Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-115-RC1, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "Link between the Outgoing Longwave Radiation and the altitude where the space-borne lidar beam is fully attenuated" *by* Thibault Vaillant de Guélis et al.

Anonymous Referee #2

Received and published: 4 July 2017

In this paper the authors devise a technique for relating – with a fairly high amount of accuracy – outgoing long wave radiation (OLR) at the top of the atmosphere (TOA) to several quantities that can be acquired from space-borne lidar (i.e., CALIOP on board Calipso). These quantities are the the radiative temperature and spatial coverage of opaque clouds and the radiative temperature, spatial coverage, and LW emissivity of thin clouds. Opaque clouds are defined as those for which the lidar beam becomes fully attenuated within the cloud, and typically have LW optical depths exceeding 1.5-2.5. Thin clouds, with LW optical depths less than this threshold, are semi-transparent and do not fully attenuate the lidar beam. The authors derive a simple semi-empirical relationship in which OLR increases by 2 W/m2 for every 1 K increase in opaque cloud

radiating temperature. For thin clouds, this 2:1 relationship is scaled by the cloud LW emissivity. OLR inferred from the lidar-derived quantities compares well with that measured directly by CERES, at a variety of spatial scales.

I found the technique described in the paper to be a clever use of the unique measurements provided by active sensors in space. Despite the presence of errors (notably for thin clouds), the OLR can be largely reproduced from 5 basic measurements, which makes it a powerful tool for relating cloud property changes to OLR. I recommend publication pending revisions based on the my concerns that are detailed below.

Major Comments: 1) My main concern with this work is that the authors may be slightly overstating the value of such an analysis, especially in regard to how it is contrasted with passive sensors. Passive sensors are rightfully criticized for often giving incorrect information about cloud vertical distribution, which active sensors retrieve with much higher accuracy. However, passive sensors are (essentially) directly retrieving the quantity that the authors need to derive here: the emission temperature of clouds. Passive retrievals may not place the cloud top at the correct physical altitude like a lidar does, but they do place it at the effective radiating temperature, which is what matters for the OLR and any TOA LW anomalies. This is basically what makes studies that relate TOA radiation to passive-derived cloud fraction histograms like Hartmann et al. (1992), Zelinka et al DOI: 10.1175/JCLI-D-11-00248.1 (2012) and Yue et al DOI: 10.1175/JCLI-D-15-0257.1, (2016) possible. The authors are sort of reverse-engineering this problem: They have highly accurate measurements of backscatter by cloud particles as a function of altitude, which they then use in a clever way to derive the effective radiating temperature, which is what you would already have if you started with passive measurements. It is not obvious to me that this is superior. I think the paper requires a clear discussion of why one would prefer this technique over one relying directly on passive measurements, and/or a discussion of how they both could complement each other. Simply asserting that active sensors retrieve the vertical profile of condensate more accurately is not compelling in this particular context. One advantage I can think of relative to existing kernel techniques is that it does indeed seem desirable to have a small set of measurements that one can get both from observations (Calipso) and models (albeit, those running the Calipso simulator) that can give a highly accurate proxy for OLR, in keeping with the analogy to APRP in the SW. This is in contrast to relying on 7x7 histogram of cloud types from ISCCP and a kernel to match. Perhaps another advantage has to do with the more practical issue of observing cloud changes over a long period of time. Few people trust ISCCP trends because of various issues that arise with splicing many individual satellites together that are poorly inter-calibrated and have non-climate related trends from satellite orbit changes, view angle changes, etc. (Norris and Evan DOI: 10.1175/JTECH-D-14-00058.1 2015). Presumably some of these issues are less relevant for lidars? If so, it would be important to distinguish these sorts of problems from those arising from the retrieval philosophy (e.g., if ISCCP was a perfect system without any artifacts, would the active approach still be superior?)

