Biogeosciences Discussions, 1, S395–S401, 2004
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European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Investigation on the trophic state of the North Sea for three years (1994–1996) simulated with the ecosystem model ERSEM – the role of a sharp NAOI decline" *by* H. J. Lenhart et al.

Anonymous Referee #1

Received and published: 28 November 2004

General comment

The authors investigated the impact of the North Atlantic Oscillation (NAO) on the biogeochemistry of the North Sea using the ERSEM ecosystem model, during three years (1994-1996). Part of the discussion is centred on the trophic state of the North Sea and its changes due to NAO.

There is an abundant literature on the inter-annual variability of physical and biogeochemical processes in open oceanic waters due to large scale climatic oscillations (ENSO, PDO, NAO) but this has seldom been investigated in continental shelf seas. In this respect, this ms is an important contribution to the understanding of the biogeo-

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Discussion Paper

chemistry of continental shelf regions.

Major comments

The authors apply a model that has already been described in other publications, and although there is nothing wrong with this, the ms does not provide all the information for the reader to fully understand the results and related discussion. This is particularly important for readers without a modelling background and that are unfamiliar with the structure of ERSEM. I suggest that the authors add a supplemental section to the ms with a detailed description of the model (something along the lines of the Vichi et al. 2004 paper).

Page 739 Lines 25-26: The authors discuss the trophic state of the North Sea based on a nitrogen budget (Fig. 5). However, the concept of autotrophy and heterotrophy is related to gross primary production (GPP), autotrophic respiration (Ra) and heterotrophic respiration (Rh). The concept of trophic state is based on the organic carbon balance and not on nitrogen fluxes (refer to Gattuso 2004). Also, the concept of autotrophy and heterotrophy in coastal environments is related to the organic carbon balance in both pelagic and benthic compartments. While discussing Figure 5 (and elsewhere in the text), the authors seem to concentrate exclusively on the pelagic compartment.

Page 736 Line 4: The authors state “Excretion does not enter the calculation of the net production”. I assume that “excretion” relates to exudation of DOC by phytoplankton; the term “net production” is imprecise and leads to confusion and is only partly relevant concerning trophic state of an ecosystem. For the latter, the only important term is net ecosystem production (NEP = GPP - Ra - Rh) and not net primary production (NPP = GPP - Ra). Also, the computation of NEP should include GPP, Ra and Rh terms in both pelagic and benthic compartments. In terms of trophic state of an ecosystem, exudation of DOC or the so-called “luxury production” can be a critical factor (e.g. Thomas et al. 1999). This also relates to the above comment on the use of fluxes of nitrogen (or other nutrients) to derive conclusions on the trophic state of an ecosystem,

BGD

1, S395–S401, 2004

since "luxury production" cannot be accounted for when looking at nutrient fluxes (e.g. Thomas et al. 1999).

Page 736 Lines 11-16: The authors mention differences on trophic state between the northern and southern North Sea (although this is not quantified, except for NPP (in the southern North Sea) that alone does not allow to conclude on trophic state). They conclude that the southern part is autotrophic and the northern part heterotrophic. The problem with this analysis is that it is based on the nitrogen budget and not on organic carbon flows. The southern part is expected to receive more organic carbon inputs from rivers than the northern one. The northern part on the other hand receives from the North Atlantic Ocean more DOC than it exports (Thomas et al. 2004). It seems very unlikely that these processes can be inferred from the TON and DIN fluxes. Thus, regional differences on trophic state in the North Sea cannot be derived from a nitrogen mass balance.

Page 732 Lines 16-17: The authors state the importance of the water budget in assessing nutrient and carbon flows in the North Sea (as also raised in the interactive discussion of the Thomas et al. 2004 paper). Thus, it would be suitable if some kind of validation can be provided for the significantly different circulation patterns and water fluxes observed between 1995 and 1996. This would strengthen the conclusions on the simulated impact of NAO on the biogeochemistry of the North Sea.

In conclusion, I strongly recommend that:

1 - the authors clarify the terminology used in the text: avoid confusing (or vague) concepts/terms such as: "biomass production", "carbon production", "production", "net production", "primary production", "net uptake"

2 - the authors avoid inferring on the trophic state of the North Sea based on nitrogen fluxes.

Minor comments

Page 726 Lines 18-19: Provide a reference for this statement. Although this statement could be true, it certainly is not as straightforward as the authors try to convey. In the case of the North Sea, this could be true for near-shore coastal waters (Radach et al. 1990) but does not seem to be the case for the more central waters (Jickells 1998).

Page 726 Line 20: More recent simulations than those of Rabouille et al. (2001) predict an opposite shift in the trophic state of coastal areas (Andersson et al. 2004; Mackenzie et al. 2004). Note that the shift towards heterotrophy predicted by the simulation of Rabouille et al. (2001) is based on the decrease of the supply of nutrients by "upwelling" due to a "natural perturbation" on which Rabouille et al. (2001) are relatively evasive. Bakun (1990) predicts that coastal Ekman pumping increases with global warming.

Page 727 Line 5: the vision of continental shelves "as efficient sinks of atmospheric CO2" as developed by Tsunogai et al. (1999) is nowadays being to some extent challenged (Cai and Dai 2004). However, I agree that is of great interest to determine how coastal seas will react to climate change; referencing to Mackenzie et al. (2000; 2004) would be appropriate at this place.

Paragraph starting page 730 Line 21: A figure with the conceptual biogeochemical model showing state variables and processes would be useful (refer also to the first major comment).

Page 733 Lines 23-24: This does not seem to be the case of near-shore coastal waters of the North Sea where phosphorus could be the limiting nutrient (van der Zee & Chou 2004).

Table 1: The cumulated freshwater discharge of the Rhine and Elbe represent about 93 km3 y-1 or 31% of the total freshwater discharge to the North Sea (300 km3 y-1, OSPARCOM 2000). Are the inputs from the other rivers accounted for in the simulations? Could the omission (?) of the other rivers explain the poor validation of the nitrate cycle for box 148 shown in Figure 2b?

Table 1: Is the difference of nitrate/phosphate loads reported for both rivers related to differences in fresh water discharge? differences of nitrate/phosphate concentrations? differences of both fresh water discharge and nitrate/phosphate concentrations?

Table 1: In the legend clarify what “Netpp North Sea incl. excretion” and “Net uptake North Sea” stand for.

Table 1: specify the surface area (km2) of the domain to which these numbers apply

Table 2: specify the surface area (km2) of each of the domains to which these numbers apply

Table 2: add DOC fluxes to table.

Table 3: “pelagic excretion and respiration” terms should be separated in the table. Summing both processes is awkward, especially in the context of trophic state. Ideally, pelagic respiration should be further separated into autotrophic and heterotrophic components.

Table 3: numbers for “POC” and “total” columns are the same. Rectify and add DOC fluxes to table. Rectify legend of table. A comparison of DOC fluxes with those reported by Thomas et al. (2004) would be interesting (comparison of orders of magnitude, since different years).

Table 3: “TOC”, “TN” and “TP” would be more clear than “total” for each of the columns.

Table 3: specify the surface area (km2) of each of the domains to which these numbers apply

Figure 1: “OSPAR” and not “QSPAR”

Figure 2 (a): Numbers of each box are very difficult (almost impossible) to read.

Figure 2 (b): Numbers of each box are very difficult (almost impossible) to read.

Figure 2 (b): Graphs are very difficult (almost impossible) to read.

Figure 2 (b): “nitrate validation” and not “nitrat validation”

Figure 5: In legend specify what deltaDIN and deltaTON stand for.

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