The reviewed manuscript uses the spectrally resolved outgoing radiances from the IASI sounder to evaluate the output of the EC-Earth climate model. The identified biases are linked to the main variables of the Earth-atmosphere system. The scientific work is topical and the methodology is sound but, in my opinion, the paper lack of structure here and there and the presentation could be improved. The analysis of the results should be sometimes more detailed. To my point of view, it will be suitable for publication after addressing/clarifying the following comments.

General comments

I think it should be made more clear, before starting the analysis of the differences between IASI and the model, what can be all the causes of the differences (biases in modeled temperature or atmospheric concentrations, methodology: effect of the cloud filtering, time matching between IASI and model, etc.). Mentioning first all the potential reasons for the presence of biases would help greatly for the clarity of the manuscript.

Subsection 2.1.1: Could the authors describe a bit more how the EC Earth model works (maybe accompanied by a conceptual flowchart)? In particular, I am not sure to understand from where are all the prescribed parameters coming from. To me, it should be explained a bit more in the manuscript. Or at least, the authors should mention clearly in what paper an extensive description of the model can be found.

3.2 Model comparison strategy:

I think this section is not clear enough and should be partly rewritten.

- For instance, "The model was run with prescribed SSTs and SIE, […]" It is not clear to me where are these information coming from (see my previous comment).
- If I understood well, the authors built monthly climatology of IASI radiance (near-nadir). Only cloud free pixels are considered for the averaging. What is the grid size of these monthly climatology? 1°x1°? Then, for the comparison with the model, from these monthly climatology, the authors only consider the grid cells that show a monthly mean cloud cover (according to CERES) lower than 30%. Is that right?
- How was this 30% threshold chosen? Why not 40% or 20% for example? I think it should be better justified.
- A map showing the grid cells that are kept for the comparison would be nice.
- Also, in the IASI monthly mean radiances, how many measurements are present on average per month in each grid cell? If the number is too low (which might be the case for some of the pixels as only near-nadir observations are considered), the comparison with the model might be biased as a few number of observations is not representative of a whole month. This should be kept in mind for the analysis. It might be good to add a postfiltering to remove from the comparison the grid cells that contain too few observations.
- <u>Lines 252-253</u>: "In fact, monthly means of observed and simulated outgoing radiances are computed over the same spatial grid by associating IASI measurements to the nearest EC-Earth grid point.". I am not sure I got the idea here. If I understood well, for the time correspondence between the model simulations and the IASI observations, only the model output of the 6-12 AM and PM time are considered? Both are then averaged to obtain a "daily" mean (and same for IASI, ascending and descending nodes are averaged to get a daily mean?)? And this is the reason why you only considered sea measurements for the analysis (because of the very high diurnal cycle of the radiance over land)?

3.3 Assessment of EC-Earth biases in simulated clear-sky radiances with respect to IASI measurements:

The descriptions in this section should be more quantitative. For example, <u>line 274</u> (but also elsewhere): "[…] a small positive bias". Of how much?

I think some additional plots could help for the interpretation of the results:

- Are the biases the same every year or is there differences from year to year? Maybe a time series showing the differences between the model and IASI (averaged on the different bands WV1, WV2, … for example) would be nice. I am also wondering if there is a difference in the biases between day and night.
- Another plot that could greatly help for the interpretation of the results and could be a nice addition for the paper is global maps of the difference between IASI and the model for a few selected channels (instead of a zonal average of the difference) (as done in Whitburn et al. 2021 for example). In particular, this would allow to highlight potential compensating biases between different regions.
- It would be nice to see Fig. 3 in W m^{-2} to evaluate more easily the impact of the differences (maybe in appendix).

Whitburn, S., Clarisse, L., Bouillon, M. *et al.* Trends in spectrally resolved outgoing longwave radiation from 10 years of satellite measurements. *npj Clim Atmos Sci* **4**, 48 (2021). https://doi.org/10.1038/s41612-021-00205-7

Specific comments

Abstract

<u>Line 15:</u> If I am not mistaken, I think it is "Metop" and "MetOp". To be checked and replace everywhere.

<u>Line 18-19:</u> "[…] while a cold bias occurs over land". The authors didn't mention that they were talking about sea just before. Please clarify.

Introduction

<u>Line 41:</u> Missing references for AIRS and IASI.

