

Interactive comment on “Turbulent mixing and heat fluxes under lake ice: the role of seiche oscillations” by Georgiy Kirillin et al.

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We thank the reviewer for positive evaluation of our study and for the valuable comments aimed at improvement of the paper readability. We agree with the bulk of the remarks and will incorporate the suggested amendments in the manuscript. The Reviewer's questions are answered below (with references to the original submission).

Q. P. 2, line 15: The following sentence has no verb: “For typical thickness of IL of ≤ 1 m, the conductive heat flux amounts at ≤ 1 W m⁻².” Please rewrite

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A. The verb was “amounts at”. Rewritten as “is $\leq 1 \text{ W m}^{-2}$ ” for clarity.

Q. Page 3 – Section 2.1. If available, it would be useful to add more information on under-ice water climatology

A. Climatological data on water temperatures are unfortunately not available for the lake.

Q. In page 4, I is defined as net PAR irradiance, but in page 6 I is defined as the kinematic flux of the short-wave solar radiation. Please be consistent. In addition explain what you mean by short-wave solar radiation and which the relation between these and the PAR radiation. Please indicate the spectral bands of the definitions.

A. The spectrum of solar short-wave (wavelengths range 200-2500 nm) radiation is strongly modified by lake waters: clear water (or ice) quickly absorbs the long-wave (infrared) part of the spectrum and yellow substance absorbs the short-wave (ultraviolet) part. As a result, at $< 1 \text{ m}$ depth, $> 95\%$ of the penetrated radiation falls within the PAR spectral range of 400-700 nm (see e.g. Jerlov, 1976). In humic brown-water lakes, like Kilpisjärvi, the equivalence between PAR and total solar radiation is even closer (e.g. Leppäranta et al., 2010). Therefore, both terms are used interchangeably in the analysis. We clarify in the revised paper the relationship between the PAR and SW radiation flux. We also introduce different symbols for dynamic and kinematic radiation fluxes for clarity.

Q. Page 4, line 27: “sampling rate 0.5 Hz”. This frequency is enough of to take in account all the relevant scales of the turbulence? Please comment.

A. The sampling rate is not critical for the velocity structure estimations of turbulence. The latter are calculated for every single “snapshot” along a corresponding acoustic beam and depend on spatial, not temporal resolution. The spatial resolution of 2 cm was apparently high enough to approach the mid- or short-wavenumber tail of the inertial interval (The Kolmogorov length scale is 0.3-1.0 cm for the dissipation rates of

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$10^{-10} - 10^{-8} \text{ W kg}^{-1}$). The high sampling rate was simply chosen to provide a higher statistical significance of results at later averaging.

Q. Page 5 line 10: It is not clear how Noise is computed?

A. Eq. (5) is a line $y = Ax + B$, where $Ax = r$, $y = C_v^{-3}D^{3/2}$, $B = \text{Noise}$. The line was fitted by least-squares to the measured $D(r)$. If Kolmogorov scaling is valid, $B \equiv 0$. Hence, B following from the LS-fit was used as an integral measure of side-effects (noise), and was applied as a quality-check parameter for the fitting. See also the works of Wiles et al. (2006) and McGinnis et al. (2015), cited in the paper.

Q. Page 5, line 11 “to discard corresponding values with subsequent interpolation between the neighboring values”. I do not understand this sentence. Please explain better.

A. The text will be replaced with: “the measurements, for which Eq. 6 was valid, were abandoned as noisy.”

Q. P5, line 14: “The estimations based on the averaging time of 20 min and $r = 0.4$ m were adopted for the further analysis 15 averaged over the three beam estimations.”. Why?

A. The tested values $r = 0.4, 0.5, 0.6$ m and $T_{\text{averaging}} = 10, 20, 30$ min were chosen as tentatively corresponding to the inertial range of turbulence, where the Kolmogorov scaling is valid (see also the reply to the question on Page 4, Line 27 above). All of them produced essentially the same output in terms of the TKE dissipation rate (see P. 5, Line 13), justifying the choice of the interval. $T_{\text{averaging}} = 20$ min was adopted as the mid-value of the tested range; the lower value of the wavelength $r = 0.4$ m was adopted to avoid occasional influence on the results of the increased instrumental noise at higher distances from the acoustic head.

Q. Page 6 line 24: “warmer is the water”. In the IL, right?

A. No. “The warmer is the CL” or “the higher is T_m ” is correct; to be refined in the final

version.

Q. P. 6, line 23: The sentence “The ice was snow-free (snow thickness < 1 cm)” seems to be in contradiction to the information in line 20: “a thick ice-cover with more than 50% of snow-ice”

A. No contradiction. Snow-ice (or “white ice”) is part of the ice cover consisting of the refrozen snow/water mix (see Kirillin et al., 2012, for details). The actual snow cover was practically absent. Text to be refined in the final version.

Q. P. 6. line 24: “a cross-section”. The same cross-section used before?

A. practically the same.

Q. Page 8, line 14: “suggestion”. Are there any evidence/reference for this suggestion?

A. No.

Q. Page 8, line 16: To integrate Eq. 3 the radiative profiles are not enough. How have you compute the other terms?

A. The other terms are calculated from measured temperatures as described in Section 2.2

Q. line 28: “The stratified IL occupied the 2-m thick layer”. According to Figure 4 the IL is thinner than 2-m

A. changed to “up to 2 m”

Q. Page 9, Line 1: “the TKE dissipation rate ε increased”. Increased relatively to what? Maybe the sentence should be rewritten beginning with the “background” values...

A. Increased relative to the previous values. As long as the time variations are discussed, the context is obvious.

Q. Page 9, Line 5: “The dataset on the TKE dissipation rate covered a longer period than the temperatures in the IL”. How, as TKE dissipation rate is computed from the

temperatures?

A. Dissipation rates are not computed from temperatures. See Section 2.3 and Fig. 6.

Q. Page 9, line 14: “ had no effect”. Based on Fig. 7, it seems that there are some effect.

A. Some effect can indeed be speculated on. We remove the sentence as having no primary importance for the discussion.

Comments on the minor remarks:

The rest of remarks and suggestions, as referred to the figure order, figure legends, and term definitions are gratefully acknowledged and will be directly incorporated into the revised version of the paper.

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