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Ocean Science Discussions

Interactive comment on "Sensitivity Analysis of an Ocean Carbon Cycle Model in the North Atlantic: an investigation of parameters affecting the air-sea CO_2 flux, primary production and export of detritus" *by* V. Scott et al.

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Response to review: Sensitivity analysis of an Ocean Carbon Cycle Model in the North Atlantic: an investigation of parameters affecting the air-sea CO2 flux, primary production and export of detritus.

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Response to reviewer 1 (S. Elliot) comments:

The authors thank the reviewer for a thoughtful set of comments, which include com-

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ments on the context and need for a range of tools for sensitivity analysis, as presented in our paper. The suggestions made by the reviewer with regards to further work are major additions to the analysis and cannot be addressed in revising this paper. However, we will include a comment on the position of the CIS in the marginal ice zone in the revised manuscript.

We will now address the reviewer's specific comments page by page:

p. 1979 line 9 – too much emphasis on 'carbon reaches the ocean bed': We agree with this comment and will rephrase to clarify – 'Sinking organic waste is largely remineralised in the water column with a tiny fraction (typically < 1%) reaching the ocean bed where it is gradually broken down, . . .'

p. 1980 – inclusion of horizontal transport. Text to be added to end of paragraph (line 5) to include model forcing and horizontal transport with a link to Appendix: 'The 1-D HadOCC-GOTM column is forced with meteorological data and uses a relaxation to a nutrient profile below the productive depth to represent horizontal transport of nutrient into the column - see Appendix B for details.'

p. 1981 – CIS recycling of particulates. A good point but unfortunately no longer feasible. The aim was to compare like with like results over an annual cycle between the sites and the deep mixing at the CIS precluded this.

p. 1982 – Chorophyll output —- yes, with a well-designed observation operator to translate the model to the observations, comparison with in situ or satellite chlorophyll could in principle be informative. In Kettle and Merchant 2008 a slightly different version of the model is compared with observations at the sites. We are unable to pursue this in the course of revising this paper.

p.1983 – Latin hypercube: An appendix (referred to in the text) explaining the Latin hypercube will be added. This will include a description of the maximin method – this maximises the minimum distance between any two points in the hyperspace to

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additionally reduce potential for 'clumping' of datapoints.

p. 1985 – 'rare outcomes'. Our intention with this section is to highlight the overall trends shown by the OAT analysis, and to show that these trends are easily understood and as expected from the model design. We will revisit this discussion to draw further insight. If the reviewer wishes to raise specific questions to be addressed, we will welcome this.

p. 1989 – apologies – revised paper will be spell-checked!

p.1990 – 1991 – We are encouraged that the reviewer shares our conclusion with regards to the limitations of NPZD type models in predicting ecosystem behaviour in a changing environment.

p. 1993 Equations order: This structure was used to match the description of GOTM-HadOCC contained in Kettle and Merchant 2008 (see refs) for consistency, but can be re-ordered as suggested.

p. 2008 Remineralisation (Rm) units. These are correct as shown with shallow in dˆ-1 and deep in mdˆ-1. This structure enables the deep remineralisation rate to decrease with depth as bacterial communities become less abundant with increasing depth – we will add a footnote to the table to explain this.

p. 2011 Certain parameter combinations produce virtually no PP – we will clarify this in the text and will amend the values to show small values.

Response to reviewer 2 (anonymous) comments:

General comment - Comparison to observations: work comparing (tuning) parameter values to observations building on our results would certainly be relevant. However, that is a separate and rather open-ended piece of work that we cannot address in revising this paper. The model focussed work presented in this paper has given useful insight that is worth sharing with the community into parameter sensitivity in an ocean ecosystem model.

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Abstract: We will redraft the abstract to contain less detail on the models used and put greater emphasis on the results.

P. 1979, Line 7. Inefficient grazing: by 'inefficient grazing' we are referring to the fact that zooplankton do not consume all the phytoplankton they eat, directly contributing to detritus before digestion. This is the messy feeding fraction represented by Fmessy. The text will be rewritten to clarify this.

