Reply to Referee #1

Overview
Overall the paper and its results represent a valuable contribution to the subject area demonstrating that for optically thicker clouds (>10) there is a solid relationship between geometric vertical extent and the top height retrievals made using A-band and thermal measurements. The paper is structured well but suffers from being careless or imprecise with explanations. The sensitivity part using homogeneous and CPR derived vertical profiles is hard to follow and, I at least, am still not sure whether it is needed or not – whether the investigation, results and explanation could not be based purely on the standard instrument retrieval results and the ARM measurements?

General reply
The authors would like to thank the referee very much for his/her comments, which are considered very valuable and significantly improved the manuscript.

In our opinion, section 2 is valuable to this study. To our knowledge, there is not another study showing in a quantitative manner the difference in impact of assumed vertical extinction profiles on a near-infrared cloud top height retrieval and a thermal infrared cloud top height retrieval. A radiative transfer model that can cover both wavelength ranges in the same manner was used. Several parts of the section was changed significantly to clarify the importance of section 2 and to make clearer the link to the other sections.

Replies to specific comments can be found below, also in blue text.

Science or technical issues
2625 – 14 Sentence "Accurate.." should have a reference as it is the key motivation for the research.

This part of the introduction was shortened, because another part was added, and sentence was left out.

2625 – 16 "In Jiang .." to end of paragraph: is not really focused and only hints at the need for vertical extent measurements. Could be more precise.

This part of the introduction was shortened, because another part was added, and sentence was left out.

2626 – 19 is it really 'poor spatial coverage' in the context of climate modelling?

Probably not in the context of climate modelling (taking into account the lack of information on diurnal variations), but in the context of several weather related applications. However, climate and weather applications would, in our opinion, still benefit greatly from requiring information on cloud vertical distribution from other instruments with both larger spatial and temporal coverage. Moreover, various long-term measurement datasets from space-born passive imagers already exist and/or are continued/planned for in the future, in contrast to space-born active instruments, which is relevant in studies on climate trends for example.

The sentence was extended:
"However, they have a poor spatial coverage due to the nadir-only measurements and, especially for weather related applications, would benefit from supplement observations on cloud vertical distributions. Moreover, in contrast to various space-born passive imagers, no long-term measurement datasets exist, which are relevant for many climate studies." (p. 4, l. 7)

2627 – 2 “vertical and latitudinal monthly mean of the vertically distributed volume cloud occurrence frequency” is too much to assimilate; could this be explained in simpler terms – especially the relevance to the study.

For clarity, the sentences were changed to:
“In Wu et al. (2009), vertical and latitudinal distributions of cloud height observations from various passive and active satellite instruments are compared. Here, also a discussion on the strengths and weaknesses of various passive CTH retrieval techniques, which depend on cloud conditions, is given.” (p. 4, l. 20)

The relevance to the study is to emphasize that in general different CTH retrieval techniques from passive imagers can show different strengths and weaknesses, depending on the cloud conditions. In our study, the difference in sensitivity is exploited to infer an additional parameter. We hope that this reasoning has become clearer now in the introduction as well as section 2.

2627 – 13 “enhancement” – need to add what this is due to...

For completeness, the sentence was changed to:
“Later in the 1960s, first satellite retrievals using the oxygen-A absorption band showed that the enhancement of photon path length due to multiple scattering inside the cloud, which in turn depends on cloud thickness and type, needs to be taken into account for accurate cloud top pressure retrievals (Saiedy, 1965; Saiedy, 1967).” (p. 4, l. 30)

2628 – 15 The phrase should be ‘large error’ and not a ‘large bias’, or is it really systematically biased?

This was changed to ‘large error’.

2630 – 8 No doubt not very significant but the surface IR emissivity should be specified.

A surface emissivity of 0.98 was used. It is now mentioned in the text. (p. 8, l. 10)

2631 – 3 ‘selected CGT and COT’?

This was changed to ‘a number of’ (p. 9, l. 9)

2633 – 10 I think it is necessary to describe in a couple of sentences how the CTT is derived from the AATSR measurements (even if a reference is provided elsewhere).

Two sentences were added:
“The forward model consists of three parts contribution to the top-of-atmosphere radiation: atmosphere, clouds and surface. The fast radiative transfer model RTTOV version 9.3 is used (Saunders et al., 2010; METOffice, 2014) to simulate clear-sky transmissions for the AATSR channels. Contributions from cloud layers and the surface to the TOA signal take into account the cloud and surface emissivities, respectively.” (p. 11, l. 17)

2635 – 10 Why is the ‘height of the top height of the highest layer’ used? Would seem to select the outlier. Why not the mean cloud height?

