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

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Response to Anonymous Referee #2

We wish to thank the reviewer for the analysis and suggestions on our manuscript.

> While I am very supportive of the objectives of this work in general, the scope of the discussion, the wording and overall presentation of this manuscript gives the impression that the model is not sufficiently mature: at this stage of development, this paper reads more like a progress report than a fine-tuned, well-thought-out, scientific manuscript. The story in this manuscript announces a great start for Mn modelling, but comes across a bit short. Specifically, excluding the biological aspect of Mn cycling is a major omission (the authors themselves acknowledge

that this is a major aspect of Mn cycling).

While the model does not include all the interesting processes, we believe that the model is sufficiently mature to publish as a first study. Concerning the biological Mn cycling, we agree and now have included a biological module in our simulation.

> As a reader, it is not totally clear what has been learned about the Mn cycle by reading this manuscript. Interesting points are made in the last few lines of the abstract, but these are unfortunately not developed fully in the text. Most of the discussion focuses instead on technical limitations of the model and on a handful of parameterizations used to modify the model to make it fit data – with variable success. Since the bulk of the model presented here is essentially a scaled version of the existing companion Fe-model, the scope of the discussion presented here is somewhat limited.

We believe that our model can be used as a starting point for studies that teach us about the Mn cycle. In other words, this model is a first model that should be primarily considered as a proof of concept and basis for further study. Such further study is seen as doing more, and other type, of field measurements, as well as further development of the simulation model. These should go hand in hand, given the currently available type and amount of field data a more complex model would be overkill. However, we do put forward several insights in the cycling of Mn in the ocean (which should be considered as further supporting the proof of concept). This could still be considered somewhat limiting, which is why we decided to include more discussion in the paper by a more extensive discussion on the GA02 transect that has not been published in a paper before.

The four bullet points at the end of the abstract follow from, or are strongly suggested by, the manuscript. Our first point concerning the high concentration in the upper ocean was already to some extend established in the literature. Our second point on the deep AMOC transport is illustrated by both the observations and the model; the model makes it more intuitive because of the high deep settling velocity. Point three on the background concentration states what the model shows, and then goes on that this supports the idea that a minimal concentration of Mn oxides are needed before significant removal occurs (see simulation *NoThreshold*). It is outside the scope of this modelling study to test this; rather laboratory and/or field experiments are needed for this (and maybe different type of modelling that includes a more mechanistic description of aggregation). Point four on the hydrothermal signal also follows from the paper (see simulation *LowHydro*).

> One aspect of the writing that is not helping convincing the reader that this is a conclusive piece of work, is the over-reliance of subjective statements to describe the simulations and on imprecise/loose statements. This undermines the results. For example, the model fields are repeatedly qualified as “realistic”, but this really doesn’t mean anything, as there are just as many paragraphs in the paper outlining model flaws. The figures also reveal quite a few regions where the model does not fit the data. There is also no real set of criteria that are presented to define what “realistic” means. The authors should instead define the features their model fits or doesn’t fit directly, without trying to oversell their product as “realistic”, and explain in a bit more detail why the model fits or doesn’t fit particular features. Other examples are: “The model reproduces observations accurately”, “This is mostly captured by the model”, “It is probably difficult to improve this feature of the model”, “Reference simulation gives a reasonably realistic distribution”, “LowHydro appears to give a much worse prediction”, “This first-order approach works well mostly, but shortcomings can be identified”, “the flux may be overall underestimated”, “this may still contribute to the dissolved pool in the bottom water”, “underestimation may be due to too strong vertical mixing”, etc. . . Some of these propositions could be tested explicitly by the authors using additional sensitivity studies or appropriate diagnostics to produce objective conclusions instead of suggestions.

In the final version we will try to refrain from subjective statements, and make them more objective with statistics or other analyses, or remove them completely. The statement "LowHydro appears to give a much worse prediction" is subjective on its own, but it is supported by our statistical analysis.

