Answer to Anonymous Referee #1

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This is a useful paper with some interesting results, including the relation between El Nino and cloud height and the fact that the impact of El Nino variations on cloud top height has contributed to downward trend in global cloud top height in the past decade. It is also interesting to see that cloud top height has increased over North Africa and Arabia while decreasing over the central Pacific. What is least convincing is the tropical/global trend to cloud top height because I am doubtful that the time series from the various instruments can be reliably concatenated. At the very least, the differing results of this study compared with others shows that tropical/global cloud top height trends are uncertain.

We thank the referee for the overall review and we address his remarks in the following.

Specific comments:

1) p. 31410 Lines 23-25: need better explanation of how high cloud absorbing and radiating warms the atmosphere (hint: high clouds are cold).

2) p. 31411 lines 1-6: The sentence structure is awkward.

The following rewording is proposed:

"The altitude of a cloud plays a cardinal role because clouds at different heights (i.e., temperatures) exert different feedbacks. High-altitude clouds absorb infrared radiation that comes from the lower atmosphere and radiate like blackbodies. Since the temperature contrast between elevated clouds and lower atmosphere is high, the clouds have a positive contribution on the local net energy balance. At the same time, their albedo is small because they are, in most cases, comparably thin. Thus, they can warm the atmosphere more than they cool it, exerting a positive feedback.

Conversely, low-altitude clouds are strongly reflecting objects owing to their high optical density but they ineffectually shield infrared radiation emitted by atmospheric gases to the outer space. The reason is that the temperature of low clouds is closer to the temperature of the ground and the local net energy balance is close to zero. These clouds in turn can cool the climate system more than they warm it and thus exert a negative feedback.

Loeb et al., 2012 observed this mechanism in a study that focused on clouds in the tropical belt. However, this situation may change when considering the Pacific Northeast over a decadal time window. Evidence of a positive feedback by low-level clouds has already been demonstrated (Clement et al., 2009). It is therefore likely that no general description is possible on a global scale and regional studies should be conducted instead."

3) p. 31412: It would be helpful to have some more detail on how cloud fraction and cloud reflectance are determined since there can be trade-offs between the two in Eq. 1. E.g., measured scene reflectance can result from either/both greater cloud fraction and greater cloud reflectance, but they may have different impacts on retrieved cloud top height.

The cloud fraction is determined analyzing PMD radiances. PMDs offer a better spatial resolution within the instrument footprint and a broader spectral coverage (from UV to NIR). Once the fractional cloud cover is determined, its value is used to scale the actual measurement within the Independent Pixel Approximation. This means that the cloud reflectance ingested in SACURA corresponds always to cloud fraction 1.

It has been demonstrated ([Kokhanovsky et al., 2007]) that the cloud reflection function it is not affected very much by horizontal photon transport, as long as cloud fraction is known from an independent source. This statement is true regardless of cloud fraction and of instrumental spatial resolution, because the algorithm makes use of spectra ratios, i.e. $(R_{758} - R_\lambda)/R_{758}$.

This effect has been demonstrated by [Lelli et al., 2012, Fig. 9, p. 1559]. The absent correlation between CF bias (defined as the difference between OCRA CF and co-located CF derived from a finer resolved instrument ATSR-2 within a GOME ground pixel) and CTH bias indirectly validates Eq. 1 of the present manuscript.

From a physical point of view, it is also clear that, on such coarse footprint scales, the TOA cloud reflectance is dominated by the contribution of photons scattered directly back to the platform. This is not true for instruments such as MODIS or (A)ATSR, for which a cloud volume may have side lengths comparable with the footprint size.

4) p. 31414 lines 5-6: Considering that getting the right correction and harmonization are critical factors for the time series analysis, more detail should be provided on how this is accomplished.

We emphasize that the time series of absolute values have been neither corrected for the impact of spatial resolution nor fitted directly. This is because the three instruments have different sensing local times. The chosen strategy has been to regress time series of anomalies, instead. This approach is analogous to the customary technique described in [Mieruch et al., 2008, Eq. 1, p. 495, and references therein]. Eq. 1 is reported below for convenience.

