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

Anonymous Referee #2

Modelling Mn in the ocean is of great interest and is very timely. The Mn cycle is very complex and modelling it is not easy. This study presents the first results from a few model simulations, one of which the authors argue is in good agreement with the few dissolved Mn concentration measurements available.

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).

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.

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.

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.

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.

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.

- 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 (L22-23, 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?

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

- 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%.

- 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.

- 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 is different ways. If that's in fact the case, why?

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

- 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"?

- 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?