Line 14 (assume referring to abstract?). Reasonable ranges: range of values that are both physically consistent with the model equations and with parameter values used in comparable models within the community. This process is explained p. 1982 Line 23 onwards.

Line 16 (abstract again?). Here we are reporting a not unsurprising result of this work – that changing parameter values has a greater effect in more productive regions. As above, this work seeks to explore the structure and behaviour of the model.

P. 1980 ESTOC is reported in the literature as exhibiting oligotrophic characteristics – e.g. Davenport et al 2002 - Primary productivity in the northern Canary Islands region as inferred from SeaWiFS imagery. doi:10.1016/S0967-0645(02)00095-4 – will reword to reflect an appropriate caveat to this

P. 1981: Inclusion of CIS: all three processes are indeed in action at the CIS, however, the action of a deep MLD in returning sinking material precludes the inclusion of net annual deep export.

Subjective terms: Here (P.1981), as an overview of sensitivity analysis is being given at this point, it is appropriate to use qualitative terms, as their quantitative meaning will be dependent on the specific application. Elsewhere, this point is fair. Figures are provided that give clarity in this respect, but we will review the text and add quantitative statements where appropriate.

P.1988: 'relative abundance' simply refers that compared to the other sites the CIS is

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better supplied with sunlight and nutrient. This will be reworded to clarify.

Line 10-16: this paragraph refers specifically to the sensitivity results for CO2 flux (Fig 4) which show that the value of Vs has a dominating effect at the PAP. Certainly sinking also occurs at the other sites but other parameters are seen to be more influential than Vs at the other sites.

P. 1989: Discussion 'as expected': the results did confirm qualitative expectations, and it remains useful to report this with the quantitative outcomes.

P. 1990: 'parameters generic to NPZD': A helpful comment – we will revisit this section.

P. 1991: We make the point that NPZD models have limited flexibility and so are not as well suited to correctly reacting to a changing environment. Reviewer 1 agreed with this view, and it is an arguable point. We refer to Le Quéré 2006 (see refs).

P. 1992: While we are focused on model analysis we agree that this section will benefit from rewording to reflect the comments made for p. 1990 above.

P. 1993. HP and HD are explained in text – line 10 'If h is the grazing rate per unit food concentration then the losses to phytoplankton and detritus are HP =hP and HD =hD respectively'

Viral mortality: this is the structure of the model – all non-predatory phytoplankton mortality is assumed to result from viral infection so that phytoplankton mortality increases with population density.

P. 1995: This Appendix addresses horizontal replenishment of nutrient to the 1D column. The meaning of productive depth is defined in this appendix.

P. 1996: Heat flux is that used in standard GOTM package – please refer to Burchard et al 1999 (see refs). We will add a statement to this effect in the model description.

P. 2011: Values determined as described in text – the aim of the work is to explore the possible parameter space of the model.

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Minor comments: P. 1978 Line 17: We feel it is important to mention the SA techniques used in the abstract – this sentence has been moved to the end of line 13 so that the abstract is clearer.

References: We will correct multiple references so that they appear in chronological order in the text. The additional incorrect 'year' at end of each reference is possibly an artefact of OS latex publishing. We will make sure that this is corrected.

Typos, and noted corrections of abbreviations will be corrected. P. 1994, Line 10 'Detritus' describes the equation below.

Tables 1 and 2: the authors previously attempted to include references using footnotes/in the caption but the result was messy and less clear than using separate tables.

Figures 1-3: removal of axes creates considerable misalignment issues in compiling that prevents easy comparison of plots. We do not feel that their inclusion detracts from the clarity of the plots.

Table 3: The units and conversions have been re-checked – no errors found. In terms of the literature sources comparison of the values with the information presented in Tables 1 and 2 shows this.

The authors thank the reviewers for their effort in review and the comments made.

Interactive comment on Ocean Sci. Discuss., 7, 1977, 2010.