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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 #2

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This manuscript provides an overview of the Community Cloud retrieval for CLimate (CC4CL) algorithm developed for the European Space Agency's (ESA) Climate Change Initiative (CCI) program. CC4CL is a cloud retrieval framework that can be applied to any number of passive satellite imagers having spectral information content sufficient to retrieve parameters such as cloud top (pressure, CTP), optical (cloud optical thickness, COT), and microphysical (cloud effective radius, CER) properties. For ESA's CCI, CC4CL is applied to AVHRR (NOAA-7 through Metop-A+NOAA-19), Terra and Aqua MODIS, ATSR-2, and AATSR, and uses a subset of solar and thermal IR spectral channels common to all sensors. Cloud identification, or masking, is performed using thresholds applied to COT derived from an artificial neural network

trained using NOAA-18 AVHRR and co-located CALIOP observations. Cloud typing (thermodynamic phase) is performed using the approach of Pavolonis et al. (2005), and cloud top, optical, and microphysical retrievals are performed simultaneously using the optimal estimation retrieval ORAC. Details on the required ancillary data, as well as the CC4CL pre-processor, are provided. The algorithm is applied to four case studies, selected for concurrent observations between the passive imagers and active CALIOP, for which retrievals of CTH (derived from CTP and ancillary atmospheric profiles) are evaluated with the co-located CALIOP cloud layer products; example CTP, COT, and CER retrieval swath images are shown for one case study. The authors show that CTH from each passive sensor is in general agreement, though there are some expected differences when compared with CALIOP.

The manuscript is clearly written, the figures provided are of excellent quality, and the sections are generally well organized. However, I have a number of comments, listed below, that should be addressed before accepting for publication. Thus the paper should be returned to the authors for revisions.

Comments

p 2, line 5: I would add cloud forward model assumptions to the list of secondary confounding factors.

p 2, lines 12-14: The CERES-MODIS products (e.g., Minnis et al., 2011a,b, IEEE TGRS) should also be included here.

p 2, lines 23-24: The MODIS C6 phase referred to here is the IR phase of Baum et al. (2012), which is in fact a quad-spectral algorithm (7.3, 8.5, 11, 12μm channels) using β ratios (the authors' description is more appropriate for the C5 algorithm). This IR phase algorithm is run in conjunction with, and is informed by, the cloud top property retrieval algorithm. The authors should be aware, and I believe that they are given the reference to Marchant et al. (2016) later in the paper, that this IR algorithm does not determine phase for the C6 cloud optical properties retrieval; phase for the optical retrieval is determined by the Marchant algorithm that uses the IR phase as one piece of information. Results from the IR and cloud optical properties phase algorithms are often at odds, specifically in cases where phase is more ambiguous.

p 2, line 24: Should probably specify that the additional spectral channels are at short-wave infrared (SWIR) wavelengths.

p 2, lines 29-30: Indeed, this is an inherent limitation of the spectral information content of passive IR channels!

p 2, lines 31-35: I assume from the references given that cloud cover refers to cloud fraction or related metrics, and not to geophysical retrievals.

p 3, line 5: Is the cloud phase bias positive or negative?

p 3, lines 6-7: See my p 2 comment above regarding MODIS phase algorithms; this statement again refers only to the IR phase.

p 3, lines 10-11: What is the difference between consistency and continuity? I can surmise that it is consistency in approach versus continuity of results, but it is not clear to the general reader.

p 3, lines 34-35: It's not initially clear why independent retrievals of COT/CER and macrophysical products are inherently radiatively inconsistent. I would guess that it depends on the approach, i.e., how (or if) one set of retrievals informs the retrieval of the other. Can the authors better explain?

p 4, line 1: Retrieval uncertainty estimates that propagate errors is not a novel feature of CC4CL. See, for instance, the MODIS C6 cloud optical properties (Platnick et al., 2017), which provide pixel-level retrieval uncertainties calculated in a manner that is mathematically consistent with that of optimal estimation (although the uncertainties are not part of the solution process).

p 4, line 6: Following on my comment above, neither the optimal estimation approach

nor the uncertainty quantification are novel features of CC4CL. As the authors themselves state on p 2, PATMOS-x uses optimal estimation theory, and the MODIS C6 (and C5) cloud optical properties provide rigorous pixel-level uncertainties.

p 4, lines 5-13: Regarding statements about consistency of the long-term, multi-platform time series, and the potential of the framework for climate studies, I don't think the authors make a convincing case for either in the text that follows. Four case studies hardly constitute a "comprehensive and detailed analysis of retrieval results," and certainly do not provide enough evidence of the potential for climate studies. Such statements require detailed analyses of long-term and large-scale inter-sensor statistical comparisons, which it appears are actually presented in a companion paper in a different journal (Stengel et al., 2017). It's thus not clear to me why the present paper was not instead a part of the Stengel paper, or vice versa. Given that the primary contributions are a brief discussion of the ancillary and data sources and a rather limited CTH analysis, I'm not convinced that this paper can or should stand on its own.

