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

O.P. Savchuk (Referee)

oleg.savchuk@su.se

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We thank Dr. Savchuk for your very good comments. We have followed all the comments from you and carefully made the improvement in our revision.

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).

Response: We have specified the aims of data assimilation in the introduction more clearly. The data assimilation meets the gap between observations and numerical modeling in this study. We aim to reproducing the ocean biogeochemical state with the help of information from both observations and a coupled physical-biogeochemical model. The results of the reanalysis can be used to estimate the water quality and ecological state with high spatial and temporal resolution in regions and during periods when no measurements are available. Regional and local model studies may use the data as initial and boundary conditions. Further, nutrient transports across selected cross-sections or between vertical layers might be calculated with high resolution and accuracy taking the complete dynamics of primitive equation models into account. This information cannot be obtained from neither observations alone or from model results without data assimilation because the latter might have large biases in both space and time. We assess the nutrient budgets of the water column and sediments, as well as of the nutrient exchanges between subbasins and between the coastal zone and the open sea. As a reanalysis can never be dynamical consistent and does not preserve mass, momentum and energy (see our response to 1.2), the calculated budgets are compared to the results of other studies to evaluate our results meant as consistency check. Hereby, we follow studies of other regions applying data assimilation for a biogeochemical reanalysis on long-term scale.

For example, Teruzzi et al. 2014. Journal of Geophysical Research, 119, 1–18.

Ciavatta, S., et al. 2016, J. Geophys. Res. Oceans , 121 , 1824–1845.

Fontana, C., et al.,2013, Ocean Sci., 9, 37-56.

In the introduction section we will further clarify the already listed limitations of data assimilation with respect to estimating nutrient budgets and we will rewrite the objectives of this study.

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.

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 with Dr. Savchuk 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.

The advantage of the data assimilation is that model variables at any station are very likely more accurate than the model output without data assimilation. For instance, time series of profiles or transports across vertical sections have very likely a smaller bias compared to observations than the corresponding model results without data assimilation. Compared to available observations the information from the model is higher resolved and homogeneous in space and time. Of course, it is difficult to evaluate the quality of model results at high resolution because independent observational data sets are usually missing. An exceptional effort to utilize independent data was done by Liu et al. (2014) showing that the statement about the added value of data assimilation is true for the available, independent cruise data at high resolution. However, one can not expect that budgets calculated from the summation of fluxes from model results with data assimilation are more accurate because usually small artificial sources and sinks from the data assimilation are becoming as important as physically motivated sources and sinks when sums of fluxes are compared. Hence, we calculated budgets with the aim to evaluate the reanalysis data and to estimate the magnitude of artificial sources and sinks by comparing our results with other studies using only observations. We are aware that it is impossible to claim that our budgets are more accurate than those budgets that are derived from observations only despite the higher temporal and spatial resolution in model outputs. Hence, the advantage of the reanalysis is that measurements are extrapolated in space and time based upon physical principles of the model. However, the disadvantage is that the reanalysis data does not obey conservation principles. We will discuss advantages and disadvantages of the reanalysis in more detail in the revised version of the manuscript.

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.

Response: RCO-SCOBI has been widely used for the Baltic Sea and the model was carefully evaluated using various observational data sets. 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. Further requirements to calculate correct transports and transformation processes in addition to optimized model equations are high-quality atmospheric and riverine forcing data, and high-quality initial and lateral boundary conditions.

We discussed already in the present version of the manuscript why FREE has so large biases compared to the results by Liu et al (2014, Tellus A) and compared to biogeochemical observations. Most of the large differences are caused by imperfect initial conditions, which can be seen from the temporal evolution of the RMSD (Figure 3).

For projections of future climate and for nutrient load abatement scenarios the reanalysis has a very high scientific value as reference data set for the historical period of the climate simulations. The evaluation of the regionalized climate (the statistics of mesoscale variability, e.g. the mean state) during the historical period can be done much more accurate based upon the reanalysis data than with sparse observational data. For instance, it is very difficult to calculate the climatological mean state just from observations that are casted only during the ice-free season of the year. Using a reanalysis as reference data for historical climate is a common method in regional climate studies of the atmosphere. Here we provide a corresponding data set for the ocean to evaluate simulated present-day climate. We will add a paragraph to the discussion to highlight the value of reanalysis data sets for climate studies.

