Point-by-point response to the reviews

Anonymous Referee #2

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.

- **Response:** We agree with the reviewer that a clear discussion of why one would prefer this technique over one relying on passive measurements is required. Thank you for your comment.
- **Change made:**
  - In Sect. 3.1 (last §): "These cloud radiative temperatures are fundamental to study the LW CRE and are different from the effective radiating temperatures measured by passive instruments which are influenced by radiation coming from below the cloud. In the case of Opaque cloud which completely absorbs upward LW radiative flux propagating from below, the effective radiating temperature measured by passive instruments should agree with the cloud radiative temperature. However, this assumes to know that the cloud is Opaque, but cloud emissivity from passive measurements is also sensitive to hypothesis made on the clear sky and surface property. Unlike passive measurements, lidar measurements robustly separate Opaque clouds and Thin clouds from the presence or not of a surface echo (Guzman et al., 2017)." has been added.

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.

- **Response:** Thank you for this comment.
- **Change made:**
  - In Introduction (8th §): "We propose to build on these studies by adding the space-borne lidar information." has been replaced by "We propose to build on these studies by adding spaceborne lidar information to obtain a simplified radiative transfer model in the LW domain that can give a highly

accurate proxy for OLR with a small set of parameters available from both observations (space-lidar) and models (space-lidar simulator). This approach is in contrast to reliance on 7×7 histograms (altitude×optical depth) of cloud types from ISCCP and use of a matching radiative kernel.".

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?)

- **Response:** Thank you for this comment.
- **Change made:**
  - In Introduction (8th §): "Moreover, a highly stable long-time observational record is essential to study clouds and climate feedback (Wielicki et al., 2013), and current passive instruments have shown limited calibration stability over decadal time scales (e.g. Evan et al., 2007; Norris and Evan, 2015; Shea et al., 2017)." has been added.

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.

- **Response:** We agree with the reviewer that it does not simplify the mathematics of feedbacks as the equation is currently as a function of T_{Opaque}. We will adapt this equation for a future study using climate model outputs with lidar simulator so that the equation will be as a function of the altitude of T_{Opaque} ($Z_{T_{Opaque}}$) considering a linear atmospheric temperature lapse rate. In that way, a change in $Z_{T_{Opaque}}$, holding all else fixed, changes CRE_{Opaque} by a quantity which, divided by the global mean raise in surface temperature, is directly the cloud altitude feedback. This will so simplify the mathematics of feedbacks.

- **Change made:**
  - In Sect. 4.2 (2nd §): "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." has been removed.

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.

- **Response:** A native English speaker copy-edited the paper.

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. . ."

- **Change made:**

- Title: "Link between the Outgoing Longwave Radiation and the altitude where the space-borne lidar beam is fully attenuated" has been replaced by "The link between Outgoing Longwave Radiation and the altitude where a spaceborne lidar beam is fully attenuated ".

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

- **Change made:**
  - Throughout the paper: "cloud altitude longwave" has been replaced by "longwave cloud altitude".

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

- **Change made:**
  - In Abstract: "The link between outgoing longwave radiation and the altitude where a spaceborne lidar beam is fully attenuated provides a simple formulation of the cloud radiative effect in the longwave domain and so helps to understand the longwave cloud altitude feedback mechanism." has been put as 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.

- **Response:** We agree with the reviewer. Thank you for this comment.
- **Change made:**
  - In Introduction (1st §): "One reason for this uncertainty is that […]" has been added.

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

- **Response:** We agree with the reviewer.
- **Change made:**
  - In Introduction (3rd §): "Such records do not exist yet. Klein and Hall (2015) suggested that some cloud feedback mechanisms, namely the "emergent constraints", could be tested with shorter records in comparing the simulated and the observed current climate interannual variabilities" has been replaced by "Such records do not exist yet, but existing records might help our understanding (Klein and Hall, 2015).".

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.

- **Response:** We wanted to focus on the fact that, so far, there was no simple mathematical expression to directly link, at different scales, cloud properties to OLR.
- **Change made:**
  - In Introduction (4th §): "Nevertheless, the cloud altitude LW feedback mechanism and its amplitude still struggle to be verified in observations. There is still no observational confirmation for the altitude LW cloud feedback mechanism because 1) there is no simple direct and robust formulation linking the observed fundamental cloud variables and the LW CRE at the TOA […]" has been replaced by "Nevertheless, the LW cloud altitude feedback mechanism and its magnitude still remain to be confidently verified with observations, because 1) there is no simple, robust, and comprehensive mathematical formulation linking the observed fundamental cloud variables and the LW CRE at the TOA […]".

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.

