Biogeosciences Discuss.,
doi:10.5194/bg-2016-301-RC1, 2016


Biogeosciences
Discussions

# *Interactive comment on* "Nutrient cycling in the Baltic Sea – results from a 30-year physical-biogeochemical reanalysis" *by* Ye Liu et al.

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Received and published: 1 October 2016

The study deals with application of data assimilation approach to reconstruction of long-term dynamics of 3D nutrient fields as a base for analysis of nutrient transport processes in the Baltic Sea. Both the approach and obtained results are significantly novel in methodological and geographical senses to deserve publishing in "Biogeosciences". However, scientific and presentation qualities should be substantially improved by the major revision of the manuscript along the lines suggested below.

1. General comments and suggestions

1.1 Objectives and applicability. The assimilation of whatever available data is fully



justified for an improvement of short-term forecasting of hydrophysical fields aiming at the search-and-rescue operations, propagation and expansion of catastrophic spills as well as management of the maritime activity. However, its applicability for long-term hindcasts of biogeochemical phenomena and properties requires careful consideration and clear explanation of the purposes/objectives of the assimilation (why and what for). Such considerations and explanations should already be given in the Introduction section, with particular attention to the limitations, especially non-conservativeness of the approach (what can and cannot be done).

1.2 Artificial non-conservation. Biogeochemical variables are non-conservative by definition, while the entire models of biogeochemical cycles are usually designed as conservative, i.e. explicitly accounting for all the external and internal sources and sinks of the matter. In such models (including the implemented RCO-SCOBI system), the dynamics of simulated nutrient fields is determined by continuous, mutually adjusted interaction of physical transport and biogeochemical transformation processes. If these 4D fields (x, y, z, t) are not absolutely identical to the corresponding fields reconstructed from observations, then an every act of "correction" of simulated towards reconstructed fields during assimilation procedure would create in the model fictitious 3D sinks and sources of the matter not generated by either transport or transformation processes. These fictitious fluxes of nutrients are then included into biogeochemical cycles, thus making the model erroneously non-conservative. Evidently, the studies of eutrophication and biological productivity in general are particularly vulnerable for these effects of data assimilation. As can be deduced, for instance, from Figs. 3-5, such effects are quite substantial.

On the other hand, with a certain confidence in simulated transport agents (water currents and mixing) supported, e.g. by the plausible dynamics of "conservative" salinity (e.g. as in Liu et al. 2013), the "corrected" fields of nutrients could be used for improving simulation of nutrient transport processes. Here, again, the discussion on how such improvement would affect simulation of transformation processes and, in turn, would be affected by them could significantly augment the scientific value of the paper. Also, the questions arises – could not the same results regarding transport processes been achieved just with the "observed" nutrient fields used for assimilation, without running and "jerking/correcting" the biogeochemical model.

In any case, the artificial non-conservativeness should be explicitly acknowledged and explained, its effects evaluated, presented, and discussed, in addition to- and, perhaps, together with analysis of biases by means of RMSD. The estimates of non-conservation and its spatial and temporal dynamics must be computed from a difference between model fields before and after acts of assimilation, starting from the initial conditions. Then the knowledge of needed "correction" can also be used in pinpointing possible deficiencies in the biogeochemical parameterizations.

1.3 Plausibility of the RCO-SCOBI model. The RCO-SCOBI model has been extensively used for forecasts (aka projections) of possible changes in the Baltic Sea biogeochemistry under different scenarios of driving forces, practically by the same authors. Therefore, the scientific value of the paper could be significantly increased by the discussion and speculations on how the model's deficiencies in simulation of transport flows and transformation fluxes, which are revealed due to the data assimilation, for instance, in the form of RMSD, could affect the predictions. Good starting point could be a statement at line 387.

1.4. Description and explanation of Methods. All the methods implemented in the manuscript must be described in more detail and, considering an intended expansion of the paper's coverage from the "hydrophysical" audience over the "Bio-Geo-Chemical" one, in somewhat more popular style.

Assimilation procedure. In addition to references to (Liu et al. 2013, 2014), several details, especially those important for magnitude and distribution of 4D fictitious fluxes, must be repeated and explicitly explained in this paper as well. The explanations should include, for instance, such details as: a) verbal description of procedure



for reconstruction of "observed" fields used further in assimilation and in calculation of RMSD in FREE and REAN experiments, b) spatially and temporally varying uncertainties of such fields determined by the scarcity and sparsity of observations, c) frequency of the assimilation acts and its possible effects on the difference between model and observation used in calculation of RSMD (Liu et al., 2014), and whatever else would be necessary for further presentation and discussion of issues from Comment 1.2 above. Without such clarifications, three sentences at lines 170-173 look as isolated abracadabra and might seem almost useless.

