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Interactive comment on “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” by V. Scott et al.

Anonymous Referee #2

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The paper by Scott and co-workers presents results of a sensitivity analysis performed with a simple NPZD model. The authors want to find out which parameters are most important for air sea CO₂ flux, primary production and export of detritus. The sensitivity analysis is shown to be a powerful tool to explore the influence of a certain parameterization onto the model results. However, I'm missing the model results compared to real observations. For me, the quality of a model is manifested best by such a comparison. Therefore I feel that the current study would gain substantially on weight, by presenting the results of the model for each site by putting the emphasis on the most important pa-

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rameters & values based on the sensitivity study and comparing them to observations. Additionally the paper is weakened by the very subjective wording when presenting the results, giving the reader little possibility to make an objective judgment. My specific comments and some minor point (both still should be included into the revised version) are listed below.

Specific comments: Abstract: I found reading the abstract quite difficult and somewhat disappointing, especially because of the numerous details about the used model and in comparison little information provided about the results of the study. I would like to encourage the authors to provide more details on the results which will be given, and move the model details to the methods. This would additionally avoid the repetitions. Page 1979, Line 7: Could you please clarify the meaning of inefficient grazing as in some regions of the world ocean grazing by zooplankton is able to keep the primary producers at low levels. So it is not what you probably mean here? Line 14: How do you define reasonable changes? Line 16: I believe that this statement should be widened and include also observations and experimental results, in the end we want to understand the real ocean, and models are one tool to do that Page 1980: I would be careful of assigning the ESTOC station to the oligotrophic waters, there are coastal effects and upwelling reported from the region, which might lead to other characteristics in the water column. I would at least ask the authors to mention it, or discuss it in the appropriate place in the MS. Page 1981: Line 1-3: I have problems following the author's arguments about the MLD at CIS; all three parameters (processes) are in action at CIS as well. I strongly suggest including CIS and elaborating on the role of the deep MLD in the discussion of the results. Line 8: not having significant effect; could you please in the method part explain briefly how you define relevant, significant, reasonable etc. Those terms are highly subjective; a clear statement would make it clearer to the reader and allow independent judgment Page 1982: Line 28 Following consultation with experts. . . Page 1985, Lines 10-14: see above. The authors tend to describe the effects of the parameters as huge, slight and negligible-those terms are really highly subjective. So I strongly again suggest to give some definition for it in

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terms of results change maybe in %, or a range, otherwise the reader has no chance to develop a objective judgment for the results Page 1988, Line 6: what is meant by relative abundance for light and nutrients? Both parameters can be exactly measured, ok, they vary throughout the year, but for each time you can provide a number. Please be more precise. Line 10-16: Why should v_s be only important at PAP? How do you transport particle downwards if v_s is not important. Considerable particle fluxes are reported from all those sites. Page 1989: The first 22 lines of the discussion part are not really new; I'm a bit frustrated to read that the sensitivity study did not provide really new information-all this we new and expected based on our observations. This is also true for the first 8 lines on Page 1990. Page 1990, Line 9: All generic to many NPZD type. . .: Most of the parameters used in models is coming from theoretical considerations and even more of them from experimental work and observations. Therefore I think you should argue that those parameters under certain condition represent the true ocean, and not that those are generic to many other models. The last does not necessary assure that the model results mirror the reality. Page 1991, line 5-to end: I cannot see really the point of all this. I would like to read here rather some statement if any, how NPZD models can be used to study the future and the effects of possible changes in the ocean ecosystem or forcing. Page 1992, Line 5-6: Again, I found it somewhat disturbing, that the authors do not think about how generic those values are if compared to observations and only argue here with models. Page 1993 h_p , h , P are used but not explained; why do you only consider virual mortality, what about natural mortality? Page 1995: Are you really arguing that upwelling and estuarine influences are important at PAP, CIS and ESTOC. Could you provide some substantial arguments/references for it. Line 18: What is productive depth?, I'm not aware of such term, please be more precise and use the right terminology. Page 1996: Which formulae did you use to calculate the heat fluxes? Page 2011: Are all those values realistic? Could you compare those to observations? There is only limited if any discussion of the values in comparison to observations, and again from my point of view this is the only way to show that the model with a chosen parameterization is showing realistic results.

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Minor comments: Page 1978, Line 17: Remove and between perturbations and using a statistical emulator In the entire MS the references should be given in chronological order and given in a consistent way Use NPZD in a consistent way throughout the MS Page 1981, Line 25: Ocean Page 1983, Line 4: what is Rm_deep, used but not defined; where is 138.9m coming from why not 138? Page 1985, Line 5: OAT is already defined so no need for one-at-the-time here Page 1986, Line 20: PAP site change from... Page 1987, Line 23: likely to be influenced Page 1988, Line 18: oligotrophic Line 25: at ESTOC Page 1994, Line 10: Detritus? Believe this should be removed References. At the end of each reference a number appears which is somehow not related to the publication, what that? Fig. 1 -3: remove y/x axis and easy. The sentence starting with "The effect..." is not necessary here, at it is repetition from text Fig. 6: add the results for CIS; I do not agree that this cannot be calculated for CIS Table 1 & 2: combine both. For example you could provide the references from T2 in the caption of T1; additionally see the footnote 26: please check the units Table 3: literature, which one, this is crucial information and should be provided

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

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