Atmos. Meas. Tech. Discuss., 8, C596–C601, 2015
www.atmos-meas-tech-discuss.net/8/C596/2015/

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Exploiting the sensitivity of two satellite cloud height retrievals to cloud vertical distribution" *by* C. K. Carbajal Henken et al.

Anonymous Referee #3

Received and published: 8 April 2015

General comments :

This paper describes the possible synergetic exploitation of two independent estimates of cloud heights in order to get enhanced information about the vertical structure of clouds. The scientific questions related to these inferences are important and a topical subject. The retrieval of cloud heights is important for climatic and meteorological applications, as well as the use of satellite to reach this goal. Questioning the sensitivity and significance of cloud top height estimates and comparing different retrievals is interesting and relevant. The comparison between estimates could indeed lead to a gain of information, and the one this study is targeting is the cloud vertical extent, which is

arguably a very interesting cloud property to retrieve. The manuscript presents new results and is in the scientific scope of AMT. It consists in a sensitivity study followed by a comparison and a new exploitation of data, applied to a case study, which is interesting. However, while the results presented seem valuable and interesting, their presentation would have benefit from more consistency between the theoretical approach (the sensitivity study, which, in addition, lacks some clarity) and the exploitation of data that uses a new algorithm. Indeed, a previous approach is used and pursued in Section 2 (comparison between HOM and CPR profile assumption), while Section 3 to 6 shows a new exploitation of data coming from a new "out of the blue" algorithm (extension of FAME-C). This inconsistency and lack of explanation gives a sense of some confusion and incompleteness of the study, all the more that what is the done in the sensitivity study in not exactly clear. I would thus recommend major revisions and efforts to make this study more complete and clear.

Specific comments

Introduction : Page6, second paragraph and line 9-10 : "In order to maximize the impact of the desired parameter, which is the cloud vertical extent (CVE), on the signal, which is here the difference between the cloud height retrievals, . . . " Thus the cloud vertical extent, CVE, is a desired parameter ? What does justify it ? It has not been clear so far in the introduction, and even later in the paper, that CVE is a highly desired parameter. Later it is said on lines 11 to 14 "For this purpose, the FAME-C algorithm was extended to also retrieve the cloud height assuming a single-layer cloud with a geometrical thickness of 20hPa, which can be considered to be close to a solid reflector for optically thick clouds." It is equally not clear in this paper why and how one could benefit from such modeling of the cloud vertical profile to get information about the cloud vertical extent ? I suggest the authors to indicate on page 5, around line 4, that "the enhancement of photon path length" is mainly related to the CVE, and a reference to Ferlay et al (2010) should be given. That is why CVE is a desired parameter. Moreover, Ferlay et al (2010) exploit the same assumption (solid reflector) that is proposed later in this study to get information about the cloud vertical extent. That would help to understand and justify the use of this assumption further in this study. This paragraph, which aim is to present the approach of this study, is finally clear with only one point : that the difference between retrievals should carry information, and that this study will follow this strategy (it is actually done in several algorithms for the detection of multilayer situation for example). It should be said here more clearly that this study will pursue a previous analysis about different sensitivities to a vertical profile (CPR vs HOM) of a cloud layer (with a given CVE ?), and, exploit a new approach (perfect reflector) because the use of it could provide information about the CVE, as it was shown in previous studies (Ferlay et al 2010, Desmons et al 2013).

Section 2 :

The current study and the current section could have helped to answer the following question : is the accuracy of the cloud top pressure retrievals more sensitive to the cloud vertical profile within a given CVE, or to the CVE with a given vertical profile ? It is not clear, in this section, if the author try to address this question or not, for the two retrievals that are AATSR based and MERIS based. It is said that (page7, line 26) two types of CVE profiles are assumed in the simulations. That (page 8, line7), as an additional LUT dimension, each cloud is modeled with varying vertical extents. But it is not clear if, in the result given in Section2, the CVE is a degree of freedom or not. Said differently : HOM-profile and CPR-profile simulations are performed. Are there differences minimized for a CTP which is thus obtained for a fixed CVE, or is the CVE itself a parameter that varies ? After having read several times section 2 and Figures 2 and 3, it is still not clear to me. It is said on page 8 line 25, that "Alternatively, the CGT of the HOM-cloud can be increased." This sentence adds some confusion, and suggests that CGTis not a variable parameter. (a remark : for clarity, the use of CGT can be kept only for monolayer clouds, and CVE for general cloud situations (included multilayer cases)) The comments of Figure 2 (given on page 8, lines 15 to 29 and page 9, lines 1 to 5) make physically sense, but the explanation about the curves in

