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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The OH^*(3–1) layer emission altitude cannot be determined unambiguously from temperature comparison with lidars" *by* Tim Dunker

Anonymous Referee #1

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This paper deals with the possibilities and limitations of co-analyzing OH Meinel nightglow and sodium resonance lidar in terms of mesospheric temperatures. Ground-based OH Meinel spectroscopy has long been an important tool for monitoring temperatures in the upper mesosphere. Question about the height, width and variability of the OH emission layer are decisive for the interpretation of resulting temperature time series. Direct comparison with co-located sodium resonance lidar can provide some constrains on the geometry of the emission layer. However, as the current paper demonstrates, the knowledge accessible about the detailed layer geometry remains limited. In particular, a simultaneous and independent assessment of the two parameters layer altitude and layer width is generally not possible.

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Discussion paper

This comparison study is not the first of its kind. The author correctly refers to the earlier work by von Zahn et al. (1985) and Pautet et al. (2014). Nevertheless, the data presented here provide a valuable contribution to assessing the limitations of OH nightglow temperature studies. As expressed in the title, the author suggests that the major conclusion of the paper is that the height of the OH emission layer cannot unambiguously be determined by this kind of study. I am not sure, however, that this really is the most interesting message from this work to the atmospheric community. I actually think that the most important result for the community is the quantification of the uncertainty of the OH temperature measurements, as shown e.g. in Figure 2. Therefore, I would like to suggest to the author to consider shifting the major message of the paper towards the actual temperature uncertainty.

My major concern with this paper is the discussion of the ambiguity between OH layer altitude and layer width. The interpretation of the data as shown e.g. in Figure 1 is not convincing and should be modified. In my opinion, this requires a major revision, after which the paper can be regarded as an interesting contribution to ACP.

Starting point of the study are coincident detailed temperature profiles with the Na lidar and "column" OH temperatures determined by ground-based Meinel spectroscopy. The basic analysis idea is then to infer a geometry of the OH layer based on the requirement that the Na temperature integrated over the OH layer must be consistent with the spectroscopic OH temperature. My concern is that the author intends to determine two unknowns (layer height and layer thickness) while there is only one observational constraint (the difference between OH temperature and integrated Na temperature). The problem is thus under-determined. This means that the statement given in the title is rather trivial: the under-determined problem will not allow for an unambiguous retrieval of the two independent parameters.

The author describes the mathematical problem as a "minimization": By minimizing the difference between OH temperature and integrated Na temperature, the two unknowns layer altitude and layer width should be fitted. This approach would be appropriate for an overdetermined system. For this underdetermined system, however, there is nothing to be minimized. On the contrary, for typical conditions there is an infinite number of combinations of OH layer altitude and layer thickness that result in exact zero difference between OH temperature and integrated Na temperature. This is obvious from Figure 1: with the exception of 2012-01-22, all plots feature a zero contour line that represents the pairs of layer altitude and thickness that generate exact solutions. On page 5, lines 11-12, the author refers to "more than one combination [...] that yield the smallest temperature difference". Why should one talk about smallest differences when there are obviously exact solutions that yield exact zero?

Hence, I urge the author to rethink the overall analysis concept and the mathematical formulation. As pointed out above, I would actually recommend the author to shift focus from the somewhat trivial question of "OH layer ambiguity" to the more interesting question of "OH temperature uncertainty".

Some other specific comments:

page 2, lines 27-29: Some basic description of the iterative procedure for temperature retrieval should be provided. Simply referring to the (hard to access) Ph.D. thesis by Lange (1982) makes it very difficult to follow the paper.

page 5, lines19-22: It is stated that for some nights reasonable solution cannot be found. I wonder whether this in part is a consequence of strong variations of temperature and/or OH layer geometry within a given night. This would make the use of nightly means problematic. To check this, it might be instructive to break up the analysis into several time intervals for a given night.

page 5, lines 29-30: It is stated that the temperature differences are not normally distributed, and that for this reason it is not possible to determine mean and standard deviation. Therefore, the author suggests to use half the difference between maximum and minimum temperature difference as a measure for uncertainty, which results in a rather large range of +/- 16 K. Even though the differences are not normally distributed, I would still argue that it might be more meaningful to talk about a "mean error", which would result in a significantly smaller measure of the uncertainty than +/- 16 K. On page 7, lines 17-20, the author uses a different notation, talking about a temperature proxy being "representative within +/- 16 K", thus referring to this interval more as "outer limits" than a conventional uncertainty interval (mean error). Please make sure to give the reader a clear feeling for what is meant by the "uncertainty interval" +/- 16 K.

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