

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

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Anonymous Referee #1 Received and published: 11 July 2007 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

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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.

Authors reply: a more in depth description of the model is given now addressing input data and model uncertainty and model performance (r^2 between validated and simulated data are given now). However we are doing scenarios here and no predictions. These scenarios can be regarded as the general path towards which nutrient concentrations and fluxes to the Baltic Sea may probably develop. This is discussed now in more detail in the ms.

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 (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

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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.

Authors reply: we added a new figure showing the performance of the model in simulating seasonal streamflow for two rivers the Råne (unperturbed boreal watershed with strong peak in spring flow) and the Oder (cultivated watershed with less specific discharge and less event like hydrology). We discuss now that even climate change might influence TN and TP fluxes from the cultivated part of the Baltic Sea catchment, though we still emphasize that climate change is not the scope of scenario iii. Graham indicates an increase in water discharge to the northern basins of some 25% and a decrease in the southern watersheds of some 15% depending on the various climate scenarios calculated by regional climate models, this might lower TN and TP fluxes from the southern watersheds and may compensate for increases due to a more intensive livestock, However, detailed parameterizations of changes in water pathways that may probably come along with warmer temperatures and changes in groundwater tables are beyond the scope of this scenario analysis.

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

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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.

Authors reply: we added a section on how riverine retention of N and P was addressed: “Since in-stream biological uptake is not described dynamically in the model, the in-stream retention was fitted during model calibration by comparing the net emissions into the streamwaters (that is diffuse inputs after soil retention and point sources after treatment) and the observed nutrient loads at the river mouth over the calibration period. The mean in-stream retention for all simulated rivers was about 15% for TN and 19% for TP. In other words, the estimated loads to rivers were about 15-20% higher on average than the measured loads at the river mouth, which is somewhat lower compared to estimates made by various model approaches compiled in the latest PLC (~30% for both TN and TP; HELCOM 2004) and by estimates in US river systems (Seitzinger et al. 2002).

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

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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).

Authors reply: It is well known that river loads of N and P increased dramatically in the 50s and 60s when intensive agricultural practices, urbanization and industrialization emerged, whereas it is not well known to what extent these various drivers caused this increase in river concentrations. However, as we mentioned in the ms, most of the Baltic Sea watersheds in transitional countries (especially Poland) received only half of the loading from agricultural sources due to low use of fertilizers compared to the Estonian and Lithuanian rivers that had indeed high livestock densities and fertilizer use comparable to western countries. The latter rivers responded fast to decreased loadings as mentioned in the ms and we argue here that it is conceivable that they will turn back to the high concentration status when livestock density and fertilizer use will increase again as may do the other rivers. This is better explained now in the text.

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

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question-marks could have significant impact on the final scenario iii results especially for N.

Authors reply: This level of detail on agricultural practices in the various riparian countries is not addressed in the current model version, but their potential impacts on model outcomes are mentioned now in the ms.

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](http://www.mare.su.se/dokument/evaluation/About%20MARE%20and%20Baltic%20Nest/Wulff%20et%20al%20(2007%20in%20press).pdf) 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.'

Authors reply: we are well aware about all these uncertainties and as a matter of fact nobody was yet able to quantify these uncertainties, thus, we formulated the outcome of the scenario analyses more careful (also in the abstract) and describe them as possible vectors of change, i.e., describe possible increase/decreases in N and P loads as an scaling exercise that give the orders of magnitude of change of a certain change in lifestyles are amendment strategies that still are crucial for management decisions, although exact data can not be given with the actual level of information.

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 ar-

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gue 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 understood 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 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?)?

Authors reply: we have explained now how manure is addressed in the model: "Nitrogen and phosphorus loads in stream flow are calculated as the product of water discharge and the specified nutrient concentrations, with the addition of three types of sources, i.e., handling of manure as well as sewage in rural respectively urban areas." And later "The effect of diffusive emissions influenced by man that were estimated in scenario iii) was parameterized as increased nutrient inputs from manure due to higher

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livestock that enter the streamwater after significant soil retention (83% for TN and 97% for TP; (Johnes et al., 1996).” This means manure is simply added as an additional load to streamwater without changing ground water type concentrations. We discuss now that this is a simplification. Ammonia volatilization is indeed occurring, but most of it is not transported long-range and can be addressed as cyclic on a watershed scale (Galloway et al., 2004), i.e., for a N budget the process of Ammonia volatilization can be disregarded since it is cyclic in nature.

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)

Authors reply: we formulate more carefully now: “The aim of this paper is first to summarize the current knowledge on riverine nutrient and carbon fluxes in the Baltic Sea catchment, i.e., to describe leading environmental variables that drive these fluxes, and secondly to give future projections on possible changes in N, P and Si land-sea fluxes as result of the adoption of EU life styles and possible changes of C fluxes as a result of climate change.” We changed to “we propose that N fluxes may increase”.

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?

Authors reply: The potential for future hydropower is discussed in (Lehner et al., 2005) that we cite.

Row 13-15. The results from the modelled biological impacts in the marine and coastal are not given in manuscript.

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Authors reply: We have extended the discussion on possible implications for phytoplankton species composition in the conclusion section (see also reply to comments from referee 2).

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).

Authors reply: It is evident that milk production varies significantly between the countries and this milk production translates directly to N emissions (Claesson and Steineck, 1991); since this information is retrievable from FAO statistics we took this into account, whereas country information on pig production etc. is not easily accessible. However, we differentiate between saws and slaughter pigs when it comes to emissions and we took these numbers as well from (Claesson et al., 1991).

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 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 23) while the authors estimate this to 20 677 731 ha. Such large deviations between sources will also affect the modelled scenario river transports!

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Authors reply: CSIM uses cultivated land as a land class that includes arable land; arable land is for crop production whereas cultivated land is arable land plus pastures and meadows where animals such as cattle and sheep graze. This explains why cultivated land is significantly larger than arable land.

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!). Authors reply: As indicated in Fig. 4b the livestock of cattle and pigs are over 30 millions in Poland and thus dominate the total livestock; that's why we haven't included sheep in the current version.

IV. The authors have estimated the per capita P emission for detergents to 0,13-0,22 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 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.

Authors reply: The per capita P emissions can indeed vary regionally; we choose a lower value than in a recent scenario analysis (Wulff et al. 2007), but also this shows that using P free detergents is definitely an option to lower P emissions to the Baltic Sea. We regard the number of 0.2 kg as reasonable since the Danube watershed has a similar mixture between western type and transitional economies as in the Baltic Sea catchment; this is discussed now in the ms.

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

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best available (maximum?) N-tertiary treatment efficiency. At least the high N-removal efficiency should be better argued for.

Authors reply: we used official numbers compiled by EUROSTART as stated already in the ms; we state now that scenario i refer to the best available technique.

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?

Authors reply: As mentioned before, we do not use CSIM simulation for detailed predic-tions how certain farm practices may influence manure distributions in time and space, we did calculate the potential gross increase in manure; the limitations to this approach are discussed in more detail now (see comments above). The Water Framework Di-rective and the Nitrate Directive require the definition of a good ecological status that still has to be defined in the EU member states; we felt it to premature to discuss our scenarios in the light of those standards that do not exist as defined concentrations or loads.

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

Authors reply: we have stated now in the ms that CSIM has been not used to simulate TOC and Si loads to the Baltic Sea.

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