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Hydrology and Earth System Sciences Discussions

Interactive comment on "Riverine transport of biogenic elements to the Baltic Sea – past and possible future perspectives" *by* C. Humborg et al.

Anonymous Referee #1

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The manuscript is a great and systematic compilation of empirical data and knowledge about nutrients, silica and carbon river fluxes in the Baltic Sea Rivers and basins. In addition, a modelling tool (CSIM) has been applied to simulate the river loads according to three scenarios.

The paper is well-organised with its main merits in its qualitative discussion. The quantitative scientific evidence is however regarded as weak.

Overall, the major criticism and weakness of manuscript are on N and P and its poor argumentation and over-simplifications, and several crucial issues are covered. These are:

- a rather poor description of the model input data and its uncertainties,

- missing description of the critical N and P processes from source/emissions to river mouth (hydro-bio-geo-chemical)
- too brief description of the model used and its performance
- weak description and assessment of the assumptions/limitations in the scenario results
- the results of scenarios is less convincingly presented and illustrated at least in scientific terms.

Part of these weaknesses is explained by that the manuscript has too wide coverage including N, P, Si and TOC in an extremely large (and heterogeneous) drainage basin. Nonetheless, it is suggested that all these missing and weakly addressed issues (see the critical remarks in more detail below) should be given (properly justified and critically assessed) in much more depth and with a better and sound scientific presentation of the scenario results (especially for N and P). Considering the remarks below will significantly increase the creditability of the results. Despite the mentioned lacks and weaknesses in the manuscript, it should be pin-pointed that the manuscript is highly relevant and very interesting. It is also ‘felt between the lines’ that the analyses are performed at high quality but not yet visible and convincingly given for the reader in its present version.

Below the most critical remarks:

1. **Hydrology.** The hydrological component in the N and P scenarios is not analysed, at least no information about this is found in manuscript. This is a weakness since the hydrological impacts is large for Si and TOC as emphasised and excellently discussed by the authors. It is well-known that hydrology (i.e. water discharge) also is the major determinant for inter-annual river nutrient loadings

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(as clearly shown in Fig. 5 in manuscript). Given the large emphasis on hydrology for future TOC-changes it is somewhat surprising to note that this is not considered by the authors in the N and P scenarios! For example, studies has shown likelihood of increased number of freezing-thawing events with indications of incidental losses of soil particles and thus also P-losses. How good is the model (originally developed in U.S) to predict hydrology in the Baltic Sea region (especially winter and snow/ice conditions)? In addition, will changes in the hydrological regimes as discussed in the TOC-section alter the N and P loads in the given scenarios (i.e., changes in seasonal distributions and thus the hydro biogeochemical processes?). These hydrological aspects should also be included or at a minimum be critically discussed and assessed in the results of the N and P scenarios. This also given that the CSIM originally is a hydrological model.

2. **The buffering and attenuation/retention** is not considered or weakly addressed in all of the 3 scenarios for N and P. It is well-known that both soils, streams, lakes and hydropower dams can retain a substantial fraction of N and P. This aspect need to be discussed more critically, given e.g., the large amount of lake areas in e.g. some of the eastern European basins (Neva is mentioned by the authors in the manuscript). Another example: On page 1102, rows 11–12: Soil retention was assumed to be 83% for TN and 97% for TP based on an English study (Johnes et al. 1996). It should be made clearer how these figures was used in the CSIM (statically or based on some more process-based algorithm?) including an assessment of possible uncertainty. It is namely well-known that soil retention is extremely variable in time and space influenced by soil type, soil aggregates, hydrometeorological conditions, soil-wetness, mineralization and denitrification potential, soil physicochemical and biological processes, hydrological pathways (surface runoff, lateral matrix flow, tile drains) etc etc. Some more consideration of these aspects will definitively increase the credibility of the results. The similar underlying processes is greatly argued for Si and TOC (see excellently written

pages 1104–1109) but regarded as weak for N and P.

