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Anonymous Referee #2

1 General Comment

This is a good paper that describes the new characteristics of the LBLRTM model, which among the generic radiative transfer models is likely to be that widely used by the science community involved in applications such as the infrared remote sensing of earth's atmosphere. The paper mostly deals with the performance of the new model, which has been assessed based on a set of night-time IASI spectra, recorded during the JAIVEx experiment. The paper is clearly written and represents a good guide for LBLRTM users and not only, and, therefore, I recommend publication. Some weak points, which could be easily handled by the authors, are here outlined:

• references could be improved in order to better represent contributions from IASI European colleagues;

Reviewer 2 wants us to include more references and Reviewer 1 wants us to cut out references. We added in some of the suggested references and others from this IASI special Issue.

• the paper does not provide a mathematical description of the LBLRTM methodology and approach, but rather its performance. I think that the paper could benefit by dealing a bit more with its physical and mathematical aspects, but I think this could be left to the initiative of authors;

This is a valid point and similar to one made by Reviewer 1. It sometimes can be difficult to determine the amount of detail to include in a paper. When writing the paper there were several places we had to make these type of decisions. For this evaluation type paper the decision was made that we should only provide the reader with a qualitative description, as the more quantitative details significantly increased the paper and would be better provided one-on-one to the users of LBLRTM that would specifically need this information.

• the paper only shows results based on eight IASI spectra, these IASI spectra have been further reduced through averaging, so that in the end only two mean spectra are considered for calculations and comparisons. On this last point, I think that the authors could make proper reference to other works published in the ACPD IASI special issue, which address similar aspects, but with a larger statistics.

We agree and have added to the discussion of the results other published work (e.g. Matricardi) from this ACP IASI special issue. The amount of discussion is somewhat limited as although these studies provide the opportunity to obtain a large number of clear-sky coincidences over a wide range of atmospheric and surface conditions the nature of the performing such a large number of comparisons with its merits is limited in the amount of detailed information it can provide in terms of evaluating the sources of systematic errors (e.g. atmospheric state, spectroscopy, instrument).

2 Specific Questions/comments

2.1 Section 1

1. Pag. 9316, lines 7 to 12: IASI has been under development since 1993. Early works to the performance and quality of the spectrometers dates back to Amato et al. 1995 and should be acknowledged here.

We agree that an early IASI reference should be included. We included the Amato reference as provided by the Reviewer.

2. The concept of closure experiment should be made explicit both in the text and in the Figure 1. The authors should mention previous closure experiments (Tobin et al. 1999 and Serio et al. 2008).

We agree that the radiance closure study concept should be made more explicit with references. The radiance closure study was first proposed by Tony Clough (ARM QME) and the first reference I can find is Brown et al., (1998). I included this reference with some other references of this approach. The section now reads as follows:

This comparison method is known as a radiance closure study, represented in Figure 1. As depicted in the illustration, the main elements of the radiance closure study are the radiating atmosphere/surface, the measurement of this atmosphere by an instrument, and the corresponding calculation of the same atmosphere using a radiative transfer model (Forward Model) with inputs that specify the atmospheric state (e.g. radiosondes, surface emissivity, etc). The radiance closure comes from evaluating the differences or residuals between the observations and model calculations. Any radiance residuals are then evaluated by investigating the radiance observations (e.g. instrument function), the forward model (e.g. spectroscopy), and the specification of the atmospheric state (e.g. radiosonde). The radiance closure study is used to identify and reduce the radiance residuals by carefully evaluating all components. With the high radiometric accuracy provided by current instruments and the recent improvements in spectroscopy great the limiting factor in reducing the residuals is often the specified of atmospheric state (e.g. the radiosonde not sampling the same atmosphere as being measured by the satellite). It is often the case that a retrieval step is needed in the radiance closure that the importance of external data

sources in the comparison (such as radiosonde measurements) is significantly reduced, allowing greater freedom from sampling issues. The approach adopted in this work has been to perform detailed analyses of a small number of carefully selected cases in order to work towards a physical explanation of remaining features in the spectral residuals. More examples and details of the approach taken to performing radiance closure studies can be found in the literature, such as Brown et al., (1998), Turner et al., (2004), and Shephard et al., (2008a).

2.2 Section 2

1. LBLRTM is one of the widely used line-by-line radiative transfer for the earth atmosphere. LBLRTM users may have benefit with the knowledge of the difference between the release 11.6 and 11.3 (the last public release available for download) of the code.

