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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Turbulence in the stratified boundary layer under ice: observations from Lake Baikal and a new similarity model" *by* Georgiy Kirillin et al.

Anonymous Referee #1

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1. Scientific Significance: Good Kirillin et al. 2019 presents an interesting approach to problems encountered in classic turbulence theory when TKE production is substantially affected by interfacial buoyancy fluxes. The authors' argue that the TKE balance, and associated fluxes, are inappropriately characterized by traditional bulk parameterizations for dynamic (sheared) and static (convective) instabilities. This is a challenging scientific problem in need of a solution and ranks among the largest challenges for numerical modelers. The authors make a compelling argument that the DO scaling approach (instead of the classic MO scaling) is practical for boundary layers generated by flows under freshwater lake ice.

2. Scientific Quality: Excellent Pg. 2 Line 8: Reference in manuscript about sea ice loss attributed "primarily" to basal ice melt (ocean-to-ice) as opposed to surface ice melt (air-to-ice) is still under debate. Ice mass balance (IMB) observations shows that the amount of surface (atmospheric) and basal (oceanic) melt varies with each year (some years the top melts about the same as the bottom). I recommend rewording this sentence to state that a significant component of sea ice volume loss occurs from the sea ice bottom... (or something like "due to ocean-to-ice heat fluxes").

Pg. 2 Lines 16-28: Great discussion in this paragraph about the uniqueness of under-ice temperature stratification under lake ice (different than my background in sea ice and salinity driven density gradients); however, I'm somewhat confused on the persistence of the interfacial layer (IL) during SML free convection. Results from Frey et al. (2011) and Light et al. (2008) show that significant solar radiation is deposited immediately beneath the sea ice; therefore, how is the IL maintained if the strongest heating (solar) is in the layer closest to the ice base. I assume this is due to either high sensible heat losses caused by the negative ice temperature gradients (thermal conductivity), or latent heat losses to the lake ice base (or combination of both in March); either way, the negative heat budget despite solar heating near the ice-water interface should briefly be addressed here. I also see that you discussed the near ice heat budget on pages 3 and 4; perhaps the best solution in the intro is to capture the dominating heat loss term during the period of your study (latent heat or sensible heat).

Pg. 2 Lines 21-23: In Arctic Ocean air-ice-water interactions, entrainment of subsurface heat (usually the near-surface temperature maximum (NSTM)) is hard to achieve with static instabilities (e.g. brine rejection), this is usually reserved for stronger dynamic (sheared) instabilities. If this statement (lake heat entrainment with static instabilities) has been demonstrated by previous work, please reference.

Pg. 3 Section 2: Regarding the geostrophic currents in Lake Baikal, request there be some background provided in this section as to the source of this current (pressure gradient force created by???? and spatial scale drives a low Rossby Number environment, etc.).

Pg. 8 Lines 17-18: Why were there more current meters deployed at S1 and not at S2?

Pg. 10 Figure 2: Perhaps I missed this in the results discussion, but why did the penetrated solar radiation drop off substantially after Feb. 22 when topside solar radiation increased (Figs 2c and 2d). Was there a snow event(s)? The only reference I can find to Fig. 2d is on page 13 and only accounts for the mean daily under-ice short-wave radiation (Io = 9.7 W mˆ-2) and a range of through-ice radiation (8-18%). These results do not match with the results in Figs. 2c and 2d where the transmissivity (solar(under-ice)/solar(top-ice)) between March 6th and March 16th appears to be well below 1% with under-ice radiation values of <2 W mˆ-2. The extremely low under-ice radiation values heavily skew the 9.7 W mˆ-2 average over the study period and likely affects the intensity of short-wave induced convective overturning in the SML. There appears to be two "modes" to this dataset: 1) light snow cover prior to 22 February with active SMLs and strong ILs; and 2) moderate-heavy snow cover after 22 Feb with inactive SMLs and weak ILs. Request clarification on how this transition in the steady state condition was handled and why it is appropriate to conduct DO scaling model validations across these varying conditions.

Pg. 17 Lines 2-4: Once again, it appears that the event (likely snow) heavily impacted these results during the 24 Feb – 07 Mar period. If the 22 Feb event is snow, I anticipate it would affect several areas of the heat budget and near interface buoyancy to include lowering ice-to-air sensible heat fluxes and destabilizing the IL (less downwelled solar radiation) allowing turbulent (shear) eddies access to the ice base. If this were indeed the case, it should probably be integrated into the discussion, if not, recommend addressing the cause of the significant change in heat balance conditions in Fig. 11 after 22 Feb (similar to the previous comment for page 10).

