Answer to Anonymous Referee #2

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Review of 'Trends in cloud top height from passive observations in the oxygen A-band' by L. Lelli, A. A. Kokhanovsky, V. V. Rozanov, M. Vountas and J. P. Burrows (acpd-13- 31409-2013)

General comments: The article is well written, and the datasets are interesting to explore optically thick clouds. However, I do not recommend a publication on this analysis for the following reasons:

1) The title is misleading, because the presented observations are only capable to determine cloud top height of a sub-sample of clouds: those which are optically thick (cloud optical depth > 5) which correspond probably to about slightly less than half of all clouds. Especially many high-level clouds are missed, because these are mostly semi-transparent. It would have been helpful for the reader to present this fact in the beginning, with a cloud fraction for low-level, mid-level and high-level clouds.

In fact, in the abstract (line 7) is stated upfront that the algorithm is based on the retrieval of cloud properties for "for optically thick clouds". This is because, within the asymptotic theory of radiative transfer, direct sunlight is neglected and the diffusive limit is reached at cloud optical thickness equal 5.

Moreover, the spectrometers used in this work, despite their known limitations, offer some unique features (such as long-term coverage and spectral resolution) and we think that our analysis is still a useful contribution to the task of cloud remote sensing and climate modeling, exactly like every other cloud data set available.

Indeed, as acknowledged by the referee, our data sets are interesting to explore thick clouds, not all clouds. Thus, in this spirit, we hope to provide our results to the scientific community, which will enrich and debate them.

The word 'trends' in the title is also misleading, because the authors show that within the uncertainties no linear trends can be found.

[Chandler and Scott, 2011] offer the following definition: "Trend is long-term temporal variation in the statistical properties of a process, where 'long-term' depends on the application". Among all statistical properties which describe time series, for the mean value and its temporal change the word 'trend' has been also proposed in past textbooks ([Chatfield 2003] and [Kendall and Ord 1990]). Therefore, it seems that trend can be used even if significance has not been found. Moreover, the length of this data set (17 years) exceeds the length of other data sets, whose analysis has been already termed trend analysis (e.g., [Marchand 2013]).

2) The authors have shown in an earlier paper (Fig. 4, Lelli et al. 2012) that a bias in cloud top height is still optical depth dependent for optical depth larger than 5, especially for ice clouds, when water clouds are situated beneath (which happens quite often according to CALIPSO-CloudSat analyses, especially in the tropics).

These cases are excluded from the analysis, owing to the forward model which assumes single-layer clouds. The text accompanying [Lelli et al., 2012, Fig. 4] reads (Sec. 3.2, p. 1556): "Given that our model assumes single-layered clouds, we would then reject retrievals flagged -3-, above a limit height of 5 km".

The algorithm flags -3- those retrievals exceeding the operational limit of geometrical thickness of 11 km and indicates the heterogeneity of the cloud scene (as demonstrated by [Rozanov et al., 2004]). In addition, it is clearly stated that retrievals flagged 3 (for a CTH $>$ 5 km) are discarded (Sec. 4.1, p. 1561, "Data selection" in [Lelli et al., 2012]).

For an additional quantitative proof that the CTH bias is not correlated with COT error, see the answer to the interactive comment by van Diedenhoven.

3) The data used from three different instruments have different foot print sizes, with a quite coarse spatial resolution. Especially the foot print size of GOME (320 km x 40 km) does not seem to be adequate to study low-level clouds, because these may appear at smaller horizontal extent. In this case a decrease in height might be linked to a decrease in horizontal extent of low-level clouds within the foot prints.

Despite the coarse footprint of GOME (as correctly pointed out by the referee), we are persuaded that the instrument has potential for the assessment of CTH long-term changes, given that CTH has been deseasonalized separately for each instrument. Our opinion is based on the following evidences:
(a) the good overlap at the beginning of 2003 with the anomaly time serie of SCIAMACHY, which has a considerably finer footprint;
(b) the GOME capability to detect the ENSO event in 1997-1998, just as good as the GOME-2 (which is again a finer-grained instrument) sensitivity for the ENSO event in 2011.

4) When using different instruments for trend analysis, calibration is also important as already indicated by B. van Diedenhoven in his interactive comment.

