We thank Referee #1 for the review, which helps us to make the manuscript clearer, including a better description of the main objective of our study and an improved presentation of the ERA5 data. Our answers below are marked with blue colours. In some of the answers, we refer to text belonging to line numbering related to the original version of the manuscript.

Referee #1

The study by Paul Glantz et al. addresses an interesting and scientifically sounded idea, namely, it looks at the effect of differential skin surface temperature trends over land and the ocean. In theory, slower warming of the ocean surface should limit water vapor supply to the atmosphere over land, and thus, cause reduction of atmospheric relative humidity and cloudiness. In practice, the atmospheric hydrological cycle is very complex, mediated by numerous poorly understood feedbacks and even less understood atmospheric dynamics, especially in the turbulent planetary boundary layer. The study does not attempt to penetrate atmospheric physics or dynamics. It focuses on a broad search for statistical evidence of terrestrial warming amplification through reduced moisture and cloudiness. If properly implemented, the study could be an important contribution to physical climatology and climate change science. Unfortunately, in my opinion, the study failed in addressing its scientific goals. The reported results are messy and do not warrant publication.

We agree with the referee that atmospheric hydrological feedbacks are relevant for ocean-land warming contrast and mechanisms for accelerated increase in temperature over land. However, even if we do not provide an analysis of the myriad processes in the atmospheric hydrological cycle, we feel that the approach we take in analysing and comparing different relevant datasets allow us to draw scientifically valid conclusions regarding the apparent humidity paradox, and thus contributes to our understanding of this phenomenon.

Concerning the positive feedback with strengthening of the greenhouse effect by water vapour, we are not able to quantify the impact on the surface energy balance. It is not possible to separate the forcing from the additional supply of absorbed solar radiation at the land surface at present, due to decreases in the clouds. Even so, ERA5 models these effects dynamically and the impact of water vapour changes on the radiation budget manifests in the major role to re-radiated infrared energy downwards. The present changes in atmospheric water vapour, in addition to enhanced solar and infrared radiation absorbed at the surface, also directly drive shifts in heat fluxes between latent and sensible within the ERA5 reanalysis. Drastic shift in heat fluxes has thus occurred over some regions on Earth, along with decrease in precipitation, which implies an even more strongly perturbed energy balance at the land surface. Since 1979, ERA5 surface net solar radiation has increased by about 7 W m^{-2} in both South America and over the areas in the Northern Hemisphere (Section 4). This regional brightening exceeds the globally averaged human-caused greenhouse gas forcing of 3.8 W m^{-2} accumulated since 1750 (AR6). Although the forcing of 3.8 W m^{-2} is a global average, substantially of the extra heat at present has ended up as increased ocean heat content (line 37 in the manuscript). The strong solar forcing over land in addition to the enhanced absorption of IR-radiation support a profound drying effect, which immediately drives a localized climate feedback loop. Furthermore, according to fundamental physics (the Clausius-Clapeyron equation) and related water vapour feedback, see also our answer to comment 6 below related to Fig. 9 in the manuscript.

We agree with the referee, about the relevance of changes in boundary layer dynamics such as severe turbulence due to increase in sensible heat flux. This suggests a deeper planetary boundary layer that dilutes whatever moisture is evaporated, which in turns suppresses clouds. This is in line with decreases in the relative humidity and cloud cover that are found in the present study. The following sentence "*An increase in sensible heat flux is as well expected to cause severe turbulence and a taller planetary boundary layer, leading to moisture dilution and cloud suppression*." has been included on line 659 in Section 4. Furthermore, at the end of Section 5 (lines 731 – 733) we mention that the present study did not investigate the initial cause of the proposed feedback loop between humidity, clouds and solar radiation. At the end of Section 4 in the original version of the manuscript we also mention that influences from internal variability can play a role for the changes identified.

