Reviewer 1:

This is a very interesting work that extends the diagnoses of the important OLR-Ts relationship in ENSO from broadband to spectral radiation. The development and application of a spectral kernel dataset is a notable strength (despite some questions detailed below). The paper shows that spectral data makes it possible to view the lagged OLR-TS relation with more clues relatable to the geophysical variables at the process level, which makes an important finding. I would recommend publication if the following comments were addressed.

- The Introduction gives a good literature review to motivate this work, with a rather complete collection of relevant previous works, despite some misses, for example, Huang and Ramaswamy 2008 (https://doi.org/10.1029/2008GL034859) which was one of the earliest data-based diagnosis of spectral OLR-Ts relationship.

We thank the reviewer to highlight this reference that we missed. It contributes to set the basis of the work and will be included in the introduction to expand the present literature review.

We have expanded the present literature reviews including the reference suggested by the reviewer in the introduction of the revised manuscript (see lines 49-50).

- On the other hand, I found the paper does not make as strong connections with the previous works when discussing the results, which potentially impedes the revelation of the novelty or difference in the findings here. For example, much emphasis of the paper is on the lagged OLR-Ts relation, for which the spectral signatures are related to geophysical drivers (Figs 2/3 and texts around Line 200). However, these findings were similarly made based on broadband kernel-decompositions (e.g., Fig 3 of Kolly & Huang). Given the objective of this paper is to demonstrate the advantage of spectral information, such comparisons can be used to discuss what additional information is brought in by the spectral data.

This touches on a very critical point of the analysis, specifically, its ultimate goal and we want to be as straightforward as possible in communicating this. The discussion of the results, as emerged from the review, needs to be improved to make this point clearer and stronger. With respect to previous studies of ENSO-driven radiative feedbacks that used broadband longwave (LW) fluxes (for example Kolly and Huang (2018)), we would like to emphasize two main outcomes. The use of the spectral dimension allowed us to investigate the spectral (wavenumber) dependency of the OLR response and the vertical propagation of ENSO signal. This information is directly contained in the measurement, without the need of decomposing it through the kernel analysis. The decomposition is performed to study the origin of the signal in the different spectral regions. In addition, as also performed in Kolly and Huang (2018), this additional information can be used to evaluate climate models performances. These points will be discussed in more depth and with more attention in relation to the existing literature when preparing the revised manuscript.

In the revised manuscript the discussion of the results (Section 4) has been rewritten including comparisons with similar results reported in the literature, specifically those obtained by Kolly and Huang 2018.

- A main, technical comment is on the kernels themselves, which are computed based on monthly profiles (Line 120) – a simplification known to introduce biases (e.g., see Huang and Ramaswamy 2009, https://doi.org/10.1175/2009JCLI2874.1). It remains to be demonstrated how well the kernels produced here explain the total spectral OLR changes. Although it is qualitatively discussed (Line 210), the biases are not quantified – better to show the residuals in closure tests of different lags. Another suggestion is to compare the kernels produced here to other kernels. For example, the broadband radiative sensitivity values spectrally integrated from the spectral kernels should reproduce broadband ERA5 kernels truthfully computed from instantaneous profiles (Huang and Huang 2023, https://doi.org/10.5194/essd-15-3001-2023). Such comparisons can be made with respect to maps of vertically integrated kernel values and with kernel-reproduced lagged OLR-Ts relationship (Fig 1). This would help provide a measure of the uncertainty in the results.

We agree with the reviewer that a closure test would be needed to quantify the biases between the observed OLR response and the one reconstructed using the kernels method.

We are aware of the shortcomings that arise from the use of monthly rather than instantaneous profiles to calculate radiative kernels. Although this simplification mostly impacts the accuracy of kernels when the all-sky scenario is considered, we performed a comparison between our kernels and the ones computed by Huang and Huang 2023 to confirm this preliminary consideration. Two tests have been made: radiative kernels used in this work have been compared against kernels provided by Huang and Huang 2023; then we computed the spectral kernels with input profiles at a higher temporal resolution. Specifically, in the new calculation, we sampled 20 time steps (corresponding to 5 days at 6-hour intervals) during one month and we averaged the derivatives computed at each time step. As an example, we show a comparison of the surface temperature kernels computed for a single month with the corresponding kernel computed by Huang and Huang (2023) integrated on a similar spectral range, Huang and Huang (2023) from 10 to 2600 cm^{-1} and this work from 105 to 2755 cm^{-1}. In the left map of Figure 1, the percentage differences are shown using the new computation, while on the right we show the results using the monthly average surface temperature.

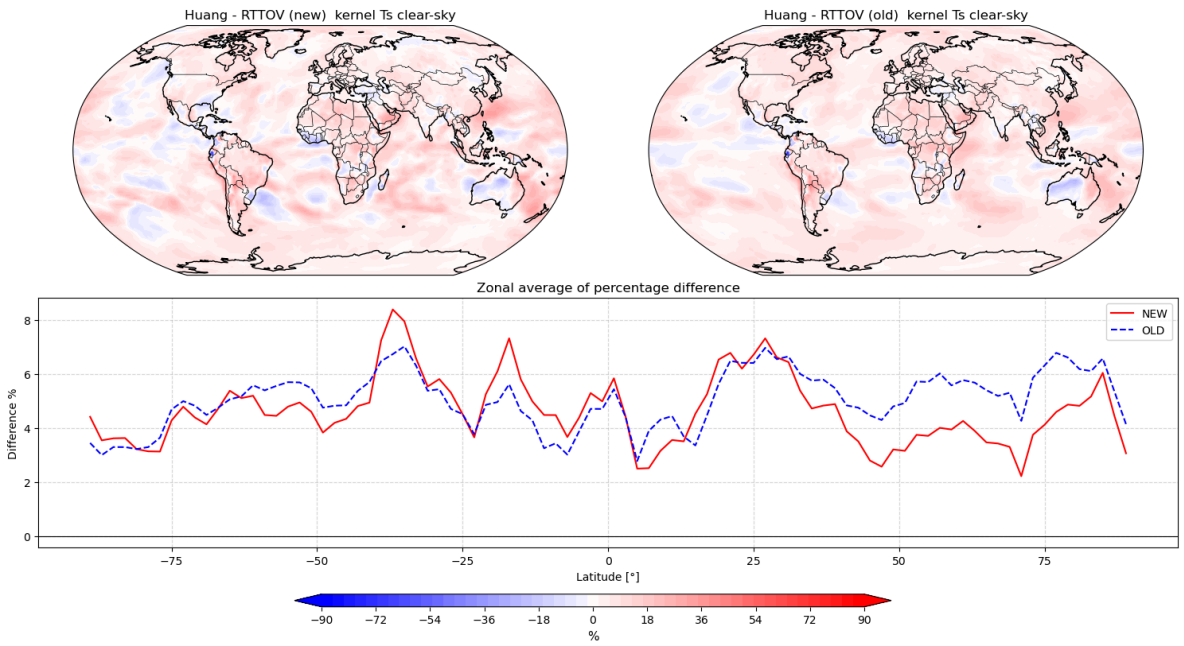

Figure 1. Differences between clear-sky surface temperature radiative kernels computed using hourly (new) and monthly (old) data with respect to radiative kernels computed by Huang and Huang (2023).

Even though this is only an example limited to a single variable, the differences are small and do not appear to be significant for the results of our analysis. Nevertheless, we will explore this aspect in more detail and discuss it further in the new version of the manuscript.

We are satisfied with the outcome of the comparison between the set of radiative kernels used for the present study and that developed by Hunag and Huang 2023. For the clear-sky scenario the differences obtained using monthly profiles yields minimal differences (lower than 1%) with respect to sampling variable fields every 6-hours.

Furthermore, we evaluated the differences between the total spectral response reproduced by radiative kernels and that obtained from observations, as suggested by the reviewer. Results are reported in Figure 2 for the tropical mean and in Figure 3 for the spatial patterns. Furthermore, a table comparing the total LW ENSO feedback obtained within different spectral regions for AIRS, IASI and the spectral kernels has been included.

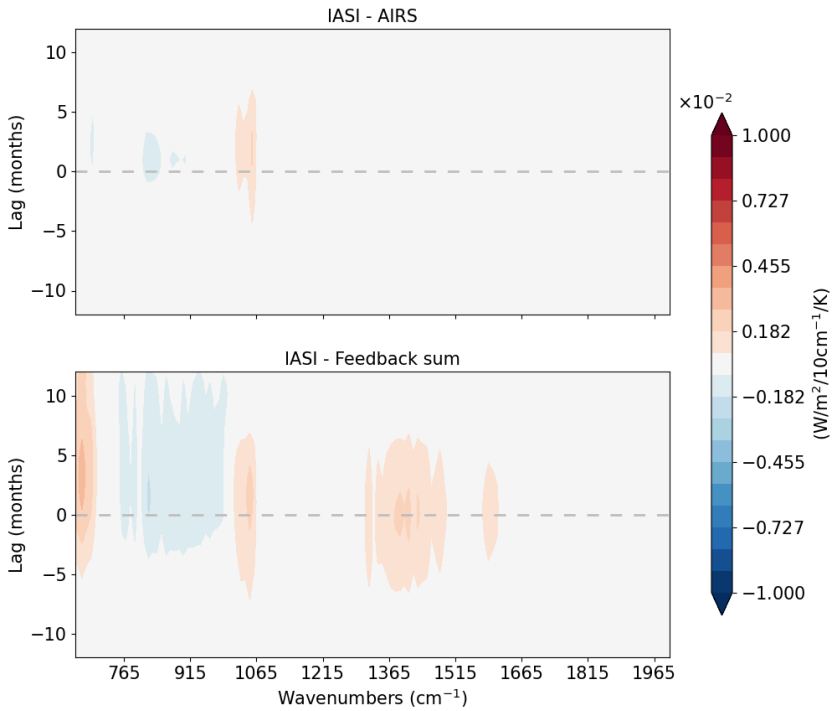

Figure 2. Differences between the total longwave response obtained from observations (IASI) and that reconstructed using spectral radiative kernels (Feedback sum) for the tropical mean.

