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 substantially, with multiple references added about the AIRS instrument,
- the introduction emphasizes our methodology is uniquely designed for climate analysis,
- the original trend figures were unintentionally smoothed. We have replaced the relevant figures with ones that have no smoothing, except for Figure 13 which is still smoothed,
- the figure labels and tickmarks now use larger fonts while the captions include more relevant information,
- we have added an Appendix detailing noise and uncertainty in the radiance time series,
- we now include estimates of for example how changing ocean temperature affects the emissivity and subsequently window BT trends.

The following pages contain our responses to the individual concerns. For ease of review, we type-faced the reviewers questions in blue. Since there are multiple changes to the manuscript, we also refer the reviewers to the differential manuscript and/or the revised manuscript, rather than include every change in the following pages.

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, included key reports, discussed re-analysis and observational datasets, added references to scientific studies using the AIRS instrument as well as a broad overview of our approach. Reviewer 2 requested a statement about earlier sounders such as HIRS, which has also been included in the rewrite. We request the reviewers look 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. In particular, it is a simple and direct comparison of observation trends versus re-analysis and L3 retrievals converted to radiance trends, independent of our retrieval errors. We have summarized and added these two points to the section, as well as rewritten/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'.

Thank you, 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.

Thank you, 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 included two more references 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.

We realized the original submission had "smoothed" versions of d(surf temp)/dt maps and dT,dt, dWVfrac/dt profiles. We have now changed the relevant figures so there is no smoothing in the plots unless noted. The reviewers will notice the profile trends (Figures 6,9,11,12,14) are barely affected by this change, since most of the smoothing happens while averaging over the 72 longitude bins. Conversely the pixelation at the tile level (3×5 grids) is now evident for the surface temperature trends (for example Figure 7).

In addition for dSKT/dt we no longer interpolate the (GISS/ ERA5/ MERRA2/ AIRSv7/ CLIMCAPS) to the center tile; instead we use the mean values of those datafields averaged over each tile. This induces very small changes. We have enlarged the discussion of the different approaches in Section 5, and have made the small changes in Figure 7 and Tables 2,3 of Section 7.1

The atmospheric temperature trends discussed in Section 7.4, has an 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 retrieval products can be influenced by the *a-priori*."

We also quickly mention here that the data used for Figure 14 are unchanged from earlier, except that the individual panels are now unsmoothed and the color axis for the left hand plots (uncertainties) are tighter than in the original submission.

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. Next generation sounders such as AIRS provide far greater vertical resolution than HIRS, and also have much longer lifetimes (greater than about 20 years). 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 reviewers look 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 operational for a long enough time to enable anomaly and trending studies for climate purposes. The daily radiance observations are routinely used for operational atmospheric state retrievals and assimilation into reanalysis models, after which climate anomaly studies are enabled. Here we use a purpose built algorithm to directly turn radiance observations into geophysical anomalies and trends with full error characterization. This unique approach for observational climate trending uses only stable low noise sounding channels, easily understood assumptions and well tested retrieval algorithms.

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.

We have changed the sentence to read "However we note that the results described in this paper used only the actual observed radiances in pristine, stable channels described in (Strow and Machado, 2021) which have been shown to produce accurate trends using CO2, SST, CH4, etc to 0.002 K/year. We have, for example, excluded A-only and B-only channels as well as all short-wave channels since they do indeed drift."

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.

Thank you, we have rewritten the first paragraph of Section 2.1, where we also clarify "channel frequency shifts that appear in the L1b product have been essentially completely removed in L1c. The frequency calibration received a further adjust after Sept. 23, 2021 for a frequency shift caused by a deep space maneuver performed for checking the radiometric polarization corrections." Please refer to the revised manuscript for further details.

We note there is some drift in two arrays due to polarization drifts, but we had already seen this in our 2020 paper and do not use any channels in these arrays

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) are 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.

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, our summed 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 deleted the sentence in the introduction "A companion paper will utilize the geophysical trend results to derive OLR trends and clear-sky feedbacks" and replaced it with the following sentence in the conclusions "Zonally averaged longwave clear sky flux trends (both outgoing top-of-atmosphere and incoming bottom-of-atmosphere) derived using sums over the flux changes arising from the AIRS_RT surface temperature, and atmospheric water vapor, ozone and temperature trends together with flux changes induced by CO_2,CH_4 forcings in general agree with those derived using observational flux trends from the Cloud and Earth's Radiant Energy (CERES) clear sky Energy Balanced and Filled (EBAF) Level 3b product [loe:18,kat:18].

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.

Thank you for this question. As mentioned previously, we have added an Appendix which discusses sources of noise in the spectral trends. It turns out that the scan angle changes per tile per 16 day interval are very small and would not impact the trends shown 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 account for the multiple observations per tile per 16 day interval.

"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 and lag-1 autocorrelations used 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 different (usually larger) than that of the other datasets. We have put in a sentence in the discussion section to mention this : "A zero *a-priori* initialization for water vapor at the surface allows a fit to the spectral trends, but the retrieved water vapor trends in the lower layers which dominate column water amounts lead to column water trends that are easily double or more than the results for the other datasets. We have adjusted the OEM water vapor covariance matrices so that the zonally averaged column water trends agree in general with 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 is a pretty large change in emissivity (about 0.015) as view angles change between 0°- 50°. This would result in an almost 1 K swing in the window channels, over every AIRS scanline. The work for the Appendix shows the average viewing geometry changes over the 09/2002 to 08/2022 time barely impact the 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.

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

We believe that according to the instructions it should be Data Availability so we will leave this for now.