p 4, line 15: Consider using Level-1 instead of L1, which for some readers implies a Lagrange point 1 orbit.

p 4, lines 21-25: Yes, replacing any AVHRR once its successor becomes available will lessen the impacts of orbital drift (and thus sampling times), but drift impacts are likely still to exist. Are these accounted for in CC4CL, specifically when constructing long-term multi-sensor time series?

p 4, line 29: Regarding filtering channel 3b data, is this to include or exclude that channel?

p 5, lines 8-10: It should be NASA Goddard Space Flight Center.

p 5, lines 21-23: "Self-calibrating" is I think a little misleading. MODIS, for instance, has a similar design (onboard black bodies and solar diffuser), yet requires a continual effort to monitor instrument stability and identify/correct calibration drifts, typically using fixed ground targets among others.

p 7, lines 3-4: Has the “gap filling” of the MCD43C1 data been validated? Is the approach similar to what is used in the MCD43B3 gap-filled product (Schaaf et al., 2011, “Aqua and Terra MODIS albedo and reflectance anisotropy products,” in Land Remote Sensing and Global Environmental Change: NASA’s Earth Observing System and the Science of ASTER and MODIS)?

p 6-7, Sections 2.2.3-2.2.4: Have the authors verified that there are not any trends in the land surface BRDF and emissivity time series during the MODIS era? If there are, wouldn’t the use of the climatology derived from all MODIS data introduce a discontinuity in the surface time series?

p 7, lines 6-7: I disagree that the surface is a minor component of the observed signal, specifically for optically thinner clouds. Thus not accounting for the spectral response functions can introduce biases, particularly in spectral regions such as the near-IR (e.g., AVHRR channel 2, MODIS channel 2) where reflectance by vegetation can change rapidly.

p 7, line 16: Resampled or aggregated?

p 7, line 16-17: I would agree that differences in sensor spatial resolution are reduced when averaging radiances/reflectances. However, this is likely not the case when averaging L2 geophysical parameters, as is done here, since the retrievals can have significantly different PDFs within a grid box due to pixel size differences alone.

p 9, line 5: How much data was used to train the ANN? Was an observation time difference filter applied to the NOAA-18/CALIOP co-location?

p 9, lines 19-21: If I understand correctly, the reflectances/radiances were adjusted to account for spectral response differences? Were the co-located observations filtered for cases in which both satellites viewed the scene at the same sun-view angle geometry? Such angle matching is important when comparing solar channels where

reflectance is strongly angularly dependent.

p 10, lines 21-22: My understanding is that the uncertainty obtained from the optimal estimation framework can be thought of as the sensitivity of the solution space at the point of the solution to the measurement uncertainty (which includes instrument, ancillary, etc., uncertainties).

p 10, line 25: This statement differs from the statement at the end of Section 3.2 (phase is determined first to reduce computation time resulting from retrieving assuming both phases).

p 11, Section 4.1, Figure 3-5. The observation date/times should be stated here. I see they are listed in Section 4.3, but it is better to include them at first reference. Also, a thermodynamic phase image would be useful.

p 11, lines 13-14: I’m guessing the peaks at 12 and 35 μm likely correspond to liquid and ice phase clouds, respectively.

p 11, line 17: The statement on cloud displacement here contradicts the statement in line 11.

p 11, Section 4.1: What about relative radiometric calibration between the different sensors? Even minor differences of a couple percent could case large retrieval differences, particularly for COT.

p 11, line 22: If median absolute CER uncertainty is 2μm, how does this correspond to a median relative uncertainty of 2% (line 24).

Figure 10: What wavelengths are used for this RGB?

p 12, Section 4.3: Hard to call this “validation” without using a much larger dataset (e.g., months, seasons, years) for statistical analyses.

p 12, line 21: What assumptions are made other than adiabaticity (e.g., extinction profile, etc.)? Also, what does adiabaticity mean for an ice phase cloud?

p 12, Case Study NA1: Need to include the Figure number in the text.

p 13, lines 25-26: Which existing algorithms were compared to these results?

p 14, lines 10-11: Why not show the extensive validation here?

p 15, lines 6-11: For the optimal estimation retrieval, are the spectral response differences handled similar to the ANN cloud mask (i.e., adjustment factors), or are they explicitly included in the forward model? What about relative radiometric calibration, could that be playing a role in the large MODIS-AATSR retrieval differences?

p 15, line 18: Here calibration deficiencies are acknowledged. Relative calibration should be explored as a cause of the retrieval differences.

p 15, lines 29-30: Can the authors provide references for these user applications?

p 16, line 29: “radiatively effective rather than physical cloud top”

p 17, line 9: The MODIS C6 phase referred to here is that of the cloud optical properties algorithm, not the IR phase referred to earlier in the paper.

p 18, lines 10-12: Perhaps this is worded poorly? I would imagine that real, complex vertical cloud structure is in fact a large source of retrieval errors, but the analytical approach to retrieval uncertainty used here (and in other retrievals) cannot account for this.

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