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Comment on acp-2022-398

Anonymous Referee #1

Referee comment on "Turbulent structure of the Arctic boundary layer in early summer driven by stability, wind shear and cloud-top radiative cooling: ACLOUD airborne observations" by Dmitry G. Chechin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-398-RC1, 2022

This paper takes advantage of the ACLOUD airborne observations, and partly also from PASCAL on RV Polarstern, to explore different Arctic cloud turbulence dynamics. It is a very useful contributions and although it doesn't really reveal anything previously unknown, it is unique in the sense that it rests on actual in-situ profiling of turbulent properties whereas most previous studies has relies on either indirect evidence, based on the vertical structure of mean parameters, or on retrievals from remote sensing, both obvious limitations. The study uses both level flight legs in a traditional sense but also analysis of slant profiles which adds substantial value and I commend this choise. This is a welcome contribution and I have no doubt that it should eventually be published, however, it still needs some more work to become acceptable; hence I recommend that it is ***accepted after a major revision***.

Major concerns

When reading the main part of the paper, there is plenty of references to previous studies of Arctic stratocumulus, but the physics of Arctic liquid-bearing stratocumulus is no different from the physics of subtropical stratocumulus. The differences lies in the mixed-phase properties often occurring in Arctic clouds and in the surface charateristics, but much of the turbulence dynamics studied here is not sensitive to the presence of ice - neother in or below the clouds. Both cloud-top cooling and the associated buoyancy generation and cloud-forced mixing and the presence of cloud decoupling are features also present in subtropical stratocumulus. It would therefore be much more useful to see the contrasting of these results to the wealth of data that exists from stratocumulus outside of the Arctic, e.g. in the subtropics. How is cloud-top cooling and mixing different here from that in the subtropical “cloud cousins” and how are the decoupling different?

The analysis of the mean profiles (Figures 3, 7, 11 & 14) and for the turbulent properties

(Figures 5, 8, 12 & 15) are different and I wonder why? I would suggest that these plots are made exactly the same, with the same variables/properties in the same spots so they can be put side by side for comparison. I also wonder why the four different case studies are not chronological, starting with 2 June, then 5 June and finally 14 and 20 June? While the logical structure is based on dynamics characteristics, the text is very case oriented and I recommend that the two cases with multiple cloud layers are merged into one sub-section and analyzed together, following the logical narrative: cloud-driven, surface-driven and dual-layer cases. Or perhaps drop the second two-layer case all together if there's not enough data; having two separate cases is just a bit awkward.

The turbulence profiles are shown in scaled heights using the total boundary-layer top and indicating the cloud layer with gray shading. However, while incorporating the slant profiles covering substantial geographical distance means that there is no single cloud geometry and I suspect that the increased spread in some turbulence statistics around the cloud base is in fact due to the cloud bases – or rather the base of the cloud-mixed layer - for the different individual profiles is different. I would therefore suggest a two-layer scaling where the cloud layer is scaled separately from the sub-cloud layer.

The total manuscript is made rather long by the addition of two appendices and I wonder if these are really necessary. Obviously, one need to trust the observations before trusting the analysis but I wonder why this is accounted for here and not in a separate paper or report. This type of tests should be standard background information for the platforms and should not need to be repeated differently in different papers. Also, no amount of filtering or correlation while flying up and down in slant profiles trough the top of the cloud changes the fact that different parts of a profile is flown in dynamically completely different domains and analyzing turbulence incorporating both is questionable. The same is the case for the mixed-layer model; surely there are references to mixed layer models that could be used; moreover, the concept of a single mixed-layer is questionable when there may in fact be two different mix-layers on top of each other when the cloud-mixed layer is completely or partly decoupled from the surface-mixed layer.

Finally, I would wish for the paper to be more concisely written with a clearer narrative and there are also some language issues here an there. Hence, I have numerous minor comments; see below.

Minor comments

Page 5, lines 17-19: What about LWP from the remote sensing on Polar 5?

P6, l5: I think "accuracy" is the wrong word here; data from slant profiles is as accurate as any other data. The question instead is what they represent.

P7, l1-16: Are all the formulas really necessary?