We have changed the end of Section 5 to the following text: "*Given the highly coupled nature of these variables, we employ a multi-dataset observational constraint framework to evaluate their co-dependent equilibrium trends. By confirming that the ERA5 thermodynamic trends on the whole map cleanly onto independent satellite (CERES, CLARA-A3) and observational (HadISDH, GPCP, GPCC) products, our analysis method provides a physically consistent real-world footprint supporting the apparent humidity paradox. Based on 36 climate model simulations, Qu et al. (2014) found that the response of low clouds due to climate change is the largest source of uncertainty driving differences in climate sensitivity between the models; our observationally-verified trend analysis provides an essential empirical baseline to help constrain these model uncertainties. Overall, the topic requires more investigation. Opposite trends in precipitation between ERA5 and GPCP/GPCC appear in Asia, North America and Africa (Sect. 3.6). In addition, t*he present study did not isolate the initial trigger of the proposed feedback loop—specifically, whether surface humidity drops or cloud clearance acted as the primary driver*.*"

The major of the study is that it does not apply proper statistical instruments for the analysis. It rather loosely browses across different statistics derived from different diverse datasets. We find here model-based atmospheric reanalysis (ERA5), satellite products of different quality, resolution, and coverage (CLARA, CERES), in situ datasets (HadISDN, GPCP, GPCC). What does this zoo of datasets make to the question in scope? It makes the text long, logically fractured, and complicated to read. I don't understand the needs for such complications. Just opposite, in my opinion, if ERA5 – as an internally consistent and complete dataset – demonstrates the constraining effects of differential warming on the hydrological cycle, it should be sufficient to analyze ERA5, and then, in Discussion, to show how good/bad the reanalysis is with respect to observational datasets (and whether it does/doesn't matter).

So, the most important part of any study – **the method** – is actually missing in the Data and Method section. This is for a good reason though because the authors do not have any logically justified analysis method for their study, but just a more or less trivial set of popular statistical indicators, mostly trends, of more or less relevant variables. The presented set of statistics does not support or question the main hypothesis of this study.

By comparing ERA5 reanalysis data against multiple independent, high-quality observational datasets, our methodology addresses possible reanalysis biases and establishes confidence in the trend analysis. We believe this is a robust, state-of-the-art approach for studying global land surface climate trends from 1979 onward. While individual trends of variables like humidity or precipitation have been studied in isolation, the aim with our study is to simultaneously evaluate ERA5 across three distinct earth system components (humidity, radiation, precipitation) using independent observations (HadISDH, CERES/CLARA-A3, GPCP/GPCC) to investigate a possible feedback loop driven by the accelerated ocean-land warming contrast. By analysing the Bowen ratio links moisture availability with temperature trends and a shift in the heat fluxes, which expose underlying mechanisms of land-atmosphere coupling, thus, whether available energy is used to evaporate water or heat the air. Investigating water vapor response to temperature directly addresses the Clausius-Clapeyron relationship, which gives an indication of possible moisture deficit over land.

To describe the objective of this study more clearly, we have included the following text at the end of Section 1: *The objective is to investigate a possible feedback loop between ERA5 humidity, clouds, and solar radiation driven by the accelerated ocean-land warming contrast. Given the highly coupled nature of these variables, we employ a multi-dataset observational constraint framework to evaluate their co-dependent equilibrium trends. While individual trends of the current variables have been studied in isolation, the aim with our study is to simultaneously evaluate ERA5 across three distinct earth system components (humidity, radiation, precipitation) using independent observations (HadISDH, CERES/CLARA-A3, GPCP/GPCC) to investigate a possible feedback loop driven by the accelerated ocean-land warming contrast. Finally, we incorporate recent studies on rapid global land-warming to corroborate our findings."*

We have also included the following sentence in the beginning of Section 3: "*The comprehensive outcomes with a weak analysis are presented in this section, while the primary findings relevant to the present objective are discussed and contextualized in Section 4*."

ERA5 utilizes a highly sophisticated data assimilation method that combines model physics with global observations, which means the raw ERA5 outputs are in general reliable for broad-scale spatial and temporal trends, providing a standard baseline in climate science. By confirming that the ERA5 thermodynamic trends on the whole map cleanly onto independent satellite and observational products, our analysis demonstrates that the observed signal is a robust physical footprint rather than an artifact of reanalysis data assimilation. This cross-platform agreement significantly reduces structural uncertainty. The following sentence related to ERA5 has been included at the end of Section 2.1: "*Variables like humidity, radiative fluxes and precipitation are not directly constrained by the assimilation of observations, they act as products of the underlying model physics. To asses them against independent observations is therefore essential to ensure their reliability*.".