- **Response:** We agree with the reviewer.
- **Change made:**
  - In Introduction (7th §): "[…] and only retrieve the cloud top pressure and estimates of high-level, mid-level, and low-level cloud covers. These last estimates have been coupled with ranges of cloud optical depth to define different cloud types (Hartmann et al., 1992) associated to different values of CRE." has been replaced by "[…] and instead retrieve single-layer effective cloud heights, often summarized as cloud fraction in seven cloud top pressure bins. Hartmann et al. (1992) used these pressure bins

coupled with ranges of cloud optical depth to define different cloud types associated to different values of CRE.".

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

- **Response:** Thank you for this suggestion.
- **Change made:**
  - In Introduction (7th §): "Zhou et al., 2013" and "Yue et al., 2017" have been added.

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.

- **Response:** We agree with the reviewer.
- **Change made:**
  - In Introduction (7th §): "[…], as well as the International Satellite Cloud Climatology Project (ISCCP) and the Pathfinder Atmospheres Extended (PATMOS-x) (Marvel et al., 2015; Norris et al., 2016) in order to identify LW CRE changes associated to cloud properties changes." has been replaced by "[…]. Recently, Marvel et al. (2015) and Norris et al. (2016) analyzed data from the International Satellite Cloud Climatology Project (ISCCP) and the Pathfinder Atmospheres Extended (PATMOS-x) datasets in terms of these cloud types to search for trends in LW CRE which would be associated with changes in cloud properties.".

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

- **Response:** We agree with the reviewer.
- **Change made:**
  - In Introduction (8th §): "Mace et al., 2011" has been added.

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

- **Change made:**
  - In Sect. 2.1 (3rd §): "Thin cloud emissivity $\varepsilon_{Thin}^{|}$ of a *Thin cloud single column* is inferred from the mean attenuated scattering ratio of levels flagged as "Clear" below the cloud, that we note $\langle SR' \rangle_{below}$ and which approximately corresponds to the apparent two-way transmittance through the cloud. Indeed, considering a fixed multiple scattering factor $\eta = 0.6$, we retrieve the Thin cloud visible optical depth δ_{Thin}^{VIS} (Garnier et al., 2015)." has been replaced by "Thin cloud emissivity $\varepsilon_{Thin}^{|}$ of a *Thin cloud single column* is inferred from the attenuated scattering ratio of clear sky layers measured by the lidar below the cloud. This is approximately equal to the apparent two-way transmittance through the cloud which, considering a fixed multiple scattering factor $\eta = 0.6$, allows retrieval of the Thin cloud visible optical depth τ_{Thin}^{VIS} (Garnier et al., 2015). As cloud particles are much larger than the wavelengths of visible and infrared light, and assuming there is no absorption by cloud particles in the visible domain, the Thin cloud LW optical depth τ_{Thin}^{LW} is approximately half of τ_{Thin}^{VIS} (Garnier et al., 2015).".

Line 183: should be "sea ice"

- **Change made:**
  - In Sect. 2.1 (last §): "iced sea" has been replaced by "sea ice".

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

- **Change made:**
  - In Sect. 2.2 (title): "Fluxes observations collocated with lidar clouds observations" has been replaced by "Flux observations collocated with lidar cloud observations".

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

- **Response:** Yes, indeed. Thank you.
- **Change made:**
  - In Sect. 3 (1st §): "such as" has been replaced by "such that".

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

- **Response:** Passive sensors do not allow a clear separation of Opaque clouds and Thin clouds as done with the lidar. Moreover, it does not find the same cloud occurrence. Cloud emissivity retrieval depends on hypothesis on clear sky and surface properties. Comparison with classical product derived from passive sensor is not obvious. An equivalent comparison was done by Stubenrauch et al. (2010) with collocated measurements from CALIOP and the passive sounder AIRS: they compared the height of the cloud emission temperatures determined by AIRS with the "apparent middle" of the cloud sounded by CALIOP, which is actually our definition of where the emission temperature of the cloud is. They show very good agreement.

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

- **Response:** We agree this precision makes the reading difficult.
- **Change made:**
  - Throughout the paper: "among Opaque clouds" and "among Thin clouds" have been removed from the main text but left into figure captions and the 1st § of Sect. 3.2 to avoid misunderstanding.

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

- **Change made:**
  - In Sect. 3.2 (2nd §): "These Opaque clouds will have a mid-effect on the local OLR," has been replaced by "The local radiative effect of these Opaque clouds is weaker than the effect if they were in tropical ascending regions.".

Line 288: "pick" should be "peak"

- **Response:** Thank you.
- **Change made:**
  - In Sect. 3.2 (3rd §): "pick" has been replaced by "peak".

