Review of acp-2014-453 “Can IASI be used to simulate the total spectrum of outgoing longwave radiation?” by Turner E. C., H.-T. Lee, and S. F. B. Tett

This study first employs a channel-based regression method to use selected IASI mid-IR spectral radiances to predict radiance in the far-IR, a spectrum region not covered by the IASI measurement. Spectral fluxes were then computed by simply multiplying a factor of pi with the nadir-view spectral radiance in each channel (usually known as the isotropic radiation assumption). The SUM of such spectral fluxes, i.e, OLR is compared with CERES OLR for the cases of SNO (simultaneous nadir overpass), which is only available in the polar regions. The study claimed a good agreement between the broadband OLR derived in this way and the CERES broadband OLR. The study further used the spectral fluxes computed in this way to study certain climatology such as the contribution of far-IR to the OLR and the cloud radiative effect in the far-IR in the subtropics.

The estimation of far-IR radiance based on mid-IR radiance definitely interesting and might be worth of a publication. However, this study has a fatal physical mistake and several fatal flaws. The way of converting radiance to flux is directly contradictory to the radiation transfer equation and the well-established diffusive approximation. The error analysis and characterization of the entire algorithm is nearly completely missing. I recommend rejection of this manuscript in current form with encouragement of resubmission. If the author seek to publish this study, I would suggest the authors to substantively revise the article, make it concise, be aware of the fundamentally incorrect assumption of isotropic radiation for the problems studied here, carry out detailed error analysis for the regression algorithms, avoid overstating the comparison results, and acknowledge intrinsic flaws/caveats of their algorithms.

Below I only list fatal mistakes and major issues, as they are already more than enough to support my recommendation.

1. Incorrect assumption of isotropic radiation

Based on radiation transfer equation, it can be easily shown that in general case the upward radiance field at the TOA is not isotropic. Thus spectral flux cannot be computed accurately by simply multiplying a factor of pi. The author supported the usage of this incorrect assumption by stating the globally average difference between CERES-IASI OLR is small. But this heavily averaged difference over the entire LW spectrum cannot justify the usage of this assumption at all, for so many compensating error sources can contribute to this broadband flux difference (I will discuss in detail about this in Comments #2 and #3).

In atmospheric radiation, there is a widely used and well-established approximation, the diffusivity approximation, which can be found in nearly all the atmospheric radiation textbook. It states that the flux can be approximated by the radiance at 53 degree (diffusivity factor 1.66) multiplying pi. This diffusive approximation has been first introduced by Elsasser in 1941 by examining the radiation chart and has been widely used ever since, in both observation and modeling. Li (2000) gave theoretical explanation of this approximation. Virtually all the GCM radiation schemes employ this approximation to compute the LW flux since they cannot afford to compute radiance at multiple zenith angles and then integrate them to obtain flux. **If what the authors used in this study were true, it would be equivalently saying that this well-established diffusive approximation were wrong and every scheme could simply compute radiance at nadir view.** Such direct contradiction to the well-established and well-verified approximation to compute LW flux is not even mentioned, let alone justified or proved.

Furthermore, in one reference cited by the authors, Huang et al. (2008) clearly shows how each spectral channel can deviate from the isotropic radiation assumption for the clear-sky situation. Figure 2 in Huang et al. (2008) shows that, in some spectral channels, the anisotropic factor is as large as 1.2, which means that a 20% error would be introduced if the spectral flux of such channel were estimated as in this study. Note the same Figure 2 in Huang et al. (2008) also corroborates that the anisotropic factor for the diffusivity angle suggested by Elsasser is indeed much closer to one than that of nadir-view angle for all the LW spectral channels.

In another reference cited by the authors, Huang et al. (2006), such nadir-view radiance from old dataset IRIS was used to multiply with pi in the second part of the study. But Huang et al. (2006) carefully defined it as “nadir flux” and used this term in all figure captions and relevant discussions to distinguish from OLR or flux as commonly defined. The “nadir flux” was never used to compare with actual OLR observation or OLR simulation in the entire text of Huang et al. (2006).

In a nutshell, it is fundamentally wrong to compute OLR in the way done in this study. It contradicts well-established and well-verified diffusivity approximation and the equation of LW radiative transfer. The author failed to show any proof why they can do so. The “flux” derived in this way has a dimension of flux, but physically is not the same quantity as the OLR obtained by CERES or simulated by any GCMs. Thus, all the consequent comparisons with CERES OLR and analysis of such results in the context of OLR (or spectral OLR) are groundless. The author can define this as a flux quantity, but by no means it is OLR.