The statistical indicators used in the present study are standard/common ones used to evaluate applied regression, predictive and evaluation statistics. The metrics RMSD and MBE serve different roles and used together provide a complete picture of data set's accuracy. To obtain an objective evaluation, a standard hypothesis test is used here as well to determine if observed results are meaningful trends or natural fluctuations.

My other comments are less significant but still pinpoint serios weaknesses and problems with the manuscript.

1. The manuscript is poorly written. Many sentences and even paragraphs are impossible to understand. An unconventional terminology is used in several places. E.g., what is "the humidity paradox"? I do not see any "paradox" in the discussed effect. What is "climatology scene"? E.g., line 180, "data product … mounted on the Terra and Aqua spacecraft". How could a data product be mounted on a satellite? How CERES could simultaneously be a data product, a project, and an instrument? E.g., line 200, "The irradiances are calculated based on temperature …" Are irradiances measured or calculated? How could cloud fraction be corrected "in the same way", based on temperature?

   We agree with the referee. We think the correct descriptive phrase needs to be used. With "apparent" it meets the definition of a false paradox, while the statement sounds impossible at first glance. To explain the apparent paradox and improve the first paragraph of Section 5 we have changed to: "*The increase in absorbed solar radiation by the land surface in addition to the enhanced greenhouse effect at present implies thus even more stress on the terrestrial water cycle. The slower warming of the oceans at present means a limitation in evaporated* *moisture to keep pace with the rapid increase in the temperature over land. The apparent humidity paradox describes a superficial contradiction in the changing climate system: while global warming drives increased oceanic evaporation and a higher absolute atmospheric water vapor capacity, relative humidity and cloud cover over land are simultaneously decreasing. By modifying this phenomenon as 'apparent,' we explicitly frame it as a veridical or false paradox—an intuitive contradiction that can be entirely resolved by evaluating the distinct thermodynamic constraints of land-atmosphere coupling. In the regions we studied, the increase in evaporative demand, caused by enhanced absorption of radiation at the surface, is most obvious where precipitation decreases. We assume that the ocean-land warming contrast doesn't exist during more gradual natural climate change since it goes slow enough in the ocean and over land so crossing of the vapour-liquid phase is not perturbed. The exception to a slow change in the climate is extreme volcano eruptions*."

Both terms are widely understood, but we have changed to "*climatological scenes*", which means geographical representations that display the spatial distribution of climate data over the entire Earth. Instead of showing daily weather, fluctuations are averaged out to reveal overarching characteristics, here with respect to total annual precipitation.

We have changed to "*CERES Energy Balanced Filled (EBAF) TOA Edition 4.2 data product (Doelling et al., 2013, 2016; Loeb et al., 2018) has been used in our analysis, where the CERES instruments were mounted on the Terra and Aqua spacecrafts launched in 1999 and 2002, respectively*.".

Yes, the CERES EBAF-surface radiation product uses measured solar radiation at the TOA as its foundational constraint. Surface fluxes are calculated based on radiative transfer models that are driven by satellite and reanalysis data. The outputs are adjusted so that the computed TOA values strictly align with measured TOA fluxes from the CERES instrument.

We have changed the phrase "*in the same way*" to "*in a similar way*". The primary is also here to balance energy at the TOA and the cloud fraction is therefore integrated into radiative transfer models. The algorithm calculates irradiances based on the CALIPSO/CloudSat-adjusted cloud fractions, forces the results to match observed CERES TOA fluxes using mathematical frameworks (e.g., Lagrange multipliers).

2. Physical interpretations are frequently unclear or directly incorrect in the manuscript. E.g., around line 35, "The specific heat capacity is fairly insensitive … and cannot explain amplified land warming", just opposite, it is sensitive as follows from the heat balance equation, and it can partially explain the amplified warming, and that it exactly what S. Manabe showed and many other studies confirmed after him. g., around line 45, "Moist air … has a higher heat capacity and resists changes in temperature". This is true, but this is minor effect; the major effect is that water vapor is the greenhouse gas by itself.

