Biogeosciences Discuss., 4, S491–S495, 2007
www.biogeosciences-discuss.net/4/S491/2007/

Biogeosciences
Discussions

BGD

4, S491–S495, 2007

Interactive
Comment

Interactive comment on “Iron profiles and speciation of the upper water column at the Bermuda Atlantic time-series Study site: a model based sensitivity study” *by* L. Weber et al.

Anonymous Referee #1

Received and published: 11 May 2007

This paper provides an exploration of an iron model coupled to a NPZD model in a 1-dimensional "BATS" setup. The iron model considers the cycling of iron between truly dissolved free forms, a complexed form, colloidal, particulate and biological forms. Since there is still much debate on how best to model iron chemistry in ocean models, this paper has some insight to offer the reader, however I do have some serious problems with the paper and potentially (unless reasonable justification by the authors) with the model study itself. I do think a version of the paper, possibly with new model runs will be worth publishing.

Although the authors do conclude with the statement that the strong sensitivity of the

EGU

model to parameter choices means that "we are far away from understanding the influence of iron in the marine ecosystem", I think that this is a significant problem with the paper and should be addressed far more.

One of the main conclusions I drew from the paper was that the "tuning" of the model parameters to BATS data was very dependent on the physics assumptions. Some parameters used in a 0-D model version of the model (Weber et al 2005) were completely wrong for the 1-D version. Which then begs the question of what happens when the model is tried in a 3-D model, and how then between eddy-resolving and non-eddy resolving (especially as much of the biogeochemsitry at BATS appears to be strongly influenced by the passage of eddies). Can some of the inferences made in this 1-D version - the parameter values chosen and their sensitivities - merely be a reflection of the missing physics?

In a very first attempt to address this dimensional-sensitivity: what are the results of this version of the iron/NPZD model with Run A parameters, but in the 0-D setup. Do the large parameter changes make a huge (and negative) changes to the 0-D results? It is instructive to see the difference in sensitivities shown in table 4. Possible more should be made of this and an expansion on how additional dimensions will affect these too.

A second mis-giving I have with the model configuration, is that possible iron limitation did not feed back to the NPZD model. The authors state that BATS is commonly thought not to be iron limited, which is why they do neglect iron limitation in phytoplankton nitrogen uptake, yet they do have iron limitation in the iron uptake by phytoplankton. More importantly, using the original 0-D parameter values they land up with very low iron (they found N:Fe ratio unacceptable)– would it be as low if the biological uptake was regulated by iron.

It is not entirely clear, either, what form of iron is bioavailable. Is it Fe(III)' or Fe(II)'? - figure 3 suggests Fe(III)', but equations from Weber at al 2005 show the bio uptake coming from the Fe (II)' Is justified that only free iron is bio-available? It seems that at

BGD

4, S491–S495, 2007

least some phytoplankton appear to take up FeL? Fig.2 indicates that while dFe might always be relatively high in the model, Fe (III)' and Fe(II)' are very low during parts of a 24 hour period, might the free form be limiting some parts of the day-light hours. In the appendix, iron uptake by phytoplankton is modified by iron limitation – why have this here, but not in the nitrogen uptake part of the model? Also - why use dFe and sFe in this mu_Fe? If only Fe(III)' is bio-available, why not use that? And why use dFe in numerator, but sFe in denominator? There needs to be further discussion and considerable clarification here. And some calculations that the assumptions (free vs ligand, no iron limitation) do not affect the results. Potentially even further runs if these do prove to be important (see specific comments).

Very little connection is made back to the NPZD model. It might be nice to know what order of magnitude the deficiencies in the NPZD model results make on the iron. How sensitive are the results to changes in the NPZD model parameters? Do these sensitivities swamp the iron parameter sensitivities? In which case this tuning the iron parameters is even more problematic.

Specific Comments: ——————

pg 825 line 3-12: several biogeochemical models (including Dutkiewicz et al 2005, Gregg et al, 2003 (DSR II, 50), Moore et al 2004 (GBC GB4028)) have included iron chemistry in their models - albeit with various simpler chemistry models - most use at least monthly dust forcing, and some even daily. Aumont et al 2006 (GBC, GB2017) has even looked at iron fertilization. I'm not sure it is entirely accurate to say that they cannot address episodic events. Certainly they do not capture diurnal variations in the way that Weber et al do. (As an aside though, it would be nice - but beyond the scope of this paper - to see how much of the iron chemistry of Weber et al needs to be kept to look at large scale long term processes: ie. how much are the above modelling studies missing?)

pg 828 line 20: "without feedback through changing export production": but this has to

BGD

4, S491–S495, 2007

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

be important to the iron cycling! If you tune model parameters to best match observations - and the observations include a biological uptake dependence - then you need to include this in your model.

pg 829 line 11-14: not obvious what you are referring to "oceanic type B"

pg 829 line 16-20: very badly written.

pg 830 section 4: since the iron cycling pays particular attention to the diurnal cycle between Fe(II), Fe(III) and FeL and colloidal forms, it would be good to know how well the NPZD model captures the diurnal cycling of the ecosystem. I assume that it is important to get the hourly uptake of iron biologically as well, if these results are to be believed. (And hence also my concern that if at some point in the day plankton are iron limited, not including this in the model could be problematic).

pg 830 section 4: are you using the same parameters in the NPZD model as you did in the 0-D case? Or has some additional tuning been necessary here as well?

pg 832 section 5: could you specify what you are assuming as dFe here?

pg 832 line 15-17: There could also be some significant impact of biological uptake that is, or is not taken into account?

pg 833 line 5: "loss losses"

pg 834 line 10: Do these values compare to observations? If your model results are a little off, how will it affect your decisions here?

pg 834 line 17: You could also change "R"...how sure are you about this value. Could you try change it too and see how sensitive results are to this?

pg 835 and 836: section 5.3: I'm not sure that either discussion is a "justification". Yes - these are extremely unconstrained parameters - but still explain why so different in the 0-D case (would be good to run the current version of NPZD/iron in the 0-D case, with these parameter choices, and see how sensitive those results are). Since the 1-D version is so sensitive – what is to say that 3-D won't change these values completely again...in which case what are you really saying about these values? Anything?

pg 837 section 6: would be nice to know how diurnal cycling of the biology impacts the results of this section as well. How important is biological uptake of the Fe(III)' in the cycles you show here? could you show living-organic iron as well?

pg 838 line 24: "depencedependencise"

pg 838 line 27: "stastablisation" - there are several similar misspellings further on through the text. Maybe a product of a file format conversion? It is in both the web view and the print version. See also 839, line 7, 840 line 10. 841 line17, 842 line 2,4

pg 839 line 12: "ar" and "simular"

pg 839 line 19-20: but 0.02 for Fe(III)' might be limiting!

pg 843 line 21: how specific to BATS is this conclusion?

pg 846 line 10: how does this affect the bio-availability of Fe?

pg 848 line 25: 'to" would be better "as"

pg 850 Section 9: How specific to the 1-d results are most of these conclusions?

pg 851 appendix: might be nice to include all equations in here... save the reader having to go back to Weber et al 2005.

Interactive comment on Biogeosciences Discuss., 4, 823, 2007.