Response to Reviewer #1's Comments:

Yuxin Zhao et al. (Author)

We appreciate the constructive suggestions and insights provided by the Reviewer #1, which have helped us identify areas for improvement in our manuscript. We acknowledge that our manuscript overly elaborated on possible mechanisms in the results section, thereby detracting from the clarity of the main points and the novelty of the study. To address this, we have relocated some of the discussions of the possible mechanisms to the "Conclusion and Discussion" section. We have revised the manuscript according to the Reviewer #1's comments. Please see our point-by-point response to the comments. All revisions are shown in revised manuscript by using track changes.

General responses:

This is a comprehensive and robust characteristics of deep convective cloud features, radiative effects and controls over a region with particularly interesting geography. The paper is long, and it is hard to keep track of what results are novel versus recounting literature. Ideally, the paper would have been written with a clear and focused results section without referring to so much literature, and then discussed the literature in a dedicated discussion section. Broadly speaking the authors seems to confirm that the differences they see between the clouds in the two regions are consistent with understanding in the literature.

I can't say I find the paper particularly novel or easy to read. However, I can see it being a useful characteristics for other scientists to build upon. So, apart from the need to address the minor comments below, I can see value in publication.

Response: We greatly appreciate the reviewer's insightful comments for our work. We fully agree with the reviewer's comments. Indeed, our manuscript is very long, aiming to comprehensively analyze the structural characteristics, cloud radiative effects, precipitation, and the influences of meteorological fields and aerosols on deep convection systems across various regions. We also consider that extensive previous findings and references have potentially masked our own results in our manuscript. In response to the reviewer's comments and suggestions, we have revised the discussion of potential mechanisms and references to literature in the "Results" section, as well as adjusted the description in the manuscript. Discussions for potential mechanisms relevant to our findings have been moved to the "Conclusion and Discussion" section. Detailed modifications can be found in the revised manuscript we have submitted.

Minor comments

1. Title – I haven't seen anything in the paper that makes me thing the clouds on TP are "unique", please remove the word or justify with respect to deep convective clouds in general.

   Response: Thank you very much for the reviewer's helpful comments. We agree that the word "unique" is too strong. So, it is changed to "distinct" in the revised manuscript. In this study, we aim to demonstrate that the different structural characteristics, cloud radiative effects, precipitation, and the impacts of meteorological fields and aerosols of deep convection systems over the TP compared with the TO. Specifically: 1) Due to the influence of terrain forcing and other factors, the DCSs over the TP are significantly thinner, but their anvils are denser (geometrically thin but with high optical depth), resulting in a stronger shortwave radiative cooling effect. 2) The dry, cold surface of the TP results in less upwelling longwave flux emitted by the surface under clear-sky condition, making the radiative heating of the DCSs more efficient at the surface. 3) Even when the cloud base of the DCS is close to the surface (distance < 3 km), the cloud base temperature over TP can still be partially below 0°C, which is rarely observed in lower-altitude regions. The colder cloud base also contributes to the unique impact of aerosols on the development and precipitation of DCS over the TP. We apologize that our initial presentation may not have effectively conveyed our viewpoint. To address this, we have added the discussion in the "Conclusion and Discussion" section. Please refer to the revised manuscript for specific modifications.

2. Title – "Tropical oceans" is much broader than the use here. At best tropical "Indian" ocean could be used.

Response: We appreciate the reviewer's advice. It is revised to "tropical Indian Ocean". In fact, our definition of the TO region was intended to select tropical oceanic areas adjacent to the TP while maintaining consistency with the TP region's area.

3. L19 - "competition between invigoration and radiative effects of aerosols" I think you should be specific about what radiative effect is competing with invigoration. This is a bit vague for an abstract. Perhaps "direct" radiative effect?

   Response: In fact, not only does the direct radiative effect (i.e., aerosols blocking incoming solar radiation at the surface) exert a suppressing influence, but the semi-direct radiative effect also does. The absorbing aerosols heat the atmosphere, enhancing its stability and consequently inhibiting convection development. Simultaneously, this process promotes the evaporation of cloud droplets, further suppressing convection (Ackerman et al., 2000). Generally, this mechanism is referred to as the aerosol semi-direct effect. To keep the abstract as clear as possible, we revise the "radiative effects" to "direct/semi-direct radiative effects".

   See the line: 19.

