**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.

S. Elliott (Referee)

sme@lanl.gov

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The Ocean Science Discussion paper "Sensitivity Analysis of an Ocean Carbon Cycle Model. . ." by Scott et al. represents an unusually detailed exploration of the basin scale, marine ecodynamic parameter space. The challenge of sampling is met through a combination of site-specific one dimensional transport with interpolation across the hyperspace. In part, the results are reassuring because many of the examples presented are invariant. Others and in particular the export production field vary widely. My suspicion is that a next step for this group should be to cull the hypervolume based

on community understanding of global ecological reality. But analyses such as this one are certainly useful to begin, and they may be thought provoking into the bargain. Although the NPZD framework applied is simpler than those in many other contemporary carbon cycle simulators, the authors argue the merits of its speed. And clearly they have demonstrated amply that there are distinct advantages in the assessment of sensitivities for complex biotic systems. The Scott et al. paper should be published in something close to its original form, but with a number of small errors corrected as listed in the detail section toward the bottom of this response.

The work under consideration is organized as a set of research site comparisons, and one of them is located in the Irminger Sea. Much of the eastern coast of Greenland in fact lies within the seasonal or marginal ice zone. The methods put forward by Scott et al. are sufficiently intriguing that I respectfully suggest they could be extended to a timely new subdiscipline –study of the carbon cycle as it is manifested in and around sea ice. For the moment, most global ocean biogeochemistry simulations ignore the role played by ice algae. But the Earth System research community is on the verge of wrestling with these issues in earnest. Sensitivity testing will need to be as sophisticated as possible, and simultaneously the inclusion of ice-bound ecosystems will complicate exploration of the total parameter space. Coupling of OAT, hypercube and emulator methods as described by the authors may be just the ticket.

In the northern hemisphere biological activity in sea ice is confined mainly to the lowest few centimeters of the pack, a porous medium known as the "bottom" or "skeletal" layer (Arrigo, 2003). To a set of first approximations, production begins in the spring as light limitation is lifted rapidly by progression of the seasons. The algae functioning best in the bottom layer are typically pennate diatoms, and they bloom rapidly where nutrient distributions permit. Regions of ice algal dominance tend to ring the Arctic but are concentrated at the Pacific and Atlantic inflow regions or else within mixing range of the continental shelf. In many cases a population explosion ensues within the porous zone, leading rapidly to solute flux limitation. Physics of the below-pack fluid boundary

dictate that a transfer velocity for resource entry is about 0.1 m/d (Lavoie et al. 2005). The limiting nutrient, whether silicate or nitrate, is typically present above the value 1 millimole/m3. Sometimes the levels are an order of magnitude higher. The reader can thus compute readily that primary production in the ice column may be 1 millimole/m2d in a selected nutrient currency, translating to about 100 mg C/m2d. A typical Arctic ice bloom lasts more than one month and is only terminated by the onset of the melt (Deal et al. 2010). Carbon fixation may thus be greater than several g on an annual basis, and since the diatoms are heavy and sink fast much of this is exportable. The reader may contrast my quick estimate with the Scott et al. figures, or in fact the available general maps of open water primary and export production. The ice system is likely to be competitive both temporally and regionally, provided it can obtain the necessary nutrient inputs.

In fact such computations can be integrated over the entire Arctic and indicate that while ice algae are relatively small contributors to the global marine carbon cycle, they will be critical in the high latitude regime (Legendre et al. 1992). Beneath the pack itself, they are likely to be the whole ball game. Add to this a series of fascinating geochemical nuances including (Elliott et al. 2010): 1.) exotic new carbonate minerals form within the ice column off the coast of Greenland in winter, altering DIC and alkalinity in brine channels and leading to a seasonal in-out pumping of CO2 relative to the ice, 2.) the extremely rich epontic systems appear to leak a variety of organic aerosol precursors into leads, creating microlayers that can influence cloud structures once the molecules are lofted by the wind, 3.) chlorophyll implied in the ice can absorb and redistribute energy thus promoting the melt and positive climate change feedbacks, plus finally 3.) with inevitable overall thinning of the average Arctic pack, snow fall may lead to widespread depression and surface flooding. Algal systems may thus shift toward upper levels of the solid matrix, becoming Southern-Hemisphere like (Arrigo, 2003). Their composition may no longer be dominantly diatomaceous. In areas where coverage transitions from permanent to seasonal status, there may be gross trophic structure shifts in the biota (Bluhm and Gradinger, 2008). The pennates melting from

bottom layers have always tended to sink rapidly after ablation. Detritus associated with them thus remineralizes into the benthos. Ecologists believe that in a global warming world, replacement ecosystems will be driven by small, open water organisms recycling primarily at the surface. This provides support exclusively to fish/sea bird food webs.

Atlantic carbon cycle simulations including only NPZD ecodynamic frames and only open water geochemistry cannot capture these subtleties. In the era of rapid Arctic climate change, many of them will prove to be critical. Scott et al. acknowledge this indirectly in their conclusions. I have merely taken the opportunity here to make the matter explicit.

