**Geoscientific**
**Model Development**
Discussions

# *Interactive comment on* "PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies" *by* O. Aumont et al.

**Anonymous Referee #2**

Received and published: 29 May 2015

This manuscript summarizes recent development efforts and the current state of the a top global-scale ocean biogeochemical model. The paper is generally well-written and the results clearly presented. It is long, but I think this type of manuscript should be long to provide enough details to clearly illustrate the assumptions and controlling processes built into the model. I strongly support publication after some minor modifications and clarifications, discussed below.

In many places the authors describe an assumption using the word "supposed". This sounds very awkward (at least to American ears), I'd suggest replacing with , "It is assumed that . . ..".

Abstract Replace silicium with silicon

Page 3, suggest explicitly noting that balanced growth, or now luxury uptake for these nutrients

Page 5 top, discussion of where PISCES is currently embedded and available, Is this PISCES-V1 or –V2?

Page 5 bottom, note that is an overview of major changes, with more details in the following sections.

Page 6 define IOM

Page 13, middle, says nanophytoplankton, should say "diatom" in line ~13

Section 4.14 – What is the resulting range in Fe/C ratios or quotas?

Section 4.15. – what is the resulting range in si/C or Si quotas?

For both of these are the general spatial patterns consistent with observations?

Section 4.3 – are the DOM pools kept at the same quasi –Redfield ratio 122/16/1?

page 47 several typos in the references listed

page 48 seems somewhat problematic that a key limiting nutrient like Fe is not conserved with regards to sea ice. . . In the standard simulation, is sea ice a net source or sink of dissolved iron? And what is the size of the net source or sink?

Page 52 – could the authors also give the export flux at 100m depth, which is often reported.

Page 55 – To be clear, there is an arbitrary iron source that kicks in when simulated iron falls below

0.01nM, (is this right or is it actually 0.1 nM?). What is the size of this iron source on an annual basis?

An additional sensitivity simulation showing the effects of removing this threshold would

be a nice addition to the paper.

page 63, top, "charlot"?

page 67, I note that the N2O cycling is not described in the primary literature, why not add brief description to this manuscript?

Page 67, line 29, please revise this sentence.

I like the overview of actual code modules provided in the appendix.

It there is one global metric I'd suggest adding to the paper it would be something on OMZ volume and distribution.

How is the routing of Fe handled when the prey Fe/C is lower than the constant zooplankton Fe/C?

Figures

In general, the text/labeling within figures is too small.

Figure 10, suggest adding at least one more low end contour (i.e. 1.0)

Figure 11. definitely needs more resolution/contours in the color scale on the low end, almost no structure is visible in the surface plot

After writing the above, but before posting my comments I examined the comments from reviewer #1. I agree with many of the points made by reviewer #1, including more information how did the sensitivity experiments impact key metrics, such as statistical match to satellite chlorophyll. I do think that some of the additional justification and information on the impacts of the various parameterizations in the model is asking too much. To address each of the topics/parameterizations listed in the review would greatly expand the length of an already long paper, and would also require a lot of computer simulation to individually turn on/off each process/parameterization. I agree that some additional information on the effects of some of these would be beneficial,

but maybe as hinted at by reviewer #1, in some cases just a sentence on the qualitative impacts needs to be added.