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Ocean Science Discussions

Interactive comment on "Structure and drivers of ocean mixing north of Svalbard in summer and fall 2018" *by* Zoe Koenig et al.

Anonymous Referee #1

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This manuscript presents a particularly interesting set of turbulence observations from north of Svalbard in the Arctic that cover the 2018 summer and autumn period. The authors investigate the vertical structure of mixing, heat fluxes and seasonal changes, and identify the processes driving the variability in the turbulence field. Both the wind and tidal supplies of energy are estimated with parameterizations derived and discussed. An attempt to extrapolate to the whole Eurasian Basin is made and interesting areas are identified that could be investigated in further work. The current lack of turbulence measurements in the Arctic is highlighted as the main limitation to pan-Arctic parameterization as well as the difficulty in accounting for lateral processes and fluxes, and for extreme events such as storms.

The quality of the English in the text is excellent. The Abstract and Introduction are

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Discussion paper

good, the Data & Methods and Observations sections are excellent. The Upper layer Dynamics section is fine. The section on Mixing in the AW layer is very interesting. The Tidal Mixing section presents a very nice analysis and tools. The Discussion is hard to link to this particular study's findings. The Summary section is excellent. The figures are excellent and have great detail. It was a pleasure to read through this work.

Major comments:

-Introduction: Sort out the introduction part on the various sources and intensity of turbulence in the Arctic (see individual comments further down).

-Discussion: Currently the discussion section reads in parts (see individual comments) more like a literature review than a discussion around how your findings fit in current research and their wider impact and implications. You have excellent results and just need to rewrite this section a little.

In its current form, the manuscript is already very good and presents a trove of findings for this region on the topic of turbulence. However, the manuscript would benefit from some sorting in parts, better highlight of key findings throughout (done well in the Summary), and better framing of this study's results in the discussion. I recommend that the manuscript is accepted subject to minor revision and look forward to seeing a revised version.

——————————————————————————————— Individual comments

Abstract:

- Well written overall. The first sentence could do with rewriting to better reflect the beginning of your introduction. Right now you fit too much in that sentence and loose some of the meaning.

1.Introduction:

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- L23: You state ‘In the near future we may enter a new regime, in which the interior Arctic Ocean is entirely ice free in summer and sea ice is thinner and more mobile in winter’. I would argue that ‘may’ here is inappropriate and ‘will’ is more suitable. ‘May’ creates doubts around the likelihood of this happening. Please rephrase to better reflect current research findings such as the latest estimate from Guarino et al. (Guarino, M., Sime, L.C., Schröeder, D. et al. Sea-ice-free Arctic during the Last Interglacial supports fast future loss. Nat. Clim. Chang. (2020). https://doi.org/10.1038/s41558-020-0865-2) of 2035 for first ice free summer, or average from CMIP6 models of 2046 with a range of roughly 2030-2065.

- L31-37 and L38-46: In both these paragraphs, you describe the various sources and intensity of mixing in the Arctic. These two sections could do with merging and a better ordering of the different sources and intensity discussed.

- L65: Consider adding the following reference somewhere here: ‘The lack of sea ice is mainly due to heat from the Atlantic layer reaching the surface’. Duarte, P., Sundfjord, A., Meyer, A., Hudson, S. R., Spreen, G., & Smedsrud, L. H. (2020). Warm Atlantic water explains observed sea ice melt rates north of Svalbard. Journal of Geophysical Research: Oceans, 125, e2019JC015662. https://doi.org/10.1029/2019JC015662

2. Data and Methods:

- L105: Unclear what ‘In total, we collected 31 profiles.’ Do you mean ship CTD profiles? Or VMP profiles or ? This doesn’t match other number of VMP profiles stated earlier in the manuscript.

- L126: Pls define ‘g’ in equation (4) if not previously defined.L129-130: You state here that ‘We used the profiles collected from the ship’s CTD system (Sea-Bird Scientific, SBE 911plus on both cruises) to check and correct the temperature and salinity from the VMP’. But earlier on L107 you state ‘A good agreement was observed and no correction was made.’. Please rewrite to make both statement consistent.

3.Overview of observations:

- L172-173: Unsure you need this statement here considering you have explained it clearly it in the figure caption. - Figure 3: Add what the red line is MLD in the caption.