This is how the CVE was defined: the difference between the two cloud boundaries (cloud top height minus cloud base height).

2635 – 15 Some justification for the (5 min) and 9x9 averaging regions would be good.

This was found to be a good balance between the number of observations available for appropriate statistics and the fact that clouds fields may change in time and be spatially inhomogeneous, thus complicating the CTH comparison from satellite to ground-based instruments. When computing mean cloud properties from observations within too large time periods or spatial domains, they are more likely not to be representative for the cloud property of the center observation.

A sentence was added:
"The choice of the size of the pixel box for the FAME-C data and the time period of the ARSCL data is the result of pursuing a balance between the number of observations available for appropriate statistics and the mean cloud properties being representative for the center observation, taking into account that cloud properties can vary strongly in space and time." (p. 13, l. 26)

2635 - 21 Unclear description of the selection criteria. Should 'More precisely' read 'Moreover' or 'In addition'? What is the FAME-C 'cost'?

To clarify this, a sentence was added:
"Successfully retrieved cloud height products are defined as the cloud top heights of those satellite pixels for which the FAME-C cloud top height retrieval converged successfully during the minimization of a retrieval cost function J, which in turn is defined as J<20 within a maximum allowed number of iterations. For further information on technical details of the retrieval set-up it is referred to Carbajal Henken et al. (2014)." (p. 14, l. 6)

2366 – 21 Why is it so sure that the AATSR CTT is incorrect and not a MERIS CTH error?

Indeed, this is not sure, but one of possibly several reasons. From experience, the MERIS-CTH using the HOM profile usually tends to be underestimated, while for optically thin clouds AATSR-CTH can be retrieved that is too low.

To clarify that this might only be an explanation for some of the cases, the sentence was changed:
"One of the possible reasons for this is that the AATSR-CTT might be incorrect due to wrong assumptions in the forward model, which are related to estimates of the cloud emissivity and ignoring multiple scattering." (p. 15, l. 8)

2367 – 2 Variability will have a contribution also from errors in the retrievals.

This is a good point and is now mentioned in the text:
"Variability around the fitted lines present an indication of the variability of cloud vertical profiles/distributions that occur in nature. However, the variability will also have contributions from errors in the retrievals as well as incorrect matching of the observations (not observing the same cloud volume)." (p. 15, l. 15)

2367 – 11 The multi-layer case might also have a contribution from AATSR CTHs falling towards the middle of the layers?

This is a good point, especially in case of an optically thin, upper cloud layer. A sentence was added to discuss this:

"In case of an optically thin, upper cloud, the AATSR-CTH can fall towards the middle of the upper and lower cloud layers, which possibly further weakens the relationship between ΔCTH and CVE." (p. 15, l. 28)

2637 – 17 et seq. I do not understand this paragraph: neither the relevance to the previous nice demonstration of the relation of CVE to dCTH nor the fact there seems to be no supporting figures or tables. If it is needed, perhaps it can be expanded to explain its relevance and give the supporting figures etc.

A Reference to table 1 was missing, but is now included and the part was slightly rephrased. It is a second way of demonstrating the impact of assumed cloud vertical extinction profiles on the two cloud height retrievals, using the measurements. It shows the large underestimation for MERIS HOM-CTP, while AATSR CTP is less affected. (p. 16, l. 5)

2638 -8 True color? What is input to the RGB?

It is a color composite consisting of the MERIS band 2, 3 and 4. This was adjusted in the text and figure caption.

2638 - 11 What saturation occurs in AATSR? BTs below 190K?

AATSR was primarily designed for sea surface temperatures. The 12 micron channel tends to saturate at very cold temperatures, which for example occur in the presence of clouds with cold cloud tops. This is now specified in the text. (p. 16, l. 21)

2638 – 18 The sentence 'The overpass..' is out of place and 'Further..' seems irrelevant or out of place.

The former sentence is moved upward and the latter sentence was removed.

2641 – 4 'center of the cloud', not 'cloud top'.

This was corrected.

2641 – 9 'estimate' and 'qualitative' are not really compatible. I think the method shows a quantitative relationship.

Since the estimates are accompanied with large uncertainties, the interpretation of is *at least* qualitatively.

2641 – 13 .. and I think the plausibility comes more from the ARSCL data comparisons than from the poorly collocated CPR/ENVISAT.