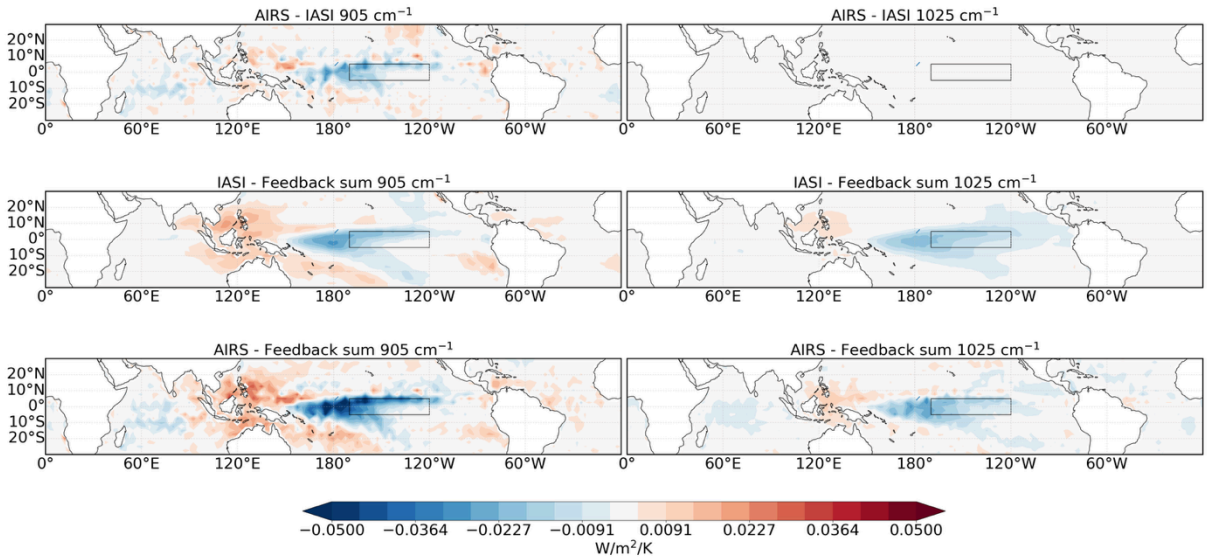

Figure 3. Spatial pattern of the total longwave OLR response to ENSO at 905 cm-1 and 1025 cm-1. Differences between AIRS and IASI (top panels), IASI and the feedback sum (middle panel) and AIRS and the feedback sum (bottom panel).

- Given the results are exclusively for clear-sky. It is better to indicate this in the paper title.

We agree with the reviewer and will modify the title accordingly. A new title could be: "An analysis of ENSO driven OLR variability in clear-sky conditions from a spectral point of view" or "A spectral perspective of the clear-sky OLR variability driven by ENSO"

A new title highlighting that the analysis is based on clear-sky OLR has been chosen: “A spectral perspective of the clear-sky OLR variability driven by ENSO”.

- Lastly, there are numerous grammatical errors. The English writing needs to be thoroughly proofed/edited.

We thank the reviewer for the accurate reading. The revised manuscript will undergo a substantial revision/rewriting with respect to English grammar and terminology.

The grammar and clarity of the sentences have been improved by the substantial rewriting of the revised manuscript.

Reviewer 2:

The authors investigate the radiative response to ENSO from a spectrally resolved perspective. The underlying idea is that spectrally resolved OLR holds information about individual contributions to radiative changes from temperature, humidity, and ozone, which cannot be resolved by broad-band OLR. The authors compute spectral kernels combining ERA5 data with a radiative transfer model. Comparing the spectral kernels with the satellite observations allows to identify changes in surface temperature, air temperature, humidity, and ozone, and to further decompose these changes into contributions from different atmospheric levels. The authors find the changes one would expect from ENSO in the spectrally resolved OLR product.

I read this paper with great interest and recommend publication of the study under the condition that the comments below are addressed. I divide my comments roughly into major, minor, and suggestions that I don't require to be addressed. I have some expertise with radiative feedbacks and tropical dynamics, but almost no experience with satellite observations.

"major"

- The authors missed the opportunity to strictly decompose the observed spectral OLR into contributions from the individual feedback processes using spectral fingerprints, as was done for example in Huang et al. 2010 (https://doi.org/10.1029/2009JD012766). While I would be very interested to see this, it would be exaggerated to ask the authors to perform this analysis, and the paper contains valuable insights even without this. However, I still mention this here, because it implies some limitations to the interpretations that the authors give.

We thank the reviewer for the suggestion. Indeed, Huang et al. (2010) used an optimal detection method to calculate the spectral fingerprint of the climate change induced by a doubling of the CO_2 concentration. They use the partial radiative perturbation (PRP) method as a reference to test optimal detection results. While in this work we calculate a spectral fingerprint by using spectral radiative kernels – an alternative method to PRP to separate individual contributions of radiative feedbacks – the application of a similar optimal detection method, as the one described in in Huang et al., (2010), would allow us to directly calculate radiative feedback from IASI and AIRS observed spectra without the use of radiative kernels. This is an interesting opportunity that can be explored in the future.

- l. 9 – 10: This is the first example where it matters. I strongly suggest to reword this sentence in the abstract because there was no strict mathematical "decomposition". Rather, the satellite observations were compared by eye to the kernels to identify the feedback contributions.

We agree that the term decomposition might be misleading in this context. We will modify this throughout the manuscript.

The sentence has been rewritten as: "The spectral fingerprint of water vapor, surface and air temperature, and ozone feedback is then calculated using a set of spectral kernels to evaluate the role of individual processes building the overall response" (lines 9-10).

- Huang et al. 2010 should be cited in the introduction as an example for how spectral OLR can be used to identify individual feedbacks

This article will be mentioned to expand the current discussion with a description of the different methods that can be used to study radiative feedbacks. As previously mentioned, the main advantage of this study is that it enables the spectral fingerprint of radiative feedback to be estimated directly from the observed data. Thank you for bringing this to our attention.

The suggested reference (Huang et al. 2010) has been added to the present literature review (see lines 55-56).

- l. 69, l. 329: I don't quite understand what is meant here by "diagnostic". If it refers to the decomposition into feedbacks using spectrally resolved OLR, then this should not be referred to as new, because it has already been done by Huang et al.

In the context of lines 69 and 329, we would like to emphasize the future implications of this study, which is the comparison of the ENSO spectral fingerprint obtained from data such as IASI and AIRS spectral fluxes to the same quantity simulated by general circulation models (GCMs). The term "diagnostic" refers to the spectral fingerprint of ENSO obtained from IASI and AIRS observations, which can be compared to that reconstructed from climate model outputs. As the original sentence was too generic, the revised manuscript will expand on this aspect, in a similar way to what has been done here.

We removed the sentence from the introduction (line 69) and provided more context in the Conclusion section to clarify the term "diagnostic":

"This work sets the basis for a future investigation of ENSO-induced variability of the OLR as simulated by climate models. The spectral dimension will allow to disentangle the various processes contributing to ENSO OLR response avoiding compensating errors that may arise from the use of broadband OLR. In an upcoming study, we plan to apply this diagnostic to evaluate the climate models participating in the Coupled Model Intercomparison Project Phase 6 (CMIP6)" (lines 368-373).

- l. 115 – 122: It is not clear to me how the kernels are computed from the data. In particular, I would like to know how the derivatives were computed. A (finite) partial derivative needs radiation computations from at least two different atmospheric states, and I don't understand from the description which these are. I assume that in each partial derivative computation only one variable was perturbed, but how / to what value?

The finite partial derivatives have been calculated using the RTTOV model, for which this calculation is implemented. More details on the computation of the spectral kernels used for this work are now provided by Della Fera et al., (2025).

Section 2.2 has been rewritten to clarify the points highlighted by the reviewer. In addition, the reference to a more detailed description of the computation of the spectral radiative kernels employed has been added (line 111).

- l. 155: I found no further investigation of this, can you point me to where this is done?

We thank the reviewer for pointing this error out. Indeed, this is mentioned in Section 3.4, Lines 235–236, and not in Section 4 as stated. The correct reference will be inserted in the revised manuscript. However, we would also like to take this opportunity to highlight the implications of the different clear-sky scene selection of AIRS and IASI in the Discussion or Conclusions section.

The sentence has been rewritten as: "This particular aspect will be more evident in Section 3.4, when we will examine the spatial pattern of the ENSO feedbacks" (line 210-211).

- All figures: "slope" is not a very descriptive y-label. l. 110 – 112 state that this slope is referred to as "ENSO feedback", so maybe "feedback" in Fig. 1, or "spectral feedback" in Fig. 2-4 could be an option?