4. intro/methods – The lack of map in the main text doesn't help the reader. That's your choice, but please at least refer to figS1 early on so the reader can look at where you are studying. I also do not know the motivation behind the specific TO region you've picked, the topical ocean is much more general that box. What's special about that part?

   Response: Thank you very much for the reviewer's comments. To enhance the readability of the manuscript, we have moved the study area map originally presented in FigS1 to the beginning of the Methods section. Additionally, we have included a description of the study area in the Introduction section. Of course, the tropical ocean is a very large area. To make the description more precise. Following the reviewer's suggestion (Comment #2), we have revised the description of this region to the tropical Indian Ocean. We did not have any specific intention in choosing this box as the study area. We simply wanted to select a nearby ocean region for land-sea comparison and for comparing different latitudes. We selected only the region 0°N~7°N and 68°E~93°E to maintain consistency with the TP region's area, ensuring meaningful comparisons of frequency analysis. We have added the rationale behind our partition selection in both the Introduction and Methods sections. Please refer to the revised manuscript for details.

See the lines: 60-61 and 99-100.

5. FigS1 – please mark on the sub-divisions of TP that you use in table 2.

   Response: Thank you for your suggestion. We have now marked the sub-region divisions from Table 2 in the original Fig. S1, which is now Fig. 1.

6. Section 2.2 – Have ERA5 or MERRA2 been evaluated over the tibetan plateau. This seems important. ERA5 is ultimately a model with a spatial resolution that may be affected by large gradients in terrain in the region.

   Response: We fully agree with the reviewer's perspective. It is necessary to assess the uncertainty of the data before using it. The high and complex topography of the Tibetan Plateau indeed induces challenges for model simulations. However, the meteorological data from ERA5 is still reliable. For example, Han et al. (2021) found in the evaluation of meteorological parameters derived from ERA5 based on radiosonde measurements on the Tibetan Plateau that ERA5 data has good reliability for atmospheric parameters in the free atmosphere. The bias and root mean square error (RMSE) for temperature are generally less than 1.2 K, and for wind speed, the bias and RMSE are generally less than 2 m/s.

   The aerosol data provided by MERRA-2 has also been evaluated over the Tibetan Plateau in the past. For example, Xu et al. (2020) found that MERRA-2 aerosol optical depth (AOD) was consistent with Aerosol Robotic Network (AERONET) and Multi-angle Imaging Spectro Radiometer (MISR) over the Tibetan Plateau. The correlation coefficients were 0.73–0.88 and 0.94, respectively. Here, AERONET provides ground-based observation data, with aerosol retrieval accuracy reaching 0.01-0.02 (Xia et al., 2004), commonly used to validate the accuracy of benchmark data from remote sensing retrievals. Of course, due to the lack of observation data over the Tibetan Plateau, reanalysis data contains a certain degree of uncertainty Li et al. (2024). However, considering our study requires aerosol mass concentration data with complete spatial coverage and high spatial and temporal resolution, MERRA-2 is the most suitable dataset to meet this requirement.

We have added the evaluation of reanalysis data over the Tibetan Plateau in Section 2.2.

See the line: 157-160 and 171-173.

7. L168 – Presumably there's a third criteria that there is cloud present between the base and the top?

   Response: We greatly appreciate the reviewer's comments. Perhaps our description was not clear enough, which caused confusion for the reviewer. However, the third criteria—cloud presence between the top and base—is not necessary. When selecting DCS samples, the cloud base height and cloud top height were obtained from the parameters "cloud base height" and "cloud top height" provided by 2B-CLDCLASS-LIDAR, which is one of cloud layer products from CloudSat and CALIPSO. The continuous vertical range of clouds is considered one layer; any interruption indicates a different layer. Therefore, the presence of clouds is guaranteed between the cloud base and cloud top, and no additional requirements are needed. Furthermore, the "bwboundaries" function was applied to verify connectivity when selecting DCS samples, ensuring that DCC also represents continuous cloud presence without interruption.

8. L179 – No "high cloud" in the image despite cloud tops over 15km. What's the definition of "high cloud"?

   Response: The "high cloud" refers to cirrus, cirrocumulus and cirrostratus. The classification is from 2B-CLDCLASS-LIDAR datasets, based on cloud height and phase, maximum effective radar reflectivity factor, and temperature, as well as the presence of precipitation reaching the surface. The detailed cloud features for "high cloud" are as follows: (1) cloud base height more than 7 km. (2) no rain. (3) horizontal dimension is 1 to 10^3 km. (4) vertical dimension is moderate. (5) liquid water path (LWP) = 0.