In our opinion it is a further demonstration of the plausibility of the approach and shows a nice example of a cloudy scene and its estimated CVE. This is why a scene was chosen that includes several different cloud vertical distributions that can be interpreted fairly easily.

2641 – 14 'A limited..' – what is this referring to?

To clarify this, the sentence was extended:
“In the comparison of the FAME-C ΔCTH to CVE observations from ground-based instruments, a limited number of cases was exploited mainly due to filtering out observations of inhomogeneous cloud fields in space and time.” (p. 19, l. 11)

Pure editorial issues
2624 - 20 “similarly to” not “analogously to” 2626 TBD! 2627 > clouds observed from different viewing angles Fig 3 Same ordinate scale would be nice. 2636 – 5 X,Y,Z! 2638 Fig 7 not 6?

Corrections have been applied.

Reply to Referee #3

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.

General reply
The authors would like to thank the referee for his/her comments and are convinced that adjustments accordingly have significantly improved the manuscript.

We agree that especially Sect. 2 lacks clarity and many efforts were made to rephrase significant parts of this section and the introduction as well as to make clear the motivations and justifications of why the studies were performed and how they are linked to the other Sections.

Replies to specific comments can be found below, also in blue text.

Specific comments:
Introduction: Page 6, 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)

We have addressed more clearly the enhancement of the photon penetration depth by extending the sentence:
"Later in the 1960s, first satellite retrievals using the oxygen-A absorption band showed that the enhancement of photon path length due to multiple scattering inside the cloud, which in turn depends on cloud thickness and type, needs to be taken into account for accurate CTP retrievals (Saiedy et al., 1965; Saiedy et al. 1967). "(p. 4, l. 30)

Furthermore, we acknowledge the work of Ferlay et al. (2010) and Desmons et al. (2013) on the impact of cloud vertical extent on the oxygen-A band based cloud pressure retrievals. Indeed, we build further on this, but our approach is different due to combining it with a thermal infrared cloud height retrieval. A significant part of the introduction was changed to motivate and justify more clearly why CVE is our desired parameter.

The following sentences were added:
"The sensitivity of Oxygen-A band based cloud pressure retrievals to cloud geometrical thickness was exploited by Ferlay et al. (2010) to infer cloud geometrical thickness. They showed that for a wide range of cloud pressure retrievals from multi-angular POLDER measurements in the oxygen-A absorption band, for which multi-scattering inside the clouds is neglected, the retrieved cloud pressures are close to the pressure in the middle of a single-layer clouds. In those cases the photon penetration depth is close to one-half of the cloud geometrical thickness, which is especially true for optically thick and geometrically thin clouds, which act like solid reflectors. Building on this work, Desmons et al. (2013) showed that a first estimate of cloud vertical extent (CVE) can be inferred from the difference between retrievals of cloud top pressure and cloud middle pressure, which was found to be close to one-half of the CVE." (p. 5, l. 13)

The out-of-the-blue algorithm consists only of changing the forward model for MERIS CTP retrieval. It is Instead of using the vertically inhomogeneous extinction profiles (CPR profiles), a homogeneous single-layer cloud is assumed. The sentence was adjusted:
"For this purpose, the MERIS forward model in the FAME-C algorithm was adjusted to retrieve the cloud height assuming a single-layer cloud with a geometrical thickness of 20 hPa, which can be considered to be close to a solid reflector for optically thick clouds." (p. 7, l. 1)

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 cannot 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. Moreover, 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.

The aim of this sensitivity study is to demonstrate, in a quantitative way, the impact of the assumed vertical extinction profiles in the radiative transfer simulations, which serve as a forward model in the FAME-C algorithm, on the TOA signal (for both NIR and TIR) and thus also on retrieved CTPs. In the radiative transfer simulations, will serve as a basis for the forward model in our cloud height retrievals, the vertical extinction profiles have to be described both in their shape and vertical extent. We do and cannot know the cloud vertical profile (its shape) nor the CVE for each measurement. However, in our retrieval we have to assume something. This was the motivation for taking the more realistic CPR profiles! They have, for each of the mentioned nine cloud types, fixed shapes and vertical extent. Previous studies as well as the comparison to CTH from radar presented in Table 1 show that in this way the bias is reduced by a large amount.