We agree that "feedback" or "spectral feedback" is a more descriptive label that could improve the readability of figures showing the regression coefficients of lagged regressions. We take the suggestion made by the reviewer and the term "ENSO feedback" will be used as the y-label in Figure 1, while "spectral feedback" will be used in Figures 2–4.

The label of the y axis has been changed from "slope" to "ENSO feedback" in Figures 2-5.

- Figs. 2, 3, 4: The color map should have linearly increasing lightness and brightness. The current color bar artificially suggests strong gradients where there are none. For example, with increasing values the colors get darker until 0.455 in Fig. 2 and then brighter again, creating false perceptions of the actual values. Some hues even seem to repeat. I find https://doi.org/10.1038/s41467-020-19160-7 to be a very helpful resource for picking a color map.

We thank the reviewer for suggesting a resource for contour plot color bars and for highlighting this important aspect of presenting our results. In the revised manuscript a proper color bar with linearly increasing colors will be used to improve the readability of figures showing a gradient.

All the plots and map have been modified using a linearly increasing color map.

- Figs. 2 and 3 should use the same value range so that they can be accurately compared to each other

As well as a more proper color bar, the same value range will also be used to enable a better comparison between different plots.

All the color bars of the contour plots have been set within the -1.0×10^{-3} and 1.0×10^{-3} value range to ensure a straightforward comparison.

- l. 182: The fact that there is a transition from analysing satellite observations to analysing something derived from the spectral kernels and reanalysis should be made clear.

This passage will be highlighted at the beginning of Section 3.3 “Atmospheric drivers of the radiative response”.

The transition from the analysis of satellite observations to a simulated product has been highlighted at the beginning of Section 3.3:

“The spectral fluxes of IASI and AIRS allowed us to study the total LW ENSO feedback associated with characteristic spectral regions and relate it to atmospheric and surface variables to which radiation is sensitive to. Now, we use the spectral radiative kernels to calculate the individual radiative contribution associated with changes in surface and atmospheric temperature, water vapor and ozone, to investigate the origin of the ENSO signal in more depth” (lines 212-216).

- Section 3.4, Fig. 4, also l. 284-286: How sure can we be that this signal comes from ozone? While this spectral band surely represents ozone where ozone is optically thick, the positive values in the Eastern Pacific could also originate from increased surface emissions through the atmospheric window if ozone is not optically thick there. A fingerprint analysis might have been able to clearly separate between ozone emissions and surface emissions. Given that this has not been done, can we be sure how much of this is ozone signal and how much is surface signal? My doubt is intensified by the fact that the 905 cm^-1 channel which purely represents surface emissions also shows increases in the Eastern Pacific.

This is an important question to be addressed for discussing the results of our analysis. We provide additional results below that may help with the discussion. Figure 3 shows the feedback across the Tropical Pacific at 905 cm^{-1} (left panel) and 1025 cm^{-1} (right panel). We want to recall that spectral kernels (as well as IASI and AIRS spectral fluxes) provide the flux integrated over 10 cm^{-1} bands ($W/m^2/10\ cm^{-1}$). Therefore, the two wavenumbers selected are not single spectral channels.

At 1025 cm^{-1} the signal of ozone and Planck surface feedback are simultaneously present in the central and eastern tropical Pacific. While their magnitude is almost the same, their spatial pattern differs. The signal related to surface temperature is limited to the equator, while the ozone signal extends off to the north and south of it. Instead, at 905 cm^{-1} the Planck surface feedback is higher in magnitude than at 1025 cm^{-1} and the ozone feedback is almost null. Based on the kernel analysis, it can be concluded that at 1025 cm^{-1} both the Planck surface and ozone feedback contribute to the TOA OLR response to ENSO, but the ozone feedback dominates.

To make this point clearer and help our discussion, we added the maps reporting the spatial pattern of all the ENSO feedbacks at the two selected spectral channels, 905 cm^{-1} and 1025 cm^{-1} (Figure 3). These maps show the region of the Tropical Pacific Ocean where each feedback originates.

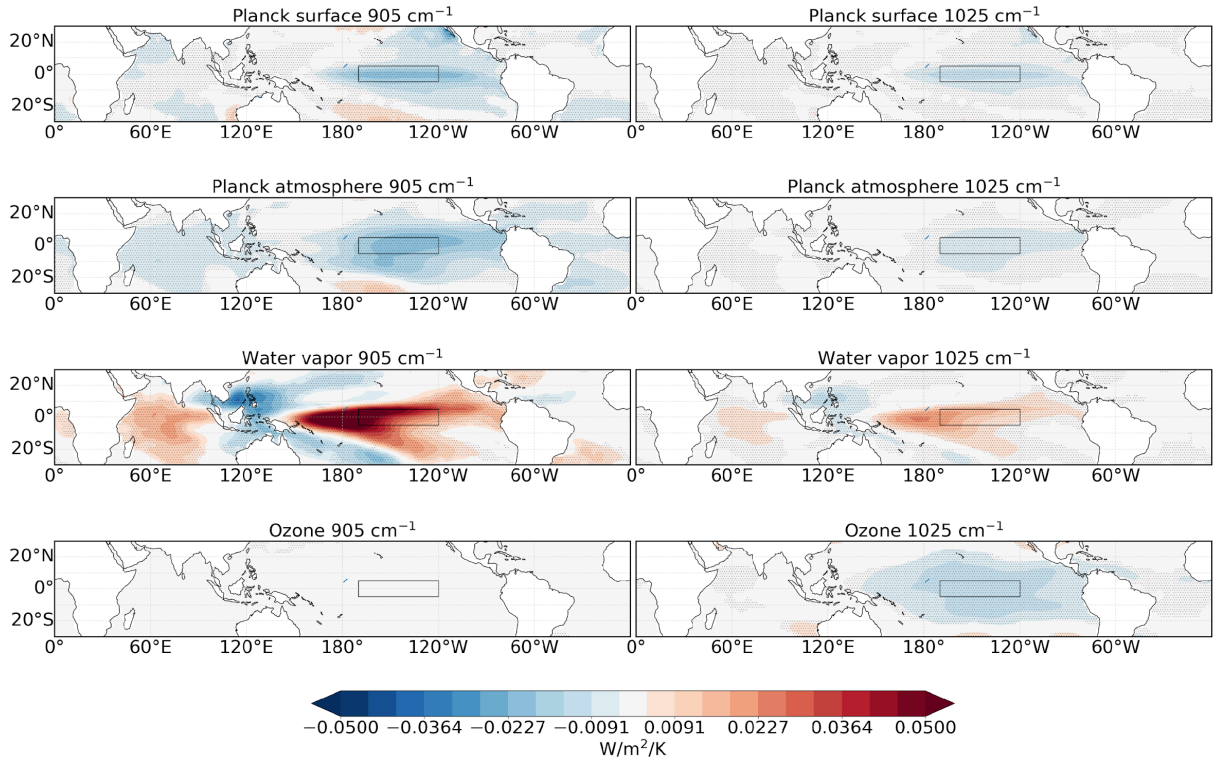

Figure 3. Spatial pattern of the Planck surface and atmosphere, water vapor and ozone ENSO at 905 cm^{-1} and 1025 cm^{-1}.

- l. 272: The humidity changes from ENSO are complicated and may also arise from the generally warmed atmosphere. Without further analysis or citations it is not totally clear to me that the positive water vapor feedback comes from the ascending branch of the Walker circulation. In particular in the dry regions, radiation is also extremely sensitive to small humidity changes. Furthermore, even though this study treats clear-sky regions, cloud (un)masking can play a role for apparent water vapor feedback.

We thank the reviewer for the insight. We agree with the reviewer that the conclusion drawn from line 272 is oversimplifying, we will rephrase and extend the discussion on this point in the revised manuscript to consider other possible explanations.

The sentence has been rewritten as:

"It is important to note that water vapor concentration is affected by humidity changes arising from the anomalous atmospheric circulation during ENSO Susskind et al. (2012)" (lines 303-304).

- l. 277-279: Where does this statement come from? Why tropospheric cooling? Doesn't an increased OLR in the FIR imply a warmed troposphere? The sentence as a whole is confusing to me. Cooling cannot maximize in a spectral region, only radiation can.

This statement is incorrect as far as it is formulated and misleading, we therefore thank the reviewer for commenting on that.

Within the preparation of the revised manuscript we decided to remove this sentence.

- Major edits are needed with respect to writing and grammar

We thank the reviewer for the accurate reading. The revised manuscript will undergo a substantial revision/rewriting with respect to English grammar and terminology.

The grammar and clarity of the sentences have been improved by the substantial rewriting of the revised manuscript.

- I suggest to indicate that clear-sky OLR is studied (and not simply OLR) in the title

We agree with the reviewer that a change in the title is necessary, given the implications of the use of clear-sky observations (e.g. different cloud masking). As already anticipated within the answers to the first review, a new title could be: "An analysis of ENSO driven OLR variability in clear-sky conditions from a spectral point of view" or "A spectral perspective of the clear-sky OLR variability driven by ENSO".

A new title highlighting that the analysis is based on clear-sky OLR has been chosen: "A spectral perspective of the clear-sky OLR variability driven by ENSO".

- l. 48: reproduced by models, I assume?

