# Response to Anonymous Referee #3

**Thank you very much for your constructive and careful comments. It was greatly helpful to improve the quality of the draft.**

****Note) Following the request of Anonymous Referee #3, Figure 1 was divided into Figure 1 and 2. So, the figure numbers of subsequent figures were increased by 1.**

**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).**

- ➔ Following the comment, we relabeled all the cloud types, as explained in Table 2. Please see P5L10-12 in the tracked-change version.

**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.**

- ➔ We used the term “proxy” for the LTS and EIS, in order to keep consistency with our previous paper (Park and Shin 2019; PS19) which already used LTS and EIS as one of LCA proxies. At least in stratiform cloud regions, LTS and EIS have been used as proxies of LCA in many papers. Many readers will be familiar with this and there won't be much difficulty in understanding the concept. However, we agree that several statements could confuse some readers. Thus, following the comment, we modified the following.

**①“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.”**

- ➔ We agree with the comment and included this explanation in P2L14-15.

**② “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."**

- ➔ Following the comment, we rephrased this sentence. Please see P10L1-3.

**③ “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. "**

- ➔ We agree that the word “undesirable” is not appropriate here. Thus, we changed the word "undesirable" to "unexpected".
- ➔ We also noted that the strong positive correlation between LTS/EIS and noCL FQ might indicate a non-linear response of clouds to the inversion strength or the existence of other factors controlling noCL. Please see P10L27-30 in the tracked change version.

- ➔ In addition, we stated that the target areas of LTS, EIS, and ECTEI are over the ocean. Please see P7L29-30 and P11L16-17 in the tracked-change version.

**③ 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.**

➔ Following the comment, we removed ECTEI from the abstract. Although not shown, the analysis results of ECTEI are **almost identical** to EIS as mentioned at the end of the Methods section (P5L25). Thus, we did not include the results of ECTEI in the supplement.

**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.**

➔ Because deep convection is controlled by similar physical processes as shallow convection (Park 2014a,b), it is unnecessary to use separate formulation for shallow and deep convections. In addition, at least in terms of cloud fraction, we thought that a decoupling hypothesis can describe the changes in cloud fraction from the well mixed (Sc), partially decoupled (Sc-Cu), and fully decoupled (Cu, Cb) conditions. This is the philosophy of ELF. We briefly included this explanation in P5L13-14.

**4. Missing variable in the derivation of ELF**

**Many times in the manuscript, the authors refer to and analyze a factor (1 - β2), 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.**

- ➔ Following the comment, the definition of (1 - β2) is added in P3L22-23 in the tracked-change version. We also reorganized the structure of explaining the definition of ELF (P3L21-P4L6). We did not add very detailed derivation of ELF here, because it requires a lengthy explanation of the conceptual framework with a diagram.

**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.**

- ➔ Following the comment, we divided Figure 1 to Figure 1 and Figure 2.

**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.**

- ➔ Following the comment, we divided Figures 3, 4, and 5 (previously Figures 2-4) and panels of $z_{LCL}$, $z_{inv}$, α, and 1-$\beta_2$ are moved to supplement (S1, S2, S3).

**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 the caption of Figure 6, we explained that the color scheme used is the same as that used in Figure 5. The open and closed symbols are explained too.
- ➔ Following the comment, we also added squared regression coefficients (R^2) without Fog (CL11) in parenthesis. A corresponding explanation is written in P8L31-34 in the tracked-change version.

**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.**

- ➔ In the caption of figure 9, we specified that the adjustable scale height is shown as shading and 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.**

- ➔ It seems that the reviewer misunderstood. Tables 3 and 4 do not show the correlations between proxies and LCA; they show the correlations between proxies and **the frequency (FQ)** of individual cloud type. If any proxy is perfect, the correlation between the perfect proxy and CL FQ should be identical to the correlation between the LCA and CL FQ.
- ➔ As an example: The global correlation between cumuli's LCA and FQ is 0.10. ELF has a similar correlation of -0.03. LTS and EIS have the correlation values of -0.45 and -0.75. In this case, ELF is a better proxy for LCA than LTS and EIS.

**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.**

- ➔ We changed "last decade" to "past few decades". Please see P1L17 in the tracked-change version.

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

- ➔ Following the comment, we added the definition of ECTEI in the Method section. Please see lines P3L17-20 in the tracked-change version.

**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?**

- ➔ Following the comment, the range of the inversion height is added in Eq. (6). Please see P3L25 in the tracked-change version. As you said, the inversion height cannot exceed LCL plus scale height, but since scale height is $\Delta zs$ = 2750m, the upper limit of inversion height can easily exceed the height of 700hPa.

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

- ➔ We specified that "f" is an increasing function of water vapor. Please see P4L14 in the tracked-change version.

**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.**

- ➔ We rewrote the sentence. Please see P5L1-3 in the tracked-change version.

**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.**

- ➔ The combination of the cloud types are explained in Table2. Please see P5L10-13 in the tracked-change version.

**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).**

- ➔ We specified that the moisture supply is "one of the important factors", rather than "primary factor". Please see P5L29-30 in the tracked-change version.

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

- ➔ In the far northern continents and Arctic area, the freezedry factor, which is a function of the absolute moisture amount, becomes very important for the onset of noCL. The relative humidity is also important but the amount of moisture is a more comprehensive concept.

**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.**

- ➔ First, we briefly explained why ELF is improved by the freezedry factor in P6L11-12. The quantitative improvements are already investigated in our previous study, so we cited the paper (PS19) here.
- ➔ The effect of the freezedry factor is discussed many times in subsequent sections (e.g. P7L13, P8L23).

**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.**

- ➔ The composite is not shown here because it will be included in the paper we are preparing. We cited the paper so future readers could find corresponding figures. Please see P6L23 in the tracked-change version.

**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.**

- ➔ As Klein and Hartmann (1993) shown, stratiform clouds in the Namibian and Peruvian stratocumulus decks tend to peak in SON. Since the detailed analysis on the seasonal cycle is not the scope of our paper, we just cited Klein and Hartmann (1993) here. Please see P6L28-30 in the tracked change version.

**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.**

- ➔ We explained why the non-centered correlation is computed here. Please see P7L7-9 in the tracked change version.

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

- ➔ Corrected. Please see P8L28 in the tracked-change version.

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

- ➔ We extended the range of x-axis of Figure 6, so the scatter located outside of the plot is now located inside of the plot. Please see Figure 6, and also see P8L31-34 in the tracked-change version.

**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.**

- ➔ Following the comment, we added an explanation of the difference between LCA and AMT in Section 3.4. Please see P10L32-33 in the tracked-change version.

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

- ➔ Corrected. Please see P11L20 in the tracked-change version.

**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?**

- ➔ "Increase in" is mistakenly omitted here, so we corrected it. Please see P14L19 in the tracked-change version.

**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?**

- ➔ Following the comment, we explained the physical meaning of a negative decoupled layer depth at P12L1-2 in the tracked-change version.
- ➔ As you commented, it can be one option to define ELF piecewise by separating the cases where a decoupled layer has negative depth or positive depth. However, such a strategy does not seem to work well when we tested it. Probably because the calculation of the inversion height is not accurate.

**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?**

- ➔ It seems like "if any" is unnecessary here, so it is deleted. Please see P15L7 in the tracked-change version.

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

- ➔ Following the comment, we listed the download site for the ERA data. Please see P15L18-19 in the tracked-change version.