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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 #2

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General comments

The authors present a series of observations of turbulent dissipation from measurements taken during two separate research cruises in the region along the slope north of Svalbard. The study considers wind forcing and tides as drivers to mix heat that is concentrated in warm Atlantic-origin water that resides in the mid-depths of the water column. Vertical profiles of turbulent dissipation, diffusivity, and heat and buoyancy fluxes are presented and tied to seasonal changes and input work from both winds and tides. Near the end of the paper, the authors extrapolate their ideas across a broader region.

This manuscript makes an important addition to the body of literature on turbulence and mixing in a key Arctic region. While the results and analysis are interesting and

merit publication, the manuscript would be greatly improved by more cohesive linking of the different ideas. As presented, the study reads as a nice collection of related results, but parts of the discussion do more to highlight some of the background and motivation than to link to those results, and many of the results are considered independently despite parallels in the analysis. Consequently, the study lacks a coherent story. There is enough detail in the manuscript already that this should not require any further analysis, but the authors should consider some reorganization of the discussion section to tie together different aspects of the study.

One potential approach to this reorganization would be to rethink the presentation of sections 4-6. Currently, these sections are organized to step vertically down through the water column from the upper ocean (§4) to Atlantic Water (§5), to the bottom boundary layer (§6). However, the wind forcing and tides are presented as the main drivers of vertical mixing while, in some capacity, the Atlantic Water is what is being mixed. It may be better to move some of the ideas from section 5 to the discussion, and use it to unify and compare/contrast the different results from sections 4 and 6 (e.g., is the structure seen in figure 7 a consequence of the results in sections 4 or 6?). Then presenting wind forcing and tidal forcing back-to-back will better highlight the parallels between the analysis in each of those sections.

Specific comments

- L106-107: Here you state that VMP measurements of temperature and salinity agreed with ship CTD profiles, so no corrections were made. But in L129-130 you discuss using the ship CTD to correct the VMP temperature. Please ensure your statements are consistent.

- L116-122: There are a wide number of parameterizations and methods for determining diapycnal diffusivity. You should discuss the sensitivity of your results to the choice of the Bouffard and Boegman (2013) method compared (at least) to the more common Osborn (1980) method with Gamma = 0.2.

- L134: The measurement height for wind speed should be mentioned here, along with the correction to 10m, instead of L248-249.

- L135-137: Have tidal current measurements from the Arc5km2018 model been verified in this region?

- L143-146: These lines about the number of profiles could be moved into the methods section (2.1).

- L156-160: These lines seem out of place here.

- L178-180: Equation 5 doesn't represent the surface layer depth from Randelhoff et al. (2017). It represents the scaled vertical coordinate those authors use, and the surface layer depth corresponds to a specific value of r. This isn't clear in your text. - L178-183: There is a lot of detail here for a surface layer definition that you ultimately don't use. This could be simplified by trimming out a number of intervening sentences and leaving only the beginning and end: "We also estimate a surface layer depth following Randelhoff et al. (2017); however, the mixed layer depth and the surface layer depth are very similar (not shown), so in the rest of the study. . .".

- L186-187: This warm water is difficult to identify in the sections (especially panels a and c).

- L191-193: Are these averages of profiles from both July and September cruises? If not, which set are these? Please clarify in your text. Also, if surface stratification and buoyancy flux are significantly different in July and September (e.g., section 4.1), then I would expect the shallow parts of those profiles to be fairly distinct between seasons and not appropriate to average. Ensure that you comment on that in the text.

- Figure 3: The red line showing mixed-layer depth is very difficult to see. In the left panels, it blends into the temperature field and in the right panels it is obscured by other details. This is also partly due to how close to the surface the mixed layer is relative to the scale of the plot. In the left panels, the scale in the upper 100m differs from the

rest of the plot to better show upper ocean details, but they are still hard to see and the scale change could be further exaggerated. I did not immediately realize that the vertical scale change was not included in the right panels. I would also appreciate if the Atlantic water was somehow better identified or more visible in this figure – I don't clearly see it in all sections.

- Figure 4: Some of the subfigure tick-labels are overlapping and hard to read. The legend is small and difficult to read. Also, it may be helpful to replace legend labels with "inshore, shelf break, offshore" as are used in the text. It's difficult to see the details in the upper water column (below $\sim$100m); you may consider using a different vertical scale (as in figure 3), or providing insets that zoom in on the surface of each panel.

- L206-208: Here you say that the core of the Atlantic water current is between 400 m and 600 m, but in L186-187 you associate the Atlantic water with 500 m to 1100 m depths. Throughout the text you use the 800 m isobath as a reference for Altantic water, which is consistent with L186-187 but not with L206-208. Please clarify this and ensure consistency throughout.

- L208: The only mention of current measurements throughout the rest of the paper are the modeled tidal currents, but this sentence is about water column currents. Are these measured with a shipboard ADCP during the cruise? Or is this sentence a reference to known characteristics of the Atlantic water layer from other studies (e.g., the submitted work by Kolås et al., that you reference in L196)?

