Review of the revised version of the AMT mansuscript:

Tropopause altitude determination from temperature profile measurements of reduced vertical resolution

by: Nils König, Peter Braesicke, and Thomas von Clarmann

Overall the manuscript is substantially improved. The author did a good job by adding a couple of stations like suggested, which even show interesting differences to the mid-latitude tropopause, and by a more precise and detailed description of their results. In addition, the analysis of the water vapor entry value - suggested by the 2nd reviewer - is a value-added for the manuscript.
I have only minor comments, questions, and suggestions. Consequently, this novel approach and analysis of limits in the tropopause determination should be ready for publication in AMT with only little effort for the authors.

General comment:

How do double or multiple tropopause events influence the results of the analysis? If two tropopauses are very close to each other (e.g. Mehta et al., 2011), then degraded vertical resolutions and TP criteria - like applied in the manuscript - may results in quite different results for the 1st tropopause. I would like to suggest, that the authors comment on the potential effects (e.g. biases) by double tropopause events. Maybe the positive outlier in Fig. 3 for Nairobi is affected by this effect.

Specific comments:

All line and page numbers are attributed to the track-changes version of the manuscript.

Abstract: Is it not a general rule for AMT to introduce acronyms already in the abstract (here: MIPAS)?

Abstract: The term 'tropopause displacement within each sample' might be a bit misleading. Do you mean 'each profile'?

Page 2, line 20: The Spang et al. (2015) is only analyzing ERA-i temperature profiles. The reference should move to the sentence before. The paper critically discussed, how accurate the 'true' tropopause height can be retrieved from the ERA-i temperature profiles.

p2,l23: 'assess' instead of 'asses' ?

p5, l27: The new sentence is confusing me a bit. The cold point tropopause is a separate analysis and there is no mixing of both analyses types, correct?

Fig. 1: Could you please add a horizontal line for the 'true' TPH, and for each degraded profile the numbers of the TPH into the legend box.

P7, l9: 'Figures 6', is the order of the figures correct? Fig.6 mentioned before Fig.3 is slightly confusing.

Fig. 4: Are the smaller peaks of the averaging kernels related to the tropopause location (~16 km)?

If yes, then please comment on this fact and it may be helpful to add the TP height to this figure as well.

p4, l7: 'MIPAS data are sampled on a 1-km grid' sounds like a sampling for MIPAS TP measurements of 1km. I think this is not correct. The minimal sampling is 1.5 km. I guess you mean something different. Please clarify.

p14, l10 ff:
'preferred pathway of tropospheric pathways into the stratosphere': the author should give a little more details on this topic (references and some text). For example, Anderson et al. (2012) postulated a severe imprint of deep convection on stratospheric water vapor and ozone in Summer over the USA.

Fig 8.: Any explanation why Nairobi looks so different to Hilo (drift to much larger dH2O/km with coarser resolution)?

Technical comment:

p22, l12: 'appropriate' sounds better to me than 'apt'

Fig. 3,7, and 8: Please, enlarge symbols for Mean and Min/Max, which are hard to spot on printout and screen.

References:

Metha, S. K., et al.: Multiple tropopauses in the tropics: A cold point approach, J. Geophys. Res., vol. 116, D20105, 2011.

Anderson, J. G., et al.: UV Dosage Levels in Summer: Increased Risk of Ozone Loss from Convectively Injected Water Vapor, DOI: 10.1126/science.1222978, Science 337, 835, 2012.