Authors responses to JGR-Atmosphere Submission 2025JD043501

Geophysical Trends inferred from 20 years of AIRS infrared global observations

by DeSouza-Machado et. al.

We thank the anonymous referees for their detailed read of the paper and providing introspective comments. The resulting changes to the manuscript have substantially improved it. We highlight some changes here :

- the introduction has been rewritten sbstantially, with more referenes added about the AIRS instrument,
- we have replaced the surface temperature and profile temperature/water vapor figures, which in the original manuscript were smoothed, with ones that have no smoothing (unless otherwise noted),
- we have added an Appendix detailing noise and uncertainty in the radiance time series,
- the paper also now includes estimates of for example how changing ocean temperature affects the emissivity.

Below we detail our responses to their individual concerns. For ease of review, we type-faced the reviewers questions in blue.

Reviewer 1

We thank the Reviewer for the time taken to read our manuscript. The concerns fell mostly into two categories : the first seven of which were a significant rewrite of the Introduction, which we have done. The remainder (starting from item 8) were after the Introduction, which we address below :

Introduction

1-7) Rewrite and restructure the Introduction

Thank you for the suggestion. We have completely rewritten the Introduction, starting with the motivation, including key reports, disccussed re-analysis and observational datasets, and then a broad overview of our approach. Reviewer 2 wanted a statement about earlier sounders such as HIRS, so that has also been included in the rewrite. We request the reviewer looks at the revised manuscript and/or the differential file.

Remaining comments

8) Figure 1: please reduce the number of lines to make this figure clearer.

Done, and text/captions amended accordingly.

9) It is not completely clear why the spectral closure section is needed as section 4. Please clarify why that is essential for the 'flow' of the manuscript. Is it really necessary? Could it be deleted, or moved to some other place in the paper or to an appendix?

This is an important section, since it highlights the fact that the spectral trends produced by the other datasets, when run through an accurate RTA, cannot reproduce the observed data trends from AIRS, a highly stable instrument. We have added the above sentence to the section, as well as shortened the section (by eg moving the description of the papers by X.Huang and S. Raghuraman to the introduction).

10) It is not clear why the authors refer to 'NWP' at places in the paper. Why is it important to refer to 'NWP reanalysis'? why not just reanalysis? It appears that at places, the authors refer to 'reanalysis' as 'NWP models'! Please correct. In general, avoid 'NWP'.

We have gone through the paper and removed the term NWP.

11) While this is a common practice in some communities, please do not use the word 'data' as a replacement for 'observations'. Data could also be model data or reanalysis data. Use only 'model data', 'reanalysis data' or 'observations' (or 'observational data'). It can be confusing to some readers if you mix them up.

We have gone through the paper and fixed this as appropriate (observations/observational data, reanalysis model fields, L2 and L3 retrieved products)

12) At places the manuscript could be improved if the authors would be a bit more formal with their text (e.g., sentences like 'a lot of cooling ...' should be avoided).

We have fixed this particular sentence as well as others as we found them.

13) Please make sure that the figure and table captions are as complete as possible including units.

We have done this.

14) Please make sure that any potential issues/confusion in the context of 'clear sky', 'cloud cleared' distinctions between the different datasets, are clarified at different places in the manuscript.

We have changed the wordings of a few relevant sentences, and hope these further clarify CCR versus clear sky. The introduction now has "...The cloud clearing method takes in the raw observed allsky radiances and solves for an estimate of clear column radiances by examining adjacent Fields of View (FOVs) to estimate the cloud effects on the observations. The method assumes any differences are solely due to different cloud amounts in each FOV, and significantly increases geophysical retrieval yields (to about 50-60%) [smi:23]. The resulting cloud cleared radiances (CCR), distinct from clear sky radiances which are obtained under nominally clear conditions, have increased noise especially in the lower atmosphere sounding channels ..."

15) Please add references for the MLS instrument.

We have added two more, in addition to the one that was there earlier.

16) The AIRS-RT data appears smoother (with less sharp discontinuities/gradients) when compared to the other datasets. This could potentially be a key point to mention, highlighting the fact that the other datasets show discontinuities that are most likely not realistic. From a thermodynamics (and atmospheric dynamics) perspective, it is plausible that the temperature trends in the atmosphere should not exhibit discontinuities as sharp as shown in the other datasets.

