

Interactive comment on “Investigation of the adiabatic assumption for estimating cloud micro- and macrophysical properties from satellite and ground” by D. Merk et al.

Anonymous Referee #2

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This paper describes an effort to derive cloud properties such as cloud depth and droplet concentration from several different sets of ground-based and satellite-based retrieval techniques. Its focus is on understanding the sub-adiabatic nature of the clouds under investigation, and to understand the differences between the various retrieval techniques. Although most of the used methods are reasonable the paper is written in a chaotic style, has an unclear structure and demonstrates a disappointing lack of precision in many of its statements. Therefore it makes for very hard reading.

In order to be acceptable the paper needs to be improved on many fronts:

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STRUCTURE:

The theory is spread out over several subsections: 3.1, 3.2, 3.3 and 3.4. It applies the adiabatic assumption as the rule and the sub-adiabatic state as the exception. So we get Eq (1) and (2) about the adiabatic state, and then the sub-adiabatic state as an afterthought on line 204 and beyond.

- In Eq (4), (5) and (6) it is unclear whether we are dealing with an adiabatic state or a sub-adiabatic state.

- The following lines do nothing to clarify, as they would be largely incomprehensible to most readers: According to the authors, the factors A1 and A2 are both dependent on the adiabatic factor (line 219), and then in the next line they are not (line 220). In fact, if (6) considers the adiabatic value for the cloud depth, then A2 cannot be dependent on the adiabatic factor.

- In the next line (221) it mentions that the uncertainty in A2 is discussed elsewhere, but they do not quantify it. Instead they jump to the factor k in the next line (line 222) and specify its uncertainty.

- In 3.3.1 they discuss the Remillard retrieval method but its assumptions are unclear: adiabatic? Sub adiabatic?

- In 3.3.2 there are two OE techniques, one which seems to be describing a sub adiabatic model (OE1), the other an adiabatic model (OE2), although it takes a long time to figure that out.

- Eq (9) and (10) come out of a Wood (2006) reference, but this reference is not sufficiently specified at the back of the paper in the bibliography.

- Furthermore, (9) and (10) have an implicit assumption about the cloud structure (inhomogeneous mixing, or homogeneous mixing, see the Boers 2006 paper) but the authors say nothing about it.

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This type of unstructured introduction into the theory does not help the reader understand the overall content. It would be much preferable to redo the theory entirely as a separate section (possibly before the data) and start with a set of general equations (such as a general version of (9) and (10), plus the sub adiabatic version of (1)) and derive all the other equations from it.

Next, discuss the adiabatic structure as the exception to the general sub adiabatic state. In that way it becomes clear that the power laws (4), (5) and (6) are transparent evolutions from these basic equations.

Next the data: the list you have is : cloud base [ceilometer], cloud top[radar], N [OE or Remillard], LWP from microwave data, and (τ , r_{eff}) from satellite. It then would become clear that there is only a single method to derive the adiabatic factor, namely through equation (8) by using the radar and lidar to get cloud dimension and using LWP from the microwave radiometer. This is the key.

Next a discussion of parameters you want to compare:

- a) N_{OE} with $N_{\text{Remillard}}$,
- b) f_{ad} with f_{OE} [the latter you should be able to derive from OE1 is it not?]
- c) N and h [ground-based and sat-based]

And so on.

THE USE OF OE_2

OE_2 is introduced on page 10 in a very unclear fashion. It is in fact almost incomprehensible to me. I gather between the lines that it is an linear adiabatic version of OE1. So it begs the question why one wants to use it, if the assumption on which it is based, namely the adiabatic state, is manifest incorrect. In my opinion OE2 should not be used, so that the section that deals with the intercomparison between OE1 and OE2 can be cleared out almost entirely (in section 4.1.2, and figure 5, which is only

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partly explained anyway).

IMPRECISION OF STATEMENTS

There are many examples of this. When writing a paper, it is important to be precise about what you say. I just give a few examples, leaving the coauthors, some of whom are experienced hands, to clean out the rest.

a) Line 13: The best match between satellite and ground perspectives. : No idea what this means; possibly: When satellite-based and ground-based retrievals are compared the best agreement was found for one of the homogeneous cloud cases, namely a 15% . . . in cloud geometric depth and a 27% . . . in cloud droplet concentration.

b) Line 16: The estimation of is especially sensitive to radar reflectivity for . . . and to effective radius. . . . for the satellite retrieval. This should be: The estimation of is especially sensitive to variations in radar reflectivity for . . . and to variations in effective radius. . . . for the satellite retrieval.

c) Line 360:points to thicker clouds in general. No idea what this means.

d) Line 366 – 369: These lines form an unclear introduction to the next set of lines because line 370 starts with the adiabatic factor, not with H or with a vertical velocity.

e) Lines 453 – 455. . . .the largest differences in adiabatic cloud depth .. show up as differences in QL. . . as both differences are linearly linked: Cloud depth differences show up as differences in QL, that is apples and oranges for me. In fact, read 453 – 465 out aloud and you will appreciate that this is an incomprehensible set of statements. 'Former' and 'latter' are used incorrectly too.

f) Line 483: never start an complete new section with the word 'Also'. Also is used when you have already discussed something else.

g) Again, lines 503 – 513: A complete chaos: 'real cloud do not follow this relationship'. What 'relationship'? What are 'real' clouds? What are 'pure' adiabatic clouds? Do

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you have impure adiabatic clouds? Line 210: 'The smallest mean absolute difference of effective radius of all channels'? What is that? Line 513: Intercomparison ...only results in ...differences with 0.68 m and 0.51.. Differences with what?

h) Line 531: Why would you want to multiply N seviri by an adiabatic factor? No theoretical background is provided. [This should follow out of a complete revamp of the theory, though.]

i) Line 542: 'A blending of received signals': no idea what you mean.

j) Line 545: Destroys the reliability? What is that?

k) Line 561: both perspectives. What do you mean?

l) Line 571: Virtual adiabatic one? Besides a pure adiabatic 'one', we now have a 'virtual' adiabatic one? What does that mean?

m) Line 588: Ground retrieved 'one'. What?

And on it goes. In conjunction with the co-authors, the principal author should carefully evaluate each and every sentence they write down and screen on its significance, style and coherence and logical placement in the whole text. This was clearly not done in preparation of this manuscript.

OTHER:

a) Unless I missed it, it seems that Cahalan's work on homogeneity is introduced in the table 1 only, not in the text. Furthermore, you have homogeneous / inhomogeneous clouds, and the homogeneous mixing and the inhomogeneous mixing assumption. These terms are mixed throughout the paper and it is not always clear what is meant by what.

b) In the print-out that I made, Table 1 and table 2 appear in the text, rather than at the end of it.

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c) Acronyms are not always introduced: SEVIRI, MODIS, MIRA, HATPRO. They are mixed with acronyms that are introduced: LACROS, DFOV etc etc.

d) Equation (13) this is not an equation when you use the sign ':'

e) The colors in the figures are insufficiently separated. Green en blue hues, then something yellow or reddish. The result is that one needs a microscope to see the differences

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