Reply to review by Igor Polyakov

"*Mixing, heat fluxes and heat content evolution of the Arctic Ocean mixed layer*"

A. Sirevaag, S. de la Rosa, I. Fer, M. Nicolaus, M. Tjernström, and M.G. McPhee.

We thank the reviewer for the positive feedback on our manuscript and for the constructive comments and suggestions which have improved the manuscript. In the following, we have replied to each comment and provided extended explanations and/or changes that will be made in a revised manuscript. Reviewer's comments are given below with our response in italic.

General:

I found this study very interesting, well written and organized. New measurements of heat fluxes in the upper mixed layer, through ice and in the upper atmosphere are of high value and demand.

That is why I think that the manuscript should be published. I have several rather minor comments and suggestions, which are summarized below. Addressing these issues will not require tremendous efforts but would help the authors build a nice story. From the comments, my identity (Igor Polyakov) may be quite obvious.

Comments:

1. The abstract is well written.

2. The last paragraph of the Introduction stands out. May require better transition.
The last paragraph is removed and instead a statement regarding the importance of the sea ice is added earlier in the introduction (after line 61)

3. Line 139 states 2m whereas Figure 3b states 3.8m.
Instruments were aligned 2 m below the 1.8 m thick ice, hence at 3.8 m depth and both 3.8 m and 2 m where used in the manuscript. We have revised this and use 3.8 m depth throughout the manuscript for consistency.

4. Line 146. Please remove space between 2 and %.
Corrected in revised manuscript.

5. Line 161, please remove «)».
Corrected in the revised manuscript.

6. Line 195. Please define the value of υ.
The value used for viscosity is a function of temperature, however the mean value used (1.9 x 10^{-6} m^2 s^{-1}) is added in the revised manuscript.

7. Line 232. I am not a native English speaker, but «aligned manually towards» does not sound as the best choice of words here.
Sentence is rephrased to "...was oriented with sensors directed towards the mean current..."

8. Line 237. Are there any estimates of sensitivity of the authors' results to selected width of the window (15' window is mentioned).
Choosing the "realisation interval" for covariance estimate is a compromise between choosing long enough intervals to capture the energy containing eddies and short enough to ensure that the temporal variability of the mean properties (temperature and salinity) and

current is small or negligible. Through sensitivity analyses for an extensive numbers of experiments, it is found (McPhee, 2008) that using 15 minutes interval length and further averaging into at least 1 hour averages, is reasonable for stable flux intervals. For e.g. the heat flux estimate presented in this manuscript, changing the realisation interval to 10 min (20 min) will reduce (increase) the turbulent heat flux with around 6 - 7%. Instead of adding more of the sensitivity analyses regarding the choice of realisation interval, which will probably not be of interest for readers in general, we have added a reference to the monograph by Miles McPhee (McPhee, 2008) where a thorough discussion is made.

9. Lines 310-311. The authors state «The temperature minimum» and put both T and S in the brackets. Rewording may help.
Since this is referred to as the remaining core of Atlantic Water, also the salinity value is given since this might be of general interest. The sentence is rewritten in the revised manuscript for better clarity.

10. Lines 365-366. Please define the depth range for pycnocline. «over the 8m thick layer centered at D» is difficult to read.
In the original manuscript we have often used the term pycnocline when we actually refer to the base of the mixed layer. Since the base of the mixed layer is defined as a specific depth, while the pycnocline covers a depth range, we have replaced pycnocline in the revised manuscript with base of the mixed layer or mixed layer depth for consistency and to avoid confusion. We have also added the range of D_{ml} *in the line referred to above and rewritten for better clarity.*

11. Lines 383-385. This statement is true in the absence of advection which, according to the authors, play some role in their analyses.
Although the first paragraph of section 4.1 was intended to be a very general introduction, we have specified that this is valid for the case of horizontal homogeneity in the revised version.

12. Lines 394-395. In Fig. 10, please show depth ranges of both mixed layer and upper cold halocline.
The cold halocline layer extends down to where the temperature increase limits the increase in density. For the average profile in Fig 4, this was around 100 m. In the manuscript, we have used the term upper cold halocline layer for the part of the cold halocline that resides above the depth of the winter mixed layer. In the revised manuscript, a better description of the extent of the "total" cold halocline layer is given and the practical use of the upper cold halocline term, is given section 3.1

13. Line 400. Please define the relation between FWC and latent heat content.
The definition of latent heat content, calculated from FWC is added to line 400 in the revised manuscript.