   The large heat capacity of the ocean is not the primary reason for the enhanced warming over land. The long-term global warming contrast (continents are warming significantly faster than oceans) is thus fairly explained by the specific heat capacity, since it is primarily a transient factor. The primary, permanent drivers of the ocean-land warming contrast are discussed and presented in the manuscript. To make it clearer we have changed the related text to*: "The specific heat capacity of water is fairly insensitive to increasing radiative forcing and cannot, therefore, explain the amplified land warming relative to the ocean in a changing climate (Manabe et al., 1991; Sutton et al., 2007). Instead, it is the large heat capacity of the* *ocean (including the deeper mixed layers) that drives the transient warming lag. An increase in globally integrated ocean heat content of the layer 0–2000m (Ishii et al., 2017) has contributed to an increase in the ratio between land and ocean warming."*

For us to better understand this comment we would like the referee to explain why water vapor as a greenhouse gas being relevant to cause the ocean-land warming contrast at present. If the referee instead means strengthening of the greenhouse effect, then we agree this is the original cause. We are also clear with that in the beginning of the same paragraph in Section 1.

3. Discussion of cloud effects is very shallow and problematic. Different types and layers of clouds have different effects and feedbacks. Those are not discussed. Also the important fact that convective clouds cover only a small fraction of the ERA5 grid cell and therefore have only a limited effect on the radiation budget, has not been discussed.

   Convective clouds correctly cover only a small fraction of a grid cell, but there is a potential opposite bias effect on the radiation budget than what the referee expect. Sub-grid convective clouds are treated in ERA5 using mass-flux parameterization and statistical cloud-cover schemes. The updraft and downdraft mass fluxes are used to calculate the fractional area covered by active convective clouds. The reanalysis model treats then the clouds as fractional features, ensuring intense thermodynamic effects do not artificially saturate or alter the entire grid cell. To handle cloud overlapping the Monte Carlo Independent Column Approach (McICA, Section 2.5.5b) is used in ERA5. The algorithm effectively treats sub-grid variability by ensuring cloud properties overlap realistically across different vertical layers instead of unfairly blocking the entire column. The radiation scheme in ERA5 (Section 2.4.2) is called separately and receives only the fractional area of the convective clouds. This prevents single convective updrafts from overwhelming the large-scale radiation budget (such as excessive solar reflection or downward shortwave blocking).

   We agree with the referee's description of different effects from the clouds in the atmosphere. We find the study by Yao (2020) where the authors found that ERA5 total cloud cover essentially capture the spatiotemporal characteristics of measured cloud coverage, based on MODIS data, with regard to the climatic scale. This outcome supports the results in Figure 6 in our study, comparisons between total cloud cover and TOA net solar ration, and between total cloud cover and relative humidity in the study by Liu et al. (2023), that we refer to in the manuscript. However, we don't find any previous study that have evaluated ERA5 low cloud cover for the global perspective. This is an additional reason to compare short-wave solar radiation at the TOA from ERA5 reanalysis against observations when investigating impact on the surface energy balance at the land surface and trends over the investigation period. We have decided not to include additional information about the ERA5 clouds in the manuscript.

4. Statistical analysis (supposed to be in section 2.7) should be central for the statistical study, and hence, it must be carefully described and justified, but it wasn't. I found only a few trivial equations for some statistical indicators. As minimum, the following are required:

   A. How were different datasets homogenized to provide comparable statistical quantities? I see that they were not as relative humidity at 1000 mb in ERA5 is compared with the surface values in HadISDN (Figure 3)

When comparing different meteorological variables then homogenization is relevant to acts as a critical standardization step across multiple domains. The HadISDH humidity variables are homogenized. The Met Office Hadley Centre's HadISDH dataset quality controls and homogenizes its multi-variable humidity suite. The dataset processes hourly station records (spanning dew point and air temperature) into monthly means, which are then run through homogenization techniques to account for non-climatic shifts—like station moves, instrument changes, and changes in observing practices.

