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# *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 #2

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Review of Weber et al.

This paper represents a useful modelling exercise for iron particular, as the authors note, because it highlights the scale of our ignorance of some processes that appear from the model to be very important. Several of the processes identified, for example the suggestion of diurnal variations in dissolved, colloidal and particulate iron, are not even measurable at the moment. I believe the paper should be published but the authors need to be careful to emphasise that the model results are not real data (e.g. p840 line 11 and elsewhere).

Detailed comments P827 You may need to define colloids as you use the term. The

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assumption in the model of excess ligand, governs all subsequent results and in some ways represents the most intriguing issue in oceanic iron cycling. The model emphasises the potential importance of photoreduction reactions but it is not clear to me which rate is used for this process in which parts of modelling work and why. I am also unclear what if anything is assumed about the photochemistry of colloidal iron. The N component of the model. It has historically been difficult to produce a balanced N cycle at the BATS site (or elsewhere). I note both Jenkins and Williams have separately suggested that lateral N transport is important, it is not clear to me if this is also the case for Fe. I note also the N2 fixation is ignored here although it may well be important to the N budget and also be limited by Fe supply. P829 line 21 is sinking organic matter only regenerated in the lowest box, as implied,? that seems unlikely, there is regeneration at all depths. P830 DIN at BATS in summer is $\ll$1uMN, I'm not sure if this is important but getting N $<$1uM is not a particularly rigorous test. This may also be true of the chlorophyll data on p831. P831 Excluding DOC and its sinking will underestimate export I think, which is not what is implied here. P832 Given the model has several classes of Fe, the authors need to be very clear about what they mean when they say "modelled iron concentrations" (line 2) is this dFE or dFe+colloidal etc. and elsewhere. P833 and later. Residence times as calculated from field data usually refer to residence times with respect to the overall loss from the euphotic zone. Again the authors need to be specific about the residence time they calculate with respect to what processes (p830&840). The model results can also be compared to estimates of iron residence times provided the model results are averaged over an appropriately long enough time scale. P833 line 24 "dissociation" of what? I found other parts of this section rather confusing. P834 line 11 I suspect its surface area not number of particles that matter. P839 line 17 is it 0.6nM? P840 Residence times of 30,000 days seem unreasonably long to me (depending on the definition used as noted earlier) I suspect the particle concentration field may be the problem and I realise I don't know how this is generated in the model. Certainly in summer dust deposition increases particle concentrations but the effect is modest. Deuser and coworkers reported data

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on the particlulate matter at this site which can be used to test this (Mar Chem 29, 203 1990) .

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