Figure 2 can not be understood without more clarity. This lack in clarity makes it also confusing the interest of Figures 3 and 4, as the sensitivity that is given there "was computed by simply applying a linear fit to each line" (page 9 line 8) of Figure 2. My understanding is that Figure 2 gives, with the HOM assumption, a CTP that minimizes the satellite signals simulated with two assumptions about the vertical profile (CPR and HOM profile), but with the same cloud geometrical thickness in the two simulations. Is it the case ? It should be clearly written. Concerning Figure 3 : it gives the sensitivity of the effective MERIS and AATSR HOM-CTP to an increase of CGT of 50 hPa. But how can this sensitivity be understood and used ? This is an "effective" HOM-CTP, "effective" in the sense that it minimizes the difference with the signal simulated with CPR-CTP profile assumption. But isn't CPR-CTP calculation also sensitive to CGT ? So what is the sense of this sensitivity ? I observe that in the rest of study and the data comparison, HOM-CTP estimates are evaluated, but not the "effective" HOM-CTP estimates. And there is no exploitation of the sensitivity of effective HOM-CTP to CGT : an interesting correlation between measured CGT and satellite signals is shown, but it uses AATSR CTH and HOM CTH; this latter comes from a new assumption of solid reflector (SR) (CGT = 20 hPa) for the cloud, having nothing to do with the previously analyzed HOM-CTP assumption. One could wonder why the difference (AATSR CTH - HOM "SR" CTH), which is exploited in Sections 4 and 5 and in Figure 5 is not part of this theoretical sensitivity study : it could have been very interesting to find this correlation in data that come from simulations on one hand, and from measurements on the other hand. And to compare them. More over, there is nothing said about the possible effect of the angular conditions that are chosen in this sensitivity study, conditions that are certainly variable for each satellite measurements compared with the ground based active measurements. Thus, conclusion of this Section (page10, lines 3 to 5) appears poor to me. It is written : "can expect cloud height retrievals from MERIS to be more affected... " With which assumption (HOM, CPR ?) is it demonstrated here ? A link between Section 2 and the rest of the paper should be made.

- On page 7 line 26 to 29 and page 8 line 1 and 2 : It may be useful and convenient for the reader to see here again the CPR profile. It should be clear for the reader that the effort to obtain the CPR profile is not part of this study, but comes from Henken et al (2013).

- Page 9, line 22 to 25 : a reference could be given.

Section 3.1 :

Page 10 line 15 and 16 : a reference should be given (how are the two BT used ?) Page 11, line 1 and 2 : the use of the new assumption (thin cloud layer of 20 hPa thickness) should be better introduced (see my previous comment about the paragraph in the introduction). Why one of the ambitions of this section about data comparison is now to investigate the possibility to retrieve the CVE ?

Section 3.2 :

Just a question to the authors : is it possible to obtain some average cloud profiles from the ground based measurements that are exploited here, and is it possible to compare them with the CPR vertical profile, used as assumption to get AATSR CPR CTH and MERIS CPR CTH ? The consistency or inconsistency between the profiles could be interesting and open discussion.

Section 4 :

On page 4 , line 23 " how is the "cost" defined ?

Section 5 :

Last paragraph of this section, on line 16 of page 14 : the sentence "Indeed, AATSR-CTH shows a negative bias" should be rephrased. The choice of "Indeed" is incorrect, and a reference should be again to the table.

Section 6 :

It is interesting to apply the estimate of CVE on one case study. However, as no

pixel-based comparison is possible, the comparison is difficult. One can read "The estimated CVE along the black line can be qualitatively compared to observations from CPR." on line 11 and 12 of page 15 : it seems that the choice of this black line should motivated. What happens for the data that correspond to an other line ? As there is a 3 hour difference between CloudSat and ENVISAT overpasses, what would be valuable is the statistics of the AATSR -MERIS cloud vertical extent and the ones from CloudSat. I would encourage the authors to show for comparison histograms of these two quantities, which should not represent an important effort to produce. This section should make reference to Figure 7.

Section "Summary and outlook"

This section should be modified according to the change in the revised version of the paper. Again, the paper lacks in clarity and consistency. Some explanations about the new strategy proposed in this study in order to enhance information obtained from the synergy MERIS and AATSR find arguments in this last section. It would have been preferable to find them mentioned above. One wonder again why the synergy between MERIS and AATSR has not been done theoretically with the help of the simulation of the satellite data. My opinion is that this conclusion is a bit too long; part of it could be moved above.

Technical corrections :

page 5, line 21 : "were" instead of "where" page 7, line 12 : "emission" instead of "essimion" page 8, line 16 : "deviaition" page 10, line 3 : "excercise" page 11, line 15 : "Measuremetn", again on line 19 page 12, line 23 : "smaller than" instead of "<" page 13, line 27 : "fittet" page 16, line 3 : "variablity"

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2623, 2015.