Pg. 19 Lines 14-16: Not entirely accurate, oceanic fluxes during the 2014 MIZ experiment in the Beaufort Sea were >100 W mˆ-2 with Autonomous Ocean Flux Buoys

(Gallaher et al., 2016) and nearly 200 W mˆ-2 in the Greenland Sea during the 1983/84 MIZ experiment (McPhee et al., 1987).

Pg. 21 Line 6: I did not see an isothermal/isopycnal (homogeneous) layer mixed layer in the data (Fig. 3); perhaps, near-homogeneous is more appropriate.

Pg. 22 Line 16: Interesting idea to scale this DO scaling approach to sea ice modeling; however, near-freezing freshwater and seawater ice-water boundary layers have notable differences. Things that come immediately to mind are: 1) the rotational Ekman layer plays an important role (which was not tested in your study) in the deeper dynamically developed ocean boundary layers (20-35 cm/sec free drift ice speeds); 2) temperature becomes the equivalent of a passive tracer (no buoyancy contribution) in seawater above $\sim$25 ppt; and, 3) bulk parameterizations using MO scaling have worked pretty well when validated against eddy correlation and thermal dissipation observations. I will admit, that during calm wind conditions in the presence of significant meltwater (melt pond drainage), this parameterization does not perform well and is similar to your study minus the temperature stratification from solar heating. For this paragraph, I would recommend rewriting to target the potential benefit of this approach during weak atmospheric forcing over sea ice during the melt season.

Pgs. 22-23 Section 6: Conclusion seems a little abbreviated, recommend recapping a few more of your findings.

3. Presentation Quality: Excellent Manuscript figures are good quality and well labeled. I only have a few recommendations regarding data visualization: Pg. 8 Figure 1: Recommend including the type of satellite imagery used (visible, LandSat, IR, SAR. . .)

Pg. 8 Figure 1: This is not strictly required, but I recommend including another figure (or figure inset) that shows the general geographic location of Lake Baikal. Because I'm not familiar with the Lake, I had to look online for some spatial context and whether it was a terrestrial (continental interior) or maritime (near coastal plain) lake.

4. Technical Comments: Pg 1 Line 10: Not sure "the latter" is required since only one topic is being referenced Pg 2 Lines 28-29: Recommend runoff or accelerated sea ice melt... Pg 3 Line 2: Recommend high water to ice heat fluxes in Lake Baikal... Pg. 3 Lines 3-5: I don't completely understand this sentence, recommend rewriting for clarity. Pg. 3 Line 15: analyze the effect of turbulent mixing... Pg. 3 Line 16: study is to establish the scaling... Pg. 3 Line 17: circulation with the seasonal ice cover dynamics and suitable parameterization for ice-water heat exchange... Pg. 3 Line 24: and markedly increase... Pg. 3 Lines 24-25: Similarly, the increase... flow can destroy the conduction layer... Pg. 3 Line 26: In the majority of freshwater lakes, the aforementioned... Pg. 3 Line 29: impurities than sea ice or river ice... Pg. 4 Line 18: does not typically... Pg. 4 Line 21: I don't understand how TKE can be supplied by the "decay of the convective motions." Shouldn't this be "by the displacement of the underlying water by density-driven static instabilities." Recommend a little further clarification in this sentence. Pg. 4 Line 31: roughness length (instead of parameter)? Pg. 5 Line 4: bulk transfer or drag coefficient... Pg. 5 Line 6: tends to be the local balance... Pg. 5 Lines 10-11: This sentence is hard to follow, perhaps something like the "the second factor influencing near boundary stratification is the destabilizing buoyancy flux due to... water column of thickness hs and is derived from..." If this isn't what you mean, recommend rewording for clarification. Pg. 6 Line 17: production of TKE is limited by two major loss processes... Pg. 8 Line 10: Three short-wave radiation sensors were deployed vertically to measure the decay of solar radiative fluxes across the air-ice-water system. Pg. 9 Line 26: while basal ice at Station S2 continued to grow at a slow rate of ∼∼0.3 cm dayˆ-1... Pg. 9 Line 26: I believe the Figure reference should be 2b not 2c. Pg. 15 Line 32: made by the single-point Pg. 17 Line 8: Long dashed line to start this line, I believe this should be deleted. Pg. 23 Line 3: currents may have a much stronger effect on lake ice melt than estimated by...

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-608/hess-2019-608-RC1-

supplement.pdf

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