Review of Shin & Park (2019), The Relationship Between Low-Level Cloud Amount and Its Proxies over the Globe by Cloud Types

Overview

In Shin & Park (2019), the authors compare correlations between their estimated low-level cloud fraction (ELF) and other metrics like LTS and EIS for different low-level cloud types (as defined by a WMO-based typology).

ELF correlates better with cloud fraction than LTS or EIS for various different cloud types. Various components of ELF are also tested. All so-called "proxies" for cloud fraction struggle to diagnose cloud-free scenes. Suggestions are made to improve the ELF formulation to better account for cloud-free conditions and conditions in which cumulus updrafts occur in a background of more stratiform clouds and to parameterize the "scale height" as a function of environmental variables.

The analysis is extensive but not always well-conceptualized and the presentation needs significant work. In particular, it is not clear why it would be expected that LTS/EIS would correlate well with deep convective cloud types given their main use in analyzing shallow convective clouds or why we should expect to be able to approximate both shallow and deep convective cloud fraction with a single equation. Some variables that appear to be important and linked to the ELF derivation are left undefined. Figures are over-crowded and difficult to read. The overuse of jargon in terms of the cloud type abbreviations makes the reader's job very difficult.

Overall, substantial revisions are necessary to re-focus the manuscript, ideally to tell a more compelling story in the main narrative and perhaps provide more information as supplementary information (I rarely recommend moving material to a supplement, but here that may be quite helpful).

A suitably revised manuscript could be quite helpful for the modeling community and anyone else interested in estimating low cloud fraction and understanding its meteorological controls. I will happily recommend a revised version of this work be published in ACP assuming the authors are able to justify some of the choices made (or reduce the focus to what can be justified) and improve the presentation and organization of the manuscript.

Major issues

1. Jargon

The almost exclusive use of cloud type numbers (e.g., CL12) makes this paper extremely difficult to follow. (As a side note, "CL" is not a terribly intuitive abbreviation of cloud type either.) Table 1 is helpful but not sufficient, and does not list the combined types defined by the authors.

The authors should standardize how they describe each major cloud classification used (e.g., CL12 could be "shallow-to-moderate cumulus") and try to pair the descriptive words with the cloud type number as often as possible. Page 8, Line 29 does this very well — something like this should be done for the entire paper (including figure captions).

2. Treatment of LTS, EIS, and ECTEI

I am confused by the authors' treatment of LTS and EIS as low cloud "proxies" rather than as cloud-controlling factors. Clearly LTS and EIS correlate with stratiform clouds, but the strength of the boundary layer inversion is really only one relevant factor among several in explaining low cloud behavior. LTS/EIS can certainly be used as proxies for low cloud fraction, but this is not their primary/sole purpose.

Similarly, LTS/EIS really don't "diagnose" anything (e.g., Page 8, Lines 19-20). They are cloud-controlling factors (one of many!), not simple diagnostics in and of themselves.

This conceptual treatment leads to several statements that sound off, at least to my ears. For instance, on Page 9, Lines 4-5, is it truly "undesirable" that we can associate particularly large values of LTS/EIS with cloud clearing? This could be a useful observation to better understand potentially non-linear cloud behavior. This seems to me like a strange way to conceptualize LTS/EIS and why one would examine these variables.

The authors mention ECTEI in the abstract and (barely) define it in the introduction before noting it is similar to EIS and therefore not shown at the end of the Methods section. I would recommend having a supplement with the ECTEI results or not mentioning it at all (or only as a parenthetical). As written, the authors appear to promise an analysis they do not deliver.

3. Definition of "low-level" cloud and its reasonableness

While the observer-based methods define deep convection as "low-level" cloud based on the cloud base, there should be some discussion/reflection of whether this is a reasonable treatment in this analysis. LTS/EIS really are meant to explain cloud behavior in shallow boundary layers, not in deep convection. I don't particularly understand why we should expect one equation or metric to apply globally for both shallow and deep convection. If the authors do have a good explanation for this, it would be very helpful to provide it.

4. Missing variable in the derivation of ELF

Many times in the manuscript, the authors refer to and analyze a factor $(1 - \beta_2)$, but this is never defined. Please address this in the methods section. It also might be possible to reorganize the section deriving ELF to be more clear, especially with an eye toward the issues brought up in the final discussion of possible improvements for an "advanced ELF." Although the finer details of the ELF calculation addressed previously do not need to be explained in great detail, it should not be expected that all readers are familiar with PS19.

5. General presentation and organization of figures

The figures are far too crowded, and each subpanel much too small, to be easily interpreted by readers. In Figures 1-3, the black contours showing the climatology are nearly illegible. For Figure 1, a suggestion could be to split the figure up by cloud type (as is done for Figures 2-3) and have an added column for the climatology in its own map.