We thank the reviewer for pointing this out. The sentence refers to climate models performances. It can be rewritten as: "Indeed, there are evidences that the radiative response to ENSO is still not completely reproduced in both its amplitude and timing by climate models (Kolly and Huang, 2018; Planton et al., 2021; Ceppi and Fueglistaler, 2021)."

The sentence now reads as follows:

"Indeed, there are evidences that the radiative response to ENSO is still not completely reproduced in both its amplitude and timing by climate models (Kolly and Huang, 2018; Planton et al., 2021; Ceppi and Fueglistaler, 2021)" (lines 61-63).

- l. 77: most of the paper is written in terms of wavenumber, but here wavelength is used. I suggest to stick to one, preferably wavenumbers

We agree with the reviewer that using the same term throughout the text would be preferable.

The sentence in question has been modified accordingly:

"CERES is a broadband radiometer (Wielicki et al., 1996) with three channels that measure radiances across the whole spectrum (50 – 3000 cm^{-1}), the shortwave (SW, 2000 – 3000 cm^{-1}) and the atmospheric window (830-1250 cm^{-1}) regions respectively" (lines 97-99).

- l. 123 – 134: This is all fine but was hard for me to understand. It could be due to my lacking expertise of satellites. For example, I only understood in the Discussion section that FORUM is a satellite mission, and even a quick google didn't help me understand what the "synthetic" in "synthetic radiance" refers to (although I could figure it out eventually). This paragraph could be improved with small edits.

The term "synthetic" in "synthetic radiance" indicates that the radiance has been simulated. This comment will be taken into account when the paragraph is improved, as the reviewer suggested.

The RTTOV model is customized to simulate radiance measurements of specific instruments operating in different spectral regions, from visible to microwaves. In our case, we exploited measurements of two different instruments to calculate the derivative across the Earth emission spectrum from 100 to 2300 cm^{-1}. We simulated radiances acquired by FORUM (Far-Infrared Outgoing Radiation Understanding and Monitoring) and IASI instruments, using their respective instrumental functions. FORUM is the Earth Explorer-9 selected by ESA, designed to cover the spectral range from 100 to 1600 cm^{-1} (Palchetti et al., 2020). While IASI measurements sample the spectral range from 650 to 2300 cm^{-1}.

However, we decided to limit the kernels description to the dataset features and leave the reader, if interested, to a more detailed description of the calculation of the spectral radiative kernels, which can be found in Della Fera et al. (2025).

- l. 161: "maps": they are not maps

The description of the contour plots has been changed. For example, the description of the figure in question in the revised manuscript is: "Figure 3 shows the spectral ENSO feedback as a function of wavenumbers and time lag (panels a and b), and for a time lag of two months (panel c), where the broadband response peaks" (lines 181-183).

"**suggestions"**

- I don't require these to be addressed.
- l. 195: This is really counter-intuitive, because a positive value corresponds to a negative feedback. I see how this arises from the fact that OLR is defined positive outward, which makes sense. Still, can this confusion be overcome? Maybe by showing -dOLR/dT in the figures, or by referring to net downward LW instead of OLR, or by explicitly stating that a positive value implies a negative feedback?

We agree with the reviewer that the opposite sign of the results presented by the contour plots makes it difficult for their comprehension, as well as their discussion. Therefore, we plan to show -dOLR/dT in the figures of the revised manuscript.

In the figures of the revised manuscript we decided to plot the quantity -dOLR/dT. As such the sign of the ENSO feedback is consistent with that used for the climate feedbacks: positive for destabilizing feedback and negative for stabilizing ones.

- Fig. 2 first panel would profit from a line at y=0
- l. 329 – 333: Without finger printing, it is not clear to me how this method can be used for a quantitative analysis to evaluate climate models. Furthermore, when comparing Figs. 2 and 3 it seems that ERA5 does a good job, except in the FIR where the satellite anyway doesn't really know what's going on, because it's extrapolated. So how would this add anything beyond directly comparing the feedback components due to surface temperature, air temperature, water vapor, etc. between ERA5 and climate models? These are directly available from the output.

We appreciate this comment because it gives us feedback on how clearly we explained the main objective of our analysis. Secondly, it enables us to improve our explanation. In

this regard, we want to emphasize that the actual diagnostic is that provided by IASI and AIRS observations. The kernels analysis is used to gain a deeper insight in the information provided by spectral satellite observations and to evaluate how well we can reconstruct the spectral response with the kernel technique. In addition, as the reviewer points out, the calculation of radiative perturbations driven by changes in specific climate variables can be performed using the simulated variable fields and radiative kernels of climate models, as we did with ERA5.

- l. 319 – 323: it could be made clearer that "The sum of the Planck surface, atmosphere, water vapor and ozone feedback" refers to ERA5 and the "observed signal" to the satellite.
- In my opinion, the spectral kernels are the coolest contribution of this paper to the scientific community. It would be amazing to have them publicly available. :)

We are glad to see that the reviewer is interested in this data product.

We added to the Data Availability statement the link where spectral radiative kernels used for this work can be downloaded:

"The spectral kernels used in this study are publicly available on Zenodo (https://doi.org/10.5281/zenodo.15487604)".

Reviewer 3

I read this manuscript with great interest. The study addresses the radiative response to ENSO from a spectrally resolved perspective, which provides valuable insights into the contributions of individual processes. By combining CERES broadband observations with spectrally resolved measurements from AIRS and IASI, and by employing radiative kernels derived from ERA5 reanalysis, the authors disentangle the roles of temperature, water vapor, and ozone in shaping the clear-sky OLR variability associated with ENSO. The spectral dimension is a particularly valuable addition, as it helps separate overlapping processes that cannot be distinguished in broadband analyses.

I found the introduction to be well written and comprehensive, setting a solid foundation for the work. However, the remainder of the manuscript often lacks rigor, and the quality of the English requires substantial revision.

My comments are structured in three categories: General**,** major**,** and minor**,** as detailed below. I would recommend publication of this manuscript provided that the authors carefully address the issues raised in this review.

General comment:

- While the study offers interesting insights into the spectral perspective of ENSO-driven OLR variability, I found that several aspects of the analysis lack the level of rigor and quantitative support.

Thank you for your careful reading and highlighting these important issues that we had overlooked.

The analysis has been improved including other statistical quantities for the evaluation of the results, as well as more direct comparison between results obtained from observations and the kernels analysis.

- First, the comparisons presented in the manuscript are mostly qualitative. For example, differences between instruments or between kernels and observations are described verbally but rarely quantified. Providing numerical values (e.g., absolute differences, percentage differences) or plots of differences (e.g. for Fig. 2 and Fig. 4) would significantly strengthen the robustness of the analysis.

The first objective of our analysis was to explore OLR variability driven by ENSO from a spectral point of view. As the reviewer suggested, taking a more quantitative approach — for example, quantifying the differences between the observational datasets used, and between the observations and the reanalysis results — would lead to stronger conclusions. Therefore, this aspect will be carefully checked before the submission of the revised manuscript.

A table reporting the value of the ENSO feedback obtained from IASI and AIRS observations and the spectral kernels has been included to provide a quantitative comparison and evaluation of the results.

- Second, unless I overlooked it, the authors do not discuss the impact of the difference in overpass time between AIRS and IASI as a potential explanation for the discrepancies observed in the atmospheric window regions. Even when

restricted to ocean-only observations, such differences have already been reported in the literature (e.g., Whitburn et al., 2020), and one likely contributing factor is indeed the distinct overpass time of the two instruments. This issue should be explicitly acknowledged and discussed.

We thank the reviewer for bringing this to our attention. Since the analysis results obtained by IASI and AIRS show differences within the atmospheric window (between 800 and 1000 cm^{-1}), the impact of the different overpass time of the two instruments can be discussed as a potential cause for that discrepancy. As shown by Whitburn et al., (2020) this should have a small impact on the spectral fluxes over the ocean, however, as suggested by the reviewer this issue should be acknowledged. This will be added within the "Data and Methods" section, when the datasets employed for the analysis are described.

When the differences between IASI and AIRS responses are discussed in Section 3.2 now we also mention as a possible explanation the different overpass time of the two instruments:

"One factor to consider is the difference in overpass times between AIRS and IASI. This could partly explain the observed bias within the atmospheric window, where radiance is more sensitive to surface temperature (Whitburn et al., 2020)" (lines 204-206).

- Third, it is important to note that Metop-A started drifting from its orbit in June 2017. The manuscript does not discuss the potential impact of this drift on the results. Considering Metop-B data from 2018 onwards could help avoid potential biases. Addressing this point would increase confidence in the robustness of the conclusions.

The orbital drift of Metop-A could impact the overpass time. This in turn would affect radiative fluxes within the atmospheric windows, which are the spectral regions that are more sensitive to the surface temperature. In addition to the different overpass times of IASI and AIRS, the orbital drift of the Metop-A will be mentioned as a potential cause of the bias observed between the two data products. However, we believe that the discrepancy between the IASI and AIRS data products is most likely the result of different cloud masks applied.

We have acknowledged that the Metop-A satellites drifted from its orbit since 2017:

"It should be noted that the MetOp-A platform began drifting from its nominal orbit in autumn 2017, resulting in an earlier local time of the ascending node" (lines 95-96).