P7, l4: Why is a (positive) scalar momentum flux used instead of the PBL-traditional along- and across-wind momentum flux?

P7, l21: Either it is partly transparent or it isn't. If it "appears to be", then it is; the eye's doesn't lie. And I don't understand what "almost transparent" means.

P7, l23: How can it be "rather solid" if it at the same time "appears to be almost transparent"

P9, l10: The potential temperature doesn't have a defined freezing point. The real temperature has, but depending on pressure the potential temperature can very well be above 273.15, while the temperature is below and vice versa. For example, with a zero degree near-surface temperature, the near-surface potential temperature may well be above zero and will remain above zero even when real temperature drops below zero with height.

P9, l33: Why is net LW defined positive; this is different from most other studies where it is defined as incoming minus outgoing, becoming negative at the cloud top. Moreover, what is the meaning of "positive upward" and how is that different from "negative downward"?

P10, Figure 3 and elsewhere: very different cloud thickness even when the cloud top, and hence PBL depth, are much more similar. Think about other ways to do the scaling of the turbulence profiles.

P11, l1: At what level is the heat flux significant, or do you mean "substantial" here? I have found it useful to only use "significant" when actually discussing significance.

P11, l3: "cloud base" is better than "cloud bottom".

P11, l4: One flight leg agrees with the mean of the slant profiles while one is much larger; could be anything. And what is entrainment(?); just another word for turbulence!

P11, l8: The lant profile TKE is height constant, but not that from the flight legs.

P11, l13-14: Change order in the sentence "The difference … upper part". Start with what you see here and then how that is different from Lenschows old results. Then conclude on the difference…

P12, F5 and elsewhere: The outlined cloud is only valid for one location; consider a two-layer scaling or indicate how the normalized-base height varies for the different profiles.

P13, l6: How is a "negative downward transport" not an upward transport?

P15, l11: Awkward "However, the … 5 June"; consider revising text.

P16, l7-8: Awkward "s_w shows … 2 June"; consider revising the text, e.g. "The cloud layer values of "s_w shows …".

P16, l11: The wind speed increases with decreasing altitude in all the profiles but more in T5 & T6 than in the other profiles. However, the reduction in these profiles closer to the surface, probably giving rise to the mentioning of a low-level jet, is only due to the fact that these profiles reaches lower than the other. If you want this to be a "low-level jet" you'll have to be more distinct in what you mean and how you can be sure none of the other slant profiles does not also have one, albeit weaker.

P16, l29-34: Might as well drop this; adds nothing of value to this paper.

P17, F8: How do you explain that the crosswind variance is larger than the alongwind variance ner the surface, when the stratification is stable and the wind direction nearly constant with height?

P20-21, F11-12: In the first figure the potential temperature seems to be linearly increasing with height below the capping inversion, while in the second figure it is constant up to z/h ~0.6-0.7?

P22, l1: Is it more interesting to compare with 5 June than with 2 June? Both have a similar O(10 W/m^2) heat flux at the cloud top, driven by cloud-top cooling, absent in the two layer case.

P22, l3: “half as large” is better than “twice as small”

P22, l5-7: There are two things at play here the presence or absence of cloud-top cooling and of substantial surface friction (i.e. wind speed).

P25, l2-3: So how is the PBL depth defined; I seem to have missed that or it isn’t described. I would have though that z/h would be unity at the inversion base?

P25, l11-14: Drop this; this is not the place. If you here feel a need to convince the reader that your turbulence data is actually accurate you need to go back and consider the whole manuscript.

P27, l17-27: Too much text devoted to discussing and ascertaining the accuracy of the methods, especially the slant profiles. This has been tested and used before many times by other authors; nit needed any more!

P29, l3: So is it low here? Turbulence in the cloud is driven by cloud-top cooling; is that lower because the cloud is over sea ice rather than open water?

P33, l19-29: Not quite sure what is going on here. Regardless if you fly in a vertical gradient or not, if you keep the same altitude you stay at the same temperature and therefore need no correction. Or you fly up and down, and then you’re not at the same place in the vertical anymore. Or the layer thickness varies along the flight. So explain this better.