Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "Synergy of stereo cloud top height and ORAC optimal estimation cloud retrieval: evaluation and application to AATSR" *by* D. Fisher et al.

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We would like to thank Referee 3 for their helpful and constructive comments. We address each specific comment in turn below.

Referee Comment 1: Intro (sections 1-4): pp 5286: need to mention resolution of ATSR here pp 5287: "detectors are low noise and well calibrated"- relative to what? Citation needed (Smith et al., 2012?)

Response: We thank the referee for their suggestions. We will include the resolution of ATSR in pp 5287-l9. We will introduce the Smith et al. (2012) reference in the

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sentence indicated by the referee.

Referee Comment 2: Instruments (section 3) pp 5290: "can be defined" change to "are inherent"

Response: We will make this change.

Referee Comment 3: pp 5290-5291: Advantages of stereo are mentioned, but not disadvantages. Need to mention geo-registration. Need to introduce later discussion of stereo smoothing relative to radiometric heights. Should consider introducing question of whether fundamental differences between stereo and radiometric height create problems for using stereo height as prior to ORAC. The assertion that multi-layer clouds do not introduce a bias for stereo is not proven. Citation, clarification, or justification needed. (At the very least, multi-layer clouds introduce sampling bias to stereo CTH.) "approach has been demonstrated to be the most effective area based stereo matcher" citation needed

Response: Thank you for the suggestions. We will provide details of the drawbacks of stereo after the advantages. We may consider addressing the fundamental differences between the heights retrieved by stereo and the radiometric approach, but this would be included in the use of stereo as a priori in section 4.

With regard to biases, we are incorrect in our assertion. Stereo retrieved heights do still show low bias (irrespective of the number of cloud layers) and do not retrieve the true cloud top, but rather the height where the COT is sufficient to provide enough cloud detail for effective matching (see Marchand et al., 2010). We will clarify this. The point we aim to make is that in the case of multi-layer clouds, stereo should retrieve cloud height with a reduced bias. Figures 4 and 6 go some way to demonstrate this (Stereo retrieved heights show relatively consistent errors between plots, whilst ORAC shows a substantial increase in error in multi-layer cloud situations). We will improve our discussion and analysis of this effect and remove the assertion that stereo is "bias free".

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We will introduce the Hirschmuller and Scharstein (2009) reference, which demonstrates that the census transform is the most effective area, based stereo matcher for scenes with radiometric distortions.

Referee Comment 4: pp 5292: use of "radius" as descriptor is not correct. I think the algorithm uses a 7x7 pixel domain, not a circular domain

Response: We will make this change.

Referee Comment 5: pp 5295: "error in the stereo matching is dominated by . . ." need rephrasing for clarity

Response: We will attempt to improve the clarity of this sentence

Referee Comment 6: pp 5295: conversion of stereo CTH to CTP does not belong as a topic here, needs to be in section 5.

Response: We will introduce a new bullet after pp 5297-l25 which discusses the conversion of the a priori stereo heights from CTH to CTP using the ECMWF reanalyses.

Referee Comment 7: Methodology (section 5.1): While the stereo technique and application within ORAC review sufficient attention, not enough detail was provided regarding the collocation of CALIOP and AATSR. More detail about the collocation sampling is warranted, including total number of collocations, a map of collocations, the maximum time difference between observations and how that affects comparison, etc. Also the study barely mentions height->pressure conversion and what role that plays in their analysis. If assumed to be negligible, that (debateable) assumption needs to be explicit. I would also move discussion of Figure 3 and Figure 9 to this section or immediately after, so as to demonstrate to the reader what individual collocated scenes look like before presenting statistics from those scenes.

Response: We plan to remove the map from Figure 3 and will replace this will replace this with a dedicated figure mapping all of the CALIOP orbits used with a color LUT applied that defines time difference from the corresponding AATSR orbit. When we

make reference to this figure we will discuss the impact of time difference on the comparison. We will also make reference to and discuss Figures 3 and 9 as appropriate in this section. On pp 5298-l20 we will discuss in more detail the specifics of the inter-comparison dataset including max sample size. On pp 5297-l25, where we will move the discussion on the conversion of CTH to CTP, we will discuss the role that height-to-pressure conversion plays in the analysis. This will include as discussion on errors and the fact that the same profiles are used when converting the ORAC CTP into CTH values (which is then used in the analysis).