For Figures 2-3, I would also recommend subdividing further. One solution could be to have one figure include ELF and comparisons to LTS/EIS in one figure and the components of ELF in another. This could also help structure the discussion — first the differences between ELF, LTS, and EIS can be discussed, and then the contributions of the different components of ELF can be discussed.

It may also be a good idea to split up Figure 4 in a similar manner.

In Figure 5, the caption should explain that the color scheme is the same as that used in Figure 4. The open versus closed symbols also are not defined, although I assume they relate to day and night.

For the regressions in Figure 5, it would be good to address to what extent CL11 drives the regressions. Especially for subpanels b) and d), the scatter of points excluding CL11 (and CL0 and CLIM) do not appear to be very strongly correlated.

In Figure 8, the caption should make more clear that the adjustable scale height as a function of the environmental variables in g) and h) is shown as the "viridis" shading and is in units of meters.

6. Interpretation of ELF correlation with cumulus cloud fraction in Tables 2 and 3

On Page 12, Line 12, the authors write that ELF captures variations in cumulus clouds (CL12) better than LTS and EIS. Unless there is a typo in the tables, this is contradicted by the evidence provided in Tables 2 and 3. The global correlation of ELF with CL12 is ~0.03 whereas it is between -0.45 and -0.75 for LTS and EIS. Or is this sentence actually referring to CL84? In that case, the correlations are more all over the map. In any event, this is another good example of where the elimination of jargon in favor of clearly indicating which cloud type is being discussed would be helpful.

Specific issues

Page 1, Line 18: As the citation of Klein & Hartmann (1993) suggests, the efforts to quantify low cloud effects on Earth's climate long predate the last decade.

Page 2, Line 14: If you do choose to include ECTEI, its definition needs more exposition here.

Page 3, Eq. (5): It would be helpful to discuss that you then force the inversion height to lie between the LCL and the LCL plus a scale height in your analysis here. It's easy to miss as written. Also, for shallow convection, there's essentially no way for the inversion height to exceed the LCL plus scale height, right?

Page 4, Line 9: "f" does not denote the amount of water vapor, it is a function of water vapor.

Page 4, Line 25: Individual components of ELF really aren't "proxies" for low cloud fraction by themselves. It would be more straightforward to just discuss these as components of ELF.

Page 4, Line 32: It would be helpful to explain that cloud types 12, 84, and 39 are actually combinations of types 1+2, 8+4, and 3+9.

Page 5, Lines 15-16: Moisture supply is not the only difference between marine and continental boundary layers (different responses to diurnal solar heating comes to mind as potentially being important here too).

Page 5, Line 25: I would expect the relative humidity to matter more than the total amount of moisture here, no?

Page 5, Lines 28-29: It would be helpful here to discuss how much of the advantage ELF has over LTS/EIS/ECTEI is due to the freezedry factor alone.

Page 6, Lines 5-7: Why isn't the composite analysis shown? It could at least be included in a supplement. The result isn't particularly surprising but would be interesting to see.

Page 6, Line 10: Why is there no hemispheric asymmetry in stratocumulus amount? If meteorology is the main driver, one would expect the hemispheric trends to be out of phase. In the Southern Hemisphere, the seasonal cycle tends to peak in spring and trough in fall whereas the Northern Hemisphere tends to peak in summer and trough in winter, so perhaps only looking at JJA-DJF differences doesn't capture the Southern Hemisphere seasonality well. Discussing SON and MAM seasonality (even if not shown, or just put in supplement) could be useful here.

Page 6, Line 20: It would be helpful to explain why the non-centered correlation is computed in some sections a centered correlation is computed in others, and whether this has any implication for the interpretation of your results.

Page 7, Lines 25-27: The latent cooling effect of evaporation should also matter for lowering the LCL.

Page 7, Line 31: Please either indicate what the outlier value is on the plot or report it here.

Page 8, Section 3.3: It would be helpful somewhere here to explain clearly what the difference between LCA and AMT is and how this should be interpreted.

Page 9, Line 28: "What is necessary" should replace "What are necessary".

Page 12, Line 24: What does the “(stratiform clouds FQ)” mean here in context? Is it supposed to refer to an increase in stratiform clouds as cumuliform cloud FQ decreases?

Page 13, Line 6: What would a negative depth for the decoupled layer mean physically? Wouldn’t it just make more sense to define ELF piecewise rather than as a continuous function to account for these types of circumstances?

Page 13, Line 12: I do not understand what the “if any” means here. Surely you believe there is some appropriate variable, or why even discuss parameterizations of the scale height?

Page 13, Line 18: It would be good to list the download site for the ERA data here as well.