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Comment on acp-2022-817

Anonymous Referee #1

Referee comment on "Opposing trends of cloud coverage over land and ocean under global warming" by Huan Liu et al., Atmos. Chem. Phys. Discuss.,
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Review of "**Opposing trends of cloud coverage over land and ocean under global warming**" by Liu et al. submitted to Atmospheric Chemistry and Physics

This paper analyses long-term trends and decadal scale variability over the globe using ERA5 reanalysis data for the last 40+ years. The authors argue for opposing trends in cloud cover over continents and over the oceans and try to associate this phenomenon with the changes in the relative humidity. Generally the topic is extremely interesting, as the analyses of interannual and longer-scale dynamics of cloud cover are still limited compared to the other meteorological variables. However, I have serious caveats preventing acceptance of this paper in its present state. They are both of conceptual and methodological nature. I suggest that major and mandatory revisions should be performed before the paper is considered for publications. I will be available for the inspection of the revised version if the editor decides so.

COMMENTS

(1) Introduction is somewhat very general and does not bring a clear message outlining why this study is important. Generally I would expect from the introduction the

justification of the use of ERA5 total cloud cover for quantifying long-term changes. In this respect I would expect here or in the data section explanation of what is total cloud cover in ERA5 (and more generally) in reanalyses. A useful guide on this WRT to ERA5 is <https://www.ecmwf.int/sites/default/files/elibrary/2016/17117-part-iv-physical-processes.pdf#section.3.6>. Moreover there were several attempts to compare regional and global patterns of the cloud cover over the ocean in visual data and reanalyses (see, e.g. DOI: 10.1002/joc.1490). The major problem of the reanalyses total cloud cover is the fact that it is strongly constrained by parameterizations used (see the link above). Given problems with adequate parameterizations of hydrometeors, especially in free atmosphere, this implies large (and most importantly regionally dependent) uncertainties in reanalysis total cloud cover. Very likely that in ERA5 considerable progress was done in some respects, however in the paper nothing was said about this. Also reference to Norris et al. (2016, Nature) could be considered under this light, as they analyse CMIP5 models where parameterizations in many respects similar to those used in reanalyses.

Also I am a bit unhappy with the context under which visual observations are described in the introduction (lines 30+). In fact for the period in which the authors are interested the problems of interpretation of visual data are mostly sampling problems over the ocean. Real problems with observational practices appear for periods prior 1960s and here references to Warren et al. (2007) and Eastman et al. (2011). Regarding incomplete sampling, cloud cover data from ICOADS do experience problems, which are however quantified and can be also treated using cloud cover PDFs (DOI:10.1175/JCLI4010.1, DOI: 10.1175/JCLI-D-17-0317.1).

(2) Selection of variables. Here I am concerned about the use of Tskin as a proxy for global climate warming (my understanding). Indeed, in ERA5 Tskin is different to that in e.g. NCEP DOE and other early reanalyses. However, the extent to that this variable is relevant to represent long-term climate variability is unclear, at least from the text. Why not to use simply surface air temperature? In any case changes in Tskin should be compared to Tair-surf. Figures show that the revealed patterns are similar (by my inspection) to what we expect from surface temperature.

My other concern is the use of RH. From section 1.2 I could not get what was done in fact. Looks that a kind of column – integrated RH was retrieved. This step requires justification. Given that RH is non-linear function of temperature and specific humidity and strongly varies over vertical coordinate, this metric (vertically integrated RH) looks for me being hardly interpretable. I might be wrong, but in any case a clear explanation should be provided here.

(3) Conceptual and the main one. With no regard whether T_{skin} or T_{surf} is used for me it is unclear the context of the analysis of temperature using the same methodology as for clouds (EOFs). Figures 2 and 3 (EOFs and normalized PCs) for TS and TCC are not informative in the context of the paper title and focus. Further use of the correlations (be it Spearman or Pierson) (fig 4) reveals vague results which can be hardly interpreted. Given the focus on contrasting / opposing trends over the continents and oceans, I would expect first of all a picture(s) showing time series of cloud cover over oceans and continents (also for different regions). Figure 5 (top panel) is not showing trend significance and in this respect is hardly interpretable. But even if all trends are significant (unlikely), the pattern does not imply unconditionally the message on opposing trends. It is also unclear which units are used, whether these are percentages of cloud cover or percentages of changes per decade. In the latter case you should note that mean values are different over ocean and land. For me the most novel picture is the mid panel in Fig 5 (correlations with RH), however it is unclear whether these are correlations before or after the de-trending.

(4) Links with different parameters (e.g. surface temperature, RH) could be revealed by using e.g. canonical correlation analysis performed on the basis of EOF decompositions. This approach may demonstrate patterns which are mostly correlated with each other, In your case very likely this would distinguish between the first and the second EOFs, as I assume that the first canonical pair may be composed by the first EOF of one variable and the second of the other. However, this is just a hypothesis, needs to be tested.

Misc - There are quite few technical and minor comments, however I assume (again, if the editor decides so) the major reworking of the MS and they can be addressed next. For now, I suggest to drop technical descriptions of the methodologies which are widely used and well known and concentrate on the results and outcomes.

