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Ocean Science Discussions



# *Interactive comment on* "Short-term variations of thermohaline structure in the Gulf of Finland" *by* T. Liblik and U. Lips

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Thank you for the review and very useful comments!

We hope that the explanations below and changes made in the manuscript will substantiate the approach used for quantification of some physical processes contributing to the observed changes in stratification and TS-distribution. We stress that the main aim of the manuscript was to define distinct stratification patterns (which in turn influence the pelagic ecosystem) and the approach for quantification of some physical processes was introduced to explain what would be behind of formation of such patterns. In the new version of the manuscript we added estimates of magnitudes of terms in equation (2) to clarify this issue in more detail and added discussion on applicability of the equation (2) as well as future steps to improve it.



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Interactive Discussion

Discussion Paper

Figures have been updated according to the suggestion and the north component of current velocity is added in Fig. 2.

We thank the referee for the suggestion to change the title, and agree to replace the title as follows: “Variability of synoptic-scale quasi-stationary thermohaline stratification patterns in the Gulf of Finland in summer 2009”, if possible.

We changed wording in abstract as suggested: “The periods with distinct layered flow structures and current oscillations were revealed. A conceptual 1-d model, where the heat flux through the sea surface, wind mixing, wind induced transport (parallel to the horizontal salinity gradient) in the upper layer and estuarine circulation were taken into account, reproduced the observed changes in the vertical stratification reasonably well.”

Discussion about the applicability of formulae is added into the new version of the manuscript.

Answers to specific comments

p. 879, line 13. The base of thermocline was defined as the maximum depth, where the temperature was $\geq$ 5 °C. The thickness of the thermocline was defined as the difference between base of thermocline and upper mixed layer depth.

p. 879, last paragraph: Thank you for this comment. Similar sentences have appeared in many publications, but you are right, in the initial form it was a bit odd. We changed the sentence and refer to a publication by Laanemets et al. (2011, Boreal Env Res) where it was estimated that mesoscale fluctuations contain 66 % of the total kinetic energy.

p. 881, lines 22-24: As stated in the manuscript the raw data acquired with a vertical resolution of 10 cm were processed and stored for further analysis with a vertical resolution of 50 cm. The pre-processing takes into account the time constant of the temperature sensor and calculates salinity and density values. However, the main



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problem is related to different (and variable one for conductivity sensor depending on lowering speed) time constants of sensors and variable lowering speed due to waves. The vertical profiles analysed are obtainedd by a simple averaging of pre-processed data within the 50 cm depthintervals.

p. 882, lines 17-18: It's correct, temperature in the bottom layer could be >5 °C. But our CTD-profiler reached only down to 50 m depth, where temperature didn't exceed 5 °C.

p. 883, line 6: We omitted respective sentence.

p. 884, line 18: Constant value of alpha is replaced by alpha as a function of temperature and calculations made according to that. The surface temperature was changing from 12 to 20 degrees, thus, the results were altered only slightly.

p. 884, last formula: This is a very important point in the manuscript, and we still think that formula (5) is applicable to estimate the wind induced changes in the stratification due to the along-gulf transport (where the flow below the thermocline is directed opposite to the mean flow in the surface layer). It is well known that the estuarine circulation in the Gulf of Finland is altered by local wind forcing (see e.g. Elken et al., 2003). The explanation, why the direction of 25 degrees from north (which is about 45 degrees in relation to the gulf's axis) has the best correlation with the changes in vertical stratification (as found for instance Liblik and Lips, 2011) is as follows: direct wind driven currents are directed out from the gulf or towards the northern coast. The latter induces some sealevel difference between the northern and southern gulf which in turn accelerates the outflow in the surface layer even more. At the same time the currents are directed into the gulf below the pycnocline (as a compensating flow). We agree that the whole approach (equation (2)) is a first rough approximation (we added this explanation into the text). Some major processes were not taken into account since we were restricting the analysis to the temporal scales days to weeks. It is stressed in the paper that the found discrepancies between the observed and estimated potential en-



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ergy anomaly were caused by the water movements across the gulf and corresponding vertical displacement of isopycnals due to the coastal upwelling or downwelling events (mesoscale processes).

p. 885, formula (6): Since we omitted upper layer temperature calculation, total heat flux is now only used in buoyancy flux calculation (formula (3)). In that respect advective heat fluxes can be neglected. We improved the argumentation in text.

p. 885, formula (9): We omitted formula (9).

p. 888, line 24: We revised the text according to your suggestion and replaced the term “gradient” by “difference” where necessary.

p. 893, line 1-11: We omitted upper layer temperature calculation.

p. 895, line 5: We revised the text.

p. 896, line 16: We partly agree with the comment and tried to improve the text/manuscript accordingly: it is defined more precisely that our aim was to identify the distinct stratification patterns. The approach used to explain the processes behind these patterns are introduced mainly to quantify the influence of some of the processes. Mesoscale processes are left out in this study from the equation (2) (since the data available did not to allow estimation of their contribution) but their importance is stressed in the manuscript. In the new version we have added more discussion about the physical background of the approach used as well as its applicability and steps to improve it.

Weak point of the periods description 3.1.2: We think the wind induced transport is taken into account quite well due to the fact that the atmospheric forcing does not differ very much for the Gulf of Finland and northern Baltic Proper in temporal scales of our interest (days to weeks). Inertial oscillations are out of the scope as well. To understand the sealevel changes and variability of currents with a temporal scale 1-2 days the barotropic oscillations have to be taken into account. But this will not influence



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the vertical stratification much. In order to explain what is not taken into account and how this could influence the results, we added some text into the discussion.

We revised Fig. 2 as suggested and added also the north component.

We took all your comments on the text into account. Thank you!

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Interactive comment on Ocean Sci. Discuss., 9, 877, 2012.