

Review of *Sensitivity of aerosol and cloud properties to coupling strength of marine boundary layer clouds over the northwest Atlantic* by Zeider et al. (2024)

Suggestion: Major Revision

Zeider et al. use aircraft measurements from NASA's ACTIVATE field campaign between 2020 and 2022 to investigate aerosol and cloud properties in the marine boundary layer (MBL) for four different coupling regimes. The authors analyze what they refer to as strongly, weakly and two sets of moderately coupled MBLs, instead of only differentiating coupled and decoupled MBLs. These coupling regimes are based on potential temperature and total water mixing ratio differences between cloud base and minimum flight altitude. The authors present findings that are novel for the investigated region and their results highlight the importance of investigating the degree of coupling.

Overall, the paper is well-written, easy to follow, concise and has important findings regarding coupling in the marine boundary layer. Thus, I believe that the manuscript merits publication. Nevertheless, there are some comments mostly regarding the robustness of the results that should be addressed by the authors before publication.

Major comments

- 1. Impact of measurement uncertainty on results:** Figure S1 shows that many data points are very close to the boundary of their regime. Given measurement uncertainties it is possible that some data points belong to a different regime. I suggest that the authors test the sensitivity of their results by varying the $\Delta\theta_l$ and Δq_t values (leading to different regimes for some data) based on measurement uncertainty. Other approaches to account for the measurement uncertainty would be possible as well. I want to add that I found Figure S1 more informative than Figure 4. If the authors want to retain Figure 4, I suggest using white color for pixels with no data points.
- 2. Missing significance testing in section 3.2:** In section 3.3 the authors test the statistical significance of their results, however, this is missing in section 3.2. For some parameters, standard deviations are relatively large and in general the sample sizes of the non-strongly coupled regimes is quite small. I suggest conducting statistical significance tests for the values in Table 4. At least for the comparisons done in the text some level of statistical significance should be mentioned. Instead of displaying the data in Table 4, the authors could also consider presenting this data similar to the display shown in Figure 5.
- 3. Moderate coupling cases with very large $\Delta\theta_l$:** Figure S1 and 4 show moderately coupled cases with $\Delta\theta_l$ of 5-10 K. For such a substantial difference of θ_l can one still

Speak of a (moderately) coupled boundary layer? I guess what I am wondering is whether the data from these (admittedly) few points is consistent with the rest of the data in this coupling regime?

Specific Comments

4. While most of the paper is well-written, I found the abstract a bit hard to read on the first read. There are very long and wordy sentences. It might make sense to revisit the abstract and edit it for easier readability.
5. $\Delta\theta_l$ is sometimes using different fonts. For example, when comparing the abstract to Table 1. Please use consistent font.
6. 79: Probably it is better to say 'lower troposphere' since usually not the whole troposphere is unstable in CAOs.
7. 86-89: Is there a reference for this?
8. 150-153: I'm not sure if I understand how the vertical profile data is actually retrieved since it is written somewhat confusingly. Please try to edit this sentence. Maybe the authors can include these points in Figure 1 as well.
9. 223-224: Can you state any conclusions from this examination? Was any comparison done to test the robustness?
10. Figure 2: In (a) and (d) there is a substantial difference between the BCB height and the actual cloud base. For (a) it looks like that for a higher BCB leg the coupling classification might have been different. Did the authors check how frequent such cases are?
11. 291: I suggest adding the 37.5N line to Figure 3. Also add headings to the sub-figures to indicate which coupling regime they belong to.
12. Table 3 could be removed and the numbers could just be added to Figure 3 to save space.
13. 327-329: Do the authors have an idea of how representative the sampling is compared to climatology? A short sentence about this could be added.
14. 367: Consider using something different than 'difference calculations'. I was confused about this at first. Maybe 'lowest Δ values'.
15. 381-383 (and in other instances before): I suggest rounding these percentages to full numbers, i.e., 59.70% to 60%; the decimals do not provide much information in this case.
16. 429: The authors should add that this is based on just 4 samples.

Typographical

17. 198: Use Greek symbol for 'kappa'