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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

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Comment 1: Page 3, line 19: What is the impact of the ship on these measurements?

Response: Unlike the radiosonde data, the surface meteorological measurements were checked for quality control before and after each cruise, as explained in individual cruise reports that are cited in Page 2; Lines 5-8 of the revised manuscript. The surface air temperature (SAT), in particular, was measured at two different locations (starboard and port sides) of the compass deck, thus minimizing negative effects from shadows and other local variations. Most importantly, all surface meteorological observations were compared with multiple instruments for quality and accuracy. Thus, we are sure that they are free from the impact of the ship, making them reliable for use in

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our study.

Comment 2: 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).

Response: Our goal is to identify the sources of convective mixing for the well-mixed Arctic BL, which is found to occur more than 90% of the time in the cruise ship observations used in our study. As the ocean is typically ice-free during this period, the Reviewer is correct in estimating that the conditions of stratification are rare (less than 10% frequency). Therefore, the use of the parcel-based method is generally acceptable. We understand that the reviewer is concerned about cases where the mixed layer is formed due to cloud-generated turbulence, and is decoupled from the surface. In the following paragraph, we will explain how and why such cases are excluded from our analysis. We use a bottom-up parcel method to identify the boundary layer top, therefore we can safely assume that the well-mixed layers are, in fact, coupled to the surface. If the well-mixed layer does not include a cloud, only the surface fluxes are assumed to control the boundary layer development, as explained in Section 4.4 (Pg. 9, Line 25) of the revised manuscript. Whereas in cases where a cloud layer (e.g. stratus or stratocumulus) occurs within the boundary layer, the mixing can be caused due to both surface heat fluxes and cloud-generated turbulence. Even if the latter is dominant, the boundary layer will remain coupled to the surface. This is because the adiabatic temperature profile in the boundary layer, which is a pre-requisite for the parcel-based method, ensures free flow and exchange of heat and momentum with the surface. Thus, we are confident to use the bottom-up parcel-based method as it systematically excludes all decoupled cases while identifying the well-mixed boundary layer top.

Comment 3: 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.

Response: As explained in the previous response, we use a bottom-up parcel method and therefore only consider boundary layers that are coupled to the surface. The term “decoupled” was intended to convey the stronger influence of clouds on BL mixing compared to the surface. However, this term contradicts the very definition of a boundary layer which is assumed to always be coupled to the surface. Therefore, the sentence has been modified, and the use of the word “decoupled” is avoided here and elsewhere in the text. Thank you for bringing this misnomer to our attention.

Comment 4: 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?

Response: See response to Comments 2 and 3.

Comment 5: 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?).

Response: Yes, the R/V Mirai does feature Doppler Radar and Ceilometer observations, but we use radiosonde profiles only to estimate boundary layer and cloud properties. As clouds can have large variability even at the microscale, we use a single source (radiosonde) dataset to study its relationship with BL height, mainly to avoid errors/inconsistencies in spatiotemporal sampling due to the use of multiple data sources. A RH threshold of 90% is chosen in order to account for ice- or mixed phase clouds which often form under sub-saturated (RH < 100%) conditions in the cold Arctic region. The frequent occurrence of 95% BL cloud thickness in Fig. 5 (a) is not concerning as these cases are likely associated with the persistent stratus fog in the shallow Arctic BL (Nilsson and Bigg 1996). Nevertheless, we examined the sensitivity of the cloud characteristics in Fig. 5 against different RH thresholds (89,90,97%), and found that our results and analysis is robust. As expected, the BL cloud occurrence frequency decreases when the RH threshold is increased beyond 90%, and vice-versa. The three distinct BL cloud thickness peaks shown in Fig. 5 (a) continue to exist at various RH thresholds (80,90,97%). The major difference is in the skewness of the distribution. For RH threshold of 97% (80%), this leads to a leftward (rightward) shift in the median, resulting in more positive (negative) skewness as compared to Fig. 5 (a). For example, when 97% RH threshold is used, the occurrence frequency of the maximum BL cloud thickness drops from 35 to 12%, whereas that of the minimum BL cloud thickness increases from 12 to 35%. While this does not affect the overall statistics of the stratus cloud regime, it appears to negatively impact the identification of stratocumulus clouds which typically form under colder air temperature conditions. Some cases of cold air advection, with RH > 90% but less than 97%, are (perhaps wrongly) classified as "dry" boundary layers with zero cloud layer thickness. The occurrence frequency of stratocumulus clouds reduces from 44% to ∼30%, while that of "dry" BL conditions (uplift regime) increases from 15% to ∼35%, which does not agree well with past studies cited in Section 4.2.1. On the other hand, the use of 90% RH threshold yields a reasonable distribution of cloudy vs. "dry" or cloud-free BLs, that agrees with past studies (Barton et al. 2012). Due to a better alignment of the occurrence frequency of cloud regimes with previous literature, and to account for ice or mixed-phase clouds that form under cold, sub-saturated conditions, we deem the use of 90% RH as the appropriate cut-off threshold in our study.