<u>Line 46:</u> Also mention the existence of a spectrally resolved OLR product at 10 cm^{-1} derived from AIRS measurements (e.g. Huang et al. 2008, Chen et al. 2016).
https://disc.gsfc.nasa.gov/datasets/AIRSIL3MSOLR_6.1/summary?keywords=AIRSIL3MSOLR_6.1

<u>Appendix A:</u> I don't really see the added value of Appendix A for the paper. The information presented are very general and the different formula can be found in many atmospheric science books and published papers.

Data and Methods

<u>Line 91:</u> Please defined IFS.

<u>Line 132:</u> FCDR: missing citation. Also, mention that this dataset is reprocessed with the latest version of the L1C and is consistent over the whole time series. I guess this is why you chose to use it?

<u>Line 136:</u> […] the 8 pixels closest to the nadir view. Thus, the clear-sky […]. There is a problem with the transition between the two sentences here.

<u>Line 137:</u> AVHRR cloud dataset: cite Guidard et al. (2011). Is there a reason why this cloud product was chosen over the L2 cloud product? For consistency over the whole time series? Maybe it would be nice to justify it.

<u>Line 141:</u> missing citation for the CERES_SYN1deg product.

Results and Discussion

Line 180: "Figure 1 shows a spectrum of OLR [...]". It is a directional radiance that the authors show on Fig. 1, not the OLR (as the authors define it before in the manuscript, the OLR is the radiance intensities integrated over all the angle directions).

Line 181: the authors write "The spectral ranges measured by IASI and FORUM are highlighted". For IASI, the spectral range extend to 2760 cm^{-1} while Fig. 1 stops at 2250 cm^{-1}. Why stop at 2250? It should be mentioned in the text.

Same for line 185: "we focus here on [...] by IASI (645-2760 cm^{-1}) from 2006 onward" while in fact the authors limit to 2250 cm^{-1} and focus on the period 2008-2016.

Line 189: when discussing the emission level, the authors could cite for example the paper of Whitburn et al. (2021) on the trends in spectrally resolved OLR.

Line 202: I agree that AW1 is more transparent than AW2, but to me this is not clearly visible on Fig. 2.

Fig. 2:
- It would be nice to add dashed lines to separate the different spectral regions on the two panels. This would help for the visualization.
- N_2O is not visible on the bottom panel (I guess it is below CH_4). Also, CH_4 and Temperature should have different colors.

Line 260: Why are the limits set to 60° N/S. This should be justified.

Line 270: Mention this is a zonal average.

Line 282: If the bias in the O_3 band is due to the bias of the temperature of the lower atmospheric layers, I don't understand how it could be higher than the biases observed in the window regions?

Line 284: Why is the focus on tropical regions? I think it is important to justify it more clearly.

Line 285: Is it a weighted average on the Figure 4? I guess this is not so important because the focus is on tropical regions, but it might change slightly the results. If I understand correctly, Fig. 4 is simply an average of Fig. 3?

Line 290: Is the comparison with CERES based on a daily average OLR?

Lines 292-298: I agree with the explanation, but I think this paragraph should be moved elsewhere in the discussion.

Fig. 3: Maybe Fig. 3 and 4. could be merged into a two panel figures.

Line 307: What are the CO_2 concentrations used? (I mean, where have they been taken from).

Line 312: "[...] for the selected spectral channels of Table 2" → "averaged over the spectral bands of Table 2."

Fig. 5 and lines 313-319: To me what is especially interesting in fig. 5 is more that the bias seems rather constant over the year, except for panel D where a positive then a negative bias is observed with indeed an amplified seasonality. I think this change in the sign of the bias should be investigated.

Fig. 5: Panel C. Why this almost 1K bias is not visible on Fig. 3 and 4.

Lines 320-330: Here I think maps of H_2O columns (from ERA5 for example) could greatly help for the interpretation of the results. Same for Ozone.

Fig. 7 (caption): Distribution of sea surface temperatures from model?

Line 356: ERA5 analysis: what latitude/longitude considered? Sea only? Why is the period considered not the same as in the rest of the paper (2008-2016).

Technical corrections

Line 52: Missing year in the ref to Harries et al.

Line 112: O3, CO2, N2O, […] Format the text to put all numbers in indices.

Line 124: 9.30 AM and PM.

Line 208: "in the wings of the […]"