Major comments:

- Lines 104-105: The manuscript mentions "[...] removing [...] the linear trend for the whole period." Could the authors provide more details on the method used to remove the linear trend? In particular, it would be important to confirm and explicitly emphasize in the manuscript that this step is intended to remove the effect of increasing greenhouse gas concentrations over the study period.
  At the same time, the authors should discuss the implications of this methodological choice. Assuming a uniform linear trend may not fully capture the real evolution of the climate system, which can be non-linear (e.g., acceleration of

warming in recent decades). There is also a risk that part of the signal interacting with ENSO could be inadvertently removed. While detrending is a reasonable approach to isolate interannual variability, the rationale and limitations of this choice should be clearly stated.

The linear trend (LT) of the OLR anomaly has been calculated in the following way. A linear regression has been performed between the OLR anomaly time series and the monthly dates:

$$OLR\ anomaly\ TS\ =\ m \cdot monthly\ dates\ +\ q$$

Where, *OLR anomaly TS* is the time series (TS) of the OLR anomaly from 2008 to 2020 and *monthly dates* are the years from 2008 to 2020 considered by the analysis. The regression coefficients *m* and *q* obtained have been used to calculate the linear trend (LT):

$$T\ =\ m \cdot monthly\ dates\ +\ q$$

the LT reconstructed has then been subtracted to the OLR anomaly TS:

$$OLR\ anomaly\ TS\ detrended\ =\ OLR\ anomaly\ TS\ -\ LT$$

To discuss the implications and limitations for the analysis, the lagged regressions of IASI and AIRS data will be performed for both the OLR anomaly TS with and without the LT removed.

We compared the difference between the lagged regressions performed on OLR without removing the linear trend (Figure 4) and detrend OLR anomaly (Figure 5) , the difference is reported in Figure 6. The most remarkable difference is observed for the carbon dioxide absorption band, where the detrending leads to a slightly higher ENSO signal. In the revised manuscript we decided to present the results obtained using the detrended OLR anomaly, motivating our choice as suggested by the reviewer:

"In addition, the monthly OLR anomalies have been detrended by subtracting the linear trend, in order to isolate the internal variability from the signal of increased GHG concentrations over the given period" (lines 134-136).

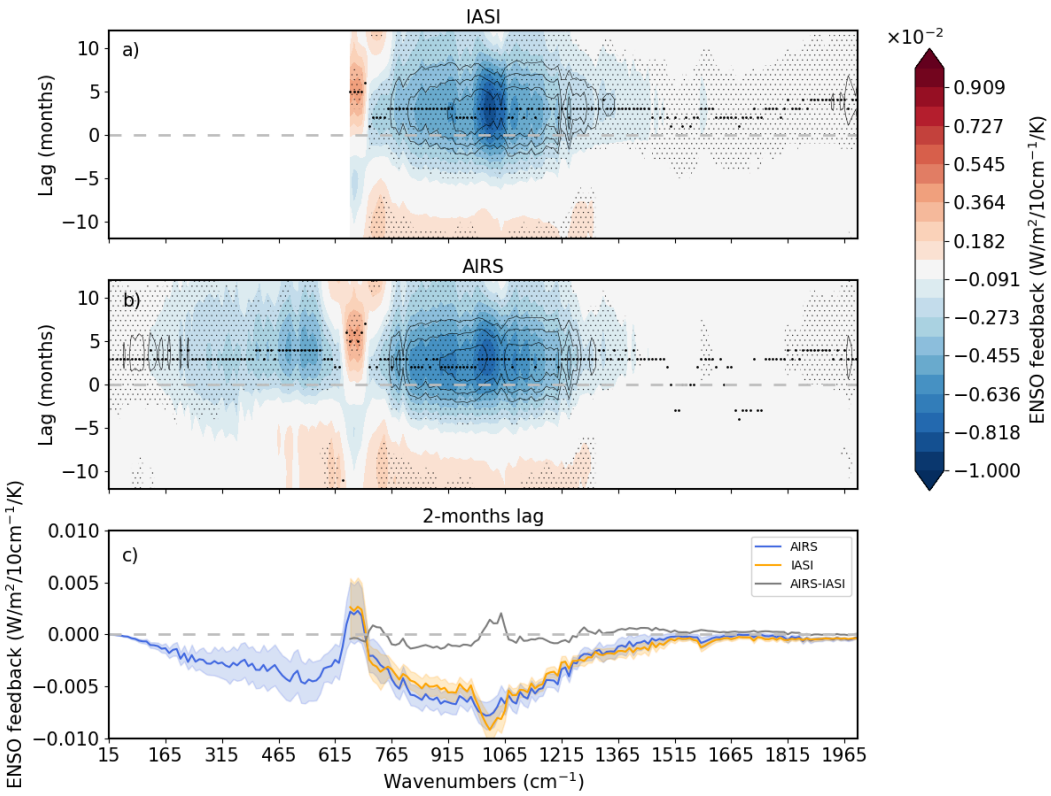

Figure 4. Lagged regression analysis performed on OLR anomaly without removing the linear trend.

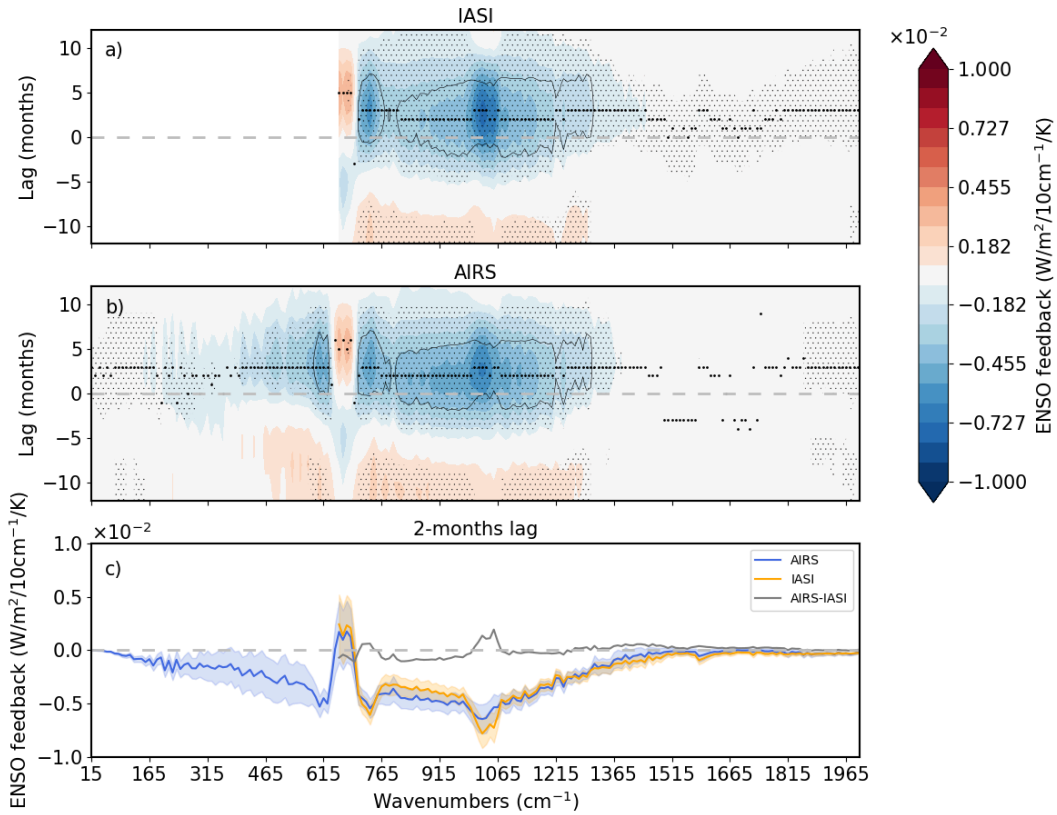

Figure 5. Lagged regression analysis performed on OLR anomaly with the linear trend removed.

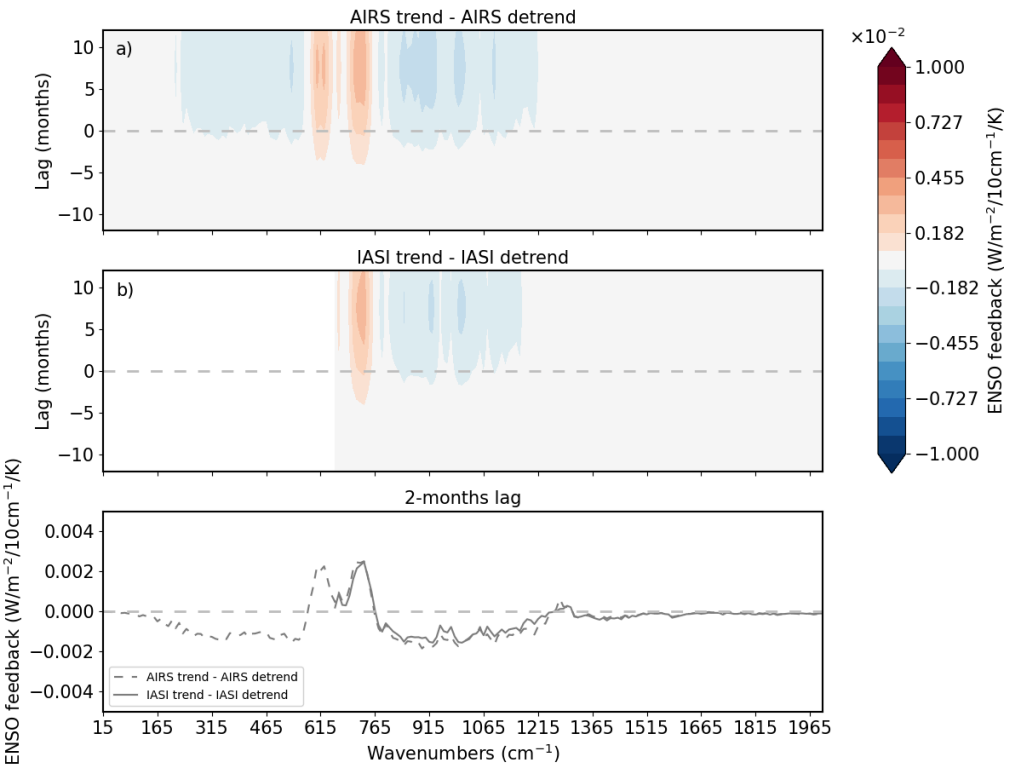

Figure 6. Differences between the lagged regression analysis performed on OLR anomaly without removing the linear trend and detrended.