As mentioned in the first page, we double checked and realized originally we submitted "smoothed" versions of d(surf temp)/dt maps and dT,dt, dWVfrac/dt profiles. We have now changed the relevant plots so there is no smoothing. The reviewers will notice the profile trends are barely affected by this, since most of the smoothing is done during the averaging over the 72 longitude bins. Conversely the pixelation at the tile level (3×5 grids) is now visible for the surface temperature trends. In addition for dSKT/dt we have now changed from interpolating the (GISS/ ERA5/ MERRA2/ AIRSv7/ CLIMCAPS) to the center tile, and instead we used the mean values of those datafields averaged over each tile. There is barely any change, but we have made note of this in Section 5, together with small changes in Figures 7,8 and Table 2,3 of Section 7.1

Section 7.4, when we discuss the atmospheric temperature trends, has this additional sentence : "We highlight that our results are smoother than those of the other datasets, while the other sets have noticeable discontinuities that may not be physical under the thermodynamics or fluid dynamics frameworks. In addition the reanalysis models ingest many observational datasets, while the L2/L3 retreieval products can be influenced by the *a-priori*."

Reviewer 2

We thank the Reviewer for the time taken to read our manuscript. Below are our responses to the concerns :

Specific comments

1) Line 17, change 20202 to 2002

Done.

2) Perhaps include in the introduction the importance of spectral resolution and calibration accuracy and stability for this type of work. E.g. Why is this not possible with HIRS data?

Thank you for the suggestion. We have now added a paragraph in the introduction to address this. In addition Reviewer 1 requested a rewrite of the Introduction with more references included, so we request the reviewer looks at the revised manuscript and/or the differential file.

3) Line 35. Plain Language Summary. I would perhaps suggest an even stronger concluding sentence, such as this type of analysis and data is what should be used for climate trending and climate model testing, as opposed to (or in addition to) the previously mentioned L3 retrieval and NWP methodology.

Thank you for the suggestion. We have edited the summary to read as follows : The current generation of infrared sounders, designed for weather forecasting purposes, have been in orbiting for a long enough time to enable anomaly and trending studies for climate purposes. Their daily obtained radiance data are typically used for operational atmospheric state retrievals, or assimilation into Numerical Weather Prediction models, after which climate anomaly studies are made. Here we use the raw radiance spectral data to form radiance anomalies and trends, followed by a one step atmospheric state retrieval with full error characterization. This novel approach uses only stable channels, easily understood assumptions and well tested algorithms for the trend or anomaly geophysical retrieval, and is designed expressly for observational climate trending.

4) Line 51/52. CrIS is Cross-track Infrared Sounder

Done

5) Line 58. carbon dioxide CO2 .... remove the first

Done

6) Paragraphs in Section 2.1 on AIRS data quality, in particular its noise. Even after L1C, there are many unusual noise artifacts in the AIRS data. For example very high levels of spectrally correlated noise (half the NEDT is correlated for some arrays), lack of spatial/spectral "purity", and significant NEDN (not just NEDT) dependence on scene temperature. "Pristine, stable" channels are basically non-existent. With the averaging you are doing, these effects are probably not relevant to your study. But some comments of this would be more accurate. As is, what is written is incomplete and gives an incorrect picture. Also it would be good to discuss these effects/considerations in Section 6.1.

Need help with this!

7) What is meant by "re-calibrated" on line 177? There is a spectral calibration

that is handled in L1C. Is there also a radiance re-calibration, which this sentence might imply. Suggest being more clear here.

Need help with this!

8) Line 188. "that" are stable in time. Done

9) Line 387. "Clouds in the infrared are not changing much". Is this true even for the last several years. Various data sources suggest a slow decrease in clouds in the last decade and there are noticeable changes in cloud cover in the Northern hemisphere particularly in the last two years.

The top right hand panel of Figure 3 show that the globally averaged trends for the different quantiles are essentially the same, though the bottom right panel shows that cloud variability manifests in the uncertainty in the trends increasing as we allow more clouds into the quantiles. Nevertheless this is a global average and is not true regionally, so we have slightly amended the sentences to read "This implies that clouds effects in the infrared produce the largest variability (blue curve). Globally on average for the infrared the spectral trends for all quantiles, ranging from clearest (Q0.97) to allsky (Q0.50 very similar, but differences are seen on regional scales. This implies the +0.022 K yr^{-1} window region trends are dominated by surface temperatures changes and to a lesser extent by water vapor changes."

10) Paragraphs starting on lines 370 and 380. Overall, OLR has increased over the past 20 years. Please describe how your results are in line with this, or not.

Need help with this!

Correct, we have separately shown that when we add together the OLR changes due to our retrieved surface temperature, atmospheric temperature and atmospheric water vapor trends, and include the NOAA ESRL changes in CO_2 and CH_4 to these changes, put OLR changes are in very good agreement with the clear sky OLR trends measured by CERES. We are working on a separate paper to document this together with our derived clear sky longwave feedbacks.

