

Atmos. Meas. Tech. Discuss., referee comment RC1
<https://doi.org/10.5194/amt-2022-37-RC1>, 2022
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Comment on amt-2022-37

Glen Jaross (Referee)

Referee comment on "Spectral replacement using machine learning methods for continuous mapping of the Geostationary Environment Monitoring Spectrometer (GEMS)" by Yeeun Lee et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-37-RC1>, 2022

I have difficulty understanding the goals of this paper. There seems to be a mismatch between what the authors are trying to achieve and the investigation they describe in the paper. The authors describe a problem on GEMS whereby "bad" pixels, i.e. missing pixel radiances, cause problems in the ensemble of measured radiances. These problems are not fully described. The authors refer to a previous approach to deal with the missing pixels involving radiance interpolation across the missing areas. That approach does not yield good results according to their description. There is no mention of what criteria are used to assess the results.

But what is the real problem that the authors are trying to solve? In an instrument such as GEMS, designed to measure trace gas composition of the atmosphere through hyperspectral measurements, the goal is probably to produce trace gas products without spatial gaps caused by missing radiances. Does anyone really care if the missing radiance fields are filled with more realistic values if they do not result in more accurate representations of the trace gas fields? The authors do not discuss the issue of trace gas retrievals or other products derived from their predicted radiances. Is there any improvement at all in those products?

Furthermore, the authors fail to discuss a more fundamental question. Is replacement of missing radiances with predicted radiances of any value? These are not measurements, yet they may be provided to the retrieval algorithms as though they are measurements. Is this what the producers of GEMS atmospheric products really want? Surely those producers do not want to report an atmospheric parameter as a measurement when in fact it is merely a prediction based on previous measurements. If their goal is to merely remove spatial gaps in maps of the trace gases, this can be accomplished more effectively and efficiently by manipulating the gas concentration values directly, i.e. spatial smoothing techniques.

The investigation the authors describe is not without merit. From an academic perspective the question of how well ML techniques can describe Earth backscattered radiances is an interesting one. If the authors approached this paper from that perspective they might provide a useful contribution to our ability to characterize the atmosphere through numerical techniques. But in doing so they must take a more rigorous approach to evaluating their radiance predictions.

The first thing the authors should do is to forget about the GEMS pixel defects. These are of no use in evaluating the efficacy of the technique since the true radiances remain unknown for these regions. Instead, choose regions of the detector where there are good measurements and treat them as missing for the purpose of deriving errors. There can be a variety of region shapes and sizes, including ones that look very similar to Defects 1, 2, and 3.

The authors must also devise evaluation criteria that are more robust and quantitative than "these spectra look realistic." Since the goal for GEMS radiances is to derive atmospheric products such as trace gases, perhaps these trace gas retrievals can be used as the metric. Merely stating that predicted radiances agree on average with measured radiances to within X% ignores the subtle spectroscopic sensitivity of trace gases such as NO₂, where the exact relationship between wavelengths is of utmost importance.

I hope the authors can revise this investigation so that the results are more meaningful. It has the potential to be an interesting paper, but in its current form it represents incomplete work.