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

Anonymous Referee #2

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Overall impression

The authors note, correctly, the sparsity of far-infrared hyperspectral radiance measurements compared to the mid-infrared. Given the important role of the far-infrared in climate and energy budget studies it is a worthwhile endeavour to seek to improve our knowledge of the full infrared spectrum. The methodology of Turner et al. (2015), which entails using mid-infrared predictor channels to infer far-infrared spectral radiances, is applied to a dataset of Antarctic downwelling longwave radiances (REFIR-PAD). The paper tests whether the methodology can successfully extend/extrapolate mid-infrared spectra into the far-infrared.

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Discussion paper

Within certain conditions the authors show reasonable skill in predicting the FIR spectrum from MIR data. Notwithstanding some points about how this works in practice (see below) I think the general idea has applicability in climate science where FIR data are lacking. The authors note that there is renewed interest in the remote sensing community in spectrally resolved FIR measurements (specifically the candidate ESA mission FORUM). I hope the authors can address the more detailed points raised below.

Substantive points

1. Page 3, line 27. “In the study, only clear-sky cases from 2013 are used.” There may be good reasons for this choice, but these are not clear. Please explain why this data selection was made.

2. Methodology, page 5. There are various differences between the published method (T15) and the way it is applied here. In T15 IASI data (upwelling radiances) are used cf. downwelling REFIR-PAD data; T15 data are restricted (it seems, on quick read) to between 30S and 60N cf. cold/dry Antarctic conditions here; T15 find maximum correlations either side of peak water vapour absorption at 1600 cm-1 while REFIR-PAD is restricted to below 1400 cm-1. These are not commented upon. In particular I would like the authors to comment on the general applicability of their scheme for global climate studies when this paper deals exclusively with polar conditions.

3. The method relies on finding a single predictor MIR wavenumber for each FIR wavenumber. This is straightforward and as published in T15. However, there may be potential downsides to using single frequencies, particularly when the same predictor is selected multiple times and in cases of instrument noise. Have the authors considered whether a weighted average or linear combination of the most highly correlated channels might work better as a predictor?

4. Some plots (Figs 4, 6) show standard errors to represent the uncertainty. I am uncomfortable with this, as it implies the expected error in the method can be reduced to the mean error with an arbitrarily large sample. The uncertainty is better represented

as the example in Fig. 4 (a) where the difference between spectrum and extension in microwindows near 240/260/270 cm-1 is about 10 r.u. Elsewhere (Table 1) you quote one standard deviation which seems more appropriate to me.

5. Page 8, Eq. (3). You add noise according to the REFIR-PAD standard deviation, but you discount correlated errors e.g. the calibration error in Fig. 1. If you want to represent the instrument uncertainty shouldn't this be included?

6. You show (Fig. 5) very different choices of predictor channels when using noiseless and noise-added LBLRTM spectra (the latter look very like those found from REFIR-PAD in Fig. 2). You say in the discussion and conclusions that including the effects of noise is important to improve the prediction model. But I think this avoids the interesting question of why the noise-free LBL extension fails (but why LBN and LBC seem to work). Plausibly it comes down to the choice of predictor channels which are heavily skewed towards 1400 cm-1 in the LBL case. Just because channels are correlated doesn't mean they have a linear relationship. It might be interesting to see an equivalent of Fig. 3 for an LBL case.

7. If instrument (random) noise is critical in your method there may be benefit in noise filtering the measured spectra. Methods exist for doing this, e.g. via principal component analysis. I anticipate this may be beyond the scope of the present study but may be worth pursuing in future work.

Minor points

1. Page 2, line 9. Typo “vvvhave”.

2. Page 3, line 22. “One calibrated spectra”, strictly “spectrum”.

3. Fig. 3, I assume the dot-dashed line is a 1:1 line but this is not stated.

4. Page 10, line 8, you refer to “figure 5 (b)” when it looks like (c)?

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