We completely agree. However, instead of putting this in the paper we decided to make this release public and put it on the website so that users can easily obtain the differences between 11.3 and 11.6. Version 11.6 was not on the website at the time of the Reviewers comments, but is there now.

2.3 Section 3

1. The authors should be cautioned that the IASI ISRF is not a Gaussian.

Our IASI Science Team colleagues (personal communication Guido Masiello and Carmine Serio) informed us that the ISRF is in not exactly Gaussian. However, they indicated that the effect is very small and would not impact our analysis. With the good agreement we are getting between the observations and the calculations there does not appear to a noticeable affect.

We are not sure if the Reviewer was just providing insight or would like us to somehow include this information in the manuscript.

2.4 Section 4

1. The IASI instrument has very good instrumental noise performance. In the spectral range between 645 and 2000 cm•1, it is, in terms of NEDT @ 280 K, less than 1 K. For this reason I do not understand why the authors averaged 4 spectra. This operation introduces a very large “interpixel variation” in the region where the IASI radiometric noise increases (□ 2400 cm•1).

This is a very good point raised by the Reviewer. By averaging the 4 pixels we further reduce the instrument random noise by a factor of 2, which in turn reduces the impact of the instrument on the noise on the residuals. This however this can come at the cost of introducing interpixel variability if the retrieval cannot account for this variability (i.e. the interpixel variability is non-linear then the retrieval will not be able to obtain a retrieved atmospheric state that represents the atmosphere being observed by the average of the four pixels). When comparing the interpixel variability in Figures 3 and 4, it indicates that the land case benefits from the averaging as there is relatively small variability compared with the ocean case.

2. In Figures 3 and 4 it is incorrect to compare interpixel variability and the NEDT@ 280 K. In the region around 2400 cm•1, the spectral brightness temperature is around 230 K. It might be better to use the radiance scale for the panel a-d, and to show the NESR in the residuals panels.

It was not to intent to directly compare the interpixel variability and the NEDT as they are two different metrics. We are just providing the reader with both pieces of information. We put them on the same plot in order to reduce having to add another panel to the figure, which already has 5 panels. We suggest leaving the figure as is, but if the Reviewer thinks it is misleading or confusing we can put them on separate panels in the figure.

The Reviewer brings up a good point that really comes down to choices that we thought about carefully. Brightness temperature is the desired quantity to work with as it provides a unit that one can relate to easily. However, when the radiance gets smaller at higher wavenumbers, the SNR decreases for single scene comparisons and computed brightness temperature less desirable (as pointed out by the Reviewer). To address this we made the choice in the paper to show the full spectral range plots in brightness temperature and the blow-up plots in the high-wavenumber region (Figure 15) in radiances. In addition, we explained this in the paper by stating: "A particularly striking feature is the large variation in brightness temperature in the coldest part of the CO_2 n_3 region (2200-2500 cm^{-1}). This part of the spectrum views the coldest part of the atmosphere (the tropopause region). The signal to noise here is not as good as it is for the lower wavenumber part of the spectrum that views the tropopause. Since the radiance values are small, any small variations in radiance are amplified to large variations in brightness temperature. Therefore in the analysis of this region, it is more constructive to view the residuals in terms of radiance."

In addition to the overall brightness temperature plots we will also include the corresponding plots in radiance.

2.5 Section 5

1. The paper needs more details for the description of the retrieval procedure.

We noticed this after the initial submission. The bottom of section 4 has been modified and now provides more descriptions of the retrieval. Here is how it now reads:

"The IASI measurements were first compared with modeled radiances using radiosonde humidity and temperature. For the ocean and land cases, the temperature profile above the top of the radiosonde was constructed by scaling tropical and US standard atmospheres respectively [Anderson et al., 1986] to match the uppermost values of the radiosonde. For the ocean case, the water vapor profile above the dropsonde was constructed by adjusting the tropical standard atmosphere to provide reasonable agreement with the IASI measured brightness temperatures. For the land case, the US standard atmosphere water vapor profile was simply added to the top of the profile. For ozone, the tropical/US (ocean/land) standard atmosphere profiles were scaled to match TOMS total column amounts. For CO_2 and N_2O, the standard profiles were scaled to match surface observations at comparable latitude. CO and CH_4 initial guess profiles were taken directly from the standard atmospheres, while profiles for OCS and HNO_3 were taken from the mid-latitude standard atmospheres constructed by Remedios et al. [2007]. All the above profiles were used as the a prior in the retrievals. The surface emissivity for the ocean case was taken from the Wu and Smith [1997] model (zero wind speed, zero viewing angle) as described in van Delst and Wu [2000]. The a priori surface emissivity used in the land case was from a previous retrieval based on eigenvector regression relations generated from infrared spectra simulated for a large ensemble of surface emissivity and atmospheric conditions [Zhou et al., 2007].