- From my understanding of the manuscript, the study treats El Niño and La Niña phases equivalently by relying on a linear regression with the Niño 3.4 index to evaluate the radiative impact. However, El Niño and La Niña are not necessarily mirror images of each other: their amplitudes, spatial structures, and temporal lags can differ significantly. Could the authors clarify whether an analysis was performed separately for El Niño years and La Niña years? If so, were the results consistent? If not, at least a discussion of this potential limitation would be important to assess the robustness of the conclusions.

We thank the reviewer for addressing this additional point of discussion. We confirm that no separate analysis of El-Niño/La-Niña events has been performed. In this regard, we acknowledge that this work investigates the overall ENSO activity, thus the results obtained are indicative of the overall behavior of this phenomenon. However, we thank the reviewer for this suggestion. A separate analysis of El Niño and La Niña events may offer new insights, and this will be considered in future developments of the study.

We decided to limit the present analysis to the overall behavior of ENSO activity. We acknowledge this limit in the Discussion:

"We analyzed the radiative response over the entire period, therefore taking into account the overall impact of successive El-Niño and La-Niña events on the tropical mean OLR anomaly" (lines 288-290).

- Lines 127-128: The spectral flux is computed using the Gaussian quadrature method with only three angles. I wonder whether this choice provides sufficient accuracy for the purposes of the study. I suggest that the authors evaluate the potential impact of using a larger number of angles (e.g., 4 or 5) on at least one or two representative cases, to demonstrate that the results are not sensitive to this assumption.

Thank you for this comment. In our work, the spectral radiative kernels are calculated starting from simulated radiances at three different angles and a Gaussian quadrature is then applied for an approximate computation of the radiative flux. As suggested, to address the impact of this method for the results of the analysis, we conducted a sensitivity test on the number of angles used for the radiance-to-flux conversion. Due to the computational time required, we performed a test on kernels computed for a single month (January 2008) using both 3 angles (as in the manuscript) and 4 angles. The sets of angles used were taken from Clough et al. (1992). As an example, Figure 7 presents the percentage differences between the surface temperature kernel computed using 4 and 3 view angles. As can be seen, these differences are limited to within 1% and are not significant. However, this may not be the case for the all-sky scenario.

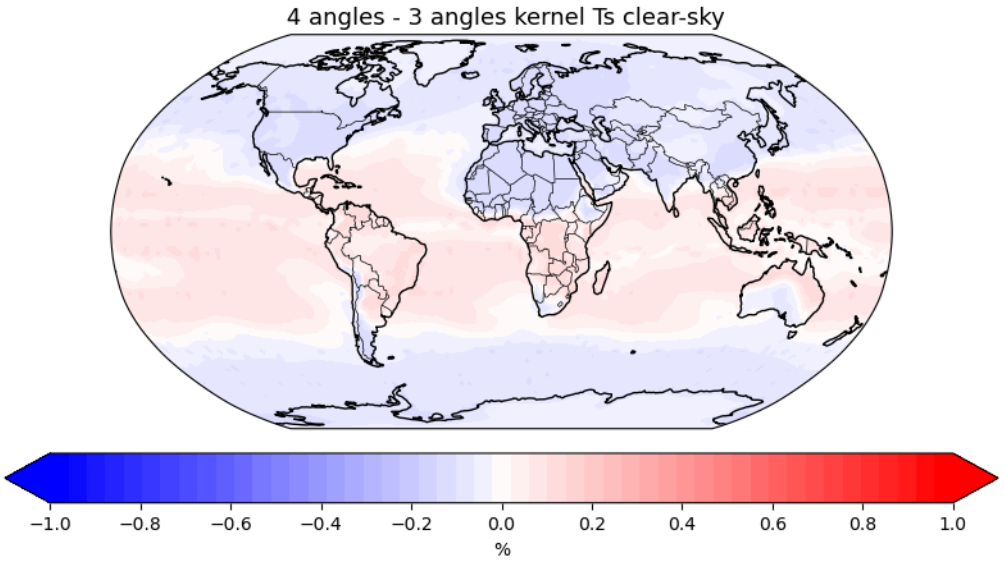

Figure 7. Percentage difference between the radiative kernel (W/m^2) of the surface temperature (Ts) for the month of January 2008 calculated using 4 and 3 angles.

We conducted a sensitivity test on the number of angles used for the radiance-to-flux conversion with the Gaussian quadrature method. We investigated the sensitivity of the surface temperature radiative kernel to 3 and 4 angles. The test was made in the month of January 2008. The change in the radiative kernels is within the 1%, as such, at least for the clear-sky scenario, the use of 3 angles can be considered sufficiently accurate.

- The manuscript primarily discusses the slopes of the lagged regressions, but the correlation coefficient (R) or the fraction of variance explained is almost never reported. Including R or R^2 would be important to assess how much of the variability in spectral fluxes is actually explained by ENSO. Reporting these values would strengthen the robustness and interpretability of the results.

As anticipated, we agree that improving the strength of the final statements through more accurate statistical considerations would benefit the analysis robustness. In this regard, we will therefore include a plot of the correlation coefficient and/or the R^2.

The correlation coefficient (R) has been added to the contour plots reporting the spectral ENSO feedback (Figure 3 and 5 of the revised manuscript). Gray contour lines mark the points where R is higher (lower) than 0.5 (-0.5).

- The slopes of the lagged regressions are expressed in $W\ m^{-2}\ 10\ cm^{-1}\ K^{-1}$. While this is certainly relevant to evaluate the radiative response to ENSO, the analysis would benefit from also discussing the changes in relative terms, for example as a percentage or in terms of Brightness Temperature. Indeed, regions where the slope is largest often correspond to wavenumbers with the highest radiance, rather than where the ENSO response itself is maximized. Including such normalized metrics would provide a clearer and more physically meaningful interpretation of the spectral response.

As anticipated, we agree that improving the strength of the final statements through more accurate statistical considerations would benefit the analysis robustness. In this regard, we will therefore include a plot of the correlation coefficient and/or the R^2.

We examined the possibility to plot the spectral response in relative terms as percentage change with respect to the mean OLR flux averaged over the year 2008, in a similar way as done in Whitburn et al., 2021. The result is shown in Figure 8. For the revised manuscript, we decided to present the ENSO feedback in terms of $W/m^2/10\ cm^{-1}$, to highlight the features of the OLR response.