Comment 6: 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?

Response: Yes, this statement has been clarified to state that more negative SATs lead to the broader SSHF distribution observed in recent years (Pg. 5, Line 11). We didn't include the figures showing the distribution of SSTs and SATs, as they are redundant, in our opinion. But these are shown here for your reference (Figs. 1 and 2).

Comment 7: 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.

Response: Thank you for pointing this out. We have made sure to remind the reader that this is the boundary layer cloud thickness here (Pg. 7, Lines 3-5), and elsewhere in the text.

Comment 8: Page 7, line 13: This "(r)" should be positioned after "correlation coefficient", not after "BL height".

Response: The correction has been made (Pg. 7, Line 9). Thank you for your attention to the details.

Comment 9: Page 7, line 17-18: Yet as a whole, this regime does have deeper boundary layers than the two regimes with smaller deltaT.

Response: Yes, we have noted this in the text of the revised manuscript (Pg. 7, Lines 13-14).

Comment 10: Page 7, line 18-19: "indicating that stratocumulus clouds likely form by saturating to the significantly colder air mass that is advected above the surface" aÌĆA ÌĘTËĞ I'm not sure I follow what this means exactly. Suggest rewording for clarity.

Response: This sentence has been modified to read more clearly as follows: “Figure 6 (a) shows that the temperature anomaly in this regime is maximized between 0.3 to 1.5 km altitudes, indicating that cold air advection occurs above the surface, where stratocumulus clouds likely form by the release of latent heat of vaporization”. This is reflected in Pg. 7, Lines 15-17 in the new manuscript. Moreover, we have added a new section (Section 4.4) in which we use the thermodynamic equation to better explain how stratocumulus clouds may form during CAA over the open Arctic Ocean.

Comment 11: Page 7, line 25: More significant in what way? Page 7, line 31: More significant in what way?

Response: For the first case, we have replaced the word significant with the word evident (Pg. 7, Line 21). Studies supporting this sentence (Barton et al. 2012; Taylor et al. 2015) are cited in Pg. 7, Lines 22-23. For the second case, we mean statistically significant at the 99% confidence level as indicated by Table 1. This has been explicitly mentioned in Pg. 7, Line 27 of the revised manuscript.

Comment 12: 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.

Response: “It” refers to the correlation coefficient (r) between the SSHF and the BL height. This has been explicitly stated in the revised manuscript (Pg. 8, Line 15).

Comment 13: 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)?

Response: Some studies have reported that the Arctic will experience more frequent cyclonic conditions and higher wind stress in the future (Hakkinen et al. 2008; Higgins and Cassano 2009; Smedsrud et al. 2011). These have been duly cited in the text (Pg. 8, Line 32).

Comment 14: Section 5: I find this section to be less of a discussion, and more of a repetition of already stated findings.

Response: Section 5 has been re-written as a discussion of our present findings in context with previous literature. Relevant citations such as Boisvert and Stroeve (2015), Boisvert et al. (2015), Brümmer (1999), Brümmer and Pohlmann (2000), Hartmann et al. (1999), Deser et al. (2010), Higgins and Cassano (2009), Jackson et al. (2010; 2012), Nilsson et al. (2001), are now included in the revised manuscript.

Comment 15: Page 9, lines 24-25: I'm confused aÌĆA ÌĘTËĞ 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?

Response: Actually, the correlation coefficient 'r' also improves with wind speeds in the CAA regime though it remains insignificant (Pg. 7, Lines 28-30). Nevertheless, we have removed this sentence as it was too speculative.

Comment 16: 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.

Response: We have carried out extensive analyses inspecting the spatial and temporal dependence of the SSHF-BL height relationship, and find that there is no sensitivity to the occurrence of negative SATs. Given that we have observations in October and September, we have looked at monthly differences and there is no evident seasonality. We are confident in our finding that the SSHF control of the BL height ($\sim$10%) is independent of the seasonal variations in ΔT. The interannual variability of observed SATs has been included here for your reference (Fig. 2).

Comment 17: 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).

Response: This entire section has been re-written (section 5 in the new manuscript), to include a more holistic discussion rather than repetition of our findings. New studies are cited (see response to Comment 14) to better synthesize our results with previous literature.

