Interactive comment on “Nutrient cycling in the Baltic Sea – results from a 30-year physical-biogeochemical reanalysis” By Ye Liu et al.

Anonymous Referee #2

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We thank you for your most helpful and thoughtful comments in the evaluation of our manuscript.

General comments

this manuscript the authors use a numerical model in combination with data assimilation to estimate nutrient fluxes within the Baltic Sea. They show that the data assimilation scheme greatly improves the results in terms of spatiotemporal concentrations fields. Without data assimilation the model have significant bias in both the annual cycle of the surface layers as well as spatial distribution of nutrient levels, but as shown, the assimilation procedure eliminate significantly of these systematic biases in a very impressive way. I am unfortunately not at all familiar with data assimilation methods. I tried to get a quick grip on what and how it is done by reading the method description in not only this manuscript, but also previous papers by the authors. Unfortunately, my background knowledge is too small to really understand even the basics of how it is done. Therefore, I hope that another reviewer is able to penetrate the technicalities of the method and judge its applicability. I can only see the end result and that the assimilated model results really do resemble the reality at the scales presented. I think given that the end results are useful for a wider community and focus on the discussion is not on the technical aspects, it would be useful if the authors include a brief paragraph describing in words how observations and model are merged in the assimilation procedure. Liu et al presents a solid reanalysis of 4 dimensional nutrient fields in the Baltic Sea. The nice correspondence with observations indicate that resulting data set is probably the best available data set and should provide useful for many purposes. Further that present interesting spatial budgets on both fine and basin-wide scales. One can, of course, question our knowledge of the certainty of the detailed source/sink calculations, but anyway the results are interesting and could definitely be considered best available. Given the journal one could have wished for deeper analysis of the results in terms of biogeochemical processes. Because of my limited understanding of the methodology I cannot really advice on how far such analysis could go, but now there is very little analysis on whether the spatial fields of sources and sinks may be due to or how they are connected to various processes. Although discussion is rather weak, I think the results are interesting enough, both in terms of the apparently excellent data quality the method results in as well as the Baltic Sea specific results on nutrient fluxes that I recommend publication.

Response: We detail and rewrite the text in the method’s description according to your comments. See the sections 4 “Methodology and Experimental Setup”, which describes how the observations and model are merged in the assimilation procedure.

In general, by relatively small effort, the manuscript text can be improved and I provide some, hopefully helpful, comments below to most sections.

Specific comments

Section 5.1 It is not surprising that the authors find some significant RMSD for e.g. ammonia in the 1970s. There are substantial temporal trends in data quality and consistent high-quality data is generally achieved only after international inter calibration became standard in the first half of the 1990s. I also believe that ammonia is one of the parameters with largest errors in the 1970s, while phosphate and nitrate was more reliable.

I do not understand “stability” of the assimilation, but that is surely due to my ignorance of the methodology.

Response: Thanks for specifying the quality of the ammonia observation.

Here we mean the assimilation results give a reliable estimation of the ocean state during the whole period. EnOI relies on the selected ensemble sample to estimate the background error covariance of model. The poor sample ensemble can cause the failure of the analysis. With the evolution of simulation, the performance of the data assimilation is different. The success of data assimilation at one time can't guarantee continued success of data assimilation at another time. Therefore, the "reliable" of a data assimilation system is key to the reanalysis. The RMSDs in Figure 3 denoted our estimation with EnOI is successful during the whole simulation period, which proved that our data assimilation system is valid and "reliable". To clarify, we instead "stability" by "reliable" in revised manuscript.

Section 5.2 The improvement in capturing the seasonal cycle is impressive. When I study figure 4 in Liu et al (2014) referred to in the text, it seems however, that the improvement is not due to the improved halocline only, but really due to the assimilation of chemical variables. In that figure DIN and DIP seem to be worse when only S and T is assimilated. I am not exactly sure how much interpretation on processes that can be done comparing different assimilated runs, but it seems that when assimilating only S and T, the model fails in using the additional In nutrients mixed up. However, I agree that a prerequisite for a deep spring bloom is a deep halocline.

Response: As shown by Liu et al. (2014), adjusting the physical condition for biogeochemical model doesn't guarantee the better biogeochemical simulation.

Requirements to calculate correct simulation in additional to optimized model equations are high-quality atmospheric and riverine forcing data, and high-quality initial and lateral boundary conditions. As any other model, RCO-SCOBI had to be calibrated because many processes including sources and sinks of nutrients are not detailed enough known. Hence, an "optimal" parameterization of unresolved processes is one of the requirements for the predictive capacity of the model. The "optimal" physical forcing field is one of conditions to guarantee the correct the biogeochemical simulation. Assimilating only S/T will possibly break the balance of physical-biogeochemical condition, which provides the "optimal" initial condition for the circulation model and maybe degrade the usage of the former "optimal" parameterization for biogeochemical model. As a result, the physical-biogeochemical simulation using only T/S assimilation is done with "non-optimal" initial condition. Therefore, both physical and biogeochemical observations are necessary to be assimilated into the model to produce the "optimal" initial condition for a coupled physical-biogeochemical model simulation.

