Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Can downwelling far-infrared radiances over Antarctica be estimated from mid-infrared information?" *by* Christophe Bellisario et al.

Anonymous Referee #3

Overall comments

The paper "Can downwelling far-infrared radiances over Antarctica be estimated from mid-infrared information?" demonstrates a method for estimating downwelling far-IR spectral radiances at the surface in Antarctica from mid-IR radiances (both measured and calculated) at that location. Although the main result in the paper is adequately presented, overall the paper suffers from significant motivational and methodological issues.

This study is presented as similar to the TOA-based study of Turner et al. (2015). The need for the results presented in the earlier study can be straightforwardly seen. There

exist many millions are mid-IR spectral radiance measurements from satellites (IASI, CrIS, AIRS, TES) without far-IR counterparts, and the construction of far-IR simulated radiances consistent with the mid-IR observed radiances could be very useful. That these observations are global and span more than a decade also attest to their utility. However, the same is not the case for surface radiances. This paper does not really attempt to make an argument that the development of this technique is likely to be relevant or the lessons learned from this study will be able to be applied to something relevant. There is no global set of mid-IR ground-based spectral radiometers for which this technique would be useful. The closest is the many AERIs that have been deployed, but the vast majority are located in regions with high enough water vapor so the far-IR is opaque and a simple surface air temperature measurement will suffice to predict the far-IR radiances. Only the few AERI datasets from high-latitude or -altitude might be spectrally extended using a variant of this technique, but even that application begs the question as to what would be the need to adequately fill in the far-IR record for these deployments. A weak motivational argument is made by alluding to the FORUM mission, but this is a satellite-based instrument. Without some adequate motivation, there is no real reason for this paper to be published.

The methodology employed is also problematic. In a dry location like Antarctica, far-IR radiances are primarily determined by the temperatures and water vapor amounts in the lowest layers of the atmospheres. Therefore, a technique to predict far-IR radiances using a single observed mid-IR radiance would ideally use a frequency that also is sensitive to these atmospheric conditions. However, the paper indicates that any such REFIR-PAD channels (1300-1400 cm-1) are too impacted by noise to be very useful for this application. Therefore the method that is arrived at predominantly uses frequencies in the CO_2 band, measurements that basically are sensitive just to the temperatures at the levels very close to the instrument. (The text incorrectly suggests that water vapor is also important in this spectral region.) Therefore, this result in the paper can be boiled down to most important consideration in simulating far-IR radiances, given the noise in REFIR-PAD mid-IR water vapor channels, is getting the near-instrument temperature

correct. Understanding the result in this context should give the authors some pause in any assessing the importance of this result – if the simulated far-IR radiances are independent of the water vapor profile, how useful and information-laden can they be?

Given the ease these days of doing non-simple optimization, it's unclear why the authors did not determine an optimal linear combination of the radiances at multiple channels for each far-IR radiance. By definition, this would obtain better results than using a single channel and allow the use of spectral points sensitive to both temperature and water vapor.

The comments above mainly apply to the results in the paper about using observed mid-IR radiances to simulate far-IR radiances. The paper also has results for using calculated mid-IR radiances to obtain far-IR radiances. It's not clear if these results would be useful since if one were already doing calculations and wanted far-IR radiances, why not compute them directly?

Specific comments by page, line. More important comments denoted by *:

Section 1

2, 3 – perhaps use "as much of as N %" instead of "significant" since "significant" is pretty subjective (and the fraction is probably less than 50%)

2,9 – some extra "v" characters are present

2, 13 – It might be worth mentioning the RHUBC campaign (papers by Turner, Mlawer)

3,1-3 – A potential satellite mission is not relevant to this paper's purpose since this analysis only applies to ground-based measurements. It may be relevant to the T15 paper, but not to this paper.

Section 2

3,15 – The REFIR_PAD goes up to only 1400 cm-1, so it's hard to see how that could be a "complete longwave dataset."

4,9 – Perhaps start this sentence "Each radiosonde records data every 2 seconds…". The current wording might be taken as sondes are launched every 2 seconds.

4,19 – Perhaps get rid of "developed by" and change Clough et al. into a regular reference (i.e. in parentheses). The current wording makes it seem like the model was developed around 2006.

5,1 – In the far-IR, the linefile has substantial modifications to the HITRAN 2016 widths following Delamere et al. and Mlawer et al. The latter study also led to MT_CKD_3.0. The text says "includes modifications", but doesn't specify what the modifications were in reference to (i.e. modifications to what?). Since the RHUBC-II results are primarily based on REFIR-PAD measurements, it is probably worth mentioning when introducing the model.

5,22 – Do the conditions have to be clear for the entire period between when the sonde is launched through the REFIR-PAD measurement time?

5,25 – random subsampling?

Section 3

6,8 and Fig. 1 – Specify whether these results are for linear or log.

*9,2 – The noise added is between -1 and 1 times the standard deviation of the measured noise at each frequency. Doesn't this underestimate the actual noise? Why not use the chosen random number to sample from a normal distribution with that standard deviation? Also, does the actual noise have spectral correlation? (i.e. if a case is higher than the mean in the MIR, is it likely to be higher than the mean in the FIR?). If so, then not taking that into account in the method may lead to inferior results for the LBLRTM+noise compared to using a pure measurement approach.

9, 25-29 – It should be made more clear that all these LBLRTM-based regression approaches are being applied to the MIR observations and not the LBLRTM simulated radiances.

10,11 – Using the mean difference may allow some cancellation of errors between the spectral points that are being averaged. It might be better to use the mean absolute value of the differences.

Section 4

10,15 – "exhibited" would be better than "exemplified". Also, "affect", not "effect".

*11, Fig 6 – I think that the result that is plotted is from a single case. If so, please label it as such. However, if true, that opens up a more serious critique. Until the paper gets to Table 1, all results shown and discussed are from an example, not from the full dataset. How can the reader know whether these results are representative of the entire dataset?

*12,9-15 – This section is puzzling. It is not up to a user's discretion whether to interpret the relative humidity measured by a sonde as being with respect to liquid or ice saturation pressure. This is determined by the sonde design and processing software, and is done with respect to liquid. Interpreting it with respect to ice is not correct. In addition, it is difficult to understand the logic behind the statement in lines 13-15 "(indirectly implies . . .)". Why would changing the poor results obtained from sonde water vapor profiles obtained by the method described in this paragraph have anything to do with applying the methods in this paper to other conditions? This entire paragraph should be deleted.

12,21 – Perhaps add a few words to clarify: ". . . the vertical resolution and assumptions made in our modelling approach without adding a chimney layer are sufficient . . ."

Section 5

*14,1 – The Rizzi et al. paper certainly shows that current spectroscopy is sufficient to match observations and is an improvement over previous spectroscopy. The results in this paper show nothing of the kind. Perhaps the LBLRTM results the authors performed also indicate this. However, these results have not been presented in this

manuscript.

*14,2-3 – The paper has not shown that an unbiased atmospheric state is essential to the approach that has been demonstrated. Water vapor profiles from sondes under very dry conditions are known to be biased (e.g. Miloshevich et al. (2009)), so the profiles that are used in this paper are likely far from unbiased.

*14,13-15 – As in the comment above, this really has not been shown. At best, the analysis about the water vapor saturation pressure over ice that is alluded to (but not really shown) suggests that this might be true, but this is far from being demonstrated.

14,24-29 – It is unclear what the results from this ground-based study have to do with the possible future FORUM mission. The authors should make their argument here more clearly or abandon it.