

Response to Anonymous reviewer 1

We thank both reviewers for their in-depth reviews and their thoughtful comments. In response to the reviewers' main criticism we have added another section and figure dealing with uncertainty estimates and outlining our argument for why we think the standard deviation is the better uncertainty estimate. Below please find our point-by-point response to the reviewer's concerns.

Major comments

1. The authors introduce the notion of the idealized stratiform boundary layer cloud model. But they leave it unclear what the difference is to the previous adiabatic assumption. Do they include a profile of sub-adiabaticity as Boers et al. (2006)? Is CDNC in this model not vertically uniform and/or the liquid water content not linearly increasing with height? What is the assumption on sub-adiabaticity?

We have clarified this more now in Section 2.2. We use a linear profile, with condensation rate calculated from cloud top temperature at 80 % of its adiabatic value. Please note, that we introduce the terminology ISBLC in order to avoid the confusion we were facing over the last ten years when using the term 'adiabatic cloud model'. We are not defining a new cloud model. Rather, we are trying to avoid ambiguities that are embedded in the word 'adiabatic'. Our experience was that half of the community accepted the term and the other half very strongly rejected it because the model is actually sub-adiabatic. This led to long and in our view possibly pointless discussions about terminology, which we hope to avoid.

2. The uncertainty analysis is highly superficial. Fundamentally, the authors just write that in a single case (VOCALS, 20 flights) the "error" as propagated from the MODIS-retrieved reff and tau uncertainty assessed by Platnick et al. (2015) is similar in magnitude as the spatial variability at the scale considered for these cases (up to 51x51 km²). As such, it is highly astonishing that the authors take the spatial variability at face value as the uncertainty for any other cloud regime as well. The result of course is foreseeable: the variability is small for stratiform, and large for broken clouds. Although it is not unlikely that the actual error behaves like this, it cannot be concluded from the analysis by Bennartz and Rausch. I suggest the authors either perform a rigorous uncertainty analysis, or else abstain from calling the variability "uncertainty", but actually call it, e.g., "sub-scale variability".

We agree with the reviewer's comment about the lack of detail on uncertainty analysis in the first version of the manuscript. We have added more detail and an additional figure on uncertainty analysis now.

Title: clarify this is only over ice-free oceans (maybe in the abstract is also sufficient)

We added this to the abstract.

p215: Studies preceding the Bennartz (2007) satellite climatology should be acknowledged, such as Han et al. Geophys. Res. Lett. 1998 (AVHRR) and Quaas et al. Atmos. Chem. Phys. 2006 (MODIS).

Fixed.

P3 I14: what is “maximum” adiabatic value here?

The maximum condensation rate of an airparcel undergoing saturated adiabatic ascent, which is a weak function of temperature and pressure. We have clarified this in the text.

P3 I16: probably something like “...a growing body of work has been devoted to understanding...”?

Fixed

P3 I20: what is the distinction of CDNC at cloud base and the one “observed”? This should be clarified, since for the assumption of vertically constant CDNC, there seems to be no difference.

We believe this difference adequately captured by the following sentence, highlighted in italics in the following excerpt (Page 3 Line 18-21, original manuscript): “Furthermore, the cloud microphysical interpretation of retrieved CDNC is also not straight forward, as ultimately one would be interested in the number of cloud droplets activated at cloud base and not the number of cloud droplets observed. *Entrainment mixing processes, precipitation formation, and additional activation above cloud base can lead to differences between these two properties.*”

P4 I6: should read km^2

Fixed

p4 I8: specify what type of observations

Fixed.

p4 I18: \cite[e.g.,][Brenquier} and later cite{Bennartz}

Fixed

p5 I3: it would be good to give the reader some insight about the aircraft-observed range in k . E.g. Freud and Rosenfeld (J. Geophys. Res. 2012) find $k = 0.93$, while values from Martin et al. 1994 and Pawlowska and Brenguier (Tellus 2003) go down to 0.67. This introduces an uncertainty of about 20%.

We have mentioned this uncertainty now (and 20% is also what we have been using in our error propagation).

p5 I16: “for the 13 years”

FIXED

p6 I1: better clarify: “liquid water cloud”

FIXED

p6 I28: some more detail is required how this series of reflectances was constructed. Which thermodynamic profiles, cloud-base heights and cloud-base updraft speeds / CCN concentrations were sampled?