For the retrievals, an optimal estimation retrieval approach is utilized to minimize the difference between the observed IASI spectral radiances and the nonlinear LBLRTM calculations of the atmospheric state subject to the constraint that the estimated state must be consistent with an a priori probability distribution for that state [Bowman et al., 2006; Clough et al., 1995; Rodgers, 2000]. The spectral regions used in the retrieval of each of these parameters are shown in Table 1. Note the spectral regions used for the temperature retrievals include spectral regions in the CO_2 ν_2 that profile the troposphere and CO_2 ν_3 region that senses the stratosphere; the Q-branch at 667 cm^{-1} was excluded as the modeling for this region is under investigation (see following discussion)."

2. About the Figures 5 to 10, the retrieved profiles are shown up to 100 hPa. Do the authors mean that the retrieval procedure is limited to this altitude range? If not, please shows the complete retrieved profiles.

The Reviewer makes a good point for certain retrievals. All the retrievals performed to the TOA. However, for nadir retrievals only temperature, ozone, and HNO3 retrievals have any sensitivity above the tropopause (100mb). We will modify the plots to reflect this for these retrieved parameters.

2.6 Section 5.1

1. The new parameterization for CO_2 and the continuum of CO_2 are two of the most important aspects in this paper. Especially for the continuum the author should provide more details on its mathematical formulation. If not yet published, these details should be described here.

As stated above in the general comments, for this evaluation type paper the decision was made that we should only provide the reader with a qualitative description, as the more quantitative details significantly increased the paper and would be better provided one-on-one to the users of LBLRTM that would specifically need this information.

2.7 Section 5.2

1. Since the water vapour continuum is defined in terms of the "local" line shape in order to use the new line strength for water vapour, the MT_CKD have to be modified according the new line parameters. Did you import the new line strengths in the water vapour continua?

This is an insightful question. The short answer is that we did not use the new line strengths to generate a new version of MT_CKD as there is not evidence yet from the closure studies to do so. It is a little difficult to discern exactly what is being asked, but it might help to think about it in the following manner.

For simplicity, we can think of the continuum coefficient at v as C(v) = S(v) * F, where S is some sort of average/integral of the line strengths that affect the continuum at v and F is the net result of the mathematical formulation of the continuum. It may be good to understand that two different sets of lines can be said to determine the continuum in different respects. The lines within a few cm-1 determine the optical depth that must come from the continuum to obtain closure, while it is primarily the lines further away than 25 cm-1 that dominate the continuum upon application of the formulation. Let's say there is a systematic change in the line strengths in a region. What will be the effect on the magnitude of continuum coefficients in that region? If we're talking about the local lines, then to keep the total optical depth the same as before, then the continuum coefficients would have to decrease. However, if this is the case, it matters whether there is enough information to constrain the continuum value at that v. If yes, then the line strength change might require us to adjust the continuum coefficients, but to what degree is unclear. If no, then there would be no reason to do anything -- any reasonable change in the continuum would be irrelevant with respect to closure. If the lines with increased strength are not local (which may be what the reviewer has in mind) then if F is unchanged then increasing S(v) would indeed lead to C(v) increasing. However, if we did redo the continuum we would use the same continuum coefficient as the constraint as before (assuming it is a place we constrain the continuum), so the F would need to decrease to compensate for the increased S(v), so the net effect on the coefficients would be small.

2. In Figure 16 a) it might be useful to indicate the absolute value of the water vapour mixing ratio.

Figure 16 is the same SGP land case in Figure 8 that shows the profile of absolute values of the water vapor mixing ratio. I added the figure reference in the following sentence to make it more clear to the reader.
"Figure 16(a) shows the difference in the retrieved profile for the IASI land case (Figure 8) obtained by using the two different sets of line parameters."

3 Typo

• Pag. 9331, line 22, change 16(a) in 16(b);

Changed, thanks!

• Pag. 9333, line 14, change Figure 18 in Figure 17.

Changed, thanks!, Also removed "both cases" at the end of the sentence and replaced them with "SGP land case" since this plot only contains one case.

References

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