"high cloud (cirrus, cirrocumulus and cirrostratus)" is added in the revised manuscript.

See the line: 109.

9. Eq1 – Another way to look at this is the abs(dV/dz). I think that's a bit more intuitive to understand. It does mean that you can identify high shear as a result of strong low level winds, and weak upper level winds. Does this occur in your data? Is it you intension to include such conditions? Do you think strong low winds with weak upper winds is likely to have the same the effect as weak low winds and strong upper winds?

Response: In our manuscript, the wind shear does not differentiate between the relative magnitudes of the upper and lower wind speeds. Instead, it is represented directly by the absolute value of the wind speed difference between the two layers. If there is a wind speed difference between the two layers of the atmosphere, it indicates relative motion between them, which is conducive to horizontal cloud development. This method of representing wind shear using the absolute value of the wind speed difference is commonly used in previous studies (e.g., Sherwood and Wahrlich, 1999; Naud et al., 2008). To address the reviewer's concerns, we removed the step of taking the absolute value and simply calculated dV/dz. The results are shown in Fig. R1.

The results show that the upper-layer wind is stronger than the lower-layer wind in most samples, although there are cases where the lower-layer wind is stronger (see the text annotation in Figure R1). Examining the relationship between wind shear and DCS width under different conditions, we find that larger wind shear promotes the horizontal development of DCS, regardless of whether the upper-layer wind or lower-layer wind is stronger. In the TP region, the results for cases with stronger lower-layer wind are not significant, likely due to the small sample size, whereas in the TO region with more samples, the relationship is more significant. Based on the above, we believe it is unnecessary to distinguish between the relative magnitudes of the upper and lower winds in the calculation of wind shear. To ensure an adequate sample size, we use abs(dV/dz) to represent wind shear. In reference to the reviewer's comments, we have revised the wind shear expression to abs(dV/dz). And we added the description of this method in the revised manuscript.

See the lines: 212-213 and 216-217.