Being Y_t the monthly mean of the variable of interest (in this context CTH) at time t (for each geolocation point on the map), $\mu\, C_t$ the offsets of the regression line, ω the desired change rate of CTH at time step X_t and N_t the noise, in the r.h.s. of the following equation

$$Y_t = \mu\, C_t + S_t + \omega\, X_t + \delta\, U_t + N_t \tag{1}$$

the term $\delta\, U_t$ describes the level shift δ in CTH, allowed when concatenating time series of GOME and SCIAMACHY at time T_0, with a step function U_t defined as

$$U_t = \begin{cases} 0, & t < T_0 \\ 1, & t \geqslant T_0 \end{cases} \tag{2}$$

Likewise, the removal of the sample CTH mean for each respective month from the time serie of absolute values allows the seasonality (the S_t in the r.h.s. of Eq. 1) to be accounted for. The term $\delta\, U_t$ is incorporated by performing this step separately for each instrument. This is because the sample mean of anomalies is centered, by definition, about zero [Wilks 2011, Sect. 3.4.2, 4.4.2] and the constant μ can be neglected (being μ the mean water vapour column of the time series at time t=0 [Mieruch et al., 2008]). Eventually, Eq. 1 reduces to Eq. 3 of the manuscript and potential autocorrelative effects are embedded in the noise term N_t (i.e., ϵ_t in our paper).

5) p. 31414 lines 15-16: I would have greater confidence in the harmonization of GOME and SCIAMACHY time series if there were more overlap between the two and they agreed well.

There is more overlap between SCIAMACHY and GOME-2, but they do not agree well, which does not give me confidence that the authors can reliably detect changes in cloud top height. Joining at June 2008 seems arbitrary, and the authors do not offer any justification for that point.

First, GOME time serie is limited to May 2003 due to a failure of the on-board tape recorder and global coverage wasn't provided from June 2003 onward. SCIAMACHY didn't reach final flight conditions before January 2003. So, only five months could be matched between GOME and SCIAMACHY.

Second, June 2008 has been chosen because it is not only the time when GOME-2 and SCIAMACHY time series converged, but also because when the PMD band definitions have been upgraded. Given that SCIAMACHY is a well calibrated instrument, we used its monthly means prior 06/2008. For the rest of the time serie, both instruments sense the dip at 2011. This feature dominates both order of magnitude and sign of the trend (the main focus of the paper), which would be preserved with SCIAMACHY monthly means anyway.

For the future, we are confident that a new reprocessing (together with a longer time coverage provided by adding MetOp-B retrievals to the time serie) will improve the significance of this kind of analysis.

6) Fig. 7: It seems like there might be too much smoothing and some shorter time scale information is lost.
7) p. 31417 lines 2-5: It's not clear what this sentence means.

First, the plot has been redone applying a running mean filter of 6 months (Fig. 1) and oscillations on shorter time scale have emerged.

Second, the sentence means that the 2-month constant lag between the column-averaged CF anomaly time serie (top plot, purple curve) and the SST anomaly can be explained by the constant lag between low-level and high-level clouds. This effect is even more evident about the extrema of the respective curves in the bottom plot of Fig. 1. We will replace the figure and reword the sentence accordingly.

8) To what extent does low/mid cloud fraction increase when high cloud fraction decreases because low/mid clouds are no longer overlapped by higher clouds? Perhaps there is no real increase in low/mid cloud fraction, but instead the clouds can be seen due to a reduction of high cloud obscuration?

This mechanism is surely true and can't be excluded, especially with such coarse footprints. Being aware of this, in the conclusions (ll. 16-18 p. 31421) we worded as follows: "In particular, it has been seen that the instrumental spatial resolution impacts the calculation of mean values of apparent cloud top height at a monthly sampling", where the word "apparent" implies the inherent limitations of the instruments.