ERA5 humidity variables are generally not homogenized, since ERA5 is a data assimilation system and ingests observational data from varying networks (such as raw radiosondes and surface stations). We come across a study (Li *et al.*, 2020) where the authors conclude that ERA5 (and ERA-Interim) have biases in surface air humidity over China due to assimilating observations changed due to a shift from manual to automatic synoptic observation in the early 2000s. Evaluation of the data used by ERA5 and the performance of its analysis scheme provides however evidence that contradicts the above conclusion. The changes in ERA5 relative humidity after 2000 was also seen in HadISDH, a dataset whose production thus employs homogenisation to reduce impacts of detected changes in measurement biases. The evaluation of ERA5 can be read in Chapter 4.7.3 in the Technical Memo 881 from ECMWF.

We agree, however, with the referee and have changed to ERA5 relative humidity at 2m height in comparison with HadISDH, which means that changes have been made in Figures 3a and 3c. The negative decreases in the time series are now closer to each other, shown in Figure 3c. The update gives, however, somewhat higher values of the evaluation statistics shown in the right low of Figure 3, while better agreement occurs of the ERA5 and HadISDH mean values. We have kept the result of changes in ERA5 relative humidity, representing as an averaged of the boundary layer (Figure 3c), since this is relevant considering relation to changes in the low-level clouds. See the updated Figure 3 below.

B. How were different spatial/time resolution of datasets handled?

ERA5 and CERES monthly area-weighted mean values have been compared in Table 1, with respect to $0.25° * 0.25°$ and $1° * 1°$ latitude-longitude grid, respectively. These spatial resolutions have been retained in the comparisons. We don't see a reason for applying interpolation of a data set when comparing time series and trends with regard to the same areas. To be able to draw conclusions on trends over time, which is the purpose of the study, evaluation of ERA5 instead of validating the reanalysis has been carried out. There are no gaps in the ERA5 and CERES time series.

ERA5 and CLARA-A3 monthly area-weighted mean values have been compared in Table 2 and Figure 2. CLARA-A3 has the same spatial resolution as ERA5. In Section 2.3 we inform about possible issue with CLARA-A3: "*We do however take extra care in interpreting the trends in the CLARA-A3 after 2020 when the drift signal could be stronger*.". Even so, we don't find any spurious changes in the results from CLARA-A3 after 2020 (Figure 2). In Section 3.2 we explain why missing data for CLARA-A3 led to shortening of time period and reduced area in the comparisons with ERA5.

For HadISDH, the humidity variables are averaged over a period of one month over 5° by 5° grid boxes. In comparisons of the humidity variables between ERA5 and HadISDH the spatial resolutions of the data sets have been retained (see how we justify this above). Even so, in Section 2.7 we write that HadiSDH data set has not been area-weighted before the analyses, due to limitation in the spatial coverage. In Section 3.3 we discuss uncertainties induced in the HadISDH annual global trend in Fig. 4 due to a lack of data in the Southern Hemisphere. There are no gaps in the time series belonging to HadISDH.

Limitation in statistics for GPCC are presented in Fig. 12b, and discussed in Sections 3.6 and 5. Limitation in statistics is not the case for ERA5 and GPCP. GPCC and GPCP has the same spatial resolution of 0.5° × 0.5° (Sections 2.5 and 2.6), while ERA5 has the resolution of 0.25° × 0.25° (Section 2.1). In comparisons of area-weighted trends from the time series in total annual precipitation with regard to the investigation areas, shown in Figure 11, these resolutions have been retained in the study (see how we justify this above).

C. How were trends calculated? How were data outliers processed?

This is described in the manuscript. For example, in caption to Fig. 1: "All trends were obtained by linear regression of the period 2001–2023.", In caption to Fig.2: "Heavy solid and dashed lines denote linear fits of the annual time series represented by the light solid and dashed lines." See also answer to question D below. Data outlies were not identified.