3. **The scenario of future agricultural development** in the Eastern European countries may be regarded as a 'worst case' scenario. The reasoning behind the assumption that these countries will adopt Danish agricultural intensity is not well justified. Secondly, as the authors write in manuscript the river loads has not changed to any particular degree despite severe changes (from high to low intensity) the last 20-years, after the break-down of the Soviet Union and Iron Curtain with significant reductions in fertiliser use and livestock. What is the mechanisms and key hydro-bio-geo-chemical processes that will increase the loads in future given the few evidences of decreased loads after decrease gross emissions? This is important to assess given the objective of paper as written in first row in abstract '*The paper reviews critical processes for the land-sea fluxes of biogenic elements...*' This is also regarded as important given one of the conclusions in paper namely that '*we propose that N fluxes will increase due to higher livestock densities...*'. In fact this conclusion should be considered to be moderated or rephrased e.g. by clearly addressing the assumptions and limitations (see also comment #4 and5).

4. **The type of livestock changes** is not mentioned. For example, is it pigs, poultry or cattle? What kind of manure handling system is anticipated? Will the manure be utilised fully as fertiliser and replacement for mineral/commercial fertilisers? Food or feed production? Grazing animals or barns? Slaughter cattle or milk cows? Concentrated large-farms like in the Soviet time or many small-scale farms like in Poland at present? Anticipated spatial distribution transport networks of manure? Level of manure fertilisers on fields vs. mineral fertilisers? Will the dominance of collective farms still be persistent in e.g. Estonia and Latvia or can we expect more private farms and in such case any changes in production and environmental management? Will the animal feed be produced within a country (i.e. affecting also the production on field)? All these question-marks could have

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significant impact on the final scenario iii results especially for N.

5. **Uncertainties** in the input data, used model and scenarios are not assessed at all! Especially the uncertainty in the agricultural scenario must be critically evaluated given the conclusion in the manuscript about increased N river fluxes. This comment is also related to points 1–4 above and the second minor remark below (ii). Here we note the comment made by the authors in another similar scenario paper in the same study area with the same model (Wulff et al., 2007; http://www.mare.su.se/dokument/evaluation/About%20MARE%20and%20Baltic%20Nest/Wulff%20et%20al%20(2007%20in%20press).pdf) where it is stated that '*The results, in terms of absolute number, produced in this article should be looked upon with great caution because of the high degree of uncertainty in the actual data behind the model used to calculate nutrient load reductions.*'

6. **The model description and results of the scenarios** are not well-addressed. The authors refers to a publication by Moerth et al (2007) (and to some extent also Wulff et al. 2007; see the short description of the model on page 1097 rows 16–19) but this is not felt satisfactory given the scope and objective of paper. At least the authors should argue about the chosen CSIM model in terms of how well it has been calibrated and validated and how well it is suited for scenario analyses (see also comment #5 above). The CSIM according to the references given by the authors (e.g. http://www.mare.su.se) assumes type concentrations for various compartments. According to these references and the figure 6 text it is understand that in the model, water from each land cover type is routed both directly to stream flow and down to the soil water compartment. From the soil water compartment water is routed to stream flow and to the groundwater compartment, and from there on to the stream. It is not clear what kind of type concentrations (and its scientific basis and assumptions) that are changed in the various compartments in the 3 scenarios. Is this done by assigning new

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static type concentrations in all the compartment boxes or is it based on some more mechanistic/process-based model runs? For example, how are the groundwater concentrations in the 2 'boxes' determined in the scenarios and what is the residence times assumed? What is the uncertainty in these assumptions (see remark #5). In addition how are likely changes in ammonia volatilization taken into consideration? More explicitly, ammonia volatilization from animal manures commence immediately after excretion of the manure, i.e., losses occur from animal houses and during storage and field application of manure. The process is governed by many factors such as the ammonia concentration, the pH of the manure, air temperature, humidity, ventilation, management and construction of the housing systems etc. How are these factors taken into consideration in the scenario? It is not well justified why the manure factor (see box of point sources) is directly routed to lake and streams (isn't the pathway via cultivated area and groundwater regarded as important?)?

