Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1130-RC1, 2017
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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Global and regional estimates of warm cloud droplet number concentration based on 13 years of AQUA-MODIS observations" *by* Ralf Bennartz and John Rausch

**Anonymous Referee #1**

Received and published: 5 February 2017

Bennartz and Rausch update and discuss a very useful satellite dataset of liquid-cloud droplet number concentration over oceans. The paper is pertinent to Atmos. Chem. Phys. and very well written. It is very useful that the authors make available their dataset.

I have two major and a couple of specific suggestions that I think the authors should address before publication in Atmos. Chem. Phys.

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 as-



sumption. 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?

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 km$^2$). 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".

Specific comments

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

p2 l 5: 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).

P3 l14: what is "maximum" adiabatic value here?

P3 l16: probably something like "...a growing body of work has been devoted to understanding..."?

P3 l20: 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.

P4 l6: should read km$ˆ2$

p4 l8: specify what type of observations

p4 l18: \citep[e.g.,][]{brenguier} and later citet{bennartz}

p5 l3: 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%.

p5 l16: "for the 13 years"

p6 l1: better clarify: "liquid water cloud"

p6 l28: 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?

P7 l21: "horizontal and vertical" ?

P7 l26: black cross? And CDNC = 500 cm-3 / H = 175 m?

P8 l5: 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.

P8 l12: correct color and numbers

p8 l13: "an optical depth"

p8 l15: the assumed single scattering albedo should be reported p8 l16: why "not shown"? It is represented in Fig. 1 as well, as far as I understand.

P9 l6: "angle increases" or "angles increase"

p9 l16: the screening criteria should be explained (reference to Tab. 3)

P9 l20: "from"



p9 l22: "in a manner that"

p10 l2: "biases" - it is not a priori clear that these are biases, or do I miss a point?

P10 l15: citet

p11 l2: The authors should report how many datapoints are sorted out by the stratification criterion.

P11 l7: From Fig. 4, it seems the problem of reduction in amount of data is mainly due to the sunglint 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.

P12 l20: This is a very brief uncertainty quantification discussion. The notion that spatial variability could fully represent the error seems implausible.

P13 l22: the trend should have a per time unit.

P13 l28: aerosol indirect effect? Or rather "radiative forcing due to aerosol-cloud interactions"?

P14 l25: traceability

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

P21, Fig. 3: The caption should explain the label "fraction of open water": is this 1 – cloud fraction? Also the scale is unclear, is this at the 1x1 km$^2$ scale?

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

P23 Fig. 5: title top panel "Difference"

p26 l8: 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.

Rather than reporting the number of missing months, I suggest to report the fraction of missing days in each 1°x1° 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.

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

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