- L250-256: The non-linear dependence of mixed-layer dissipation on wind energy input is a really interesting result, but it would be valuable to explore this concept in more detail and relate it to prior studies (either here, or in some part of the discussion). In particular, there has been some theory that looks at this relation in the wave-boundary layer and may or may not support a linear relationship (see Craig & Banner, 1994, doi: 10.1175/1520-0485(1994)024<2546:MWETIT>2.0.CO;2 and Thomson, 2016, doi:

10.1175/JPO-D-15-0130.1). It's also been considered in a bulk sense in the mixed layer (i.e., as an efficiency; see Sutherland et al., 2013 doi:10.5194/os-9-597-2013 and references therein; though this is still in the wave-breaking framework). I think there might be some richness in the fact that this analysis suggests a non-linear relationship and worth speculating about why or what that might mean (perhaps stratification or mixed-layer depth play in in some way). Additionally, it may be worth mentioning the wave conditions during the sampling in section 3.1, even if only qualitatively.

- Section 4.2: In this section you take all of the data together, but in section 4.1 you contrast some of the details of the mixed-layer between the July and September cruises. Am I correct in interpreting from figure 6 and L252-253 that there's not enough data in September to be able to be able to make meaningful comparisons of Dml between seasons? If it's possible to contrast the effects of wind forcing between the two seasons at all, it would be very interesting.

- Figure 6: Are there any noticeable relationships if you colour the points by mixed-layer depth?

- L316: What are the confidence intervals on the decay scales? Are 18 m and 22 m statistically different from each other?

- L320: "We investigate the role of two distinct contributions from tidal currents". Contributions to what? This sentence isn't clear.

- L326: Why the choice of 250 m for integrating the dissipation? Is this choice informed in any way by the estimated decay scales from earlier? Are results sensitive to other choices?

- L330-331: Why exclude wave (tidal) frequency?

- L327-347: Since the tidal-work parameter in equation 7 doesn't provide a useful correlation, you could choose to simplify the text and figure 9 by removing some of the text in the section, and simply stating that you also tried comparing D250 with the rate

of work given by Nash et al., (2006) but found no significant correlation. Then you could remove equation 7 and some of the text surrounding it and remove panels a and b from figure 9. This is a personal choice, but it would better highlight your positive results.

- L335: It's worth highlighting somewhere that equation 8 is analogous to the equation for E10 (in section 4.2), and so the nonlinear relationship between D250 and Wbotdrag is something that can be related to the nonlinear relationship between Dml and E10.

- L350: Does it make sense to compare the bottom drag coefficient to one from the Bering Strait? I wouldn't assume that the bottom morphologies of the two areas would be similar. Can you instead refer to a range of "typical" bottom drag values?

- Figure 9: I don't quite understand why there is only one red dot in panels b and d but many in panels a and c? Is this due to how you perform the u_rms calculation?

- L367-368: If you are showing only a line along the Eurasian Basin, then "Pan-Arctic" in the section title is not appropriate. (Note, the authors have already expressed plans to rename this section).

- L399-400: Maybe highlight to what extent the dissipation in panel b of Figure 10 will account for the nonlinear waves (e.g., if it did account for it, I'd expect to see peaks in D250 in panel b the correspond to the peaks of inverse Fr in panel c). Make it explicit that these are areas that warrant specific further study.

- L387-405: While the discussion of these potential non-linear wave "hotspots" is very interesting, it feels somewhat disconnected from the rest of the study. Most of the times non-linear waves present in the results before this point are references to the high dissipation event at RS2 that was already documented by Fer et al. (2020b). This section would connect more if you make more explicit comparisons between the general results and the non-linear wave results (e.g., you have RS2 points in red in figure 9, which show the associated increase in D250, but those points are presented as more of a sidenote in the text L351-353 when there's potential to make more direct

comparisons).

- Section 7.1: Overall, this is a nice extension of the ideas in section 6.

- Section 7.2: As written, there is no clear link between the ideas in the section and the results you've presented. Do your results agree with or refute any of the studies you cite? Can they be compared at all? This section provides interesting background and motivation, but without linking it explicitly to your results it is not really a discussion section. (Note, the authors have already expressed plans to remove this section).

- Section 7.3: Similar to section 7.2, this provides good background but isn't otherwise well linked to the rest of the study.

- L459-460: The different values of kappa_bot may be a stronger result to highlight in the summary than the different decay scales (or maybe include both?)

Technical corrections

- L25-26: Awkward grammar/sentence structure in the sentence starting with "The heat reservoir. . .".

- L41-42: Do you have the correct reference for the sentence "Wind-driven momentum input. . ."? Is this meant to reference Rainville and Woodgate (2009) instead of Rainville and Windsor (2008)?

- L154: "encounter" should be "encountered"

- L317: In the sentence "We use kappa_bot. . .", should that instead be kappa_bg?

- L414: Awkward grammar/sentence structure in the sentence starting with "They found. . .".

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