

***Interactive comment on* “The carbon budget of the North Sea” by H. Thomas et al.**

Anonymous Referee #2

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Referee #2 Review 2

I was hoping some response from the authors before passing onto the next stage of review, but presumably the authors plan to address the comments at a later stage. The following complies with the format expected by the editor.

General comments

The authors produce a carbon budget for the North Sea and from it draw the following conclusions.

1) the gross carbon budget is dominated by the exchange with the N. Atlantic 2) the net carbon budget is dominated by the carbon inputs from the Baltic and the atmosphere 3) the N. Sea is a sink for organic carbon 4) that the imbalance in their budget terms ($0.73 \cdot 10^{12}$ mol C/a; i.e. 0.58% of the total budget) is attributable to a non-included term - air-sea exchange, which as equal to $1.3 \text{ mol CO}_2/\text{m}^2 \text{ a}$ is in agreement to an independent assessment. 5) that the N. Sea is heterotrophic

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Specific Comments on Conclusions

1) I have no problems with this - but considering the geography of the N. Sea; it doesn't seem especially surprising to me.

2) As expressed it is clearly wrong, as the net carbon budget is a sum of all the terms, so the dominant terms in the net budget and the same as those in the gross budget. I think they are not saying what they want to say, which is that the net imbalance (0.73×10^{12} mol C/a) is less than the input from rivers (0.866×10^{12} mol C/a) and Baltic Sea (1.1×10^{12} mol C/a) and the atmosphere (which is equal to the imbalance). The first two terms are respectively 0.7 and 0.88% of the overall budget. The issue here is the statistical uncertainty of the estimate - if it is significantly less than these terms than the statement stands, if not then the statement cannot be sustained. This was the issue I raised in my first review and we must await the authors' response on this matter.

3) Fundamentally this conclusion is dependent upon the DOC+POC values and how they have been derived. At the present the DOC+POC concentrations entering are: 60mmol/m³ (English Channel) and 76.7 (N Atlantic) and leaving are 56 and 67. As the N. Atlantic flows dominate, then the figure of 76.7 (versus 56 & 67) is critical and the conclusion rests mainly on its (their) assessment - we are given no insight into the detail of how they have been derived, how the temporal and spatial heterogeneities have been accommodated and what uncertainties exist, or so far have any access to the data from which they were derived. Again these issues were raised in my earlier comment and again we must await the authors' response on these matters.

4) The observed imbalance 0.58% is small, although no error assessment has been made, which to me is the crucial issue. If the authors can present evidence that it is statistically sound then what they have achieved is technically quite remarkable. If not then they will need to be more circumspect about their findings and their implications, again we must await the authors' response.

5) There have been two comments already over the meaning of heterotrophy, but until we can quantify the uncertainties in 3) I see no point engaging in this debate.

Interactive comment on Biogeosciences Discussions, 1, 367, 2004.

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1, S233–S235, 2004

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