We have added detail to this discussion. However, since we did not start with CCN concentration, updraft speeds and CCN concentration were not considered.

P7 I21: “horizontal and vertical” ?

FIXED

P7 126: black cross? And CDNC = 500 cm-3 / H = 175 m?

FIXED

P8 15: It would be useful if the authors discussed how often the MODIS algorithm fails to diagnose partly cloudy pixels, i.e. how important this problem remains after condition (4) on p6 is enforced.

This issue is addressed in Section 4 in much detail both in terms of its impact on mean CDNC as well as in term of its impact on data density.

P8 112: correct color and numbers p8 113: “an optical depth”

FIXED

p8 115: the assumed single scattering albedo should be reported

The single scattering albedo at 0.55 micron (SSA) reported by Haywood is 0.89-0.91. The cont. poll. SSA is 0.892. These values of course depend on wavelengths. The full information can be looked up in the referenced publications, which we believe is sufficient for the study presented here.

p8 116: why “not shown”? It is represented in Fig. 1 as well, as far as I understand.

Good point. FIXED.

P9 16: “angle increases” or “angles increase”

FIXED

p9 116: the screening criteria should be explained (reference to Tab. 3)

FIXED

P9 120: “from”

FIXED

p9 122: “in a manner that”

FIXED

p10 12: “biases” - it is not a priori clear that these are biases, or do I miss a point?

We have formulated this more carefully now.

P10 115: citet

FIXED

p11 12: The authors should report how many datapoints are sorted out by the stratification criterion.

We added this information.

P11 17: From Fig. 4, it seems the problem of reduction in amount of data is mainly due to the sunlint angle > 35 criterion. However, compared to the scattering angle criterion, this one seems to be of minor importance. I suggest to split the two issues and investigate them separately.

While the sunlint angle criterion does remove a large amount of data, we could not detect any significant sensitivity of the results toward including or excluding this data. We have tried to make this

argument clearer now in the paper. It really is the scattering angle that determines systematic artifacts we are seeing. This would also make sense from the standpoint of the retrievals. Both, solar zenith angle and sunglint angle are partly correlated with scattering angle, so they do show some of the same effects because of this correlation.

P12 I20: This is a very brief uncertainty quantification discussion. The notion that spatial variability could fully represent the error seems implausible.

We agree the uncertainty quantification was too short. See our general comments further up.

P13 I22: the trend should have a per time unit.

FIXED

P13 I28: aerosol indirect effect? Or rather “radiative forcing due to aerosol-cloud interactions”?

We were thinking of the first indirect aerosol effect here

P14 I25: traceability

FIXED

p20, Fig. 2: It would be desirable to add smaller CDNC to the plot, since $CDNC < 20 \text{ cm}^{-3}$ are frequently retrieved.

We do not believe this will add value to the analysis presented here, since we are exemplarily showing results for one observation geometry and under highly idealized conditions. If a similar method were used in a true retrieval, we agree the lower limit should be changed.

P21, Fig. 3: The caption should explain the label “fraction of open water”: is this $1 - \text{cloud fraction}$? Also the scale is unclear, is this at the $1 \times 1 \text{ km}^2$ scale?

We have added a sentence to clarify this.

P22, Fig. 4: Are the curves averaged over the year and globe for given scattering/sunglint angle?

Correct. We have revised the caption slightly in order to make this clearer. See also additional discussion further up.

P23 Fig. 5: title top panel “Difference”

FIXED

p26 I8: this is the central result of the climatology, so it deserves more attention. I suggest to move the boxes from the top panel to the (less important) bottom panel.

FIXED

Rather than reporting the number of missing months, I suggest to report the fraction of missing days in each 1×1 grid box. For the climatology (top panel) a color should be chosen that better allows to distinguish CDNC at lower concentrations, possibly a non-linear color scale would be very helpful in this regard.

We re-plotted CDNC on a log-scale as suggested. We kept the missing months for the third panel as we feel this information is more useful to the user of the climatology, since it more directly relates to the actual content of the climatology.

p29 references: journal names should be abbreviated. Some titles are in upper cases.

FIXED