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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.

Nils König et al.

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Comment: *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 and 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.

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Discussion paper

Reply: We thank the reviewer for this encouraging evaluation.

Comment: *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.*

Reply: Please see our comments below.

Comment: *Major comments:*
The title of the paper promises more than the analysis and the final results can deliver.

Reply: We do not quite see the point. The title is "Tropopause altitude determination from temperature profiles of reduced altitude resolution" and this is exactly what we critically assess in the paper. As stated below, we will replace the term 'profiles' by 'measurements'.

Comment: *The tropopause (TP) determination of reduced altitude profiles - like announced - is only analyzed for one very specific instrument (MIPAS)[...]*

Reply: This is not true. We investigate into this effect for a series of idealized instruments with altitude resolutions of 1 to 5 km. This analysis is applicable to a wide range of instruments. Analyses based on averaging kernels from real instruments suffer from

the fact that the averaging kernels have to be taken as they are, and the dependence of the effect on variation of the altitude resolution thus cannot be assessed. Thus the focus of the study is on the idealized averaging kernels where the altitude resolution can be varied. MIPAS results are presented in addition as an illustrative case study.

Comment: *[...] 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.*

Reply: This study was performed with application to satellite measurements in mind. The applicability of the methodology presented is an added value but for a paper in Atmospheric Measurement Techniques we find it adequate to restrict the study to atmospheric measurements.

Comment: *Take these criticisms into account the author should change the title accordingly.*

Reply: To avoid wrong expectations we change the title to "Tropopause altitude determination from temperature profile measurements of reduced altitude resolution."

Comment: *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.

Reply: The additional analysis performed in reply to both reviews will add length to the abstract and the short sections.

Comment: *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.*

Reply: We wonder in which sense the reviewer uses the term 'significant' here. The context of sample size suggests that it is meant as a statistical technical term, while the context of 'conclusion' suggests that the term is used in a colloquial sense. In statistics only differences can be significant. In our case, the differences are considerably larger than the uncertainties of even a single profile. Thus we can say even for a single profile that the effect is significant. For any given profile, the effect is deterministic. Note that we do not recommend any inductive correction based on the mean tropopause altitude displacement. In this case significance would be defined by the standard error of the mean displacement and the displacement. But, as said above, we neither propose nor endorse such an inductive correction. Nevertheless we have increased the sample size considerably to gain a better idea on the representativeness of our results.

Comment: *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.*

Reply: We do not see how a statistic should become more robust by making the sample more inhomogeneous. Nevertheless we take this suggestion and include analyses of other sites but we do not think that it would be a good idea to merge all this into one statistic.

Comment: *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 ± 1000 km in longitudinal direction and ±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?*

Reply: This reads as if the reviewer has misunderstood our approach. Coincidence criteria would indeed be far too coarse if we had compared MIPAS profiles to radiosonde profiles but we did not do that. We only have applied MIPAS averaging kernels to the sonde profiles and compare the original sonde profiles to those where the averaging kernel has been applied. MIPAS averaging kernels are only weekly or at best moderately temperature-dependent, and thus this issue is, in this application, a higher order effect.

Comment: *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.*

Reply: The sample size will be increased. The Gaussian curves were only illustrative, that is to say, no conclusions have beed drawn from them; thus they can easily be removed. All statistics comes directly from the data without any detour over Gaussian pdfs.

Comment: *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.*

Reply: Agreed, other sites will be included.

Comment: *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).*

Reply: We are glad if the theoretical part helps also for these applications but we think that for a paper in a journal on measurement techniques it is justified to restrict the analysis to measurements.

Comment: *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).

Reply: Agreed.

Comment: *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?

Reply: This depends on the relation between the amplitudes of the fine structure and those resolved by the instrument. No general statement can be made on this.

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

Reply: With additional data available the decision how to best present the results will have to be drawn from the scratch anyway.

Comment: *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.*

Reply: More data will be included. Again, we suspect that there is a misunderstanding because no MIPAS profiles have been used, only MIPAS averaging kernels. The bulk of the study relies on idealized averaging kernels corresponding to a range of altitude resolutions.

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