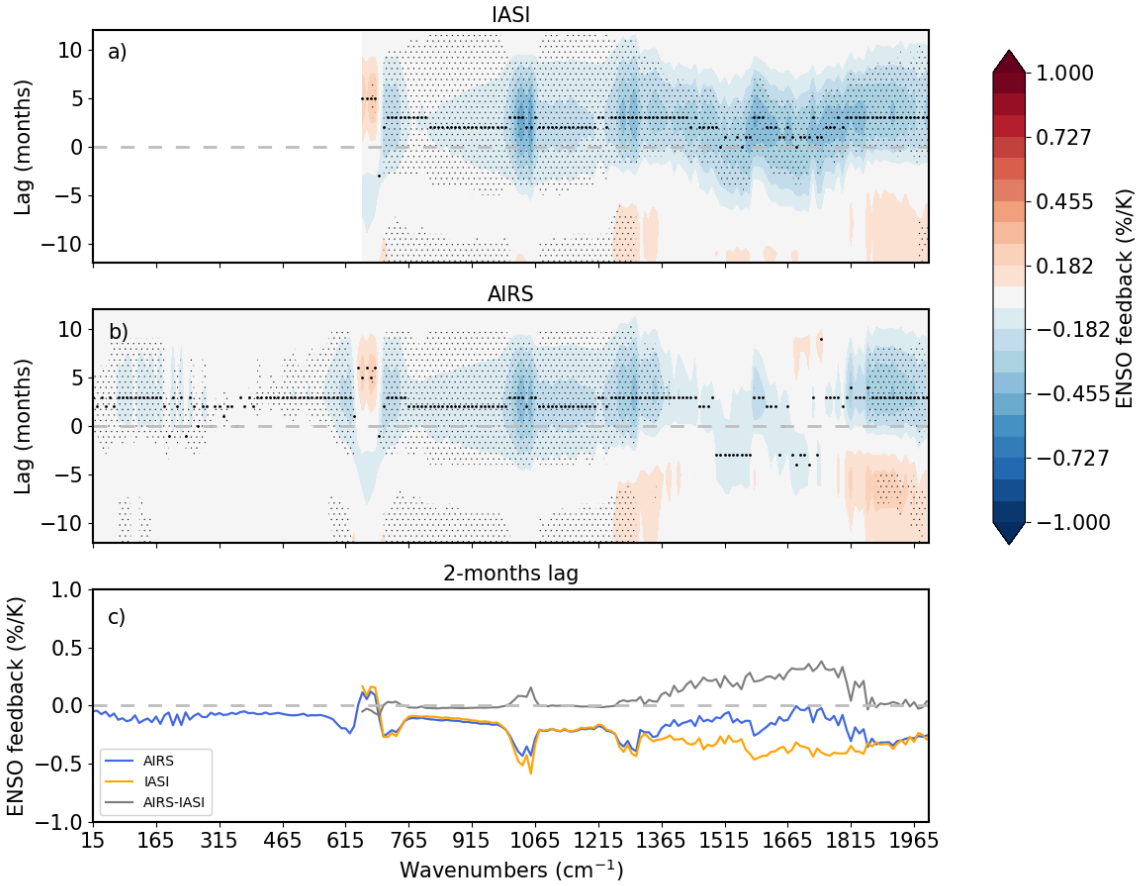

Figure 8. a) ENSO feedback as a function of wavenumbers and lag for IASI. The ENSO feedback is expressed in terms of percentage change (%/K) with respect to the mean OLR averaged over the year 2008. b) same as a, but for AIRS. c) ENSO spectral feedback for the 2-months lag for AIRS (blue) and IASI (orange), and their difference (gray). In panels a and b, thinner black dots mark 95% significance of the slopes, while thicker black dots mark the lag where the strongest radiative response occurs at each wavenumber. In panel c, shades envelope the 95% confidence interval.

- Lines 175-176: "Since AIRS and IASI differ only in these two spectral regions, the opposite biases could compensate each other [...]". Could the authors clarify whether this compensation actually occurs when the total (integrated) flux is calculated?

The compensation would occur when IASI and AIRS spectral fluxes are integrated over the same wavenumber range. This statement is based on the fact that the IASI and AIRS broadband fluxes calculated over the 645–1995 cm^{-1} wavenumber range demonstrate an excellent level of agreement. The peak of the radiative response shows the same magnitude and 2-months lag. When the regression coefficient spectra of IASI and AIRS are plotted at a 2-month lag (see Figure 2, upper panel), differences emerge in the atmospheric window between 750 and 1000 cm^{-1}, as well as within the ozone absorption band around 1043 cm^{-1}. Specifically, IASI exhibits a lower response within the atmospheric window, and a higher response within the ozone absorption band, with respect to AIRS. Given that their agreement is almost perfect when the broadband OLR is considered and that the IASI and AIRS signals are in good agreement with other wavenumbers, it was hypothesised that these biases would cancel each other out. We will

consider removing this sentence since it does not have any implication for the rest of the work.

We decided to remove this sentence in the revised manuscript. Indeed, it was confusing without further evidence to prove it.

- The manuscript does not seem to discuss the uncertainty associated with the reconstructed radiances in the spectral ranges not covered by AIRS. Could the authors provide an evaluation or at least an estimate of these uncertainties? Such information is crucial to assess the robustness and reliability of the results in those spectral regions.

We agree that it would be important to discuss the uncertainty associated with AIRS reconstructed fluxes. We will do our best to address this issue in the final manuscript.

The accuracy of the AIRS OLR spectral fluxes has been evaluated against theoretical calculations and OLR from CERES observations, as reported in Huang et al. 2008. The comparison against theoretical calculation showed that the maximum discrepancy for AIRS OLR spectral fluxes are within +/- 0.06 $W/m^2/10\ cm^{-1}$.

- Lines 223-224: To evaluate the spatial pattern of the OLR response to ENSO, the authors select a channel at 905 cm^{-1} to extract information on surface temperature and the effect of water vapor. However, no significant H_2O absorption line is present at 905 cm^{-1}. Would it not be more appropriate to consider the OLR integrated over a spectral range within the window region? This would better capture the combined effects of surface temperature and any residual water vapor absorption.

The authors recognized that there might have been a misunderstanding. As the sentence is currently formulated, it is unclear whether the spectral radiances are expressed as $W/m^2/10\ cm^{-1}$. This would mean that they are integrated over bins of 10 cm^{-1}. As such, 905 cm^{-1} is not a single wavenumber.

The fact that OLR fluxes used in the study are defined over 10 cm^{-1} bins has been acknowledge for clarity:

"We want to recall that spectral kernels, as well as IASI and AIRS spectral fluxes, are expressed as the flux integrated over 10 cm^{-1} bands ($W/m^2/10\ cm^{-1}$). Therefore, the wavenumbers selected represent the central value of the 10 cm^{-1} interval and not a single high-resolution channel" (lines 257-259).

- Lines 234–235: The manuscript states, "As already anticipated in section 3.2, the different temporal and spatial sampling between the two here becomes evident, as highlighted by the more scattered patterns of AIRS with respect to IASI." However, in lines 100–101, it is mentioned that all datasets have been regridded to a common 2.5° × 2.5° grid using bilinear interpolation, and both datasets correspond to monthly mean fluxes. It is therefore unclear why AIRS would appear more scattered than IASI. Could the authors clarify the origin of this apparent discrepancy? For instance, is it due to residual effects from cloud screening, incomplete coverage, or some other factor?

We agree that this sentence is not properly formulated, and that the words “temporal sampling” and “spatial sampling” are not correctly used in this context. Indeed, as the reviewer points out the two datasets have the same monthly temporal resolution and have been interpolated to the same horizontal grid. The main reason behind the more scattered pattern of AIRS is therefore suggested to be the cloud screening applied to the two datasets. Figure 3 reports the percentage of OLR values at each grid box over the period from January 2008 to December 2021 for the two overpass times in AIRS (upper row) and IASI (lower row) datasets. The maps show that the amount of valid points follows the cloud cover over the tropical regions. Another factor that may play a role is the strategy employed to derive AIRS fluxes, which is based on a collocation strategy with respect to CERES footprint (Huang et al., (2008)). The “different spatial sampling” would refer to this last, maybe these words are misleading and we will change them in the revised manuscript.

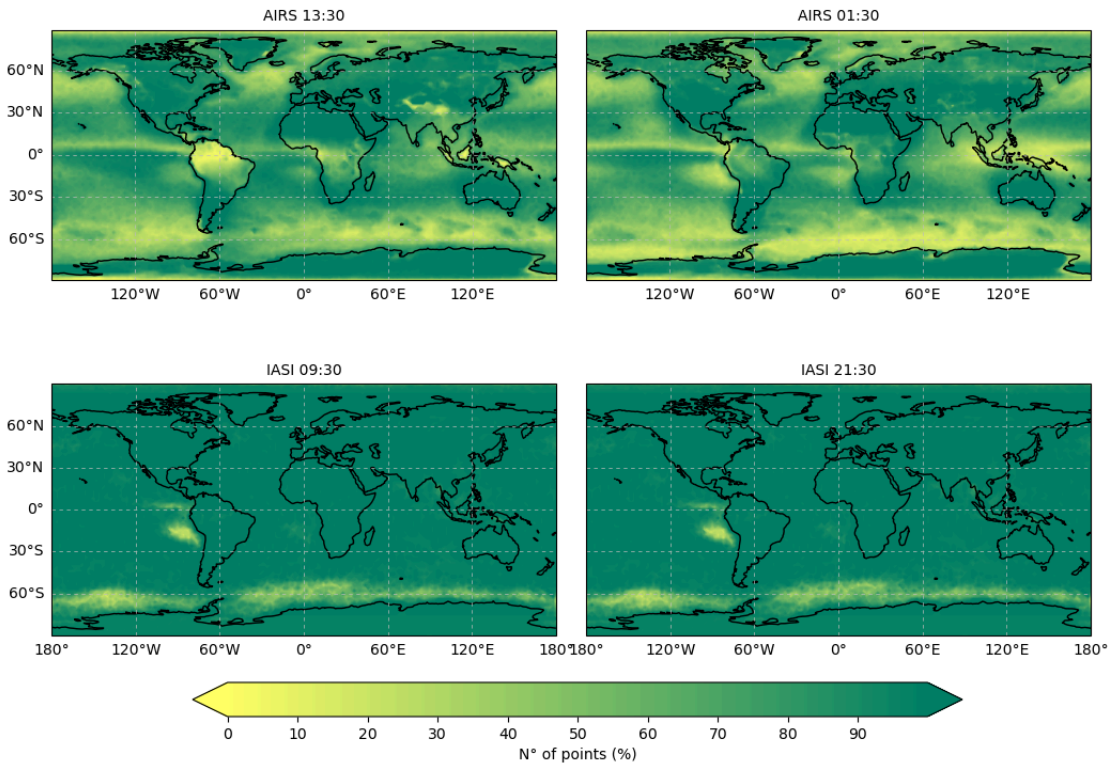

Figure 3. Number of valid points (percentage %) for each overpass times of the OLR time series from January 2008 to December 2020 in AIRS (upper row) and IASI (lower row) datasets.

The sentence has been rewritten as:

“As anticipated in Section 3.2, the impact of the clear-sky scene selection of AIRS observations is evident here. Specifically, AIRS shows a more scattered pattern with respect to IASI in the western and central tropical Pacific at 905 cm^{-1}, where AIRS signal results lower in magnitude than IASI” (lines 268-270).