We have added the following sentence to the end of that paragraph in the revision : :We note that running the retrieved surface and atmospheric temperature, water vapor trends, together with the CO_2 and CH_4 trends through a flux calculator such as ecRad [Hogan et al, 2018], and doing zonal averages, produces very good agreement with trends observed by the CERES instrument."

OR We have chosen not to mention OLR in this paper since we are still in the process of doing more work on the separate OLR/longwave feedback paper.

11)First paragraph of Section 6.1. "We ignore scan angle geometry within a tile" . "we ignore instrument changes (changes to NeDT(v)) ... " . It would be good to explain how the scan angle geometry can be ignored. Also, instrument changes are not just possible changes to NeDT. You explained how you limited this part to channels that have been determined to be stable to within some limit. However the limit is not zero, and it is not clear that even those small instrument changes do not translate into a false geophysical trend. Perhaps just explain with another sentence or two.

Need help with this! We have done additional analysis on the different sources of error, and added this to an Appendix in the paper.

12)Line 530,556. If the noise term is not really NeDT, then suggest giving it a

different variable name.

We have changed it to $NeDT_{retrieval}(\nu)$ and in the text (lines 526, 556) explain that for daily L2 retrievals this would be instrument noise, but for our trend retrievals we need to average it over the observations.

"The spectral noise $NeDT_{retrieval}(\nu)$) varies with scene temperatures and on particulars of the retrieval algorithm. For single footprint retrievals using daily observed data, the spectral noise $NeDT_{retrieval}(\nu)$) in a typical tropical "clear scene" is about 0.1 K in window region, increasing to about 1 K in the 15 μm temperature sounding channels and about 0.2 K in the 6.7 μm water vapor sounding region, and is usually larger for operational L2 retrievals which use cloud clearing."

and a few paragraphs later,

"The noise term $NeDT_{retrieval}(\nu)$ for the trend retrievals is now the uncertainty that naturally arises from the inter-annual variability when doing the linear trend fitting shown in Equation 2."

13) Line 597. "For completeness we note that a sequential retrieval (see for example Smith and Barnet (2020)) produces very similar geophysical trends." This sentence should be re-written somehow, because as-is it can be interpreted that the sequential retrievals of Smith and Barnet give very similar geophysical trends.

We have changed this to "For completeness we note that a sequential trend retrieval produces very similar geophysical trends. "

14) Line 573. Suggest not using "which sets us apart". Rather perhaps "which distinguishes this approach from other ...."

Done

15) Line 596. Over land, the constant relative humidity approximation is not well known or a good approximation. E.g. https://www.pnas.org/doi/10.1073/pnas.2302480120 Since the H2O signal you are getting is relatively small, it would be good to show results where you do not assume constant RH and/or investigate land vs ocean H2O results.

Thanks for the suggestion. We have worked with assuming zero water vapor trends as our prior, but we chose not to show the results since the paper is already quite lengthy. In essence the freedom in the lower atmosphere really impacted the column water vapor trends, which could become quite larger than what the other datasets show. We have put in a sentence in the discussion section to mention this : "If we start with zero *a-priori* for water vapor at the surface, we can fit the spectral trends but the retrieved water vapor trends in the lower layers which dominate column water amounts, can leads to column water trends that are easily double or more than the results for the other datasets.

16) Section 6.5. The ocean emissivity also has a dependence on water temperature, which is not captured by Masuda (Nalli 2022). Is this not included because the temperature changes for any given tile are so small and this is just a bias effect not effecting the trends?

Thanks for asking. To answer this, we use the plots in [Stuart Newman 2005/ Nalli 2023] to estimate the change in emissivity as a function of temperature, and then include this effect to see how brightness temperatures would change (assuming no atmosphere); it turns out to be a tiny 1e-3 K change due to surface emissivity,

and the analysis details have been included in the revised paper.

17) Perhaps comment on emissivity effects when different viewing angles are averaged together. Again is this considered a static bias that does not affect trend?

For a fixed wind speed and ocean temperature, there actually is a pretty large change in emissivity (about 0.015 between 0°- 50°), which would result in an almost 1 K swing. But this would be happening over every AIRS scanline. As we are assuming the viewing geometry does not change over the 09/2002 to 08/2022 time, this should not lead to any trends.There could be small changes in the sampling of the clearest FOVs per 16 day repeat cycle per tile, but as with the emissivity temperature dependent analysis above, this should be a very small value. In addition the Appendix now includes a section on how the changes to the mean viewing angle affect the results.

18) Line 1045. Rename this section to "Open Research"

I believe that according to the instructions it should be Data Availability so I'll leave this for now.