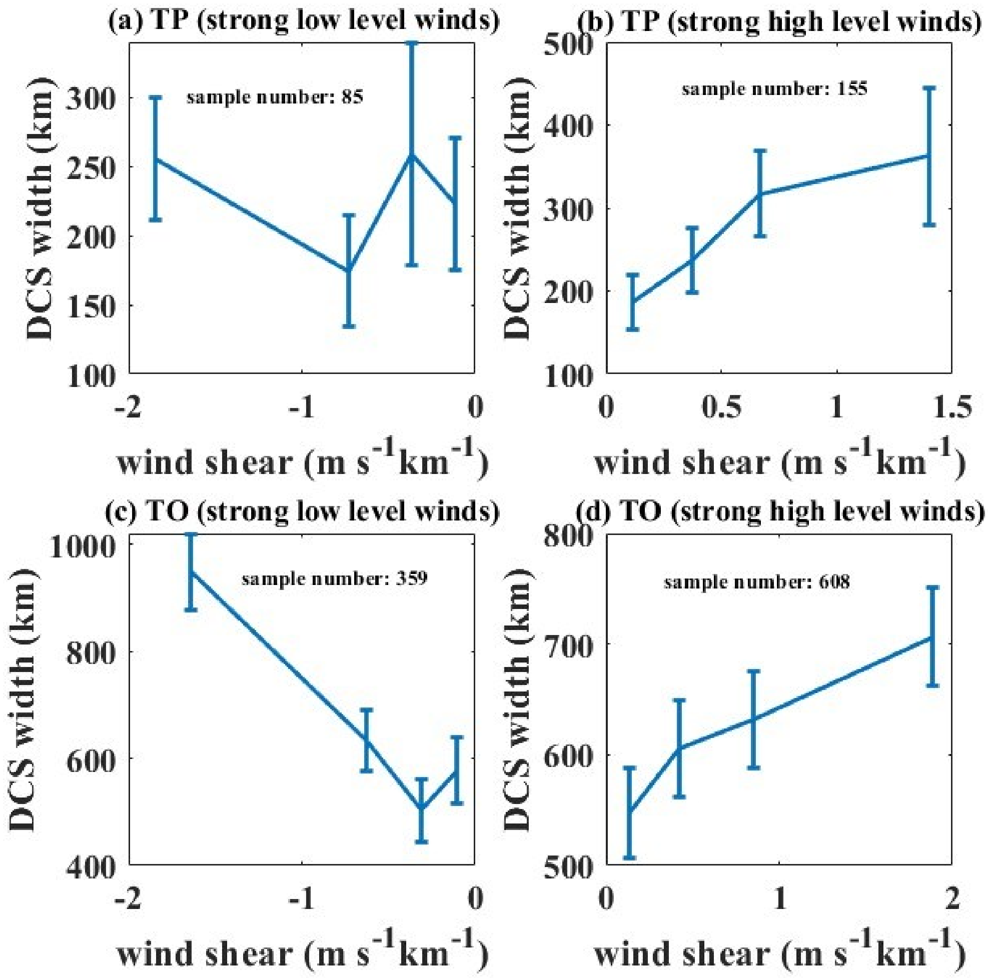

Figure R1: Bin-averaged wind shear (dV/dz; m $s^{-1}km^{-1}$) with DCSs width (km) from different subregions. The error bars represent the standard error of the mean (SEM=standard error /$\sqrt{n}$).

10. L195 – please can you describe the theoretical basis for using the gradient in theta_es to study the impact of conditional instability. I would have expected you would relate the environmental temperature to theta_es to look at stability. And for conditional stability you would need to consider the dry adiabat too (see AMS glossary on "conditional stability"). I did look at Li et al (2018) but I found no information to justify the approach.

Response: Thank you very much for the reviewer's comments. The theta_es is the "saturated equivalent potential temperature" or "pseudo-equivalent potential temperature". The theta_es is the potential temperature that takes into account the water vapor mixing ratio. For saturated air layer, the following formula can be derived:

$$\frac{\partial\theta_{se}}{\partial z} \approx \frac{\theta_{se}}{T}(\gamma_s - \Gamma)$$

where γ_s is the moist adiabatic lapse rate, and Γ is environmental lapse rate. When $\frac{\partial\theta_{se}}{\partial z}$ is positive, γ_s is larger than Γ, which means the atmosphere is stable. And the larger the $\frac{\partial\theta_{se}}{\partial z}$, the more stable the atmosphere. We take $\frac{\partial\theta_{se}}{\partial z}$ as diagnostic of the degree of atmospheric instability.

In response to the reviewer's suggestion, we have rechecked the glossary of conditional stability. Conditional instability refers to the environmental lapse rate lying between the dry and moist adiabatic lapse rate. It means that the air layer is stable for unsaturated (clear) air parcels and unstable for saturated (cloudy) air parcels. In fact, when $\frac{\partial\theta_{se}}{\partial z} > 0$, the air layer is absolutely stable. This description "conditional instability" is inappropriate here. It is revised to "atmospheric stability".

Regarding the reviewer's mention of "relate the environmental temperature to theta_es to look at stability", we wonder if it meant that the reviewer suggest that we calculate the environmental lapse rate. In fact, the calculation of theta_es requires the environmental temperature, meaning theta_es already include information about the environmental temperature. Under moist adiabat, the theta_es is constant at different altitudes. Therefore, its gradient can describe the relative magnitude of the environmental lapse rate compared to the moist adiabatic lapse rate.

Consequently, the vertical gradient of theta_es can characterize the stability of atmosphere. The gradient of theta_es has been widely applied as a criterion for atmospheric instability. For instance, it has been shown to have a significant impact on cloud overlap (Naud et al., 2008) and serves as a predictor for tropical cyclone occurrence (McDonnel and Holbrook, 2004). McDonnel and Holbrook (2004) indicated that the gradient of theta_es is a measure of the potential for cumulonimbus convection from a lapse-rate stability viewpoint.

In summary, the gradient of theta_es is a typical proxy for atmospheric stability, widely used for convective condition analysis. It can be employed in our study to describe the thermodynamic state influencing the development of deep convective clouds. For clearer expression, we have revised the description of theta_es in the revised manuscript.

See the lines: 218-222.

11. Results section – this includes a lot of references and discussion for results. I suggest just not labelling those sections as "results" or calling them "results and discussion".

    Response: Thank you for the reviewer's suggestion. Extensive discussion and citations might have interfered with our presentation of the results. We do not change the title of the results section, but we remove some of the references and move some discussions to the discussion section.

12. Table3 – I'm surprised by how negative these CRE's are, I think this is because they're daytime-only? I think it would be worth labelling them as such in the caption.