However, a more detailed explanation has been also included in Section 3.2:

“However, the different clear-sky scene selection could also play a role. Specifically, AIRS clear-sky fluxes are based on the collocated CERES measurements considered as clear sky by the scene type information provided by the Moderate Resolution Imaging

Spectroradiometer (MODIS) as described in Huang et al. (2008). We found that this strategy produced a more conservative cloud screening than that of IASI (not shown). This aspect will be more evident in Section 3.4, when we will examine the spatial pattern of the ENSO feedbacks" (lines 206-210).

- The Discussion section needs significant rewriting. Several passages are unclear or imprecise, and the analysis would benefit from more quantitative support. References to figures are often missing, making it difficult for the reader to link statements to the presented data; the authors should clearly indicate which figure panels support each point. Additionally, the last paragraph should include a reference to Whitburn et al., 2021, as similar points were discussed there.

On the basis of all the comments emerged during this first review phase, the section Discussion will surely undergo rewriting. It will benefit from a quantitative evaluation of the results, as well as stronger connections to other results presented within existing literature on related subjects.

The Section "Discussion" has been rewritten including comparisons against existing literature.

Minor comments:

- Lines 18-19: The sentence "It contains both the signature of the increased concentration of radiatively active species [...]" is slightly misleading. The spectral OLR contains the signatures of all radiatively active species, not only those whose concentrations increase. The current formulation is correct when referring specifically to OLR trends.

Maybe this can be rewritten as: "It contains both the signature of climate variables, such as radiatively active species [...]"

The sentence has been modified as follows:

"The long-term trend of the OLR bears the signal related to the increase of carbon dioxide concentration and other Greenhouse Gases (GHGs), driving the present-day climate change, as well as that of the system's radiative response, produced by the warming of the planet's surface and resulting climate feedbacks" (lines 18-20).

- Lines 87-88**:** The statement "AIRS fluxes are calculated exploiting the same angular distribution models of CERES [...]" is not fully clear. CERES provides broadband OLR, while AIRS produces spectrally resolved fluxes at 10 cm^{-1}. Could the authors clarify how the CERES ADMs are applied to AIRS data in this context?

We thank the reviewer for the careful reading. This sentence is the result of misinterpretation. The same scene type information from the CERES footprint and auxiliary information has been used for AIRS (so the ADMs are not the same as stated). This approach is based on a collocation strategy. Only data from AIRS pixels that overlap with the CERES footprint up to a certain threshold are retained for the radiance-to-flux conversion. In addition to the cloud filtering, this is another reason for the more scattered pattern of AIRS.

The sentence has been removed and rewritten where the differences between IASI and AIRS results are discussed (Section 3.2), as follows:

"However, the different clear-sky scene selection could also play a role. Specifically, AIRS clear-sky fluxes are based on the collocated CERES measurements considered as clear-sky by the scene type information provided by the Moderate Resolution Imaging Spectroradiometer (MODIS) as described in Huang et al. (2008)" (lines 206-208).

- Line 92: [...] "from 15 to 1995 cm^{-1}". Isn't it from 10 to 2000 cm^{-1}?

The dataset used for this study, that has been downloaded from https://disc.gsfc.nasa.gov/datasets/AIRSIL3MSOLR_6.1/summary, provides spectral fluxes from 15 to 1995 cm^{-1} (every 10 cm^{-1}).

- Line 95: Metop-C was launched in 2017 and not in 2013.

The authors thank the reviewer for highlighting this error. The correct year will be indicated in the revised version of the manuscript.

The correct year is now indicated in the description of IASI instrument and dataset.

- It could be useful to include a plot of the Niño 3.4 index for the study period, so that readers can better visualize the ENSO variability. In addition, a brief discussion of the relevant ENSO phases during this period would provide valuable context for the analysis.

We agree that this would contribute positively to the comprehension of the analysis. Inserting a plot of the Niño 3.4 index could help the discussion of the feedback sign and spatial pattern. As shown by Figure 4 the overall OLR response follows the SSTs anomalies of El-Niño phase: an increase in TOA OLR emission is found across the central and eastern Tropical Pacific, while a decrease is suggested to characterize the western part of the basin.

The plot of the Niño 3.4 index has been included in the revised manuscript (Figure 1), as well as a description of the major El-Niño/La-Niña events that characterized the years examined (from 2008 to 2020):

"The period examined covers the years from January 2008 to December 2020, and it is characterized by two strong El-Niño events, as suggested by the Niño 3.4 index (Figure 1). The first peaked during the winter of 2009, the second, reached the highest SST anomaly during the winter of 2015. During this period, two strong La-Niña events also occurred in 2008 and 2010" (lines 286-288).

- Lines 154–155: The manuscript mentions that the difference is "likely due to AIRS's clear-sky scene selection, as will be investigated further in section 4." However, I could not find a further discussion of this point in section 4. Could the authors clarify or correct this statement?

We thank the reviewer for pointing this error out. Indeed, this is mentioned in Section 3.4, Lines 235–236, and not in Section 4 as stated. The correct reference will be inserted in the revised manuscript. However, we would also like to take this opportunity to highlight the

implications of the different clear-sky scene selection of AIRS and IASI in the Discussion or Conclusions section.

The sentence has been rewritten stating the correct reference to Section 3.4:

"This particular aspect will be more evident in Section 3.4, when we will examine the spatial pattern of the ENSO feedbacks" (line 209-210).

- Lines 196–197: For clarity, it would be helpful to remind the reader that a negative slope corresponds to a decrease in the TOA OLR, which implies that more radiation is trapped and therefore corresponds to a positive feedback (and conversely).

These lines could be rewritten as: "The positive regression slopes that characterize the Planck and atmospheric feedback indicate an increase in OLR emission at the TOA. Consequently, they constitute a negative feedback that acts to stabilize the system. Conversely, changes in humidity cause negative regression slopes throughout the spectrum, meaning that a reduction in OLR emission takes place at the TOA, constituting positive feedback." However, we will also take into consideration to plot -dOLR/dTs.

In the figures of the revised manuscript we decided to plot the quantity -dOLR/dT. As such the sign of the ENSO feedback is consistent with that used for the climate feedbacks: positive for destabilizing feedback and negative for stabilizing ones.

- Figure 2: Why does the x-axis stop at 1515 cm^{-1}, whereas Figure 3 extends further? For consistency, it would be preferable to use the same spectral range in both figures unless there is a specific reason for this choice.

As suggested also by Review 2, the figures in the revised manuscript will be improved by changing the colour map and y-label, and by using the same magnitude scale in all plots.

The same wavenumber interval (from 15 cm^{-1} to 1965 cm^{-1}) has been used for the x-axis of all the plots.

- Lines 205–215: This paragraph is not very clear and would benefit from some rewriting. In particular, the statement *"the net signal in the FIR and MIR spectral region"* is too vague. Could the authors specify the exact wavenumber ranges referred to and indicate clearly in which panel(s) of the corresponding figure this signal can be seen?

We agree that generally referring to the "MIR" and "FIR" is too vague and the wavenumbers of the actual spectral regions will be specified in the revised manuscript. As suggested the paragraph will also undergo a careful revision to make the description of the results as clearer as possible.

The paragraph has been rewritten using the exact wavenumber intervals:

"The main discrepancy arises in the spectral regions below 600 cm^{-1} and above 1300 cm^{-1}: here AIRS fluxes indicate a negative feedback that peaks 3 months after ENSO, while the kernels reconstruction indicates a contribution close to zero at that lag (Table 2, first row) and a positive feedback that anticipates the ENSO peak (Figure 5)" (lines 243-245).

- Figure 3: For clarity, it might be better to move panels 1 and 2 to the bottom of the figure, as they are discussed later in the text. This would make it easier for the reader to follow the discussion in the correct order.

Panel 1 and 2 of Figure 3 have been separated adding a new figure, Figure 4.

- A study of ENSO teleconnections beyond 30° N/S could be an interesting addition to the conclusion or perspectives. Extending the analysis to the extratropics might provide further insights into the global radiative response to ENSO.

The authors thank the reviewer for the suggestion, which adds value to the analysis. We will take it into consideration to extend the future perspective of the study during the writing of the revised manuscript.

- In the Conclusion, the authors state: "to the best of our knowledge, this is the first time that the radiative response to ENSO is analyzed using satellite-based spectrally-resolved measurements of the OLR." This statement is not entirely accurate, as similar analyses, albeit to a lesser extent, were conducted in Whitburn et al., 2021.

We thank the reviewer for highlighting that we are missing some relevant reference that contributes to setting the context for this analysis. A comment on Whitburn et al., (2021) will be added within the Discussion and Conclusions sections.

The statement has been changed as follows:

"Results further consolidate the potential of satellite-based spectrally-resolved measurements of the OLR to constrain the fast inter-annual feedbacks and isolate the processes driving them" (lines 343-344).

The work done by Whitburn et al., (2021) has been acknowledged in the Introduction and Discussion:

"[...] the analysis of the trend of the first ten years of observations of IASI (from 2008 to 2017) revealed the spectral signature related to both the increase in GHGs concentration and to ENSO variability across channels sensitive to water vapor and temperature (Whitburn et al., 202)" (lines 52-54).

"Our analysis adds evidence to the attribution of IASI radiance trends in the ozone band in Whitburn et al. (2021) to ENSO-related ozone changes" (lines 330-331).