Aside from the potential omission of ice ecosystems, the paper "Sensitivity Analysis of an Ocean Carbon Cycle Model..." is in relatively good shape. Nonetheless I succeeded in identifying several grammatical and typographical problems per page. The authors would no doubt catch many of these themselves during revision, but I will work with OSD editors to be sure they have access to my mark ups. To conclude this response, I will now present a summary of major specific comments.

p. 1978 –The authors describe in the abstract and throughout the text an exploratory, low dimensionality framework for sensitivity testing of climate biogeochemistry. This is what I call in my own research a "Toy System Model". They have reduced the gross physical detail of a large swath of the marine system in order to focus on uncertainties. This approach is at the cutting edge. I am attempting to apply it myself to the design of high latitude ecodynamics simulations, perhaps with less success than Scott et al. demonstrate here.

p. 1979 –The introduction places too much emphasis on organics reaching the sea floor, as opposed to remineralization within the column. The problem is embodied in the comment "...reaches the ocean bed". This can easily be remedied.

p. 1980 –The best description of the authors' treatment of horizontal transport is hidden

in the appendices. But it is perfectly reasonable, and in fact clever at some levels. Please give this more prominence

p. 1981 –No doubt too late now, but the CIS recycling of particulates could readily have been simulated. This would have permitted a relative completion of all the analyses/plots.

p. 1982 –It would have been instructive to include chlorophyll output in the analysis. Whether these were assessed with respect to in situ or satellite data, it would have added another dimension to the paper. Constraints might possibly be placed on the carbon to chlorophyll ratio issue.

p. 1983 –I'm sure many readers would be interested in a quick summary of the "Latin Hypercube" approach. This looks like a fascinating way to cut through a parameter space.

p. 1985 –In several cases in the "Results" section, rare outcomes are cited as interesting but then there is no follow up. The outliers are simply left unexplained. Analysis of at least some would enhance the interest, readability and utility of the paper. This comment extends to the "Discussion" as well.

p. 1989 –Beginning at the top of the "Discussion" portion, the writing is very clear but it is riddled with small spelling errors. It is almost as though the authors forgot to proof the last few paragraphs of the paper. Please run a spell checker in these areas.

p. 1990 –Also in the "Discussion", there is a reliance on what I call "GO TO" statements, which some readers find distracting. Examples which might be edited include instances of "see sections X.X" and "As has been demonstrated in section Y.Y". This is a highly optional suggestion –it is mainly a matter of personal taste.

p. 1990 –Ultimately the authors do a good job of drawing what I view to be the most appropriate and important conclusion from their results. The NPZD approach will be a fast systems modeling starter and useful to a point, but it must ultimately give way to

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the incorporation of multiple phytoplanktonic functional types. And to multiple bacterial classes which will be critical to many geocycles, and to chemical resolution of dissolved organics so that surface microlayers and the influence on aerosols/condensation nuclei can be discerned, and more. Biotic/ecodynamic (biogeochemical) details of this sort can only be parameterized to the extent that they are later tested through detailed systems simulation. This is a superset argument relative to my most visible comments, regarding the ice algae.

p. 1991 –It would be a simple matter to functionalize behavior of the CaCO3 secretors to account for changes in pH. But my feeling is that gross changes in ecosystem structure will be the more important story, stemming from shifts in North Atlantic circulation and ice coverage.

p. 1993 –Why do the equations begin with those regarding zooplankton? They are the consumers or secondary producers. More usually a presentation of this type opens with a definition of phytoplankton terms. Also, the authors should be aware that in several cases the text lead-ins for equations are nongrammatical. For example although A1 is prefaced as "where h= f(B,Z,F,g, etc.)" A2 reads very abruptly "Phytoplankton P time partial =". This is a technicality and aesthetic point really, but OSD will be better served by a careful writing of the appendices as well as the text.

p. 2008 –Units for the parameters Rm (shallow and deep) are not consistent with one another. Probably both should be expressed as per time. Please check this.

p. 2011 –If the minimum Pmax is finite, how can Table 4 primary production values go to zero? The reader is confused here.

References

Arrigo, K.: Primary production, in Sea Ice –An Introduction to its Physics, Chemistry, Biology and Geology, edited by D.N. Thomas and G.S. Dieckmann, Chapter 5, Blackwell, London, 2003.

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Bluhm, B. and Gradinger, R.: Regional variability in food availability for arctic marine mammals, Ecol. Appl., 18(2) Supplement, S77-S96, 2008.

Deal, C., Elliott, S., Jin, M., Hunke, E., Maltrud, M., and Jeffery, N.: Large scale modeling of primary production and ice algal biomass within arctic sea ice, J. Geophys. Res., doi:10./1029/ 2010JC006409, 2010.

Elliott, S., Hunke, E., Jeffery, N., Turner, A., Maltrud, M., Deal, C. and Jin, M.: Systems level simulation of sea ice-atmosphere biogeochemistry connections, SOLAS, 11, 14-17, 2010.

Lavoie, D., Denman, K., and Michel, C.: Modeling ice algal growth and decline in a seasonally ice-covered region of the Arctic, J. Geophys. Res., 110, doi:10.1029/ 2005JC002922, 2005.

Legendre, L. and 10 others: Ecology of sea ice biota: Part 2. Global significance, Polar Biol. 12, 429-444, 1992.

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/7/C685/2011/osd-7-C685-2011-supplement.pdf

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