2) On lines 362-365, the authors state "Monitoring T_Opaque on longterm should provide important information which should help to better understand the LW cloud feedback mechanism. Moreover, because the relationship is linear, it simplifies the derivatives in mathematical expressions of feedback and will allow to construct a useful framework to study LW cloud feedback in simulations of climate models." Feedbacks are conventionally defined as the change in a given quantity holding all else fixed. In the case of altitude feedback, this would be the change in cloud altitude only, with everything including the temperature profile fixed. Mathematically, this is equivalent to comparing a control OLR with a hypothetical one computed with the cloud at a higher altitude and therefore at a lower emission temperature. Of course we know that in reality the cloud top temperature is expected to stay nearly constant with surface warming as the cloud top altitude rises with the isotherms (i.e., FAT hypothesis of Hartmann and Larson 2002). Changes in T_Opaque will depend on both the change in cloud altitude and the change in temperature profile, and constant T_Opaque may mean perfectly complementary changes in both the altitude and the temperature profile, as

one expects from FAT. If one uses your relationship between OLR and T_Opaque in computing feedbacks, then the mathematical formulation of the feedbacks will need to be changed to accommodate this. Specifically, I think one would need to compare the fixed T_Opqaue (FAT) case against a hypothetical baseline situation in which all things change except for the Z_Opaque, such that T_Opaque warms as much as a fixed altitude. While this is do-able, I disagree with the statement above that this simplifies the mathematics of feedbacks.

3) The English is very poor throughout the manuscript. There were far too many errors for me to list all of them (grammar, spelling, awkward phrasings, words that are plural that should not be, incorrect comma usage, etc.). In some places the writing was poor enough that the meaning of the sentence was unclear. This paper should be copyedited by a native English speaker before the reviewers see it again. In contrast, the figures were very clear, well-designed, and well-executed.

Minor Comments: In addition to the numerous English errors, I note the following:

Title: I would suggest deleting "the" before Outgoing and also rephrasing to "...where a space borne-lidar..."

Throughout: "cloud altitude longwave" seems awkward. Please rephrase to "longwave cloud altitude"

Abstract: This ends very abruptly. It needs a better closing sentence.

Lines 29-34: An uninformed reader of this paragraph will assume that the only reason there is uncertainty in how clouds will respond to warming is because models simulate biased clouds in the mean state. Surely this is not the only reason for low confidence in cloud feedbacks. There are a variety of recent review articles out on cloud feedbacks that may be helpful on this point.

Lines 52-54: This statement needs to be rephrased. Emergent constraints are not feedback mechanisms.

Lines 64-65: I disagree that there is no link between observed cloud variables and LW CRE. See, for example, the section on LW cloud altitude feedback in Ceppi et al doi: 10.1002/wcc.465 (2017), which points out that high cloud amount and emissivity, along with the temperature structure of the upper troposphere, govern the strength of this feedback. All of these are observable.

Lines 85-87: Cloud fraction histograms from passive sensors generally report cloud fraction on 7 cloud top pressure bins; the high, mid, and low aggregating is usually done later to simplify.

Lines 88-89: Suggest also citing Zhou et al DOI: 10.1175/JCLI-D-12-00547.1 (2013) and Yue et al 10.1002/2016JD025174 (2017), who have done this globally

Lines 90-91: These studies should be more clearly distinguished from the ones preceding it in the sentence: they have focused on trends, not interannual variability.

Line 97: Mace et al (2011) DOI: 10.1175/2010JCLI3517.1 should be cited here

Lines 168-170: I can't understand this. Please rephrase.

Line 183: should be "sea ice"

Line 185: Should be "Flux observations collocated with lidar cloud observations"

Line 216: Should "as" be "that"?

Figure 4: Is it possible to compare these cloud emission temperatures with those from passive sensors? They should be in agreement, right?

Line 273: "T_opaque among opaque clouds" is redundant. This sort of statement occurs throughout the document.

Line 282: meaning of "mid-effect" is unclear

Line 288: "pick" should be "peak"

Line 303: rephrase

Lines 422-423: Rephrase.

Figure 8: Is the shading 2-sigma? Max to min?

Line 433: "under the tropics" – rephrase

Line 453: I don't know what this statement means.

Lines 488-493: The authors seem to be implying that omega is the only variable on which the cloud properties and CRE depend, and that therefore knowing how omega change will tell one how cloud properties and CRE will change. This is incorrect, as has been discussed many times over, most notably by Bony et al DOI 10.1007/s00382-003-0369-6 (2004) where this type of analysis originally appeared. While omega changes may strongly determine regional changes in cloud properties, when averaged over the entire tropics, it is the thermodynamic sensitivity of cloud propertiesÂăwithin omega bins that emerges as the dominant driver of cloud changes.

Section 6.1: It is unclear whether this is actually an error source. The authors raise the issue then immediately downplay it. Is it a source of error? Have you actually performed a sensitivity study to determine with these assumptions matter?

Section 6.4: the impacts of these assumptions are being assessed on the global mean OLR, but I wonder whether they also influence the slope of OLR on T_Opaque.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-115, 2017.