The author shows a seemingly good agreement between heavily averaged CERES OLR and derived OLR (at SNO or global-mean), but this seemingly good agreement can be due to many compensating errors (as I will discuss this in more detail in following comments). This is not something we can argue “end justified the means”, because the “means” here is fundamentally wrong according to the physics, unless the author can approve the otherwise. Note the SNO approach is powerful for comparing radiometric quantities directly measured by the instrument, as shown in many recent calibration studies. But OLR here is not a derive quantity and compensating errors must be identified if the authors want to employ this approach.

References:
Elsasser, W. M., 1942: Heat Transfer by Infrared Radiation in the Atmosphere. Harvard Meteorological Studies, Vol. 6, Harvard University Press, 107 pp.

Li, J., 2000: Gaussian Quadrature and Its Application to Infrared Radiation, Journal of the Atmospheric Sciences, 57, 5, pp. 753-765.

2. Limitation of A “one-fit-all” regression model for all scene types over the globe

The study employs a regression model to estimate far-IR spectral flux after carefully selecting the predictor mid-IR channels, as shown in Eq. (1). My understanding is Eq. (1) is applied to the entire globe and there is no separate set of parameter derived for different scene types (e.g., ocean vs. land vs. snow surfaces, overcast vs. partial cloud etc.). It is well known that the regression model works best for the mean state and can behave badly for individual state that is largely deviated from the mean state. This is, in my opinion, why in observations like ERBE or GERB, more physics-based angular distribution model approach has been adopted instead of such statistical regression.

Physically, different scene types (surface type and cloud properties) can have very different spectral dependence, especially for mid-IR channels that are sensitive to the surface emission. Therefore, a regression model working best for ocean surface might not work for the land surface, and vice versa. Taking cloud fraction and cloud optical depth into account will further compound this issue exponentially. Even a set of regression coefficients is derived using thousands of observed profiles, there is virtually no discussion how the regression model behaves for different scene types and how the spectral emissivity of difference surface types has been obtained and incorporated into the simulation/training.

As long as the predictor channels used in the regress include channels sensitive to surface emissions, and as long as the authors want to discuss any spatial features beyond global-average fluxes, the authors are obligated to discuss the regression errors for different scene types, especially the dependence on the surface type and on the cloud fraction and cloud optical depth (or equivalently cloud emissivity).

In fact, though the globally averaged CERES-IASI OLR difference is small, Figure 8 does show that, even after heave average over one month, a large portion of globe still has OLR difference more than ±10 Wm^{-2}. Such big difference is likely attributed to more than one error sources, but oversimplified regression model is definitely a reason and its error contribution needs quantification.

The comparisons with CERES OLR beyond the SNO cases are ill defined due to the different stages of diurnal cycle covered by the Terra/Aqua CERES and IASI. However, this cannot be simply attributed as the dominant error sources for Figure 8 when other sources of errors are not quantified at all.

3. Title vs. content

The title leaves an impression that this study is to use IASI to estimate spectral flux over the entire LW spectrum (i.e. “the total spectrum of OLR” as in the title). However, the only validation done in this study is comparison with CERES OLR. A good agreement with OLR is necessary condition for a good estimate of the total spectrum of OLR, but not a sufficient condition at all, let alone the quantity derived in this study is not OLR at all (see my comment #1). There are so many possible compensations among different spectral bands that makes the total OLR correct but for utterly wrong reasons. In another word, the question posed in the title has not been convincingly answered by this study at all.

This study employs a simple and physically incorrect conversion from radiance to flux, as I discussed in comment #1. This conversion alone leads to errors in all spectral channels, mid-IR and far-IR. Then when the summation of spectral flux is computed, it is not clearly at all how much of the agreement with CERES broadband OLR is due to compensations of errors among different channels (or different bands).

Even there is no spectrally resolved observations in the far-IR that are suitable for direct validation of the algorithm, it seems the study can at least use LBLRTM to simulate far-IR spectral flux and IASI radiance simultaneously, then compare the spectral flux regressed from such simulated IASI radiance against the spectral flux computed by LBLRTM directly. Such comparison should be done for clear-sky scenes as well as cloudy sky scenes with a variety of cloud fractions.

Relevant to this issue, the text especially the long introduction reads more like the far-IR being the focus of this paper instead of spectral OLR of the LW spectrum. The far-IR, as a band, has been discussed more than any spectral details of the flux as computed in this study (which is not the OLR per se).