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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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Comments on "Synergy of stereo cloud top height and ORAC optimal estimation cloud retrieval: evaluation and application to AATSR"

Authors introduce AATSR derived stereo cloud top heights as prior constraint to ORAC retrieval that fits modeled cloud properties to observed multi-channel radiometric observations from AATSR. Results are compared with ORAC retrieval in absence of stereo prior. This is a novel and useful analysis of an AMT-relevant procedure that has clear potential value in diminishing well-known modes of error. The underlying method of comparing CALIOP, ORAC, and ORAC+Stereo heights is sound, but the presentation could be tightened up. However, authors inexplicably do not address a the highly

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relevant question of whether the stereo prior improves or degrades the cloud optical property retrieval. That question is not addressed, and needs to be, through intercomparison of ORAC, ORAC+Stereo, and some other instrument representing ground truth (why not CALIOP again?).

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?)

Instruments (section 3) pp 5290: "can be defined" change to "are inherent" pp 5290-5291: Advantages of stereo are mentioned, but not disadvantages. Need to mention georegistration. Need to introduce later discussion of stereo smoothing relative to radiometric heights. Should consider introducing question of whether fundamental differences between stereo and readiometric height create problems for using stereo height as prior to ORAC. The assertion that multi-layer clouds do not introduce to bias for stereo CTH 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 most effective area based stereo matcher" citation needed pp 5292: use of "radius" as descriptor is not correct. I think the algorithm uses a 7x7 pixel domain, not a circular domain.

pp 5295: "error in the stereo matching is dominated by ..." - needs rephrasing for clarity pp 5295: conversion of stereo CTH to CTP does not belong as a topic here, needs to be in section 5

Methodology (section 5.1): While the stereo technique and application within ORAC receive 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 as-

AMTD
8, C3655–C3658, 2015

sumed to be negligible, that (debatable) assumption needs to be explicit. I would also move discussion of 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.

Results (section 5.2) This section is difficult to follow, mostly because Figure 1 presents 20 different results in a single, disorganized 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, cloud-layers-single-vs-multiple, cloud-heights-low-mid-high. This is 2*2*2*3=24 total 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 result. (This applies to Figure 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 accomodate the results makes meaningful comparison of the quartiles and means impossible, and also prevents meaningful organization of the different classes of result. It also lacks information about 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.

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, disorganized box-whisker plots are presented (Figures 7 and 8) where organized 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. pp 5302-3: linear regressions are inappropriate and should be removed pp 5304-15: "As expected". ???

AMTD

8, C3655–C3658, 2015

Why expected? 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.)

Discussion and Conclusions (section 6-7) These are nice sections, and I suspect is would be even more compelling if the the Results section was cleaned up (and if 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.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 5283, 2015.