

The 2nd-round review of ACP-14-18421 by Turner et al.

I appreciate the substantive changes made by the authors in respond to the comments raise by the reviewers. There are still major issues that concern me, which I describe below. There are also minor issues such as inaccurate description of IR radiative transfer and incorrect reference formats. These concerns need to be satisfactorily addressed before this article can be accepted for publication.

Major comments

1. The merit of “Integrated Nadir Longwave Radiance product” INLR product. Reviewer1 raised the question about the new information in this study compared to established technique. The authors argued that “The proposed method is unique among others in that it resolves the far infrared (and near infrared) at high resolution, utilising the full breadth of the spectrally continuous IASI range” in the reply letter and “This approach has the advantage of allowing for a cleaner comparison with climate model simulated satellite products.” (Line 6 on page 9).

I am not convinced that this method has such claimed advantages compared to existing methods for data-mode comparison. Here is why

- (1) This method uses regression to predict radiance in the far-IR. Thus, it must assume all information about far-IR radiance has been included in the mid-IR radiance measurement. In another word, **the far-IR radiance estimated in this study has no extra information content compared to measured IASI radiances.**
- (2) As for data-model comparison in radiance domain, a radiance simulator must be employed because GCM does not compute radiance at all. Then the simulated IASI radiance is sufficient for comparison with observed IASI radiance. Because of (1), there is no extra information in comparing far-IR radiance, which merely increases the computational cost but does no increase information content.
- (3) The far-IR is meaningful when we discuss flux and radiation budget. Because of the large fractional contribution of far-IR to the LW flux, evaluating far-IR flux is much more meaningful and is an inseparable and irreplaceable part of radiation-budget comparison between model and satellite observations.

Please note evaluate GCMs in radiance domain are still evaluating the thermodynamic variables simulated by the GCM, not simulated radiative flux. **Having far-IR radiances merely regressed from mid-IR radiances does not provide any new information content from using mid-IR radiances alone.** The author needs to acknowledge this weakness in the article, rather than claiming it as an advantage. In my opinion, the INLR defined by the authors is an “half-done” product: it seemingly more than mid-IR radiance but really with no new information, and yet still not flux yet so it cannot be used in radiation budget studies.

2. Validation of the regression algorithm for the predicted far-IR radiance.
 - (1). As I pointed out in my previous review, the comparisons with CERES cannot fully validate the far-IR radiances derived in this study because CERES radiance is a broadband one. It is not a valid assumption to assume the mid-IR radiance from IASI being the same as the portion of mid-IR radiance in CERES unfiltered radiance. Please note even IASI and AIRS does not agree with each other that perfectly in mid-IR. For example, Larar et al. (2010) shows a 2.5K difference between IASI and AIRS for mid-IR

water vapor band and 0.5K difference for mid-IR window band. In studies in Huang et al. (2008) and (2010), they seek a “theoretical” validation by comparing the estimated far-IR flux based on synthetic AIRS radiance with the directly computed far-IR flux using the same radiative transfer model. Similar validation using LBLRTM should be done in order to validate the estimated far-IR spectral radiances in this article.

Reference: Larar et al. 2010, *Atmos. Chem. Phys.*, 10, 411–430, doi:10.5194/acp-10-411-2010.

(2). The CERES SSF data used by authors contained sophisticated scene type information (i.e., cloud fraction, surface temperature, total water vapor, etc). Table 1 should be shown for different scene types, in addition to overall daytime vs. nighttime statistics. The algorithm can be only deemed as successful when it has consistently robust performance across all scene types. Statistics for all scene types as shown in Table 1 is not convincing: the regression is trained with a variety of profiles so it is aimed to get better results when results of all scene types are counted toward the statistics. But such “one-fit-all” is not necessarily valid for individual scene types. Large compensating biases can exist among different scene types. Such possibility of compensation needs to be eliminated by examining different scene types.

3. There are key information missing or outdated regarding the forward simulation for deriving regression coefficients.

(1). In section 2.2, please describe how you handle land surface spectral emissivity in your LBLRTM simulation and which high-spectral resolution spectral emissivity database you have used for this simulation. This piece of information is critical because different land surface can have considerably different spectral emissivity in the mid-IR window region and “micro window” channels in both water vapor v2 band and far-IR band. A simple use of blackbody surface or incorrect surface emissivity will lead to systematic biases, especially for the regression involved with micro window channels in water vapor bands. Please note there are hundreds of IASI channels in H₂O v2 band sensitive to surface emission. Take channels in the window regions and CO₂ band into account, the number would be even bigger.

