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Discussions

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Interactive comment

Interactive comment on "Remote Sensing of Multiple Cloud Layer Heights using Multi-Angular Measurements" *by* Kenneth Sinclair et al.

Anonymous Referee #1

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This paper uses airborne RSP observations taken during the SEAC4RS campaign to estimate the cloud top height (CTH) of clouds overflown, using the parallax technique (based on geometric grounds: near-simultaneous observations of a scene from multiple angles). The technique is applied to measurements at 670 and 1880 nm separately, and also a combined approach using both bands, to examine the effectiveness of the various band combinations. The CTH is also compared to CPL observations, mounted on the same aircraft.

As I commented in the quick access report stage of the journal, there is nothing technically wrong with this paper (and I want to stress that: it is a nice analysis, and quite clear). The issue is I don't see the broader scientific novelty or use of it. These parallax-based methods have been applied for years (to e.g. the MISR and ATSR sensors), although one wouldn't think that was the case because little of the existing literature on

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the technique has been cited or discussed. The strengths and limitations of the technique are well-understood (as are the strengths and limitations of other techniques) so we don't learn anything that noteworthy about it from these case studies. Since this is basically a validation exercise for a few airborne case studies that don't form part of a large data set it isn't clear to me how it is scientifically interesting unless you have a specific science question related to these specific clouds seen at these specific times. As such the paper doesn't meaningfully develop or increase our understanding of the measurement technique, or answer science questions relating to aerosols or clouds in the SEAC4RS campaign. This is relevant when determining whether the submission is appropriate for journal publication.

Looking for a bigger picture, it's true that there is a need to improve the remote sensing of clouds (especially multi-layer systems) and their representation in modes. But by their nature RSP measurements can only be applied to case studies from airborne field campaigns (which probably also have a co-mounted lidar in most cases) and so will never provide us long-term large-scale statistics needed to substantially reduce uncertainties in climate prediction. Certainly not as much as we can get from existing instrument combinations using similar techniques (again the ATSRs, MISR, SLSTR) or other techniques (thermal, A-band, lidar, etc). A spaceborne version of RSP would be welcome for a great many applications, but parallax CTH from it would have additional issues not mentioned in this study, such as the fact the pixel size would be dramatically larger, limiting what can be resolved. My understanding is that current/proposed space-based polarimeters have significantly coarser resolution than imaging radiometers; for example MISR and the ATSRs are about 1 km but POLDER is about 6 km. I also understand there are more band-to-band and angle-to-angle geolocation difficulties, which would affect this type of retrieval. It also isn't clear to me whether a band in a water absorption region like 1370 or 1880 nm is currently planned for future spaceborne polarimeters, which would influence how relevant that RSP band is to future spaceborne applications.

So as a result the paper doesn't really address bigger-picture issues either. As a result I regrettably recommend rejection: while the analysis is not incorrect, I do not believe it is scientifically novel (this type of technique being well-established, and no significant methodological leap being made in this study), or particularly useful for the broader community (as noted the results are only relevant to these specific clouds at these specific times and don't have direct applicability to science questions or to future missions). Have the SEAC4RS team, for example, been doing detailed scene analysis to answer SEAC4RS science questions using the results from this study? If so, this could basically form a method/validation section for another paper. If not, then I'm struggling to see what the motivation behind the study is.

If a paper based on this analysis were to eventually be accepted, I'd feel the need to see a more demonstrated bigger-picture relevance or methodological advance. For example, I think that the ER-2 also mounts the eMAS sensor, which has thermal IR bands. One could therefore use these case studies (if eMAS is available) to develop a combined parallax (geometric) and thermal (radiometric) retrieval algorithm that hopefully is better than using either technique singly. Steps in this direction were recently made by Fisher et al (AMT 2016, doi:10.5194/amt-9-909-2016) but that was in the form of using parallax as prior information rather than in the retrieval. Such an approach would have direct current relevance as a similar combined retrieval could be applied to the MODIS/MISR combination or the ATSRs, and may provide useful input for future missions. Or as another possibility the authors could degrade the RSP capabilities to the expected resolution a spaceborne version would have, and thus simulate how well a future spaceborne sensor of this type might be expected to perform, which is important as we hope to have more multiangle polarimeters in space in the coming decade. Or as yet another possibility, could the RSP and CPL data be combined to enable the inference of information like lidar ratio for the cloud droplets, or profiles of cloud particle size or similar? (There are radiometric/polarimetric techniques to estimate cloud optical depth and particle size from RSP, for example.)

The bottom line is that I am left asking, what is really new here? Technical correctness is a prerequisite for publication but scientific novelty/utility are equally important and I don't see that here. I raised this issue with the quick access report stage but don't feel that it has been addressed. Any of the additions suggested above would probably go beyond the scope of major revisions. As a result I recommend rejection but encourage the authors to explore new applications of the available data sets and build on this work.

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