Section 5.3 Also here the improvements are impressive and the spatial variations in winter nutrient concentrations are well captured. This really gives credibility to use these results in flux calculations.

Response: thanks for your comments!

Section 5.4 Secchi depth is a complex variable including strong dependence also on coloured organic matter. It is evident that a higher Secchi depth is obtained using the assimilation, but calculating Secchi depth in the Baltic Sea from modeled algae biomass is not really well constrained so one could argue that by recalculating Secchi using somewhat different attenuation from CDOM could also give a fit to observations with the model without assimilation. Since temporal variation is not captured (which may be due to other causes than biomass), there is no way of knowing which calculation is actually the best and thus applicability of Secchi depth for validation is not very promising. Therefore I suggest that you can remove this section and the associated

Response: Following the advice of both reviewers we delete this content from the revised manuscript.

Section 5.5 I am not really sure what these horizontal fluxes tell us!

Response: The aims of presenting mean horizontal nutrient currents in the Baltic Sea is helpful to address the description of the nutrient exchanges between subbasins and between the coastal zone and the open sea in manuscript. The nutrient transport in Baltic Sea is differing from other regions because of its physical and biological condition (e.g. the shallow mean water depth, much river runoff, the weak tide, the much source/sink). The horizontal distribution of the nutrient transport gives the hint to detect the intensity and direction of the nutrient transport.

Section 5.6 Does the assimilation as such affect conservation or constitute a part of the source/sink? Baring in mind my limited understanding of the methodology, I am wondering whether by having an underlying model simulation with error, corrected by the assimilation scheme the total source/sinks may give some erroneous results? However, I guess if you just integrate currents times concentrations, there should not be any problem. These results are quite interesting, although a bit challenging to understand. Perhaps it would be somewhat easier to explain if Total P (N) and DIP (DIN) were used instead of Org P (N). The totals would then give the net source/sink of the nutrient and the inorganic show the "gross" source/sink due to net turnover. It would be easier to read if the comparison with Eilola 2012, was postponed to the discussion. Now, I think the main results from this study is unnecessary difficult to follow, because of the frequent comparison with the previous paper.

Response: In the long-term simulation, the new initial condition for an assimilation cycle differs from the ending ocean state of the last cycle when at that time observations are available. In this sense, the data assimilation introduces sources and sinks of the nutrient cycles by interrupting the model simulation and adjusting the initial condition. However, we provide the "optimal" initial condition with data assimilation for the RCO-SCOBI for every simulation cycle. It means we don't change the equations of the RCO-SCOBI and just integrate currents and concentrations. The simulation process is conservative during the simulation between two assimilation occasions.

We agree that the data assimilation affects conservation properties for the long simulation as a whole. Although the reanalysis is conserved during every "independent" simulation cycle, the adjustment of data assimilation implicitly creates unknown complementary sources or sinks to the biogeochemical model. The magnitude of these adjustments depends on the bias between model and observations. The artificial sources/sinks are directly related to the model biases. Figure 3 shows that the model has large biases during the beginning of the simulation. However, data assimilation has corrected the mismatch between model state and observation to an "optimal" level during an initial adjustment period. After the adjustment period, the mismatch between model and observation becomes small and the successive adjustment due to data assimilation also becomes small (Liu et al. 2014). Further, the adjustment of data assimilation is related to the spatial-temporal coverage of observations. Here we assimilated only observed profiles into the model.

We want to keep the discussion of internal dynamics of inorganic and organic nutrient. As mentioned earlier, the potential impact from artificial sources or sinks due to data assimilation is included in the reanalysis results. Because of the unknown impact from this "process" it is better to avoid detailed discussions about the net sources and sinks.

We move the comparison with Eilola et al. (2012) to the discussion section.

Section 5.7 To my knowledge, the model used does only include bio available nutrients. This is fine but should be clearly stated to avoid confusion. Especially for nitrogen, there is a significant net flux through the system of refractory N that is not captured here. I further assume that the budgets are made summing inorganic and organic nutrients, but adding a sentence about that makes it easier for the reader to follow. I am confused by the fact that the budgets in figs 10-11 does not add up. A small net could be attributed to changes in water column storage, but looking for example at phosphorus in Gulf of Finland the net is 8.6+54.7-50.7-6.7 = 12.6 -6.7 = 5.9 kton/yr. This is far too much to be storage change. I thought that it could be that only a part of the load was used, but looking at Gulf of Riga there is a net loss of 1.4 kton/yr. Is it a consequence of the data assimilation? In that case, how should this residual be interpreted? In any case it should be clarified and shown in figures 10-11. That gross fluxes are different between approaches are not surprising since it will depend on time-resolution as the authors point out. Oscillating flows due to various processes cause a dispersive transport that to some extent is resolved by the 3D model, but it is not given that the net effect is correct if the processes that regulate the dispersive transport such as e.g., mixing and frontal movements are appropriately modeled. Without really detailed observations of currents and concentrations one have to resort the validation of the dispersive transport to the net effect on e.g. salinity in the basin. Thus, in some sense, the estimate of net transport by a full 3D model may not be that different from the assumptions behind those of using the diagnostic Knudsen approach, i.e. a strong correlation between salinity and the constituent of interest. Having said that, the level of detail is of coarse massively different and the possibilities to make temporal and spatial analyses also greater.

