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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The open ocean sensible heat flux and its significance for Arctic boundary layer mixing during early fall" *by* Manisha Ganeshan and Dong L. Wu

Anonymous Referee #2

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This paper explores important relationships between surface sensible heat flux, cloud properties, boundary layer structure, the large scale environment and the surface condition. I commend the authors for attempting to synthesize the unique perspectives coming from the deployment of the R/V Mirai during the fall season. I think that the analysis completed is interesting and sheds some light into how the surface and atmosphere interact. I do have some reservations and questions about the manuscript, and therefore would like to see some additional work completed before this paper is accepted for publication.

Specific Comments: Page 3, line 19: What is the impact of the ship on these measurements?

Section 3.2: I question how well the parcel-based method works when applied in conditions where there may be stratification. Given that the Arctic Ocean surface is open during the analysis times, these are likely times when this technique is generally acceptable. However, I would think that this would not be appropriate in cases where there is a surface inversion, for example, or in instances where clouds have worked to develop temperature structures and are not connected to the surface condition (as is pointed out to occur on occasion in the Arctic).

Page 4, line 30: I struggle with the "boundary layer" terminology as applied. If the layer or cloud associated with it is decoupled, is it still a boundary layer? It seems that the layer may better be referred to as a "decoupled, cloud-driven mixed layer" or similar. I do understand that as defined, the "BL height" may still be located at the cloud height, but perhaps that is also justification for revisiting that definition.

Page 4, line 32: Again, are decoupled clouds really boundary layer clouds? Why would clouds at 1 km be any different than clouds at 3 km if both are decoupled?

Page 5, line 2: In my experience, the near-surface humidity can often be 90% or more. Has an evaluation been completed of the impact of this definition on true cloud statistics? Doesn't the MIRAI also feature surface-based remote sensing? The frequent occurrence of BL cloud thickness ratios of 95% or greater in figure 5 is somewhat concerning. Perhaps it would be appropriate to evaluate the sensitivity of these metrics to the RH threshold chosen (for example, how does fig. 5 change if you choose 97% RH as the threshold?).

Page 5, line 13: I think that I understand this to mean that there were colder SATs observed in the recent years, is that correct? Otherwise can you explain how the variability of the SAT would result in increased surface sensible heat flux? Also, it might be informative to show the components that go into calculating deltaT. For example, how do the SSTs compare between years?

Page 7, line 7-8: Interestingly, this is backwards from what I usually think about Arctic

clouds (thick = frontal, thinner = stratocumulus, thinnest = decoupled stratus). I think it is important to remind the reader that this is cloud thickness within the boundary layer, and not total cloud thickness.

Page 7, line 13: This "(r)" should be positioned after "correlation coefficient", not after "BL height".

Page 7, line 17-18: Yet as a whole, this regime does have deeper boundary layers than the two regimes with smaller deltaT.

Page 7, line 18-19: "indicating that stratocumulus clouds 20 likely form by saturating to the significantly colder air mass that is advected above the surface" âĂŤ I'm not sure I follow what this means exactly. Suggest rewording for clarity.

Page 7, line 25: More significant in what way?

Page 7, line 31: More significant in what way?

Page 8, line 17: Please redefine what "it" is in this sentence. I believe that you're referring to SSHF, but that should be explicitly stated in the text.

Page 9, line 3: Is there a reason for thinking that the Arctic will see higher wind speeds in a future climate (or a reference which makes a case for this)?

Section 5: I find this section to be less of a discussion, and more of a repetition of already stated findings.

Page 9, lines 24-25: I'm confused âĂŤ I thought that the higher wind speeds were shown to be a significant factor in the stratus regime, and not in the CAA/stratocumulus regime?

Page 10, lines 10-12: To what extent is this dependent upon the timing of the cruises? Does this number change under as the ocean advances towards refreezing in late October and early November, when air temperatures are colder? It might be nice to include information on the variability in observed SAT between the different years.

Page 11, lines 7-9: I realize that this is supposed to be summarizing the previous text, but this has been stated many times already throughout the manuscript. I would have liked to see some more concrete discussion which synthesizes these results with other studies (without repeating the results of the current study over and over again).

Section 6, bullet points: Again, I feel as though all of this has been stated many times already. I really don't see a need to repeat it a 3rd or 4th time.

Page 12, lines 12-13: What model physics need to be improved? The flux parameterizations? The cloud microphysics and radiation? Ocean dynamics and sea ice physics? More information on these questions would be more helpful than additional repetition of the results of the current study.

Figure 1: I'm not sure that it's necessary to show a map of the entire Arctic here. I think it would help to zoom in on the area of interest (say 60-90 N and 110W to 160 E).

Figure 7: This caption is somewhat confusing. If I understand correctly: - The left hand figure is for uplift regime, and for all wind speeds, and the relationship is derived using all cases except the one outlier. How is this determined to be an outlier? Why are the cases with high BL height and very little SSHF not also outliers? - The right hand figure is for stratus cases, and is divided into two subsets âĂŤ one for higher wind speeds (red) and one for lower (black). Why is there no relationship determined for the lower wind speeds? Please reword the caption for clarity.

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