In our study, only the CGT of the HOM clouds is varied. For a range of CTP, CGT and COT combinations we show in a quantitative way, the impact of the assumed CGT on the TOA signal, which will consequently also impact CTP retrievals. From the study it can be seen that this impact will be larger for MERIS than for AATSR, especially for optically thick clouds. To emphasize that we search for the CTP of the HOM-cloud that has a TOA signal close to the TOA signal of the reference CPR-cloud, it was renamed to equivalent HOM-CTP (instead of effective HOM-CTP). Fig. 2 shows the equivalent HOM-CTPs that were found for a CPR-cloud with CTP=600 hPa. Fig. 3 shows the values of the slopes, the sensitivity of equivalent HOM-CTP to CGT taking CPR-cloud as a reference, of the lines presented in Fig. 2, but for more COT and CTP combinations.

Now, for the further part of the manuscript, the motivation is to ignore correction of in-cloud scattering by assuming near solid reflector kind of cloud (HOM with CGT=20hPa) to get a cloud pressure somewhere in the middle of the cloud instead of an actual cloud TOP pressure and relate its difference to the CTH from AATSR to CVE, for actual measurements.

Concerning the impact of viewing geometry on the results, this could be investigated in a more extended study. The satellite viewing angle does not vary that much across the swath (max 22 degrees), an average solar zenith angle was chosen.

Large parts of this section are rewritten as well as extended to address and clarify the issues mentioned in the comments (see updated manuscript Section 2).

- 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).

It was considered, but because it is already published twice (Henken et al., 2013; Carbajal Henken et al., 2014) it was not shown again. A sentence was added to refer the reader to the papers that show the profiles.

"The resulting profiles and their incorporation into the FAME-C algorithm can be found in Henken et al., (2013) and Carbajal Henken et al. (2014)." (p. 8, l. 26)

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

It is not clear, for what a reference should be given..

Section 3.1:
Page 10 line 15 and 16: a reference should be given (how are the two BT used?)

A sentence was added:
"The fast radiative transfer model RTTOV version 9.3 is used (Saunders et al., 2010; METOffice) to simulate clear-sky transmissions for the AATSR channels. Contributions from cloud layers and the surface to the TOA signal take into account the cloud and surface emissivities, respectively." (p. 11, l. 19)

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?

This is now addressed in the previous sections.

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.

The mean CPR profiles depend on the CTP and COT, and were obtained by averaging profiles from 1 year of data on a global scale. The number of cloud profiles from ground-based measurements that are exploited here is very limited compared to the number of CPR profiles used to create the mean cloud vertical extinction profiles. Furthermore, layer cloud optical thickness and geometrical thickness were used to compute extinction values. The layer COT is not part of the ARSCL product. Such a study would be very interesting to perform, but is considered out of the scope of this work.

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

A sentence was added to clarify this:
"Successfully retrieved cloud height products are defined as the cloud top heights of those satellite pixels for which the FAME-C cloud top height retrieval converged successfully during the minimization of a retrieval cost function J, which in turn is defined as J $<$ 20 within a maximum allowed number of iterations. For further information on technical details of the FAME-C retrieval set-up it is referred to Carbajal Henken et al. (2014)." (p. 14, l. 7)

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.

The sentence has been changed and a reference to the table is added. (p. 16, l. 5)

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 another 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.

The choice of the black line: going through the main part as well as the outer skirts of the hurricane, thus covering several regimes of the hurricane with different cloud vertical structures, which can already be seen visually. Showing the CloudSat overpass is merely to get an impression of the CTH and CVE, but since the CloudSat samples for this overpass are very limited in numbers and results could heavily depend on where it exactly passes the hurricane system, any quantitative comparison is difficult. Furthermore, CloudSat can miss low-level water clouds, which would then result in small CVE if a high level cloud is above.

To emphasize that showing the CloudSat results was only considered for qualitative interpretation, the sentence was changed to:
"The estimated CVE along the black line, chosen to cover various cloud regimes of the hurricane with different cloud vertical distributions, can be compared to observations from CPR, but only in a qualitative sense." (p. 16, l. 26)

A reference to Fig. 6 was incorrect and changed to Fig. 7 (p. 17, l. 3)

In our opinion it is a further demonstration of the plausibility of the approach and shows a nice example of a cloudy scene and its estimated CVE. This is why a scene was chosen that includes several different cloud vertical distributions that can be interpreted fairly easily, because of well-known structures of hurricanes.

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.

Also for this section several adjustments were made to motivate and clarify our studies. Several sentences have been moved upward or were left out from this section to shorten it ((see updated manuscript Conclusions)

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”

Corrections have been applied.