    Response: Yes, the data for the shortwave cloud radiative effect only comes from daytime observations, around 13:30 local time. The shortwave cloud radiative effect observed at this time, when solar radiation is nearly at its peak, is very strong. In our study, the calculation of CRE is based on profiles where DCS/DCC are present, rather than being weighted by cloud cover over an area (or grid point), which represents the radiative effect of clouds over a large region (e.g., L'Ecuyer et al., 2019). Therefore, our CRE results appear predominantly negative. Different methods and results reflect varying perspectives in consideration.

13. L358 – fig S5 i and j are BOA DCS not ATM DCC.

    Response: It is corrected to "Fig. S5m" in the revised manuscript.

See the line: 410.

14. Fig6 – aerosol quantiles. I suggest you refer to 30th and 70th percentiles, opposed to the numbers you put into a code function. How do the actual values of these percentiles relate to the low and high aerosol environments discussed by Fan (are you actually spanning the range of aerosol levels they did?)

    Response: Thank you very much. We have revised the captions of Fig. 6 and Fig. 7 according to the reviewer's suggestions.

    The response to the question is as follows: Our study uses a different method for classifying clean and polluted environments compared to Fan et al., (2009). We match the aerosol mass concentrations provided by MERRA-2 with the observed DCS samples, and then classify the data into relatively clean and relatively polluted conditions based on the 30th and 70th percentiles. In contrast, Fan's research classifies clean and polluted conditions in numerical experiments using cloud droplet number concentrations (or cloud condensation nucleus concentration) ranging from 110 to 1100 cm^{-3}. Clean and polluted conditions are relative to different regions. For example, in the TP region, an aerosol mass concentration of 0.88 is considered relatively polluted, whereas this value would still be considered clean in the TO region.

15. L590 – do you mean previous studies over the TP? If so, be specific. Plenty of studies over other areas have studied the anvil CRE.

    Response: Yes, we mean that a lot of studies of deep convective clouds over the TP focus on deep convective cores. "previous studies on the CRE of deep convective clouds over the TP" is added in the revised manuscript.

    See the line: 618.

Technical comments

16. L14 – "notable" to "notably"

    Response: It is corrected in the revised manuscript.

    See the line: 14.

17. abstract – generally the abstract will need a grammar check by the journal. Issues are very minor.

Response: We sincerely appreciate the reviewer's valuable comment. Upon careful consideration, we have thoroughly reviewed the grammar of the abstract and addressed the issues accordingly.

18. L26 – "convections" is not grammatically correct. Change to something like "convective storms"?

Response: It is corrected in the revised manuscript.

See the line: 26-27.

19. L369 – Can you spell out in your methods the equations for each of the atmos/BOA/TOA CREs. You're saying "difference between all-sky and clear-sky" but it's ambiguous which way you have done the subtraction. Please spell it out so it is clear. I assume clear minus all-sky, but the phrasing suggests the opposite to me.

Response: For atmos/BOA/TOA CREs, the CREs are defined as:

$$\mathrm{CRE}_{net} = \left(F_{net}^{down} - F_{net}^{up}\right)^{all-sky} - \left(F_{net}^{down} - F_{net}^{up}\right)^{clear}, \tag{1}$$

where F_{net}^{down} and F_{net}^{up} are the downward and upward net fluxes, respectively.

$$F_{net} = F_{SW} + F_{LW}, \tag{2}$$

where F_{SW} and F_{LW} are the shortwave and longwave fluxes.

At the TOA,

$$\left(F_{net}^{down}\right)^{all-sky} = \left(F_{net}^{down}\right)^{clear}, \tag{3}$$

Thus, Eq. (1) changes to:

$$\mathrm{CRE}_{net} = \left(F_{net}^{up}\right)^{clear} - \left(F_{net}^{up}\right)^{all-sky}, \tag{4}$$

At the BOA and ATM, the CRE is calculated with Eq. (1).

The TOA CRE and BOA CRE are directly provided by 2B-FLXHR-lidar, and the calculation of ATM CRE is TOA CRE minus BOA CRE (Lv et al., 2015).

Cloud radiative heating rate (CRH) is equivalent to the ATM CRE per unit mass. Similarly, CRH is defined as the differently between the all-sky radiative heating rates and the clear-sky radiative heating rates (Haynes et al., 2013):

$$\mathrm{CRH} = \text{heating rate}^{all-sky} - \text{heating rate}^{clear}, \tag{5}$$

This Eq. (5) is added in the revised manuscript.

See the line: 421-422.

20. L648 – "investigatingthe" needs a space

    Response: It is corrected in the revised manuscript.

    See the line: 684.

Reference

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