Comment 18: 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.

Response: We have modified the section title to "Summary" as opposed to "Conclusions", and the bullet points now include only the key take-away points from our study.

Comment 19: Page 12, lines 12-13: What model physics need to be improved? The flux parameteri- zations? 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.

Response: In the revised manuscript, we have included a more comprehensive discussion of the implications of our results for model physics (Pg. 10, Lines 29-32, Pg. 11, Lines 1-2, and Pg. 11, Lines 15-21). This is provided below for your reference.

"Clouds, in turn, can have significant radiative feedbacks to the darker, ice-free ocean surface. Some model simulations of an ice-free Arctic Ocean suggest that the surface heat fluxes will dominate polar amplification during fall and early winter (Deser et al. 2010; Tietsche et al. 2011; Higgins and Cassano 2009). Our results suggest that both surface fluxes and clouds are sensitive to non-local large-scale factors, which influence their relative roles in diabatic heating of the Arctic atmosphere. This interaction between dynamic and thermodynamic variables must be duly incorporated in climate models for accurate projections of polar amplification".

"Based on our results, it appears that conditions of uplift and high surface wind speeds may favour efficient heat dissipation by SSHF, whereas episodes of CAA may not.

Nilsson et al. (2001) similarly found that the late summer/early fall turbulent heat fluxes over the Atlantic sector of the open Arctic Ocean can be sensitive to cyclone activity and cloud regimes. These dynamical triggers should be duly considered in BL parameterization schemes and surface layer schemes of climate models while evaluating future scenarios and sea ice recovery mechanisms for the Arctic. The chances of possible irreversible and more permanent feedbacks of sea ice loss also need to be seriously evaluated in models".

Comment 20: 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).

Response: Figure 1 has been revised as per your recommendation. We have now zoomed in an area covering 120W to 150E for more clarity. Thank you for the suggestion. The new figure is included here for your reference (Fig. 3).

Comment 21: 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 âA ÌĘTËĞ 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.

Response: As explained in section 4.2 (Pg. 5, Lines 25-28), the ΔT has no influence on the correlation between SSHF and BL height (r), whereas surface wind speeds have a positive influence on 'r'. For the observations of uplift regime shown in Fig. 7 (a), the point identified as an "outlier" has the maximum ΔT (6.77°C), which does not improve the linear relationship between SSHF and BL height. Whereas the red markers in Fig. 7 (b) are observations with maximum wind speeds, which has a positive influence on the linear relationship. For clarity, the caption has been modified to read as follows:

“Figure 7: The scatter plot of SSHF and BL height during (a) the uplift regime, and (b) the stratus regime. The linear relationship between the SSHF and BL height derived using the least squares method of curve-fitting for (a) all cases (except one outlier indicated by black marker), and (b) cases with surface wind speeds exceeding 9.8 ms-1 (indicated by red markers). The correlation coefficient and the coefficient of multiple determination for each linear relationship is denoted by r and R2, respectively. Note that the outlier in the uplift regime is an observation point with maximum ΔT, which has no influence on r. Conversely, the red markers in the stratus regime are observations with maximum surface wind speeds, which have a positive influence on r. See section 4.2 for explanation.”

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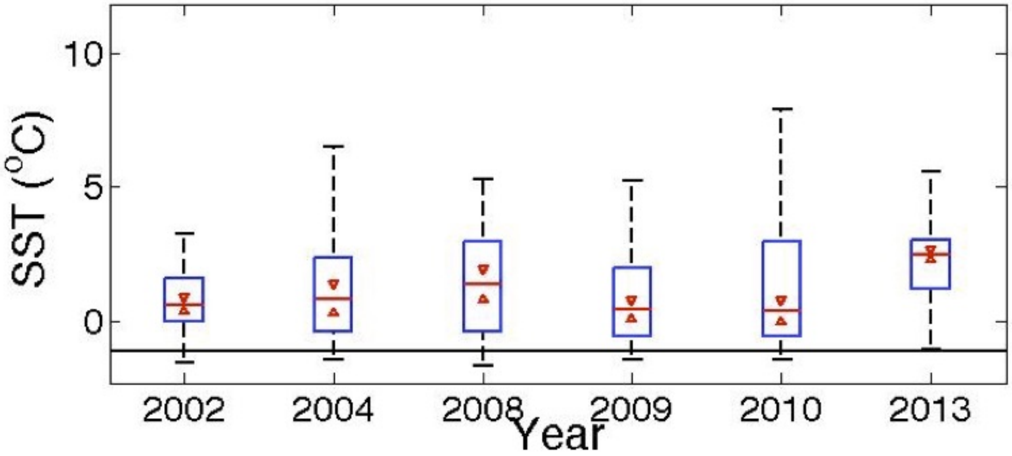

Fig. 1 The interannual variability in the distribution of the sea surface temperatures (SSTs). The solid horizontal black line represents the overall median value based on 6 years of ship data. The median for each year is represented by the horizontal red line within each boxplot, and the red notches represent the 95% confidence intervals around the same. Two medians are different at the 5% significance level if their intervals do not overlap.