7. The abstract should be revised since it covers issues that not are covered in manuscript. For example:
   Row 5 'i.e., changes in hydrological patterns' are not fully and completely addressed in paper, i.e. lacks for N and P (see also comment #1 above)
   Row 9 'we propose that N fluxes will increase'. This is regarded as speculative since it is based solely on the worst case agricultural scenario (see also comments #2 and 3 above)
   Row 11 '...with further damming...'. This is not discussed to any particular degree in manuscript. Where is the quantitative evidence and references of further damming in the Baltic Sea Region?
   Row 13–15. The results from the modelled biological impacts in the marine and coastal are not given in manuscript.

Some minor remarks:

I. Table 4. It is not clear why the emission coefficients for milk cows deviate so much between countries? For example the emission from an Estonian cow is 93 N per yr while it is only 74 kg in Denmark? Secondly, why are the coefficients for the other livestock types the same for all countries? This does not seem consistent. In addition, emission coefficients may also differ significant within one animal group (e.g small pigs vs. fat pigs).

II. Table 6. There seems to be some contradiction in the figures of the cultivated land compared to other literature sources? For example, FAO (http://www.fao.org/ag/agl/aglw/aquastat/countries/latvia/index.stm), in 1994, estimated the cultivated land in Latvia to 1.2 million ha, of which over 98% was covered by annual crops. Earthtrend (http://earthtrends.wri.org/pdf_library/country_profiles/agr_cou_428.pdf) report a cropland area in 1999 of 1,88 million ha for Latvia.
In Table 6, the authors report 3.28 million ha. Similarly for Estonia, FAO for 2004 reported 698,000 ha as agricultural area whereof 613,000 ha are arable (http://www.eastagri.org/country_detail.asp?id=39). In the manuscript it is reported 1 984 645 ha as cultivated area.
FAOSTAT reports for Poland in 2005, and agricultural land of 16,169,000 Ha (http://www.eastagri.org/country_detail.asp?id=23) while the authors estimate this to 20 677 731 ha.

Such large deviations between sources will also affect the modelled scenario river transports!

III. Table 6. Is the number of sheep's included in the model estimates? According to FAO statistics the number of sheep's only in Poland could be as high as 800,000 heads (during the late 1980s several millions!).

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IV. The authors have estimated the per capita P emission for detergents to 0,13-0-0,22 kg based on one study in the Danube river basin by Zessner and co-workers (2005). A recent European-wide study by Wind&Henkel (2007 at EWAOnline; http://www.ewaonline.de/journal/2007_03.pdf) show that these figures can be highly variable between countries. The question is then if one static value (obtained from a study in Danube river basin) applied for all the Baltic Sea countries will give the correct scenario *ii* results? See e.g., the higher per capita detergent consumption in Germany and Austria compared to Sweden and Finland in Wind&Henkel 2007.

V. In scenario i (page 1102 rows 23–25, the authors assume a removal efficiency of N in tertiary treatment of 80%. According to Zessner&Lindtner (2003), average treatment efficiencies are 50% nitrogen removal in treatment plants with nitrification and 80% in treatment plants with nitrification/denitrification. It is suggested that scenario i refers to best available (maximum?) N-tertiary treatment efficiency. At least the high N-removal efficiency should be better argued for.

VI. How is the scenarios related to expected policy implementations like WFD and Nitrates Directive? For example the code of good agricultural practises and restrictions in manure storage (the requirement for each farm to have sufficient livestock manure storage capacity for the period when they are not permitted to apply the manure to the land) and rules of spreading, e.g., $<$170 kg N organic/hectare/year given by the Nitrates Directive?

VII. It is less clear if CSIM is used also in the Si and TOC scenarios? Some explicit statement about that should be included.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 1095, 2007.