

Interactive comment on “Manganese in the world ocean: a first global model” by Marco van Hulten et al.

Anonymous Referee #3

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The manuscript by Hulten et al. presents the first global model of oceanic manganese cycling and compares the model output with two high-quality sections of dissolved manganese concentrations in the Atlantic and Southern Ocean. Since manganese is a biologically important element and can be limiting growth in some regions of the ocean, this is an important step forward and certainly of interest to the readers of Biogeosciences and should be published.

In constructing their model, the authors follow the principle that models should start as simple as possible, and include complex processes — even if it is known that they occur — only if the observational data directly shows evidence for these processes. This approach of starting simple and increasing complexity only after thorough analysis of model deficiencies is laudable. But it also means that this work is likely to be just a first step, and that more complex models will follow.

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The main victim of this approach of starting simple is that the model disregards the biological cycling of manganese, although it is known that manganese availability is necessary for oxygenic photosynthesis, and can be (co-)limiting to phytoplankton growth at times (Brand et al. 1983, Bruland et al. 1991). The authors argue that in the data they use for validating the model, there is no strong signal of uptake and remineralisation of Mn.

I must admit that I am sceptical about this assumption:

- firstly, is the argument to neglect of biological Mn cycling maybe biased by using primarily Atlantic data? The secondary maximum of dissolved Mn in the North Pacific, shown in Bruland et al. 1991 seems hard to explain without remineralisation of Mn in biological material, and — in addition — a slower Mn(II) reduction in the oxygen minimum. This at least is what I have gotten out of the 1-d modelling study by Johnson et al. 1996.
- secondly, how consistent is the neglect of biological cycling with the magnitude of other vertical fluxes of Mn in the model? At least an estimate for the magnitude of the vertical export of Mn through biology could be obtained by multiplying Mn:P ratios in phytoplankton (Sunda and Huntsman, 1998, Twining 2013) with common estimates of global export production. The vertical sinking flux of particulate Mn (only the precipitated form, not the non-dissolved lithogenic part) from the model could be compared to that number.

There are other parameterisations in this model that are less ad-hoc than they appear at first sight: The authors use a constant rate for Mn(II) oxidation and a reduction rate for Mn(IV) that switches abruptly from a high value in the euphotic zone to a low value below. Although this is qualitatively justified by the existence of photoreduction, one wonders where the values come from, until one finds in the discussion that this simple parameterization was chosen to reproduce the particulate/dissolved Mn ratio at the

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Vertex site. I think figure 11 and the corresponding explanation should be shifted to the model description. Instead, in the discussion, I think the authors could perhaps discuss what they think is the driving process for this reduction and whether it is likely that the Mn(IV)/Mn(II) ratio in the deep ocean is the same in different ocean basins.

My last criticism concerns the parameterisation of the sinking removal of oxidised Mn; contrary to the spirit of starting a model with simple assumptions, I find this parameterisation not simple at all. At the surface and at concentrations below 25 pM, Mn particles are assumed to sink at 1 m/d. Has this value been estimated from some assumption on the size of the particles, using Stokes' law, or is it just an arbitrary choice? Most of the initially formed particles probably are so small they don't sink at all on their own and need aggregation with other organic particles to sink, but most aggregates sink quite a bit faster. The model assumes that above the threshold of 25 pM, Mn particles aggregate and sink faster, but the increasing sinking rate above the threshold is made dependent on water depth, so that it does not affect the removal of Mn near the surface. Is there any justification for the depth dependency of the sinking rate? Several biogeochemical models use a vertically increasing sinking rate for their detritus compartment, as an implicit way to account for aggregation, see e.g. Kriest and Oschlies 2010, but the absolute sinking rates for aggregates in these models are much higher than assumed here. I found the choices here quite ad-hoc and wondered how strongly the model results are affected by them.

Summarizing, I think that the authors need to discuss the processes that lead to the vertical transport and ultimately removal of Mn in their model more, before the paper can be published. They have shown with their sensitivity runs that aggregation seems to be important for the removal of Mn from hydrothermal plumes, and that a lower threshold for this aggregation seems to be important for explaining the relatively uniform Mn background in the deep ocean. But if indeed aggregation is the main vector of vertical transport, then one might suspect that near the surface, biological export production plays a large role in determining the vertical Mn flux, either directly from in-

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corporated Mn, or indirectly from driving aggregation. That would lead to a much less uniform vertical transport velocity and it would be good to have an idea how much that could influence model results.

Specific comments:

page 3, line 3-4: My suspicion is that the good correlation between lithogenic particles and Mn may be caused by Mn inside lithogenic material, not so much by scavenged Mn. I would therefore be cautious to cite that as an evidence for that 'lithogenic particles are likely to play a significant role in the removal .. on Mn'.

Equation 3 for the Middelburg et al. empirical formula for denitrification is incomplete, as the units of neither the denitrification rate nor the water depth are given. Probably it is metre for the latter, but for the first?

Minor comments:

p.4, line 17: Is it on purpose that the concentration of O₂ is denoted by square brackets, but that of the OH⁻ ion by curly brackets?

In my printout of the pdf file several of the equations appear mangled, with mathematical symbols replaced by small blocks, vertical lines replaced by some dots, the vertical bracket in eq. 20 missing etc. I think this is the consequence of using the a formula editor, which relies on software-specific fonts. The formulae should be checked out not only on a computer screen, but also in a printout.

Also, in my printout of Figure 2, all arrows are replaced with strange symbols, the ligated letters ff in 'sediment diffusion' are lost etc. I think this means that the pdf relies on the presence of some fonts that are present some type of computers, but not elsewhere. The ligated 'fl' in flux is also lost in the captions of the subfigures in figure 3.

It is very convenient that the caption to Figure 4 marks the cruise names in the same colours as the dots within the figure, but is this feature supported in the final journal

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form?

Several of the citations are incomplete, e.g. Charette et al.

References not present in the original manuscript:

Kriest, I., Oeschies, A. (2011). Numerical effects on organic-matter sedimentation and remineralization in biogeochemical ocean models. *Ocean Modelling*, 39(3-4), 275–283. doi:10.1016/j.ocemod.2011.05.001

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