Fig. 1.

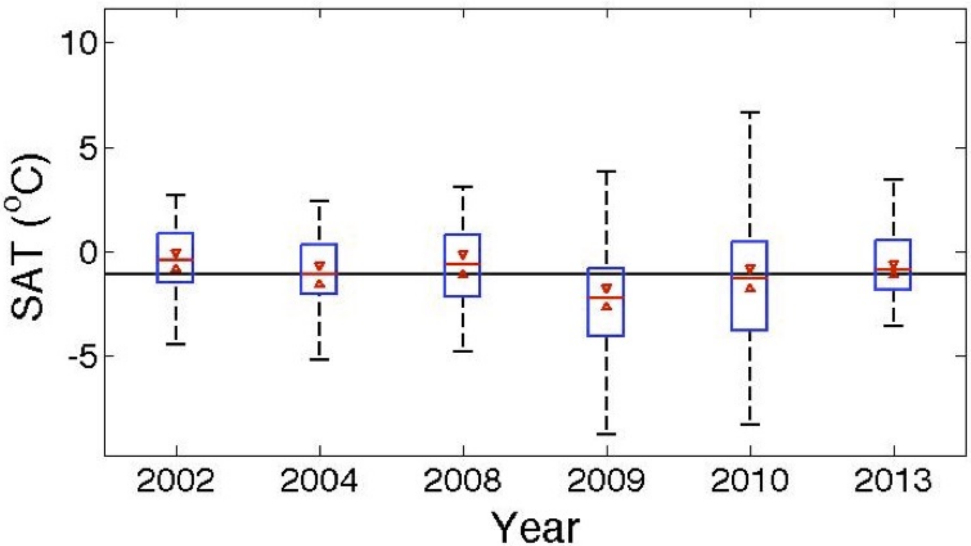

Fig. 2 The interannual variability in the distribution of the surface air temperatures (SATs). The solid horizontal black line represents the overall median value based on 6 years of ship data. The median for each year is represented by the horizontal red line within each boxplot, and the red notches represent the 95% confidence intervals around the same. Two medians are different at the 5% significance level if their intervals do not overlap.

Fig. 2.

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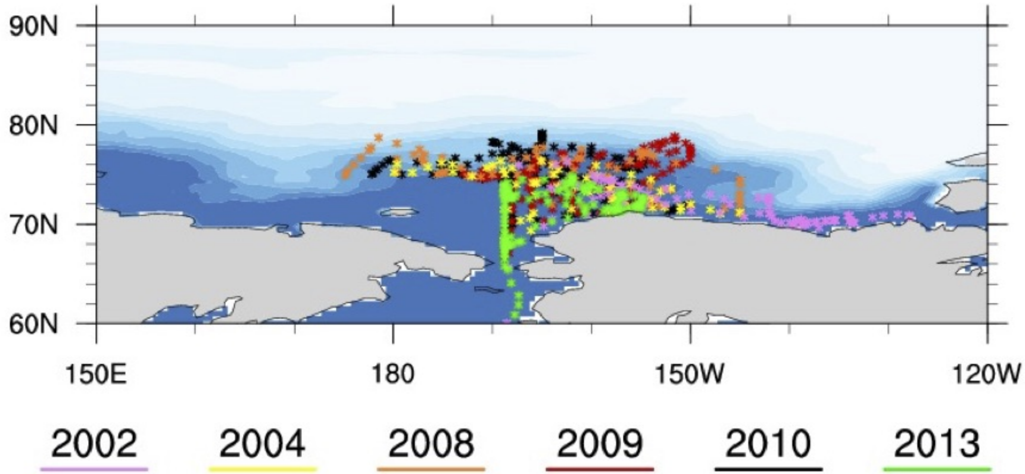

Fig. 3 Ship tracks during multi-year cruises of the R/V Mirai indicated by colored asterisk symbols. The average ice fraction (shaded white) at the time of cruise during 2008-2010 is also shown based on National Centers for Environmental Prediction/ National Center for Atmospheric Research (NCEP/NCAR) reanalyses project (Kalnay et al. 1996).

Fig. 3.