Validation currents and circulation patterns are very difficult and I do not demand that, but it could have been nice with a discussion on how confident we can be in the results of nutrient circulation and source/sink spatial variations in light of how the data assimilation improves circulation. A starting point could be the consequences of that a clear majority of the hydrochemical data has been collected at single locations usually quite central in the basins and not along the stretches of strong circulation. A naive issue that I personally wondering about is whether assimilation of point wise observations may induce spurious circulation patterns?

Response: Thanks for your comments. Yes, the budgets are made summing inorganic and organic bio available nutrients. We add text for clarifying the total nutrient in this section in revised manuscript.

The budget calculation is recalculated with new borders. The results are reliable and reasonable.

Meanwhile we corrected the mistake caused by the unit transform. The results are regarded reliable and reasonable. For example, the net phosphorus tendency for the Gulf of Finland is 24.3-22.5+8.6-6.7 = 3.7 Kton/yr. Further, in the Bothnian Bay, the net nitrogen tendency is zero. Comparison with the results of Savchuk (2005, 2007) based on Knudsen approach, the difference is mainly caused by the external supply from atmosphere and land. But phosphorus tendency in Gulf of Riga still a net loss of 0.5 Kton/yr. The difference between our result and Savchuk (2005) is due to different internal removal. Our results and Savchuk (2005, 2007) are treating different periods, the loads in the 1970s and the 1980s were larger indeed compared the loads in 1990s.

In the Baltic Sea, the mean water depth amounts to about 54m. Mainly wind forcing and topography are the factors that affect the variability of the circulation In the shallow region in the Baltic Sea, where stratification is weak, the surface circulation may affect the sea floor. Further, the topography of the sea floor plays an important role in constraining the circulation and much of the abyssal flow is funneled through passages such as the Denmark Straight. Our reanalysis changes salinity and temperature of seawater but it does not change the horizontal circulation explicitly. Further, we change the stratification in the Baltic Sea which will affect the vertical circulation in our assimilation experiment (Liu et al. 2013). Fu et al. (2011) has validated the improvement of sea level in assimilating temperature and salinity with EnOI method. In this study, we don't change the forcing. With a high-resolution circulation model, physical state variables include the sea level, temperature and salinity. We consider the impact of barotropic and baroclinic balance during the assimilation. Further, Wenzel et al. (2001) proved that, when sea level is assimilated in the circulation model in addition to temperature and salinity to adjust the small-scale variability, the large-scale circulation will not be degraded.

We estimated the assimilation increment according to optimal statistics of the water column in every grid point. The water mass is mainly controlled by the temperature and salinity. We estimated the "optimal" characteristics (temperature and salinity) of water mass in our reanalysis. The "optimal" characteristics will produce the "optimal" hydrological dynamic balance based on the model dynamic equations. As a result, we don't degrade the estimation of horizontal transport

M. Wenzel et al. (2001) Progress in Oceanography 48 73–119.

Actually, we used the Knudsen approach to calculate the water flow transports. We obtained results similar to Savchuk (2005).

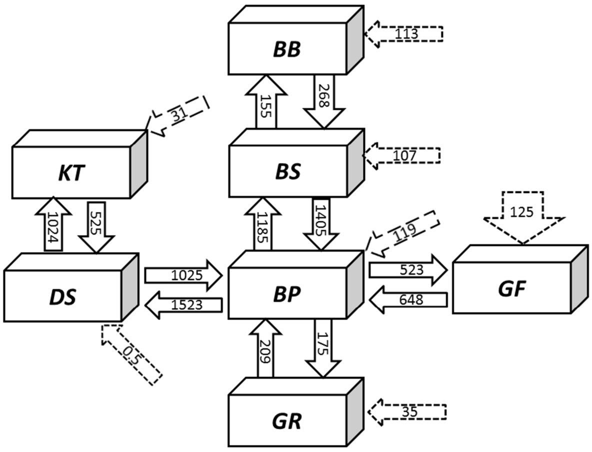

Figure. Water flows between the Baltic Sea basins (km^3 year^{-1}) averaged over 1970-1999. External water inputs are the sum of net freshwater supply from river runoff and from atmosphere.

I would argue that the sub-basin boundaries in the model of Gustafsson etc also (2012) is not arbitrary chosen. As far as possible sub-basin boundaries of this model is chosen according to dynamical constraints such as sills or fronts that can be parametrizised. A discussion of the implications of the high-resolution sink/source fields for our understanding of major processes would have been quite interesting. What does the spatial distribution of e.g. net sedimentation or denitrification imply? What are the pathways for organic matter? I am not sure how far you can take this given methodological limitations, but it could be nice here with a few things and not only referring to other model simulations.

Response: We clarify the boundaries description in Gustafsson et al. (2012). And we will consider the possibility to add some discussion about the high-resolution sink/source fields in the revised manuscript.