9) Fig. 8: Is this for the same region used in Figs. 6-7?

Yes. The information has been now added in the caption.

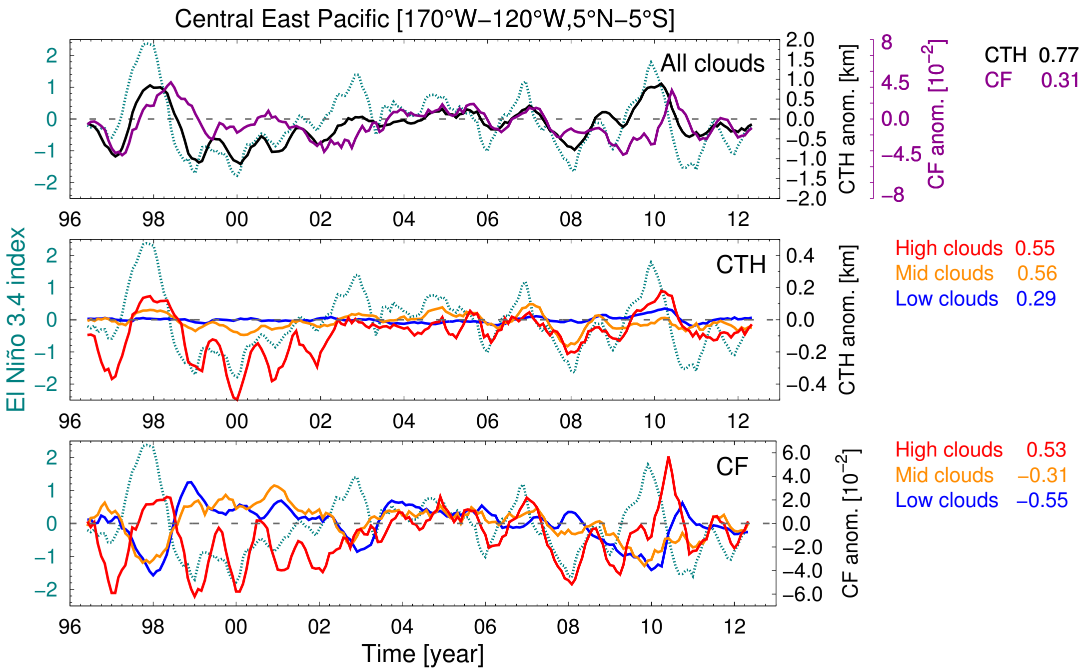

Figure 1: As in Fig. 7, p. 31438 of the manuscript, but with a running mean filter of 6 months. Time series of CTH (black curve) and CF (purple) anomaly and subsets of HC, MC and LC. All time series with their respective correlation with El Niño 3.4 index.

10) Fig. 8: I can understand how cloud fraction in a particular vertical interval can change and therefore have a correlation with Nino 3.4, but how can cloud top height in a particular vertical interval change? Isn't the vertical interval fixed? Or is average height within that interval changing?

The vertical interval is fixed. What is changing within the vertical interval is the count number of CTH retrievals per height bin k (i.e., c_k at numerator of Eq. 2 in the manuscript). This can be understood as clouds being reclassified in adjacent height bins.

11) Fig. 13: Probably what matters for high clouds is not columnar water vapor, which will be dominated by the boundary layer, but upper tropospheric humidity. One reason larger trends may be seen over North Africa and Arabia is that there are few low-level clouds in those regions due to desert conditions. Thus any increase in high-level clouds will be less diluted by the presence of low-level clouds.
12) I don't see any convincing evidence that an indirect aerosol effect is present. This is speculation on the part of the authors.

Our purpose is to provide some sketches on the possible mechanism that links aerosol production to cloud top height, mediated by water vapor. This is achieved mainly calling on papers in the literature and with some preliminary investigation. This complicated topic deserves more in-depth analysis and we would postpone it for a later publication.