14. Line 408. Related to Q13 question: please define how it was defined that 1139 MJ/m2 is due to both sensible and latent heat fluxes.
The total heat content was the sum of sensible heat present and the latent heat represented by the FWC at the end of melting season. This is specified in the revised manuscript for better clarity.

15. Lines 410-413. I would argue that this statement may not reflect the whole story. According to our study, ice melting/production and intensity of draining of freshwater from

the Arctic Ocean in response to winds are the key contributors to the freshening/salinification of the upper Arctic Ocean. (Polyakov et al. 2008, JCli). River runoff and E-P are too small to trigger changes of FWC in the central basin. These findings may be corroborated by Figure 1, which shows FWC anomalies based on 2007 observations. Red color indicates anomalies corresponding to salinification. I would argue that a plausible explanation for freshening found by the authors in the Amundsen Basin would be advection of fresh water from the Canadian Basin, not from the northern Laptev Sea (where 2007 estimates of FWC showed salinification).
We thank the reviewer for pointing out the importance of local ice production/freezing and freshwater drainage relative to the net E-P and river discharge and we have updated the paragraph to include this. The Fig 1 supplied by the reviewer shows a salinification (decrease in winter FWC) for the western Amundsen Basin, where we find a significant increase in FWC. Data sources are not given in detail in Polyakov et al (2010), but it seems that most data from this area seem to be from the drift of ITP 7 which drifted through the area in late April/May, hence it shows the same salinification for the winter mixed layer as our observations from 2008. The main point in our discussion was that the area displays both an increase in summer FWC and a signature of a saltier winter mixed layer, which indicate a stronger seasonality in mixed layer salinity

16. Lines 413-414. According to analyses by Kwok, there was no significant trend of ice export through Fram Strait. See green line in Figure 2 as an example.
We are aware of the fact that Kwok (2009) did not find a trend in ice export through the Fram Strait based on passive microwave satellite data. However, using SAR images across the Fram Strait along 79N, which have good year round coverage and improved spatial resolution compared to passive microwave data, Smedsrud et al (2008) found an increase in ice export from 2004 and onwards (so an increase prior to our 2008 measurement, but no long term trend). Also ongoing work (Smedsrud et al., submitted to The Cryosphere), shows the increase which has a preliminary maximum in winter of 2008 and shows that the increase in annual ice area export is mostly related to increase in ice export in the winter months (DJF).

17. Eq. 7. H used for heat content is somewhat confusing since very often H is used for depth. I suggest Q instead.
Changed as suggested in the revised manuscript.

18. Line 548. Why the authors chose 20m?
Fig 13 shows that there is a shift in variability before and after the snow fall/freezing, where there was a larger variability for the upper part before the freezing and lower part after freezing. The 20 m limit was chosen somewhat arbitrarily, however 20 m is the depth where the variability was roughly equal before and after the snowfall. We see that this does not follow intuitively from Fig 13 and in the revised manuscript, we have changed this limit to 25 m, since this is the first depth where the variability after the snowfall was larger. This makes it easier to follow the discussion from Fig 13 to 14 and choosing 25 m instead of 20 does not change the conclusions from Fig 14, only some of the absolute values on the bars.

19. Lines 550-551: this sentence is difficult to read (probably because of too many «from»).
The sentence has been rewritten and simplified in the revised manuscript and will hopefully read better.

20. Figure 3. «Heading» sounds somewhat confusing.
Replaced by "direction" in figure and figure caption in the revised manuscript.

21. Figure 4. These profiles look very smoothed, practically without any fine structure. Is it a result of averaging or the original profiles do not have fine structure?
Profiles in Fig 4 are averages of 7 profiles, hence they look somewhat smoothed. Although there are some fine structure in the average profile (see e.g. average profiles for upper 100m in Fig 5), this is not visible in Fig 4 since they are plotted for the full depth range (0 – 500 dbar). We have added information about the averaging in the figure caption in the revised manuscript.