Nutrient transports, trends, and budgets. The exact definitions of all the nutrient transports, trends, and budgets measures and characteristics together with algorithms of their calculation, including derived units, should be clearly presented already in Methods. This will clarify possible confusions with the usage and interpretation of the terms vs. phenomena, commented in details below, in Section 2.

2. Specific comments and suggestions.

2.1 "Cycling" in the title and similar statements to that effect elsewhere Accordingly to comments 1.1-2 above, the non-conservative model cannot be used for comprehensive studies of nutrient CYCLING. Hence, the title should be modified – consider, please, something like "Nutrient TRANSPORTS in the Baltic . . ." instead. Correspondingly, the usage of "cycling" and similar statements and expressions about transformation processes should be carefully revaluated throughout the entire text, for instance, at lines 80, 189, 310, 306-307, 362-363, 466, and throughout the entire Section 5.6,

2.2 Calculation of RMSD. Line 194 – What is the meaning of "overall" and "monthly mean" in "the overall monthly mean RMSDs" and how they were calculated – for how many fields per month? covering the entire Baltic? cell by cell for interpolated "observational" fields or only for cells with the real observations?

2.3 Nutrient transports. Explain and clarify, please, involved terms and interpretations – What does "net" (which is usually used with the word "exchange" and represents a difference between inputs/imports and outputs/exports) mean at lines 17, 259-260, 277, 300, 338, 356, 360, and 492; – Why some characteristics related to single grid cells or a grid "column" are called "net", has it something to do with the difference between in- and out- transport flows or/and is it meant to account for local changes due to transformations, causing difference between inflows to the cell (column) and outflows from it? For instance, at line 694 – How exactly the vertical averages and vertical integrals (e.g. line 259) have been computed? Why ANNUAL average is expressed in ton/ km/MONTH (Fig. 7, lines 694-697)? Would vertical averages multiplied by the depth of grid point be equal to vertical integrals? What is the point presenting/contrasting/comparing (e.g. in Fig. 7) vertically averaged transport for the locations with, for instance, 200 and 20 m depths? – Definitions and explanations for calculations of nutrient sources and sinks from integral transports would be helpful in understanding and interpretation of Section 5.6. Some consideration and discussion on how much the sinks and sources could depend on which transformation processes and how much they would be determined by fictitious fluxes might be useful too. Also, check the consistency of term's usage both in the text and, especially in legend to Figs. 8 and 9 (annual average IMPORT (transports?); again ANNUAL is expressed on per MONTH basis.

2.4 Nutrient budgets. Explain, please, how the budgets were computed: – How nutrient in- and outflows (as product of velocity and concentration) been obtained from integrals of continuous computations for period 1970-1999 or from averaging of monthly or annual integrals? – How have annual sink/sources been calculated? Have the transformation processes (sediment-water exchanges, burial, nitrogen fixation, denitrification) been accounted for? – How trends in Table 1 been estimated? What does P sources in the KT, GF, and BB (sic!) as well as N source in GF mean? – How the total amounts (pools) of nutrients were calculated, by averaging of which fields, integrated with which frequency?

These explanations are necessary but not sufficient for understanding how 30-year average annual "tendencies" (trends? deviations?) agree with pools? Most illustrative



are P sources. In BB, 0.8 Kt P/yr *30 yrs=24 Kt P comparing to the pool of 5.9 Kt P; in GF, 5.9 Kt P/yr *30 yrs=177 Kt P comparing to the pool of 29.9 Kt P. Where has such hefty P excess gone, accumulated in the sediments? Evidently, the changes of nutrient pools in sediments must be included into consideration as well regardless of how plausible they are.

– Legend to Figs. 10 and 11 says: "External nutrient inputs are separated into terrestrial and atmospheric sources. Terrestrial loads are reduced by phosphorus retentions for the coastal zones." However, external inputs are presented with single numbers. Is it a sum of terrestrial and atmospheric loads, then the word is "combined"? What is the coastal P retention, how it was estimated and which values were prescribed? Was N inputs treated in a similar way?

Similar explanations and considerations, starting from algorithm of calculation should be given also to horizontally integrated flows at transects (Fig.12, lines 349-378) with special attention paid to explanation of the purpose of their analysis in a view of complex picture of water circulation and nutrient transports in Fig. 7.

Considerations about possible contributions of transformation vs. fictitious processes would be appropriate in Section 5.7 or in discussion of presented results as well.