D. How were smoothing and filtering applied?

For the ERA5 and HadISDH time series in Figures 3 and 4 the Savitzky-Golay finite impulse response smoothing filter of polynomial order has been used. Filtering has been performed in the meaning to investigate trends over land globally, over South America and the areas in the Northern hemisphere, but not in the attempt to remove errors and exclude outliers.

5. All dataset descriptions are rather poor and unclear. Section 2.5 is not a description but rather declarative advertisement of GPCP. The description must be homogeneous and focused on the features relevant to the study.

In Section 2.5 it appears that the GPCP spatial resolution is 0.5° × 0.5° latitude–longitude and data are available for the period 1983 – 2023. Relevant information about the time resolution, monthly data, are presented as well. You can also read that high-quality GPCC gauge analyses are incorporated to vastly improve the estimates over land, which we think is then relevant information. In the section, a description of how precipitation is incorporated based on estimates from several and diverse remote and in-situ observation data is given, which should be interesting for the readers. You can also read that new data fields have been introduced in the version (Version 3.3), to better characterize precipitation, used in the present study. A description of uncertainties in the estimates at different regions are included as well, which we think is relevant to inform about. We have however removed the following sentence in Section 2.5: "*Over the long-term, GPCP represents the current state of the art*."

6. Figure 9 is supposed to be important, but I could not understand what it shows.

Information in the sentences on lines 478-484 in Section 3.5 and in the figure caption to Fig.9 are included to understand the results. Even so, we have changed "(7 % K^{-1})" on line 480 to "(theoretically 7 % K^{-1})". The figure shows thus trends in the ratio between ERA5 total column water vapour (TCWP in %) and temperature at 2 m height (T_{2m} in kelvin (K)) of the period 1979 – 2023 with the aim to present results of the water vapour response for the reality with strong ocean-land warming contrast at present. We have included the black solid line in Fig.9 that represent the theoretically value of 7% per Kelvin for the uptake of moisture in air for a global perspective. Information about the water vapor response can also be found where we refer to previous studies in the manuscript, for example on lines 68-71 for a global perspective and lines

647 – 652 considering the ocean-land warming contrast. We have included the following sentence after the latter text (with start on line 652): "*Weaker water vapour responses and even negative changes are found in many regions on Earth in the present study (Fig. 9)*."

7. Figure 10 is rather obscure to me. Does it show that since 1979 the amount of reflected solar radiation has increased by 100% almost everywhere?

As routine, we use the Greek letter delta to represent the concept of a difference, here with respect to the rate of ERA5 surface upward solar radiation between the two periods compared. The mean value of the rate taken for the current area in the figures are close to one for all the twelve cases (not shown). We presented, however, the results in percent, which means values of 100% or near that represent no or small differences between the two periods, while values substantially lower than 100% are large differences between the periods.

Even so, we agree with the referee that the results can be more clearly presented, which means to flip the colours so higher values represent larger changes between the two periods. This means, instead of calculating the rate between the two periods the difference divided by the mean of the period 1979-1988 has been calculated. We have accordingly made the changes in the sub-figures, changed text in Section 3.5 and in the figure caption to Figure 10: "*Changes in ERA5 annual surface upward solar radiation (in percent) in the Northern hemisphere, obtained by comparing differences in mean values of the periods 1979-1988 and 2014-2023 divided by mean values of the former period*." See updated Figure 10 below.

8. Figure 13 should be Figure 1 as it supposedly shows the effect (differential warming) which this study addresses.

Figure 13 could be the first figure in the manuscript, but we are presenting the differential warming already in the beginning of Section 1, while then up to 2020 based on the IPCC AR6 report (2021). We therefore believe it could be relevant to update with newer data, at the end of Section 3, with the aim to strengthening the relevance of the ocean-land warming contrast for the findings of impacts on radiation and humidity over land. We have included the following sentence in the beginning of Section 3.7: "*In this section, we extend the analysis of global land and ocean warming up to 2020 from the IPCC AR6 report (Section 1) by presenting ERA5 annual near-surface temperature trends through 2025*."

9. Discussion in lines 660-675 is very confusing. It touches the Amazon and takes results from boreal areas. I did not understand the conclusion here. Is it less absorbed radiation – stronger warming?