As correctly pointed out by the referee, it is not about an indirect aerosol effect (which has

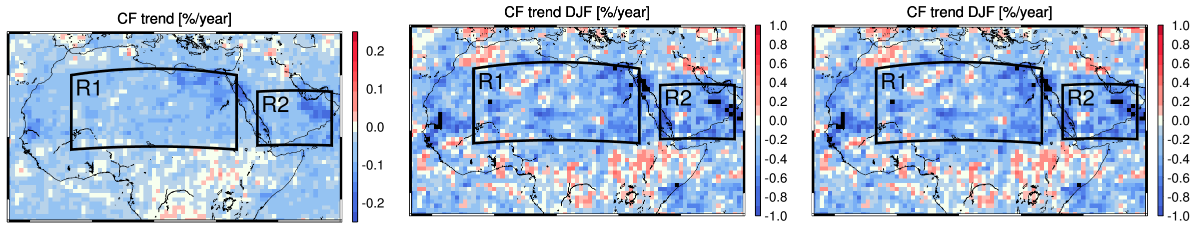

Figure 2: Trend [$\% \cdot \text{year}^{-1}$] map over North Africa (R1) and Arabian Peninsula (R2) for annual cloud fraction (left plot), summer (mid) and winter (right) months. Average cloud fraction over R1 and R2 ≈ 0.4.

a precise meaning), but a semi-direct aerosol effect (as defined in the IPCC AR4, 2007) instead. Beside the study cited in the manuscript, arguments supporting a decrease in surface insulation of the Indian Ocean have been reviewed, among others, by [Turner 2012].

In Fig. 2, the trend in cloud fraction anomalies is provided together with breakups in summer (JJA) and winter (DJF) months. Fig. 2 suggests a constant decreasing tendency of cloud fraction, which is consistent with values of CF change derived by other instruments ([Stubenrauch et al. 2013, Fig. 3.11, p. 141]) for the same region. Looking at the breakups for JJA and DJF, cloud fraction is not correlated with the seasonal Indian monsoon because its trends over R1 and R2 are commensurate and have equal sign, which wouldn't be otherwise. This finding complies with [Norris, 2001]. In addition, there are indications that high values of water vapor volume mixing ratio (and not number density) are found over desert areas, where moist air is advected upward in presence of biomass burning particles ([Kim et al., 2009]).

The question whether a change in cloud parameters (fraction, optical thickness and top height) translates into a change in columnar water vapor (due to the assumption in the algorithm of ghost column under the cloud) has been recently addressed by [du Piesanie et al., 2013]. They show that water vapor columns derived from SCIAMACHY are influenced neither by changes in cloud fraction nor in cloud optical thickness ([du Piesanie et al., 2013, Fig.4, p.2930]). The dependence of H_2O total column on changes in cloud top height has been explored for cases with CF $\geqslant 0.9$ ([du Piesanie et al., 2013, Fig.5, p.2930]). Even for these very cloudy scenes, a change in CTH of $\approx$450 m over 17 years can't explain the decrease of $\approx$ -0.85 g/cm^{-2} above Arabia for the same time span. This argument rules out algorithmic artifacts due to clouds and points to a real process, that presumably takes place over the northern Indian Ocean.

In summary, the following changes are introduced in the manuscript:
(1) The mechanism of cloud feedbacks is rephrased in the introduction.
(2) In Sect. "Data and Methods" details are given about the determination of cloud fraction and the accuracy of Eq. 1.
(3) In Sect. "Trend Model" the reduction of Eq. 1 in [Mieruch et al., 2008] to Eq. 3 of the manuscript is made explicit.
(4) In Sect. "Data and Methods", arguments for the merging of the time series will be given.
(5) Fig. 7 of the manuscript is replaced by Fig. 1.
(6) Fig. 2 will be added to Fig. 13 of the paper. The discussion about the semi-direct aerosol effect will be expanded together with references. In the conclusions, the claim about the influence of aerosols and water vapor on CTH trend will be relaxed.

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