22. Figure 6. Caption states that Figure 6b shows ε whereas color map is for $\log_{10}(\varepsilon)$. I would like to see more comprehensive comparison of MSS and TIC measurements (i.e. at least means, correlation and standard deviations).
We have corrected the figure caption to state $\log_{10}(\varepsilon)$ in the revised manuscript. We did include a comparison of dissipation rate estimated from both TIC and MSS in Fig 6 to provide some kind of basis for comparison of the two measurement techniques. Although we find this very interesting, we feel that a more comprehensive comparison of TIC and MSS is not within the scope of the present work. We also think that such a comparison is not crucial for the conclusions of this manuscript and would like to focus more on this in future work.

23. Figure 9. Please provide depth ranges for the mixed layer and pyclocline in the caption.
To avoid confusion, we have replaced "pycnocline" with "mixed layer depth" or "base of mixed layer", see also the reply to comment 10.

24. Figure 11. Why do the authors use normalized depth? For the bottom panel: do not you think that defining the change of heat content as the difference between two neighbouring profiles would work better?
The aim of Fig 11 was to show a time series of heat content evolution from the start of the experiment by subtracting the first profile from the following. This way one can see which part of the mixed layer gains and looses heat. Since the depth of the mixed layer was changing, we needed to convert to normalized depths to do this, hence this is used in a).
The lower panel is the integrated heat content for the heat content profiles in the upper panel (but integrated over the "real" extent of the mixed layer), hence this shows a time series of total heat content in the mixed layer from the start of the experiment. The change in heat content between neighbouring profiles is shown in Fig 12a.
In the revised manuscript, figure caption is updated to make the distinction clearer.

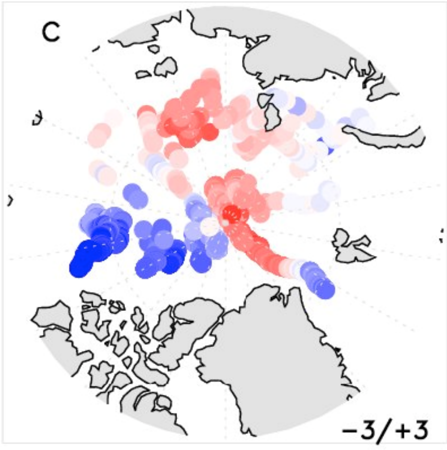

Figure 1. 2007 freshwater content (FWC, m) anomalies in the subsurface 25-75m layer of the Arctic Ocean. Colors indicate linear change of FWC anomalies between their maximum and minimum values, which are shown in the right bottom corner of the panel.

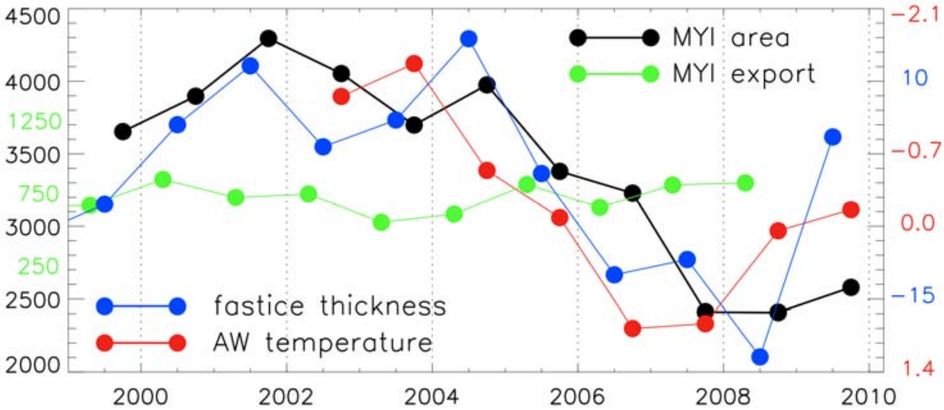

Figure 2. Arctic Ocean multiyear coverage and export (black and green, 103 km2) and atmospheric (blue) and oceanic (red) thermodynamic forcing. Atmospheric forcing is expressed as average fastice thickness anomalies (cm) from six arctic stations flanking the Laptev Sea. The oceanic thermodynamic forcing is expressed as the composite time series of normalized intermediate Atlantic Water (AW) temperature anomalies (oC, reverse vertical axis is used) obtained by averaging time series derived from four continental slope observational sites located at ~30oE, 105oE, 125oE, and 142oE.