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.

Response: We will detail and rewrite the text in the method's description according to your comments. See the sections 4 "Methodology and Experimental Setup".

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,

Response: Following your suggestions, we will change the text and use nutrient transports instead of nutrient cycles.

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?

Response: We add the following Equation to specify the calculation process of RMSD in the revised manuscript.

The overall monthly mean RMSD is calculated by the following formula:

$$RMSD = \frac{1}{N_j}\sum_{j=1}^{N_j}\sqrt{\frac{1}{N_t}\sum_{i=1}^{N_t}(\varepsilon_t^i)^2}$$

where N_t is the number of the observations at assimilation time t and N_j are the number of days observed in one month for one field for entire Baltic Sea. $\varepsilon_t^i = x_{sim}^i(t) - x_{obs}^i(t)$ represents the model-observation difference at the time t at the i^{th} observation position. x_{sim} and x_{obs} are the modeled and observed field. We calculated ε_t at only the observation position at the time t, which is calculated by mapping the corresponding model field to the observation space.

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.

Response: We add the following equation to explain the calculation process of the nutrient transports in every grid 'column' or 'cell'. The vertically averaged transport (VA_{Trans}) at every horizontal grid section at a simulation time is calculated with the following formula:

$$VA_{Trans} = \frac{1}{N_z}\sum_{j=1}^{N_z}\iint \rho_f u\,dx\,dz\,,$$

where ρ_f, u, N_z, dx and dz are the field concentrations, the current velocity normal to cross-sectional area, the number of wet grid cells in one water column, the horizontal and vertical dimensions of a grid cell (m), respectively.

Here the net transports express the difference between inflow and outflow transports. Both "net" and "exchange" are common usage in the description of transport. Just like you mention here the "net" denotes the difference between inputs/imports and outputs/exports. We will define "net" in the method part of the revised manuscript.

For example. Eilola et al . Ambio., 41, 574–585, 2012. Treguier et al.,Ocean Sci., 10, 243–255, 2014.

The "net" usages also denote the local transport change in every section or grid cell or grid 'column'.

We change the "ANNUAL average" to "Monthly average" in the corresponding text. The calculated process referred to the above Equation.

The vertically averaged nutrient transports present the direction and magnitude of the nutrient transports in every water "column" in the Baltic Sea. The Figure 7 shows the mean net horizontal nutrient transport in each cell of the horizontal model grid. From that we can get the distribution of the direction and magnitude of nutriment horizontal transport in the Baltic. For example, the magnitudes of DIP/DIN transport are stronger in the east Gotland basin than that in Gulf of Finland.

Definitions and explanations of sources and sinks have been given in the text of Section 5.6(also see our response to 2.6). Further, we give how transport is calculated in every grid cell or 'column' (see Equation in the reply to 2.4).

We changed the legend usage in the Figure 9. And we also clarify the description of net flow in the Section 5.6, which use the consistent term's usage description.

Actually, if we use the Knudsen approach (exactly as described in Savchuk 2005) to calculate the water flow transports we obtain results similar to Savchuk (2005) (see figure below).

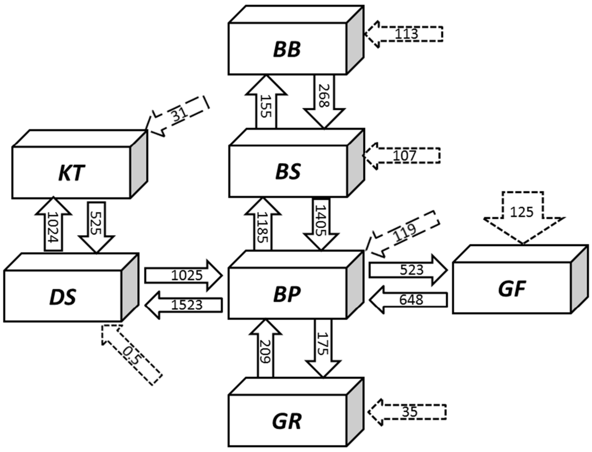

Figure. Water flows between the Baltic Sea basins (km^3 year^{-1}) averaged over 1970-1999. External water inputs are the sum of net freshwater supply from river runoff and from atmosphere.