> One way forward could also be to develop a section that discusses the observations in more detail (i.e. expand what is currently in the Introduction and Figure 1 and tie this more closely to what the modelling section aims to achieve). Appendix A spends quite a bit of effort describing how measurements were made. Showcasing these (unpublished?) measurements in more depth early in the manuscript would help build a sense of expectations with regards to what the model is expected to do or not do. That could also be used as a "roadmap" to explain how the paper is organized and why. It is also not totally clear why the authors focus on only a few data sections in their discussion. Figure 5a&b show that the model struggles to fit observation in the upper Pacific on P16. This seems certainly worthy of further examination and commenting.

It would be a good idea to discuss all the observations, and also using all of them for the statistics; but we decided to focus on the GA02 transect. You are right that we can discuss the observations in more detail. The GA02 transect on which we focus has not been published before (except as a data product on the GEOTRACES website). It now has been decided among the co-authors, that the field data and its primary interpretations, now will become part of this simulation modelling paper, instead of thus far chosen by a separate paper. We will restructure the paper such that the discussion of these observations is more extensive and prominent than it is now in the paper.

Other observations are used in a visual (more subjective) way, and detailed discussion and statistics on comparing those with the model (and GA02 observations) is not straightforward because they use other methods or are not intercalibrated. Future studies could include many more measurements for a statistical comparison; this would imply the prerequisite of evaluating every dataset for accuracy and consistency with the others. In the future there will also be more GEOTRACES sampling cruises in the Pacific Ocean and other basins. Analyses from those samples will be following the same quality protocols, thus be more suitable for model–data comparison than many of the already existing observations.

> Since the authors emphasize goodness of fit as the main theme throughout their paper, it is not clear what is gained from the simulations in sections 3.2 & 3.3. (why these experiments were chosen and not others?) It is clear from Table 5 what the results of these sensitivity experiments are. In that sense, little new insight is gained from reading section 4.2 and 4.5. Discussion section 4 is also generally focused on outlining model flaws and omissions. While this honesty from the authors is appreciated, this discussion section has less scientific value than it could have as it caters more to the model developers themselves than to the general public interested in the Mn cycle.

Sections 3.2 shows that we need the combination of high hydrothermal input and high export to get such a good simulation as described in Section 3.1. Section 3.3 shows that it is not as simple as that: the high export needs to be limited to get the correct background concentration. We could have chosen other simulations as well to test further properties of the model, but we think that the chosen sensitivity simulations are the ones that provide the most important insight in the model. A general note on this: it is often useful to start with a realistic simulation, and do sensitivity simulations based on this (good) reference simulation.

Specific comments

> - Please refrain from listing all the “in preparation” papers in the Introduction. The general public doesn’t have access to these manuscripts at this point and there is no guarantee “in prep” papers will be published. Since these “in prep” papers

seem to be review papers, a few well-chosen published references would be much more appropriate.

Most “in preparation” papers in the Introduction are to be published in a special issue of the recent Royal Society GEOTRACES conference that connects closely with the goals of this paper. That special issue is very likely to be published before our Mn modelling paper. We also think that for the Introduction these review papers are the most useful for the reader, because they give an overview to all state-of-the art knowledge, and contain useful references. We will remove the citation to the Middag et al. “in preparation” paper from the manuscript, and instead what was aimed to be in that separate paper will now also be added to the current paper.

- The first line of the Introduction argues that Mn update by phytoplankton is important, even “crucial for photosynthesis”. Yet, the argument for not modelling the biological aspect of Mn cycling is that “not enough data is available to constrain the processes (L2223, p22)” and because “there is no clear evidence for typical updake-remineralization processes" (L15, p26). It seems that testing these ideas is very much what the value of a Mn model would be? Also, how can you say Mn is “crucial for photosynthesis” in the Introduction if there is no information available on biological uptake?

We know that Mn is needed in organisms, and it plays multiple roles in photosynthesis. We also have estimates of inner-cell Mn from Twining and Baines (2013). This means that Mn is incorporated into the cell during growth, but we do not know how this process takes place and what happens with the Mn after it is incorporated. Possibly it is very similar to what happens to phosphate, so we decided to simulate a biological Mn cycle by following P, using a Mn : P ratio from Twining and Baines (2013), consistent with Middag et al. (2013). In this way, we have tested the effect of the trivial case of an uptake-remineralisation proportional to phosphate. First results of this simulation show that it may be relevant for the equatorial region and at mid-latitudes, especially in the Pacific Ocean. We will include and discuss further this result in the paper.

