Earth System Dynamics Discussions

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Comment on esd-2021-30

Anonymous Referee #1

Referee comment on "Accounting for surface waves improves gas flux estimation at high wind speed in a large lake" by Pascal Perolo et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-30-RC1, 2021

Review of "Accounting for surface waves improves gas flux estimation at high wind speed in a large lake" by Perolo and colleagues.

In this well-constructed study, Perolo and colleagues evaluated the performance of various empirical and process-based gas exchange (k) models using a combination of high-frequency k and weather measurements, intending to find the best model to accommodate high wind and high wave heights conditions often found in large lakes such as Lake Geneva. To be able to account for the major processes affecting k on such a large lake (i.e. wind shear, buoyance flux and wave motion including bubble enhancement), the authors cleverly combined and adapted existing process-based models first developed for the ocean. The chosen adapted model was proven the most accurate and flexible to a wide range of wind speeds and wave heights.

Investigating k at high wind and wave heights conditions is an important and overlooked aspect of k dynamics in the context of episodic events. The addition of waves and bubble enhancement to lakes k models is novel, to my knowledge. I particularly liked the cumulative k analysis as it clearly demonstrates the disproportionally important role of rare periods of high winds and waves. It also elegantly shows how each model responds as a whole to the distributions of wind and wave that actually occurs on a large lake. I also think that Figure 1 is very useful and accurately summarizes the main processes and the predictive models with their respective variables used.

Overall, I found the manuscript well written, well organized and easy to follow. The references are appropriate and the methods clear. I only have a few general and specific comments that could potentially improve the manuscript.

General comments:

The lack of spatial integration (due to only one measurement station) is only discussed briefly in the conclusion. Wind and waves fields are different in other parts of the lake and this can have a large impact on the conclusions made regarding CO2 fluxes. I suggest the authors expand this part and move to the discussion section.

In undersaturated CO2 conditions (high pH), there is the possibility at CO2 fluxes are enhanced chemically (aka chemical enhancement factor). This usually happens during productive periods (summer with undersaturated pCO2), where CO2 is rapidly consumed chemically (hydration) at the very surface of water (Wanninkhof and Knox, 1996), enhancing the CO2 influx from the atmosphere, but not affecting pCO2 measurements at a deeper depth. If this is the case (at about pH > 8), it would result in an overestimation of observed k values measured from pCO2 and flux chambers, especially under calm wind conditions. In the manuscript, the chemical enhancement factor was not taken into account, and I think this should be justified. I do not think this would change the main conclusions of the paper, but it may potentially slightly affect the parameterization (SD20-fit) and the models evaluations.

Significant wave heights (Hs) were not measured but predicted from U10 and fetch. Do the authors have any idea of uncertainties associated with the predicted Hs?

The terms *SD20* and *S20* (in Fig. 5) are presented in the same way as the other published models (i.e. first letter of author name and the year of publication). In this case, does SD stands for Soloviev and *D* for Deike? and why 20? Please explain in the text and/or in the figure caption.

Specific comments:

L42: I think it should be Cole and Caraco (1998), as it is commonly called, instead of Cole et al.

L138: As this is a novel method, I think more details on the automated (forced diffusion) flux chamber should be given. How this chamber is different from the more traditional floating chamber?

L235: I don’t know what the “^” symbol means here?

L280. Missing dot.

L362: But specific calibration (a and A) would also be needed in process-based models like it is the case for Lake Geneva.

Fig. C1. The models used only negative buoyancy flux, which induces turbulence by convection. I wonder what is the effect of positive buoyancy during heating on k. Could it reduce the effect of wind shear as it suppresses turbulence?