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?

Response: The calculations of nutrient budgets will be better explained in the revised version. The nutrient flow for the budgets is calculated by the similar method to the above shown integral equation at the selected borders of Baltic subbasins. We obtained the annual average nutrient flow from integrals of continuous computations for period 1970-1999.

In the nutrient budgets the P and N external sources are computed from the combined supplies from land and atmosphere. Nitrogen fixation is not included in the external supplies. The sediment sinks are calculated from the difference between the net deposition of nutrients to the sediments and the release of nutrients from the sediments.

The model includes all these transformation processes (sediment-water exchanges, burial, nitrogen fixation, denitrification). The results have taken these processes into account. (refer to Eilola et al, J. Mar. Syst., 75, 163–184, 2009 and Almroth-Rosell et al, Journal of Marine Systems, 144, 127–141, 2015.)

The potential impact from artificial sources or sinks due to data assimilation is of course also included in the results. Because of the unknown impact from this "process" it is better to avoid detailed discussions especially about the changes in the nutrient pools. The trends in Table 1 are calculated from the differences between the nutrient inputs and nutrient exports seen in Figures 9 and 10.

The total amounts (pools) of nutrients were calculated as the sum of the inorganic and organic nutrients in the water. The total amounts of nutrients for every grid cell were calculated with the averaged nutrient concentration of corresponding grid cell during the period 1970-1999 and the formula:

$$Total = \iint \rho_f \, dxdz,$$

where ρ_f, dx and dz are the field concentrations (including nutrients from phytoplankton, zooplankton, detritus and dissolved nutrient) the horizontal and vertical dimensions of a grid cell (m), respectively. And then total amounts of nutrients are the sum of the nutrients of all water grid "cell" in every subbasins.

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.

Response: We redefine the borders of the subbasins (Fig. 1) and recalculate the total nutrient budget based on the new borders. 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.

– 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?

Response: the number of external inputs is a sum of the supply from atmosphere and land. We change the word used in these figures description. We remove the text "Terrestrial loads are reduced by phosphorus retentions for the coastal zones" since our model has consider these process during the model calculating nutrient flux.

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.

Response: we have given the answer for these comments. Please refer to the reply to 1.2 and 2.3.

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.

Response: we followed the suggestion by the reviewer and deleted this section about the Secchi depth in the revised manuscript.

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

Response: As mentioned earlier, the potential impact from artificial sources or sinks due to data assimilation is of course also included in the results. Because of the unknown impact from this "process" it is better to avoid detailed discussions especially about the changes in the nutrient pools.

3. Minor things, technical corrections and language cosmetics.

We will in the revised version have several major changes in the text that may affect the interpretation of the detailed suggestions given by the reviewer. We will seriously consider and take into consideration all minor comments from the reviewer also in the reworking of the text.

Lines: 3 – I guess, it is Eilola not Eolila;

Response: we correct it in revised manuscript.

11-12 – What is "improvement in ::: concentrations"? Consider, please, something like "improved simulation/reproduction/imitation of concentrations" or similar;

Response: We change it to "…improved simulation of both oxygen and nutrient concentrations"

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)

Response: We change this sentence to "MBIs can significantly affect the biogeochemical processes in the deep basins because of the inflow of large volumes of oxygen-rich water into the Baltic Sea (e.g. Conley et al. 2009; Savchuk, 2010)."

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;

Response: We rewrite it in revised manuscript. To clarify, now we delete “In general, coupled physical-biogeochemical models use a variety of biological formulations (either empirical or mechanistic) to update biogeochemical concentrations. As a result, the model formulation and the reliability of their parameterizations play a key role in determining the simulation accuracy of biogeochemical processes. In reality these processes governing the interactions between biogeochemical compartments vary in space and time (Losa et al., 2004; Doney, 1999).” in the revised version.