Line 303: rephrase

- **Change made:**
  - In Sect. 3.2 (last §): "[…] emissivities of Thin clouds are usually small, and clouds with small emissivities have less impact on the OLR. This, once again, goes in the sense that the role that play Thin clouds on the total CRE should be significantly smaller than that of Opaque clouds." has been replaced by "[…] emissivities of Thin clouds are usually small, so they have little impact on the OLR and hence their contribution to CRE should be significantly smaller than that of Opaque clouds.".

Lines 422-423: Rephrase.

- **Change made:**
  - In Sect. 5.2 (1st §): "Interestingly, an inversion of cover predominance and colder temperature between Opaque and Thin clouds occurs around 30° latitude. " has been replaced by "There are always more Opaque clouds than Thin clouds in the extratropics (beyond 30° latitude) and they are colder than the Thin clouds. It is the opposite in the tropical belt: there are always more Thin clouds than Opaque clouds, and those are slightly warmer.".

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

- **Change made:**
  - In figure caption of Fig. 8: "(max to min)" has been added.
- **Additional change:**
  - Fig. 8 has been redrawn because an error in our script was discovered. During computation of annual means of T_{Opaque}, T_{Thin}, and ε_{Thin} on 2°x2° boxes (before averaging zonally), means were not weighted by monthly mean cover, on 2°x2° boxes, of opaque and thin clouds. It is now fixed. Changes are quite small and do not affect the conclusions.

Line 433: "under the tropics" – rephrase

- **Change made:**
  - In Sect. 5.2 (2nd §): "under the tropics" has been replaced by "in the tropics".

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

- **Response:** .
- **Change made:**
  - In Sect. 5.2 (last §): "Also, since the expression used for Thin clouds seems to give coherent results for $CRE_{Thin}^{\boxplus\,(LID)}$, it could also be used in a future work to quantify the role of a change in $C_{Thin}^{\boxplus}$, $T_{Thin}^{\boxplus}$, and $\varepsilon_{Thin}^{\boxplus}$ in the variations of $CRE_{Thin}^{\boxplus\,(LID)}$." has been replaced by "However, since the OLR expression above Thin clouds is almost as good as for the Opaque clouds, it could also be used in a future work to quantify the impact of changes in $C_{Thin}^{\boxplus}$, $T_{Thin}^{\boxplus}$, and $\varepsilon_{Thin}^{\boxplus}$ on the variations of $CRE_{Thin}^{\boxplus\,(LID)}$.".

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 propertiesÂă within omega bins that emerges as the dominant driver of cloud changes.

- **Response:** We agree with the reviewer.
- **Change made:**
  - In Sect. 5.3 (last §): "Because cloud properties seem to be invariants for dynamical regimes, a change in the tropics of the large-scale circulation should provide a change in the CRE predictable and linked to the spatial distribution (both covers and altitudes) of Opaque clouds and Thin clouds sounded by CALIOP. For example, under global warming, climate models suggest a narrowing of the ascending branch of the Hadley cell (e.g. Su et al., 2014), which means less convective regions and more subsiding regions and which should result in a decrease of the CRE predictable knowing the changes of ω_{500} all over the tropics." has been replaced by "Because cloud properties seem to be invariants for dynamical regimes between 20 hPa·day^{-1} and -100 hPa·day^{-1}, a change in the tropics of the large-scale circulation should lead to a predictable change in the CRE in regions that stay in this range of dynamical regimes, linked to the spatial distribution (both covers and altitudes) of Opaque clouds and Thin clouds sounded by CALIOP. For example, general circulation models suggest that a warmer climate will see a narrowing of the ascending branch of the Hadley cell (e.g. Su et al., 2014), which means less convective regions and more subsiding regions. This should result in a predictable decrease of the CRE, knowing the changes of ω_{500} for some part of the tropics.".

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?

- **Response:** It is a source of error. The worst case for this is the multi-layer scenario when an optically thin cloud overlap an optically opaque cloud. This is now discussed in the new subsection 6.2. We could certainly have slightly more precise results using a centroid temperature for every case but it will add complexity to our expressions. However, the aim of our study is to find a simple expression of the CRE by determining its main cloud variable driver, not to reach the maximum of accuracy in CRE estimation.
- **Change made:**
  - In Sect. 6: Sect. 6.2 Multi-layer cloud and broken cloud situations has been added.
  - In Appendix:
    - Fig. A4 has been added. It shows the decomposition of Fig. 6 in "single-layer cloud" and "multi-layer cloud" situations. The main text refers to Fig. A4 in Sect. 6.2.
    - Fig. A5 has been added. It shows improvement on OLR_{Opaque} when considering multi-layer cloud in the computation of T_{Opaque}. The main text refers to Fig. A5 in Sect. 6.2.

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.

- **Response:** As Z_{Opaque} is increased in every profile with the same amount (+480 m), and because atmospheric temperature profile is linearly dependent on the altitude, the slope of OLR_{Opaque} on T_{Opaque} is not influenced.