2.5 Secchi depth (see also comment for lines 185-186 below). The water transparency seasonal variations and long-term trends depend on too many factors that either are not included in the model (e.g. CDOM and SPM distribution and variation) or are determined by complicated feedbacks from transformation processes (e.g. primary production and sedimentation of decomposing organic matter) to be used as unequivocal indicator of improved simulation of the nutrient fields. In result, the related analysis (lines 250-253) looks weak and unconvincing, for instance, the decrease of inorganic nutrients should cause the decreased primary production and how realistic is that? Or is it a correct effect by the wrong reason? Therefore, I would recommend deleting consideration of Secchi depth from the paper entirely. However, if the authors will chose to retain these considerations then a few words about how Secchi depth is estimated in the model (what it does and does not account for) would be useful for readers.

2.6 Presentation of pelagic and sediments pools. As it appears from Comments 2.4 and lines 380-388 in Discussion, presentation of pelagic and sediment nutrient pools could help to untangle several issues in interpretation of results

3. Minor things, technical corrections and language cosmetics.

Lines: 3 – I guess, it is Eilola not Eolila; 11-12 – What is “improvement in . . . concentrations”? Consider, please, something like “improved simulation/reproduction/imitation of concentrations” or similar; 33-34 – Perhaps, not as much “living conditions” as redox dependent biogeochemical processes; here the reference to (Conley et al., 2009) or/and (Savchuk, 2010) would be appropriate in addition to- or instead of (Fu, 2013) 50-54 – poor choice of words: “. . .of BIOLOGICAL formulations (either empirical or mechanistic) to UPDATE biogeochemical concentrations” that sounds as (physical) oceanographers’ slang; why only “biological”, what is “update” and “simulation accuracy”, why “In reality..”, “applicability” to what purposes? Please, reformulate more carefully; 92 – “The reanalysis is mainly based on. . .” Consider, please, replacing something like with “The success of reanalysis. . .” or “The confidence in reanalysis is based on (or stems from). . .” or similar; 94-96 – neither ICES nor SHARK “are monitoring” the Baltic Sea, both just maintain databases with monitoring results, correct appropriately; 104 – in that context a reference to Gustafsson et al. (2012) would be more appropriate in addition to- or instead of Savchuk et al. (2008); 110-111 – is “. . .a better assessment of HISTORICAL changes in the nutrient budgets of the water column and (OS – especially) sediments. . .” , true and legitimate aim of this study? Where are historical changes then? 119 – unusual usage of “sea surface heights”, replace, please, with “sea level (variations)”; 148 vs. 165 – is it SHARK only or SHARK and BED together? If the later, then there are much more observations in BED, for instance, for the Gulf of Riga; 178-180 vs. 81-82 – repetition, delete, perhaps, from Introduction; 182 – instead of “we focus . . . on nutrient budgets and transports. . .”, perhaps, “we



focus . . . on nutrient transports and budgets derived from them. . .” would better reflect both the focus and importance of results; 185-186 – consider simplification as “. . . long- term trends in eutrophication as indicated by Secchi depth (Section 5.4)”, because if the water transparency can be used as indicator of the eutrophication as the entire phenomenon, it seems too far-fetched to use it for evaluation of the “excess of nutrients in the water column”. 198-199 – what does “. . .positive impact on the model simulation” mean, improved model-data comparability, or model-data resemblance or similar? Is it unexpected? 216 – perhaps, “. . . how data assimilation makes simulated nutrient dynamics in the Baltic proper look more realistic” would be more correct introduction to Fig. 4? 266 – concentrations should be HIGHER not GREATER 268 – Why AMPLITUDES, most common meaning is as the measure of range, fluctuation, difference between maximum and minimum, i.e. large amplitude could mean small NET transports. Maybe, MAGNITUDE? 285 – maybe, “contrast” would be better word than “contradiction”? 306 – What “uptake and deposition of DIP”, by which process (es)? 310 – “taken up” or retained? 311-313 – needs better, clearer explanation 315 – Which “vertical exchange”, in the water column or along the bottom, how estimated? 380-388 vs. 177-178 – Has not initialization somewhat adjusted the fields? In any way, these considerations once more call for presentation of sediments’ pools 428-432 – There is a confusion and misinterpretation about P loads that should be corrected. Possible underestimation of P load was guessed by Savchuk and Wulff (2007) only for the Gulf of Riga. In all other basins, HELCOM data on unfiltered samples were used and GF load of 7 Kt P/yr used by Savchuk and Wulff (2007) are actually very close to the latest compilation by Knuuttila et al. (JMS, 2016). However, the loads in the 1970s and especially, the 1980s were larger indeed. 454 – Isn’t location of halocline and, correspondingly, different volumes of hypoxia prone layers a rather important explanation? 484 – Is it denitrification and not PP? Why?

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