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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Community Cloud retrieval for Climate (CC4CL). Part I: A framework applied to multiple satellite imaging sensors" *by* Oliver Sus et al.

Anonymous Referee #3

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Review of "The Community Cloud retrieval for Climate (CC4CL). Part I: A framework applied to multiple satellite imaging sensors", by Sus et al.

The manuscript introduces a valuable approach to establish a common passive cloud retrieval applicable to a series of standard polar orbiters in order to create data sets usable for climatological studies. This would be an important step for the community and the usability of satellite products outside the satellite community. I also understand and acknowledge the need to base such an approach on well established methods instead of more experimental approaches as suggested by one of the other referees. The general presentation is of good/excellent quality. My two co-referees have elabo-

rated on a number of specific technical and scientific details already. I want to focus on a more general weakness.

What exactly is the focus of this manuscript? If I missed important, clear, early statements in the existing text, I apologize. If not, the reader needs this guideline.

In many places important details can not be given and are not explained owed to the shear extent of this project. In most cases the reader is then correctly referred to other publications where the methods of CC4CL are introduced. This way the purpose of the manuscript at hand becomes more and more unclear while reading through it. First impression is that the general method will be explained. But then the core retrieval techniques are explained elsewhere (McGarragh). Then a technical explanation of the ANN cloud mask is started, but it stays too short to be fully comprehensible. After the introduction of example cases Fig 3-5 and cross sections Fig 9-16, I expected an in-depth discussion of reason for differences and a quantitative validation (section titles containing "validation") or cross-comparison of all products, but the discussion stays very general and mostly describes differences. Proper validation is again shown elsewhere (Stengel).

The limited original content of this manuscript (correct me, if I'm wrong) is not reflected by the title and manuscript length (e.g. 8 figures 8-15 with very comparable content and not too surprising differences between active and passive sensor, but no quantitative validation). The authors should clarify the purpose of this manuscript and shorten parts published elsewhere even stronger. I suggest to consider these general points and a revision of the manuscript.

Specific major issues:

p3, line 27: "Moreover, the resulting time series are carefully validated ... (ISCCP, PATMOS-x, CM SAF, and MODIS Collection 6), reanalysis and model data (ERA-Interim and EC-Earth), ground-truth synoptic observations, and CALIOP lidar data." My understanding was that I would see that in this manuscript: You will only show

CALIOP comparisons, will you? Could you please clarify.

p8, section 4.3: I think you cannot call this chapter "validation". There is no systematic validation, only a few selected case studies, which mainly show the problems and no systematic quantitative validation. Four case studies of time height cross sections are shown only to present that lidar cth does not have much to do with passive cth? I also expected CER and COT validation somewhere.

p13, line 15: You mean, proper quantitative validation is shown in another paper ... Stengel et al 2017 ESSD? The retrieval method was shown in two other papers as well ... McGarragh 2017 at JAS and AMT. Remind me about the reason for this manuscript?

Minor issues:

p 2, line 33: AVHRR was not introduced before, was it?

p 2, line 53: What is r?

p 2, line 65: How can CTP and CTH be underestimated at the same time? Can you please comment?

p 2, line 66: What is a "cloud phase bias ... of 9%"? Cloud phase? Liquid and ice? Or cloud cover?

p 2, line 68: Low or hight bias?

p 2, line 102: It would be nice to say at this early stage what the purpose of this particular manuscript is in ESA Cloud_cci? And what other parallel publications contribute? Later on, the reader gets the impression that everything relevant is introduced elsewhere.

p 6, line 27: If this is the only description of ANNCOD available, you might at least want to cite Kox et al . 2014 (AMT, 7, doi:10.5194/amt-7-3233-2014) who introduced the idea and described in much more detail.

p6, line 51ff: This is all a slightly vague description, if it isn't detailed somewhere else. Why do you need ... after viewing angle dependency correction ... a whole set of thresholds? ANNCOD already gives an answer on the question cloud or no-cloud, doesn't it?

p7, Figure 2: y-axis. It is PEC not 1-PEC shown, isn't it? Does the graph show that, at your threshold you are only correct by about 50%?? Please discuss.

p10, line 10: "consistent". You could also say its all over the place, with different physical reasons in any single column. This is not a validation. You even tried to correct cth for cc4cl and still have big problems.

p11, Figure 7: Please make the labels consistent with the rest of the manuscript: n18->avhrr, myd->modis ...

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