(2) Line 14 on page 11, I am surprised that Haurwitz and Kuhn (1974) were used in this study for ice cloud properties. This paper was outdated and the result in this paper was far less accurate than state-of-the-art ice cloud properties deriving using accurate scattering theory of non-spherical ice crystals. There has been at least three generations of ice cloud properties after Haurwitz and Kuhn (1974). Even using ice cloud properties in current flagship GCM cloud-radiation schemes such as those in the CESM should be more justifiable than using the results in this paper published 40 years ago. If the authors want to be more accurate, the ice cloud properties used for MODIS ice cloud retrievals can be consulted.

References: Baum, B. A., et al., 2014, *J. Quant. Spectrosc. Radiat. Transfer*, 146, 123-139, doi:10.1016/j.jqsrt.2014.02.029.

Yang, P., et al., 2013: *J. Atmos. Sci.*, 70, 330-347

Minor Comments:

1. Line 3 on page 1, I do not think “theoretical” is an appropriate word here. The algorithm

is based on statistical correlations between IR channels and is derived using a finite training data set. Different training data set will give different regression coefficients, though the difference might be small. Therefore, these are statistically derived correlations, not theoretical derived ones.

2. Line 18 “Consistent with previous estimate”. This needs to be more accurate. There has been no such previous estimate regarding INLR. All previous estimate is about flux, which is a different physical quantity. So this sentence is misleading and needs to be rephrased in more accurate way.
3. Line 19 on page 1 “In terms of the spectral cloud effect (CINLR), the FIR contributes 19% and in some subtropical instances appears to be negative, results that would go unobserved with a traditional broadband analysis.” The FIR negative contribution in this region is due to the fact that, in such marine stratus and cumulus-stratus areas, the humidity above PBL in the clear-sky area can be higher than those in the adjacent cloudy area (opposite to the case of clear-sky vs. cloudy pixels in the deep convection area). This is because dry air above the PBL can help maintain the marine stratus in the PBL and adjacent clear-sky area can be where the marine stratus has dissipated and vaporized. This can be seen from Figs 4-5 in Sohn et al. (2006). Please note this issue only affects the satellite-derived CRE not the modeled CRE because the different ways of defining clear-sky flux in satellite observation and in GCM simulation.

Reference: Sohn BJ, et al., 2006, J. Climate, 19, 5570-5580. doi:
<http://dx.doi.org/10.1175/JCLI3948.1>

4. Lines 1-2 on page 8, please note there is a recent publication by the same group, in which the algorithm has been extended to all scene types (including snow surfaces) so spectral CRE can be derived for all collocated AIRS and CERES observations over the entire globe. The citation is

Huang, X. L. and Co-authors, 2014: A global climatology of outgoing longwave spectral cloud radiative effect and associated effective cloud properties, Journal of Climate, 27, 7475-7492, doi:10.1175/JCLI-D-13-00663.1, 2014.
5. Lines 6-7 on page 9, please see my major concern #1. The weakness of INLR needs to be acknowledged here.
6. Lines 24-29, page 23 the two references here are in wrong formats, there is no journal named “Journal of Geophysical Research: Atmospheres (1984-2012)”. Standard and correct reference formats should be used here.
7. Lines 11-12, page 3, “For this this (*duplicated THIS) reason emissions occur mostly in the upper tropospheric and stratospheric regions.” This statement is completely wrong in physics. Far-IR emission occurs everywhere, from the surface all the way to stratosphere. The far-IR emission from surface is much stronger than the far-IR emission in the mid- and upper-troposphere. Under normal condition, the water vapor abundance is large enough so the far-IR emission from surface can be completely absorbed by water vapor in the troposphere and none reaches the TOA (but this is not the case for very dry and cold air, e.g. over Antarctic Plateau where surface far-IR

emission can reach the TOA). The water vapor in the middle and upper troposphere absorbs the far-IR emission from below and re-emit far-IR radiation in a lower temperature, most of which can then reach the TOA due to little water vapor abundance in the stratosphere. This is the correct physical picture regarding upward far-IR emission.

8. The author did an excellent job reviewing far-IR issues in the introduction section. In 2014, there were two important publications on the relation about far-IR radiation and climate, it might be worth to include them as well in the introduction section. They are

Feldman, D. R., W. D. Collins, R. Pincus, X. L. Huang, X. H. Chen, Far-infrared surface emissivity and climate, *Proceedings of National Academy of Sciences*, doi: 10.1073/pnas.1413640111, 2014.

Chen, X. H., X. L. Huang, M. G. Flanner, Sensitivity of modeled far-IR radiation budgets in polar continents to treatments of snow surface and ice cloud radiative properties, *Geophysical Research Letters*, doi:10.1002/2014GL061216, 41(18), 6530-6537, 2014.