Referee #1

Below are Referee #1 comments reproduced in grey italic and our point-by-point replies in black. Line numbers in our response refer to the revised manuscript with changes accepted.

I confirm that Pasquier's modelling approach is very interesting, efficient and full of promises. The authors answered to my comments regarding the lack of references, and clarified some of the issues I raised. However, I still consider that there are still unclear sections.

We would like to thank Referee #1 for their positive overall opinion of our work. We hope that our responses to the comments below help to clarify the issues raised.

The goal of the manuscript is still unclear, mostly blurred by the conservativity test in the end, as well as a poor discussion of the hypothesis vs simulated parameters. The authors should clearly state in the abstract that the goal of their study is to present the model and not to establish parameter values. This is still not clear, and the last section (application to test the conservativity) is more confusing than clarifying.

The main goal of this manuscript is to describe the Global Neodymium Ocean Model (GNOM) v1.0, and highlight some of the unique attributes of this model compared with previous models of the modern ocean Nd cycle. Thus, this paper fits the GMD "Model Description" type paper.

In the abstract of the manuscript, we state that: "... in this model description paper we [are] present[ing] and describ[ing] the Global Neodymium Ocean Model (GNOM) v1.0…". We also state that our "...systematic objective optimization allow[s] us to make ***preliminary*** estimates of biogeochemical parameters." The results we present are intended to highlight the potential of this new model, but we are in agreement that some parameter values from our preliminary estimation are unrealistic. We plan to make further refinements in the future. Our hope is that by publishing the *model description* paper separately from a paper focusing on the *scientific implications* of finalized parameter values, other members of the community can, in the interim, also use this new tool and make further improvements as well.

As part of the Model Description type paper for GMD, there is a requirement that "[t]he model description should be contextualised appropriately. For example, the inclusion of discussion of the scope of applicability and limitations of the approach adopted is expected." In light of this, we feel that it is crucial to include the section on unique diagnostics that are possible with the GNOM model, such as the test of conservativeness that we present in Section 3.3. These diagnostics, which are made possible by the formulation of the model, are also mentioned in the abstract.

We have edited the abstract in an attempt to further clarify our goals in this paper.

My understanding of the hypotheses is still very confused. I would appreciate if the authors would make the effort to improve their model description and tests. For example, they are going to tinker with sediment flux parameters (let's say in Greenland) but actually, they don't optimize the sources but only the parameters associated with the sources. Particle fall velocities are considered as constant while Dutay et al showed that this parameter can strongly influence the simulation results. On how much time the velocity fields are averaged is not given, and if the AWESOME OCIM particle fields are consistent with the same dynamics is not given either.

We are somewhat confused by this comment. First, it is unclear which part of the model description Referee #1 believes is missing.

The comment on optimizing the parameters rather than the sources themselves is also confusing. If Referee #1 is suggesting that each pixel of the model should be its own parameter for the sediment source, then that would require the optimization of an additional 10,441 parameters, which is an unreasonable request. An important trait of the GNOM is that its source and sink parameterizations are based on simple mechanistic representations. For instance, the sedimentary-source parameterization of the GNOM can be boiled down to three reasonable assumptions: it varies with depth, composition (via the ε_{Nd} enhancement), and in some cases with location (Greenland enhancement).

Regarding particle sinking velocities, we are unsure which Dutay et al. study Referee #1 is referring to, but we do not dispute that particle sinking velocities can vary. As we describe on L455, our assumption of constant settling velocity (and having this not be an optimizable parameter) is a simplification. Due to the parameterization of reversible exchange itself, there is no point in independently optimizing both the settling velocity and the scavenging equilibrium constant:

> "...we do not optimize the corresponding settling velocities … because K_x and w_x can perfectly compensate each other. For example, doubling K_x while halving w_x has no effect on Nd distributions and the objective function. Only their product, $K_x\, w_x$, which sets the strength of the "scavenging pump" through the operator matrix $\mathbf{T}_{\mathrm{scav}}$, appears in the tracer equations (see Eq. (16) or, e.g., John et al., 2020), such that these parameters cannot be easily optimized independently."

Finally, we note that in a steady-state model such as the GNOM described here, variables and parameters are not averaged over specific periods of time (steady-state = no variation in time) although one could think of them as representing the climatological mean.

No changes to the manuscript were made in response to this comment.

Regarding the conservativity test. In this section, the authors declare that they will model Nd parameters while this part presents the modeling of a new tracer in itself (certainly related to Nd). Actually, a very fast passive tracer modeling tool allowing to do a lot of sensitivity tests is presented, in which they have implemented Nd and its sources. Again, it's a nice tool, but this part leaves the reader with the same feeling as for the inverse modeling above, with this "catalog aspect" where they present a rather sloppy (or at least not finished) application saying that they will do it seriously later.

As reiterated multiple times in our reply to the first reviews, our manuscript is a "model **description** paper", which aims to **describe** the GNOM framework. We respectfully reject Referee #1's claim that showcasing an advanced (and efficient) Green-function diagnosis that is seldom easily obtainable from GCMs in our model is somehow "sloppy".

It seems that Referee #1 is expecting a full geochemical study such as would be required for publication in a Geosciences journal, rather than the description of a new model as is required by GMD for a "Model Description" paper (see response above).

No changes to the manuscript were made in response to this comment.

Detailed comments

Line 34: the CHUR value has bee actualized to 0.512630 (see Bouvier et al, EPSL, 2008)

In order for ε_{Nd} measurements to be consistent with those published throughout time, it is crucial that they are all referenced to the same value. Therefore, while it is true that the CHUR value has been updated since the work of Jacobsen and Wasserburg (1980), it is preferable for us to use the Jacobsen and Wasserburg (1980) value of 0.512638 for consistency with observational data.