> - P22, L10: "the k values in our model are optimised to get values. . .". How is this "optimisation" done? Is optimisation really what was done?

No, we did not use the right terminology here. We set $k_{red,light}$ simply to the mean value found by Sunda and Huntsman (1994), then used the dissolved and particulate profiles in Fig. 4 of Bruland, Orians and Cowen (1994) to derive k_{ox} and $k_{red,dark}$.

> - For figures 5, 6, 7, 9 that compare simualtions to data, consider adding panels showing the relative errors achieved by the model simualation with respect to the data. This would be helpful to interpret the meaning of the bulk statistical fit diagnostics reported in Table 5 and understand misfit patterns in more detail. The color scheme chosen is also not that great for these figures as it is very hard to distinguish dark blues from dark greens, even if these colors would imply errors of order 200–300 %.

We already spent quite some time in producing good comparison plots, and do not think we can improve on these notably. The statistics support the comparison in a quantitative manner.

> - How does the Mn inventory evolve from one sensitivity study to the next? These sensitivity runs would make more sense if the global inventory of Mn was kept constant. Maybe discuss the inventories as part of Table 3.

We will add the inventories to Table 3. If one does a sensitivity simulation, it is not straightforward what is the best way to keep the inventory invariant compared to the reference simulation, or if this would be very useful. In the case of *NoThreshold* we want to see the change of inventory.

- Appendix B, p29. From what I understand in this section, it seems that the interpolation of the model simulations to the sampling location is done in different ways. If that's in fact the case, why?

Yes, for the horizontal coordinates we used the sampling locations, while for the vertical we did either nothing (for the plots) or used the model grid (for statistical comparison). For the plotting the reason for interpolating the model to the the station coordinates was that there needed to be some kind of interpolation onto a grid that includes the sampling locations; the simplest choice was the minimal one where the new grid is defined by the station coordinates. The reason for the statistics is that while we preferred a comparison of the model at the sampling locations, this would be less trivial in the vertical, since the sample depths vary per station. Since the vertical spacing of the model is similar to that of the samples, we decided to interpolate the observations onto the vertical model grid.

- Why is the dependency of Mn on O2 not considered? (p11, L9)

It would be better to include such a dependency, as the effect is clear in, for instance, the Pacific Ocean. Johnson et al. (1996) had tested this on a local scale. We decided not to repeat this exercise, even though it would be sensible to include it at some point in global Mn models. We found it not to be essential for our purposes at this point. The downside of adding such a dependency is that it would make the model more complicated. It would imply relying on the circulation and oxygen distribution of a biogeochemical carbon model that has uncertainties of its own. So far it has shown quite difficult to simulation OMZs and its effects in an accurate way. However, as a first test we have introduced biological cycling of Mn.

- Section 2.2.6. Can Mn scavenge on other particles besides autigenically formed Mn oxides in this model? Is the lack of particles in deep water what you are trying to approximate with the "aggregation threshold"?

This may be the case. We believe that a more mechanistic model than the one presented in the current manuscript would be much more difficult, as it should include several size classes of particles, different types of manganese and other complications. This should be done, but it is unfeasible for the current study.

> - On p11, L21, you say "manganese oxide is buried when arriving at the ocean floor, which means that it is removed from the model domain". If that is the case, what is the source of the sedimentary Mn from section 2.2.3, eq (3)? Is the amount of Mn buried substantially less/more(?) than the sedimentary Mn source? how do these compare? Is the sediment source totally decoupled from the burial sink in this model? Is that decoupling justified?

It is indeed decoupled. Coupling with a sediment model may be useful for future studies. We will compare burial with forced Mn input and say something about it in the updated manuscript. If useful, we might add a burial figure as well. However, the expectation is that the correlation with the forced flux is very bad: sediment source is large at shallow sediments, while the hydrothermal source would generate most of the Mn burial. This is probably realistic, and suggests that coupling water column and sediment may not be useful, or very difficult, for a global study.

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Discussion paper