We don't use results related to conditions in boreal areas and apply it to the situation in the Amazon, if this is what the reviewer implies with the comment. We write that "*the overall biophysical effect of deforestation leads to cooling in the boreal zone.*", but in tropical deforested areas surface warming was instead found due to the prevailing impact of evapotranspiration (Luo et al., 2024b). Alterations in surface albedo, however, partially counteract the warming in tropics. The latter phrases with the situation for Amazon can be related to the beginning of the paragraph (line 661) where we write "*Measurements in Amazon rainforest show that replacement of forest by pastures leads to a decrease in evaporation and consequently an increase in sensible heat flux (Gash and Nobre, 1997)*." So, it has about reduced evaporate cooling due to decrease in evaporation and increase in sensible heat flux that warm the near surface air directly. The process with "reduced evaporative cooling" is discussed on lines 492 and 643 in the original version of the manuscript.

Figure 3 updated, concerning the comment by the referee above:

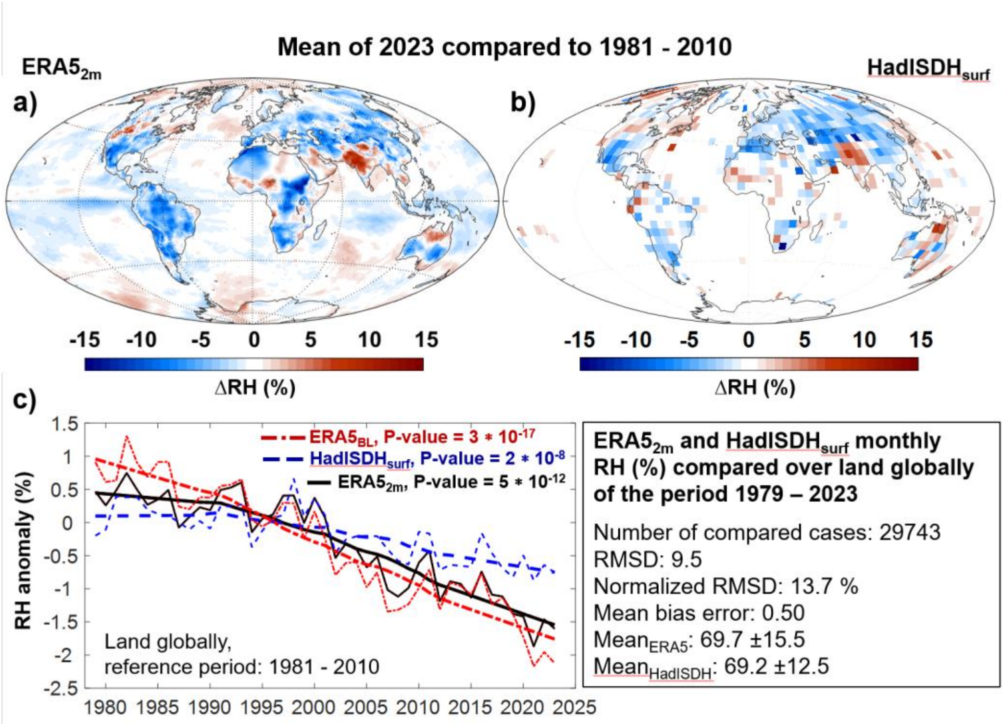

Figure 3. Comparison of relative humidity (RH) in the lower troposphere between a) ERA5 and b) HadISDH for 2023 relative to the period 1981–2010. Heavy black solid and blue dashed lines for ERA5 and HadISDH, respectively, in c) denote trend lines where the Savitzky-Golay finite impulse response smoothing filter of polynomial order has been applied on annual and global land time series represented by the light black solid and blue dashed lines. P-values of the trends are obtained by the Mann-Kendall test at the 95% confidence interval applied to the annual values. The ~~descriptive~~evaluation statistics in the lower right were obtained by comparing the monthly mean values of the ERA5 and HadISDH time series.