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;

Response: We change it to “:::The success of reanalysis is mainly based on a reliable model:::”

94-96 – neither ICES nor SHARK “are monitoring” the Baltic Sea, both just maintain databases with monitoring results, correct appropriately;

Response: We change it to “For example, the International Council for the Exploration of the Sea (ICES) (http://www.ices.dk) and the Swedish Oceanographic Data Centre (SHARK) (http://sharkweb.smhi.se) are collecting the observations with the aim to monitor the Baltic Sea. Furthermore, the Baltic Sea Operational Oceanographic System (BOOS) (http://www.boos.org/) is providing near real-time observations.”

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);

Response: We replace the reference to Savchuk et al. (2008) by Gustafsson et al. (2012).

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?

Response: we change description of the aim of this study. Please see the reply to 1.1.

119 – unusual usage of “sea surface heights”, replace, please, with “sea level (variations)”;

Response: we replace the “sea surface heights” by “sea level elevation”

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;

Response: Yes, data from SHARK are assimilated into RCO-SCOBI. But data from both SHARK and BED are used for validation. We correct it in revised manuscript.

178-180 vs. 81-82 – repetition, delete, perhaps, from Introduction;

Response: we delete the “However, in Liu et al. (2014), only a shorter assimilation experiment for a 10-year period is presented, and so far the stability of the assimilation scheme in multi-decadal simulations has not been shown.” in introduction section.

182 – instead of “we focus ::: on nutrient budgets and transports ::: ”, perhaps, “we ::: on nutrient transports and budgets derived from them ::: ” would better reflect both the focus and importance of results;

Response: we accept your comments and change it in the revised manuscript.

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".

Response: we delete this sentence: "and long-term trends in eutrophication (excess of nutrients in the water column) as indicated by Secchi depth (Section 5.4)".

198-199 – what does " ::: positive impact on the model simulation" mean, improved model-data comparability, or model-data resemblance or similar? Is it unexpected?

Response: the positive impact means reanalysis results closer data relative to FREE, which reduce the uncertainly of model simulation.

216 – perhaps, " ::: how data assimilation makes simulated nutrient dynamics in the Baltic proper look more realistic" would be more correct introduction to Fig. 4?

Response: we change it according to your comment.

266 – concentrations should be HIGHER not GREATER.

Response: We change the word "greater" to "higher".

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?

Response: We change the word "amplitude" to "magnitude". Thanks for your kind comment.

285 – maybe, "contrast" would be better word than "contradiction"?

Response: We change the word "contradiction" to "contrast".

306 – What "uptake and deposition of DIP", by which process (es)?

Response: We change this sentence by "This result might be explained by local processes causing the phytoplankton uptake and sediment deposition of DIP.".

310 – "taken up" or retained?

Response: it should be "retained".

311-313 – needs better, clearer explanation.

Response: The phosphorus sink may also be partly caused by oxygen dependent water–sediment fluxes that bind DIP to ironbound phosphorus in oxic sediments (Almroth et al., 2015). This effect is not included in the Eilola et al. (2012), but might potentially be accounted for by the adjusted DIP transports in REANA. The results of REANA indicate that there is an additional sink but the relative importance of different processes causing this sink (data assimilation or sediment processes) is, however, not possible to evaluate from the reanalysis data set.

315 – Which "vertical exchange", in the water column or along the bottom, how estimated?

Response: the "vertical exchange profile" description is related to the internal nutrient sink/source at different water depth (Figure 8). But for clarification, we delete "vertical" in the revised manuscript.

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.

Response: Both REANA and FREE take the start initial condition from the same earlier run. However, to REANA, we firstly use the data assimilation method to "optimize" the initial condition and then forward the integration. FREE forward the integration based on the non-"optimal" the initial condition.

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.

Response: we clarify it by delete this sentence: "However, their total phosphorus load, for example to the Gulf of Finland, is underestimated because the particulate phosphorus fraction is neglected (Savchuk et al., 2012)."

454 – Isn't location of halocline and, correspondingly, different volumes of hypoxia prone layers a rather important explanation?

Response: Yes, we also think it is good explanation of model biases. We add it into revised manuscript.

484 – Is it denitrification and not PP? Why?

Response: Thank you for the comment. The high productivity in the shallow areas effectively transfers DIN to OrgN. The denitrification act on larger scales and decrease the exports of nitrogen from coastal areas to the deeper areas. The potential impact from artificial sources or sinks due to data assimilation is also included in the results. The discussion in the manuscript will be revised accordingly.