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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tropopause altitude determination from temperature profiles of reduced altitude resolution" *by* Nils König et al.

Anonymous Referee #2

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General remarks:

The manuscript by König et al. presents interesting aspects for the tropopause determination from temperatures profiles of restricted vertical resolution. This topic is not only crucial for satellite based temperature sounding but also for frequent analyses and applications of meteorological data sets. The mathematical background based on Rodgers (2000) is described in detail and allows a nearly correct an elegant description of the problem. In addition, it allows an accurate description and quantification of resulting error sources. The paper is well organized and written, and the scientific and technical objectives will fit to the scope of AMT. However, I have strong concerns that the paper in the current form is adequately addressing the scientific and technical standards of AMT, I am generally missing a more in depth

analysis and larger statistics (e.g. a larger set of radiosonde stations) to draw robust and meaningful conclusions. I can only recommend the publication of the study of König et al. after some major revisions and improvements. More detailed suggestions for improvements and comments are specified in the following sections.

Major comments:

The title of the paper promises more than the analysis and the final results can deliver. The tropopause (TP) determination of reduced altitude profiles - like announced - in only analyzed for one very specific instrument (MIPAS), but is a quite general problem and especially important for many studies taken into account meteorological data sets like ERA-Interim or MERRA. Very similar problems occur for these type of data, if you like to quantify the error in the TP determination for the relatively coarse vertical sampling around the TP compared the typical fine resolution of the radiosonde data. TP heights are not part of the meteorological data sets. It would be by far more interesting to apply the methodology in a more general approach , for example to the problem outlined above. Take these criticisms into account the author should change the title accordingly.

The abstract is extremely short and includes even repetitions ('3 km vertical resolution'). The reader may ask, are there as little results? This is also true for the conclusions and unfortunately my final impression of the presented study, although there seems a high potential in the formalism.

The data base and the statistical analyses have a couple of limitations, which need improvements in a new version of the paper:

a) The number of 30 radiosonde profile for only one station (Nairobi) is far too small for significant conclusions based on the presented analyses and statistics.

b) The selection of only one station representative for the tropics seems also critical. Are there any references for this simplification? I guess continental and coastal area can have quite different temperature profile (wave activity), also regions with strong and low convection activity. If the study likes to stay with its focus on the tropics, then more stations and coincidences with MIPAS should be taken into account.

c) More stations and profiles (by taken a longer time period) would also help to bypass the very coarse coincidence criteria applied in this study. A $\pm$ 1000 km in longitudinal direction and $\pm$500 km in latitude is by far too coarse to define a proper coincidence. In addition, I am missing a miss-time criterion in the manuscript?

d) All statistic plots suffer on the general problem of the study of rather low number of profiles/coincidences. For me it makes no sense to fit Gauss distributions to histograms or to present box-whisker plots for such low ensemble numbers.

Overall, I would recommend to apply the methodology not only to the tropics, because TP determination it is a general problem at all latitudes, which would give the study a much broader scientific relevance. In addition, the authors should think about to apply the formalism to temperature profiles of meteorological analyses, which would give a much broader scientific community a tool or reference to quantify uncertainties in the tropopause determination (e.g. for tropopause related coordinates, definition of the transport barrier).

Minor comments:

The authors should reference in the introduction to other limb based remote sensing analyses in former publications or to more general publications highlighting the difficulties and importance of an accurate tropopause determination (e.g. Pan and Munchak, 2011, Peevy et al., 2012, or Spang et al., 2015).

What is the effect of the higher resolved and vertically more structured *a priori* profiles (e.g. ERA interim) on the MIPAS temperature retrievals and finally on the MIPAS TP determination. Can you quantify this effect with your methodology?

Why do the authors include Figures 6 with no additional information compared to Figure 1, what's new or has to be highlighted here? Tropopause heights (radiosonde and potentially for different degraded resolutions) should be superimposed in both Figures.

Section 4 on the feasibility of correction schemes is missing a detailed analysis and the description is too short. This section has currently not the substance for a full section in a paper, it's just a result for a paragraph. Again the number of profiles is not sufficient. I am wondering why the author made the analysis with such a limited data set of radiosonde profiles and MIPAS profiles. It will be easy - but of coarse additional work - to extend the complete study to a larger database and to draw better and more profound conclusions.

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