Updated Figure 10 (see comment 7 by the referee above):

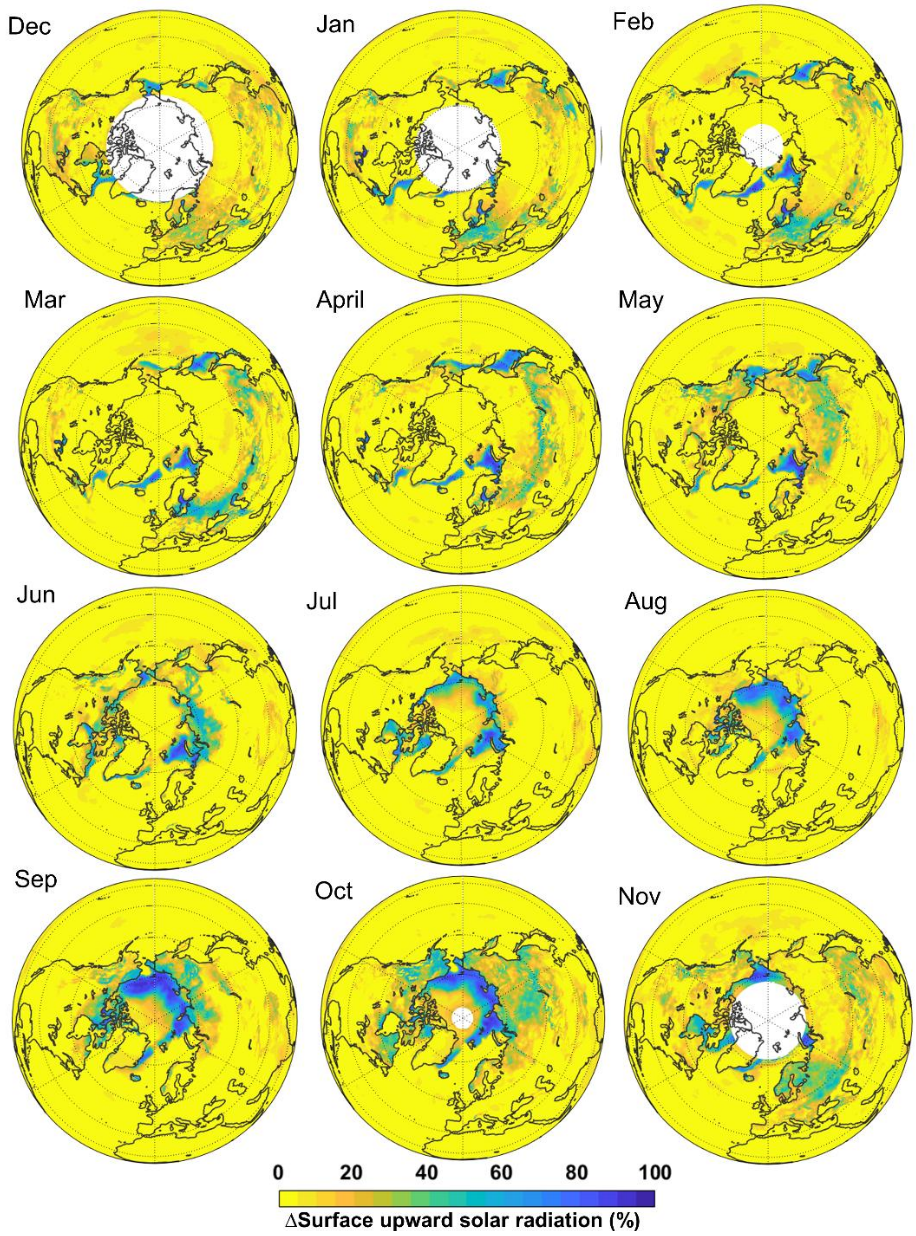

Figure 10. Changes in ~~annual~~ ERA5 annual surface upward solar radiation (in percent) in the Northern hemisphere, obtained by comparing ~~the rates of~~ differences in mean values ~~(in percent)~~ of the periods 1979~~2014~~-1988~~2023~~ and ~~1979~~2014-2023~~1988~~ divided by mean values of the former period. No data are available during the polar night, denoted with white colour.