4. Upper layer dynamics:

- L252: Add the definition of Dml in the text. Currently it only appears in Fig.6 caption. Can you make it clearer in the text how you obtained your estimate of the relationship between Dml and E10: it's a linear fit of Dml from the VMP data and E10 from the ship wind speed measurements.

5. Mixing in the Atlantic Water layer:

- L264: Should 'in present conditions of a warming Arctic' not be 'in the new conditions of a warming Arctic'?

- Fig.7 is great

- L274-275: This statement is confusing 'vertical turbulent heat fluxes are negative (less than 5Wm-2)' You might want to rephrase to 'vertical turbulent heat fluxes are negative (0 to -5Wm-2)'

- L282: Which section are you speaking about when you say '. . . the heat loss due to vertical turbulent heat fluxes is about . . . across the section'?

- L282-285: Why is your estimate of heat loss due to vertical turbulent heat fluxes (1.2x10ˆ5 W/m) so much lower than Kolas estimates from the same cruise (9.1x10ˆ7 W/m and 9.6x10ˆ6 W/m)?

6. Tidal mixing:

- Fig. 8 caption: 'Average profiles of a) dissipation rate, b) turbulent heat flux and c) diapycnal diffusivity k for small' Also add Espi and F_H after the variable's names.

- L326-366: Nice analysis of the vertically integrated dissipation rate in bottom 250m.

7. Discussion:

- Fig.10 caption: I suggest removing the first word ‘Typical’. Also, what is the background shading on the small map, topography? This map is useful and should be listed in the caption.

- L358: Subsection title ‘Pan-Arctic estimates of tidally-driven dissipation rates’ is not representative of results presented which are ‘instead of presenting Arctic-wide maps we concentrate on the Eurasian Basin from north of Svalbard into the East Siberian Sea’. Please change section title to represent better the content. Also edit L355 in the previous section announcing the ‘pan-Arctic estimate’.

- L398-405: Great findings.

- L410: Rephrase sentence ‘In the future, sea ice meltwater is expected to increase and turbulent mixing near the surface to decrease’ to better justify/explain the expected decrease in mixing (due to increase stratification).

- L 423: ‘and an earlier onset of stratification which might be indirectly linked to bloom development’ . . . due to . . . . Please add details.

- Section 7.2: I m unsure about the contribution your results make in this theme of ‘impact of meltwater on the near surface mixing’. Consider better linking to your observations or moving this section as context in your introduction in a condensed form.

- L433: I m unsure about how this statement ‘Vertical turbulent heat fluxes are not the main source of cooling of the Atlantic Water layer in the Arctic. Ivanov and Timokhov (2019) reviewed that from the Yermak Plateau to the Lomonosov ridge, 41% of the Atlantic Water heat is lost to the atmosphere, 31% to the deep ocean and 20% is lost laterally.’ fits with the previous ‘heat loss due to turbulent vertical mixing represents less than 10% of the total heat loss of the Atlantic Water’ . Would the 10% not be part of the 31% deep ocean and 20% laterally? You seem to imply they are different when you state ‘Vertical turbulent heat fluxes are not the main source of cooling of the

Atlantic Water layer in the Arctic'. Please tidy up these two paragraphs so the reader can follow your thoughts. Again, further down you discuss eddies and their roles. But is the heat export from eddies not included in the 20% lost laterally from Ivanov and Timokhov (2019)?

- L444 and 445: The numbers you quote there (10ˆ-8 and 40W/mˆ2), are they from Kolas and Fer or from this study? Again, how does this section of the discussion (7.3 AW heat loss) exactly links with your findings. Currently this reads a lot like an (excellent) literature review, rather than you putting your new findings in context. . .

8. Summary:

- L459-460: Consider adding 'The vertical decay scale of the diffusivity is 22m *for those strong tidal currents*, compared to 18m for weaker tidal currents.'

- L470: Consider adding details 'More in situ observations from different sites *in the Eurasian Basin and elsewhere in the Arctic* are needed to confirm our results.'

- L475: Can you add 'of the *expected/estimated* total heat loss of the Atlantic Water layer'.

- L475-476: Can you explain better the relation between the first part of the sentence and the later part? I understand you mean to say that increased vertical mixing during storms might partially close the budget but don't make up the whole 'missing' heat loss which might be mostly lateral fluxes. So that both lateral fluxes and extreme conditions such as storms, frontal systems etc should be investigated. But this will not super clear in the current form of the sentence.

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