Referee Comment 7: Results (section 5.2) This section is difficult to follow, mostly because Figure 1 presents 20 different results in a single, disorganised plot- and the discussion reads as a non-concise description of that plot. Those results are statistics from comparisons classed by underlying-ice-vs-water, cloud-type-ice-vs-water, clouds-layers-single-vs-multiple, cloud-heights-low-mid-high. This is 2*2*2*3=24 minus 4 combinations that have no sampling. This is too much information to present without organizational structure. I strongly recommend that this data be provided as a table (ideally with cells colored as a heatmap), so that a reader can quickly understand how the 4 different classifications affect the results. (This applies to figures 7 and 8 as well.) As an added point, I see no value in presenting these results as a box-whisker. The y-axis scale needed to accommodate the results makes meaningful comparison of the quartiles and means impossible, and also prevents meaningful organisation of the different classes of results. It also lacks information about the number of samples that could easily be added to a table (along with any other supplementary statistics.) Figure 2: This is a nice figure, and I think variations of this figure broken down into cloud-type, underlying-type, and/or single-multi-layer might also be helpful

Response: We like the suggestion for Figure 1 (also 7 and 8). We agree that a table would provide a far better summary of the data than the current box-whisker plots. We will implement this approach. We will then attempt to provide a more concise description of the plot focussing on the key results. If still deemed necessary following

the improvement to Figure 1, we will introduce more plots in to Figure 2.

Referee Comment 8: Results (section 5.3) These are key results more important than the height comparison, but have been given less detailed discussion, less figures, and no ground-truth comparison point from separate instrument. And as before, disorganised box-whisker plots are presented (Figres 7 and 8) where organised tables are needed. In the case of Figure 7, I'd also like to see a regression of the change in height versus the change in optical property between ORAC and ORAC+Stereo. In the end though, I'm not sure whether any of these results are useful without ground truth.

Response: Here we restate our response to Referee 1: we agree that the analysis of the cloud microphysics should be extended to include inter-comparisons against CALIOP. For the phase analysis we will apply the approach the referee (1) suggested in comment 10. For the COT we will provide an analysis similar to that employed for CTH Figure 2, with a joint histogram of ORAC vs. ORAC+STEREO. We will also an inter-compare ORAC and ORAC+STEREO against CALIOP COT retrievals for optically thin clouds (where CALIOP is not saturated) and display the results in a table format (as suggested by referee 3). We will produce similar figures for effective radius.

In addition we will apply regression analysis to change in height vs. change in optical property for REF and COT for both ORAC and ORAC+STEREO.

With these analyses in place (and associated discussion) this should address the remainder of the comments from the referee.

Referee Comment 9: Pp 5302-3: linear regressions are inappropriate and should be removed

Response: We will do as suggested.

Referee Comment 10: Pp 5304-15: "As expected". ??? Why expected?

Response: Yes, we agree that this statement is invalid and that, as mentioned above, we need to provide far more detailed analysis in this section.

Referee Comment 10: Pp 5305: from line 5 on, there are a number of speculative statements that are not corroborated by the data. No citations given. In particular, "This is to be expected and indicates more clearly that in many of these cases the cloud is multi-layered". This assertion (and following logic) is poorly corroborated. (Authors might have corroborated this point with their analysis, but ,if so, it was not explained with sufficient clarity.)

Response: Again, with the further microphysical analysis we plan to undertake we hope to better corroborate the assertions made.

Referee Comment 11: Discussion and Conclusions (section 6-7): These are nice sections, and I suspect would be even more compelling if the the Results section was cleaned up (and is some of the content in the results was moved to discussion.) However, I believe the key point of this study should be how using prior stereo heights improves/degrades the optical properties. That analysis is missing and needs to be added.

Response: Thank you for these comments.