Please, see the appropriate answer to the short comment by van Diedenhoven.

Points 1-4 indicate that it will be very difficult to use these datasets for a linear trend analysis (and why should cloud trends be linear?) in cloud height. Indeed, the authors show that within the uncertainties no linear trends can be found. The maps with trends and their significance (Figs 11-13) are difficult to believe, considering the possible optical depth, vertical structure and foot print size dependent biases listed in points 1-3.

Clearly, we disagree with the conclusions given by the referee. While is not completely true that no trends have been found, because physically consistent patterns can be detected on a regional scale, we think that our retrievals are accurate enough with respect to: optical depth dependency (pt. 2 of this answer; answer to interactive comment by van Diedenhoven); calibration errors (answer to interactive comment by van Diedenhoven); and spatial resolution issues (analysis of anomalies).

In addition, we would like to provide more insight on the suitability of a linear model for the assessment of trends in CTH, despite the fact that it might be out of scope for this work.
We note that the condition of normality is reasonably satisfied even in the region of highest autocorrelation (i.e. Central-East Pacific, Niño 3.4 index region), as can be seen in the left plot of Fig. (1), where an analytical gaussian PDF (red curve) has been fitted to the trend distribution. The right plot of Fig. (1) displays the theoretical residual quantiles (estimated after regression with the linear model and application of the parameter estimates $\hat{\alpha}$ and $\hat{\beta}$ of Eq. 3 of the manuscript) against the sample distribution of global CTH anomalies. Since the majority of points cluster about the straight line, the linear model seems to be a reasonable assumption, given that the autocorrelation functions of Fig. 4 of the manuscript drops almost to 0 after one month.
Therefore, the major process that still can influence the time serie is the quasi-stationary ENSO. [Laken et al., 2012] indirectly came to similar conclusions, while analysing the effect of solar activity on cloud altitudes with MODIS Terra and Aqua measurements (on the decadal time scale). [Norris 2005] comes to the same conclusion as well. The ENSO periodicity is still matter of on-going research (as an example, see [Solomon and Newman 2012]) and it is not the focus of the present paper.

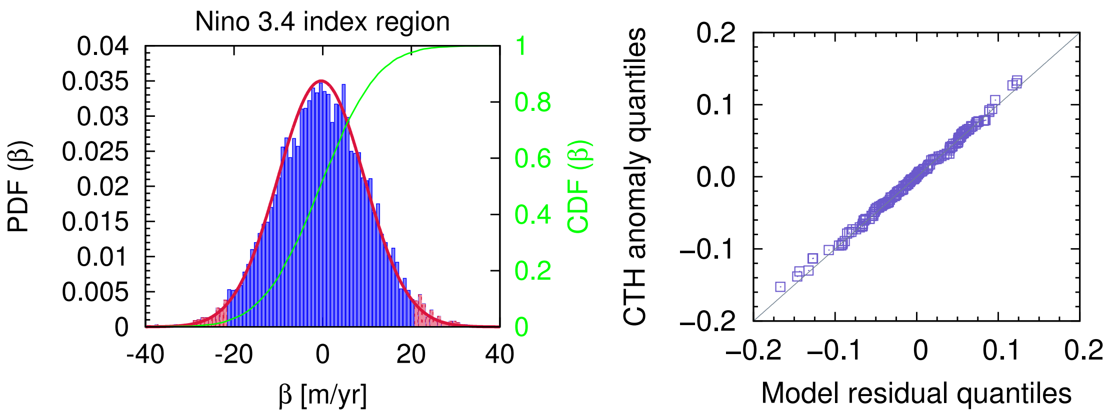

Figure 1: (Left) Trend PDF for the Central-East Pacific region, where residual autocorrelation (≈ 0.12-0.13) has been found. (Right) Normal Q-Q plot for global CTH anomalies.

In summary, the following changes are introduced in the manuscript: **(1)** update the paper title: "Linear trends in cloud top height from passive observations in the oxygen A-band"; **(2)** add a sentence in Sect. 2 ("Data and methods") about the insensitivity of the algorithm to radiometric calibration; **(3)** insert the left plot of Fig. 1 in Fig. 5, p. 31436 of the manuscript and add the reasoning about the linearity of the trend model (together with references) in the conclusions.

References

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