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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

Anonymous Referee #3

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The manuscript reports a new set of hydro-physical field data obtained by an autonomous buoy profiler and a bottom mounted ADCP in the Gulf of Finland (the Baltic Sea) during 2 months of summer-2009. It presents a substantial contribution to the Baltic Sea/Gulf of Finland investigation.

Measurement equipment and methods are ok, however, a substantiation of physical approach used for quantification and explanation of the processes, manifested in the data, is strongly recommended (see Comments for further details).

Figs. 2 and 3 have to be larger – axes, legends are not readable on printouts. The English is understandable – however with quite many mistakes and wrong expressions.

1. Does the paper address relevant scientific questions within the scope of OS? ==>

yes

2. Does the paper present novel concepts, ideas, tools, or data? ==> new data are presented

3. Are substantial conclusions reached? ==> The only substantial conclusion is that it is possible to find in the data a 4-15-days-long periods of quasi-stationary situations. Other conclusions still require further substantiation.

4. Are the scientific methods and assumptions valid and clearly outlined? ==> Measurement methods are OK, whilst the assumptions must be outlined more explicitly.

5. Are the results sufficient to support the interpretations and conclusions? ==> Not always - see the Comments below.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? ==> yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? ==> yes

8. Does the title clearly reflect the contents of the paper? ==> I may suggest another title: "Variability of synoptic-scale quasi-stationary thermohaline stratification patterns in the Gulf of Finland in summer 2009"

9. Does the abstract provide a concise and complete summary? ==> Abstract Line 15: I'd not use words "model" and "simulated" here – they usually suggest numerical simulations, while in the paper there are rather estimations. Another choice – directly define the model here in Abstract, e.g., "a conceptual 1-d model. . . reproduced well. . .".

The revealed "period of 26 h" appears in Abstract (thus, it is considered as one of the main results) – however only one sentence is devoted to this result in the entire paper (p.892, lines 12-14),); no spectrum is presented, no discussion. Either omit from the Abstract – or show more in the paper.

OSD
9, C268–C273, 2012

10. Is the overall presentation well structured and clear? ==> yes

11. Is the language fluent and precise? ==> needs improvement

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? ==> the applicability of the used formulae is not discussed

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? ==> yes, I'd suggest elimination of the "temperature criterium", see Comments

14. Are the number and quality of references appropriate? ==> yes

Specific comments:

p.879, line 13: what is understood under "the base of the thermocline" and "the thickness of the thermocline"?

p.879, last paragraph: "Due to..." – the sentence is physically confusing: the Ocean is also well larger than the internal Rossby radius; why mesoscale processes are do not dominate also there??

p.881, lines 22-24: not clear: were the data smoothed over 50 cm? interpolated? how? (important for p.882, lines 20-21)

p.882, line 17-18: "The base of the thermocline was defined as the maximum depth where the temperature was >=5C" ⇒ isn't it the very bottom?

p.883, line 6: "the basic idea... to show that it is possible to detect a number of ... patterns" sounds not too serious for the researcher who possesses so rich data sets! Better to avoid such formulations – and just omit this sentence.

p.884, line 18: alpha is taken as constant, while water temperature varies from 20 C to 3 C (what is already close to the Tmd) – why so? It is easy to take alpha(T).

p.884, last formula (5): if I understand correctly, the formula is based on the sugges-

OSD

9, C268–C273, 2012

Interactive Comment

tion that the flow is in a kind of the geostrophic balance. However a few pages earlier the authors argued that, in their case, the mesoscale processes in narrow and wind-influenced GoF are dominant. Thus, the formula (5) is not applicable in this case. Almost the same remark to p.886, lines 3-7: the authors take as “the most appropriate” wind direction – almost the same as that of the estuarine density gradient, what is obviously in contradiction with the geostrophic balance. The applicability of the formulae (5) in the given situation must be physically substantiated!

p.885, formula (6): why to neglect the advective heat fluxes? – just a line before at least the estuarine circulation WAS taken into account. Here again physical arguments are missing: in principle, the heat transport can be small when compared with other terms of eq. (6); this, however, must be shown by evaluations – otherwise, there is no reason/logic to neglect the advection.

p.885, formula (9): physically wrong: valid only for cooling or complete-mixing episodes in summer , while the authors apply it “considering strong stratification in summer”. Temperature of WHAT is meant here then? p.887, line 12: “on average” – and no return flow to the south? Needs clarification about 3- dimensionality of the circulation, otherwise this is nonsense.

p.888, line 24 and further: gradient and difference are confused – they are mathematically different quantities!

p.893, lines 1-11: here in fact – explaining why so many discrepancies - the authors find that formula (9) is indeed not applicable... What for it is used in the paper then? I suggest this part of “the model” is omitted from the paper – it is useless.

p.895, line 5: two graphs of Fig. 6 cannot be considered as a substantial proof for the conclusion that ”changes in stratification can be modeled as proposed by Simpson et al. (1990)” also in the case of wind-induced reversal of the estuarine circulation; much more detailed consideration is required here.

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Interactive Discussion

Discussion Paper

p.896, line 16: “the existing model (Eq.2)...” – physically wrong expression. “Model” in science is based, before the all, on some idea/concept/suggestion that, for the given process/situation, say, (a),(b),(c) are important and (d),(e),(f) – not important. This leads to a certain equation(s), which CANNOT be used without substantiation, that this very “model” is applicable in this particular case. In this paper, the logic is reversed: the underlying physical model for the used equations (and thus - limits of their applicability) is not discussed at all, an equation is called “a model”, and both agreements and disagreements between some calculations and real measurements are considered as a proof of the model applicability. This logic is to be improved: (1) physical background for Eqs.(2)-(9) and their applicability for the given situation is to be provided and (2) the terminology should be checked.

Weak point of the periods description in 3.1.2: the authors relate all the changes of TS structure in the given area with local forcings only (in fact - with local wind solely), whilst GoF is just a large bay of the Baltic sea, which has a wide open entrance from the side of prevailing winds. Thus, influence of the Baltic sea water dynamics (wind-induced transport, internal waves, etc.) should be at least mentioned, better – taken into account. The same with variability of river drain – why it is also neglected?

Fig. 2: why only E-W current component is shown? Just for simplicity of the description? The circulation under investigation is obviously 3-dimensional, and here it is reduced to even 1D. It seems, that real processes are far too complicated, and the authors try to avoid the details. If so, I suggest some smoothing of the current data is performed – but anyway both horizontal current components are shown.

Comments on the text:

p.879, line 6: “separated from the surface by two. . .” – to add two words p.879, lines 9-11: for summer time p.880, line 27: “narrow width” – bad Engl. p.881, line 4: high-frequency p.881, line 7: “down to the depth of 40-50 m” (the same at p.883, line 17). The entire paragraph requires the language check. p.884, line 6: delete the word “re-

OSD

9, C268–C273, 2012

Interactive Comment

spectively". p.884, lines 5-6: "increase or decrease of stratification" – physically wrong phrase; better to say 'time rate of change of potential energy of the water column due to heating/cooling, etc. ". p.884, line 10: "increases" => "reinforces" p.884, lines 5, 11: "right" => "right-hand side" p.885, line 8, the very end: 10 is missing. p.885, line 18: Stefan–Boltzmann - dash is needed (the law is named after two different people - Jožef Stefan has deduced it on the basis of experimental measurements, and Ludwig Boltzmann derived it later from theoretical considerations). p.886, last line: considerably p.888, line 13: show here the dates explicitly p.893, lines 12-21 (and throughout the paper): "decrease/increase of stratification" is wrong expression

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