We also note that the Jacobsen and Wasserburg (1980) CHUR value differs from that of Bouvier et al. (2008) only in the sixth significant digit, which is within their reported uncertainty of ±0.000011, and that swapping one for the other results in a mere 0.15 ‱ change in ε_{Nd}, which is within error of most measurements (and within the ±0.2 ‱ uncertainty as propagated from the Bouvier et al. (2008) uncertainty).

We have updated L34 to read:
"For consistency with previously published data, we use R_{CHUR} = 0.512638 from Jacobsen and Wasserburg (1980), rather than the updated value from Bouvier et al. (2008)."

Line 44: the interpretation "powerful palaeoceanographic tracer" is challenged by all the studies trying to tackle the "missing parameter". This should also be mentioned here (for reasons of fairness and rigor).

Referee #1 ignored the adjective "potentially" in their quote of our manuscript, which reads: "**potentially** powerful paleoceanographic tracer". We strongly disagree with Referee #1's views here. There have been clear paleoceanographic advances based on Nd, as demonstrated by the plethora of scientific discoveries in the literature. Every paleo proxy has complications and limits. In fact, in our view, although we did not do so, it would have been legitimate to forgo the modifier "potentially" and simply call Nd isotopes a powerful paleoceanographic tracer.

No changes to the manuscript were made in response to this comment.

Line 49: the Geotraces IDP 2021 is published yet. Although not used in this work, could be mentioned.

The Geotraces IDP 2021 was published after our original submission and model runs. In future versions of the model, we will certainly include this updated dataset. We would like to note that we took extreme care to include all other *published* ε_{Nd} data that came out after the IDP 2017 but before our manuscript submission.

No changes to the manuscript were made in response to this comment.

Line 78: to my knowledge, only Gu et al (2019,2020) attempted to mathematically optimize a Nd-Cycling. Rempfer as Arsouze (or others) only made artisanal tests. The authors could moderate this sentence.

Re-examining the text and supplemental material of Gu et al. (2019), we see that they performed a very similar optimization to Rempfer et al. (2011) (two parameters were varied in each study).

We have updated L78 to read:
"To our knowledge, only Rempfer et al. (2011) and Gu et al. (2019) have attempted to optimize a Nd-cycling model, using the low-resolution Bern3D OGCM and 3° resolution Community Earth System Model (CESM1.3), respectively, and each optimizing only two parameters."

Line 117: I disagree with the following sentence, for 2 reasons:

"These preliminary diagnostics already reveal important information. They help quantify the conservativeness of epsNd along water pathways and unveil the underlying mechanisms by evaluating the effect of local sources and sinks."
The first reason is that it is contradictory with the initial goal which is (e.g. line 530) "we emphasize that it is not the goal of this manuscript to establish estimates of the GNOM parameters and that we welcome future GNOM users to apply narrower ranges for those parameters for which they have better constraints (for example, restricting dust Nd solubilities to values below 10 %)"
The whole manuscript would benefit if the goals are better and clearer written (as I suggest, starting in the abstract). The authors should not declare that they "unveil" source parameters having dust solubilities of 80% …which is so far from the observations!
The second reason is that conservativity is not tested with exactly the same model (see above). Actually, I'd strongly suggest to remove the conservativity section (3.3.4) which will have many advantages 1) make the whole work clearer and explicitly dedicated to the presentation of GNOM; 2) inverse model are developed to constrain the sources that explain the distribution of any parameter in a given reservoir, which is discussed in the other sections (no problem with that) but testing conservativity would gain with a Lagrangian modelling, and not with a wobbly inversion and 3) this will shorten the manuscript, which is very long.
Thus, I'd replace the sentence line 117 by another one underlining again that the goal of this work is not to establish the GNOM parameters…"

Referee #1 seems confused about a couple of points and misrepresents some of our claims.

Firstly, we want to make clear that the diagnostics are tested in exactly the same model as is used for all the work presented in this manuscript. See L10 of the abstract.

We have edited L580 of the main text to reiterate that the diagnostics are implemented within the GNOM model.

We feel the line quoted about our preliminary diagnostics is already mild in tone, but perhaps the reviewer is suggesting that our model diagnostics cannot reveal *any* information about the system if the optimized parameters are not finalized. We disagree. Again, the main point of this paper is to highlight the capabilities of our new model and the diagnostics are an important part of the model. Indeed, we *specifically do not* include much interpretation of the diagnostics we present, as that would require more finalized parameter values. In future science-focused papers, with finalized parameter values, we plan to discuss the scientific implications of the parameter values themselves and the Green-function-based diagnostics.

We have tried to make this point clearer on L119:
"Detailed investigations of these diagnostics are out of the scope of this study and will be carried out in future work using a subsequent version of the GNOM with more finalized parameter values."

Again, our manuscript is quoted here without context. Nowhere do we claim that we "unveil source parameters". We merely say that the partitions allowed by our diagnostics "...help quantify the conservativeness of ε_{Nd} along water pathways and **unveil the underlying mechanisms** by evaluating the effect of local sources and sinks".

We have edited this sentence (L118) to now read:
"...help quantify the conservativeness of εNd along water pathways and unveil underlying mechanisms by evaluating the effects of local sources and sinks".

All of the co-authors agree that the suggestion by Referee #1 to remove Section 3.3.4 is unreasonable, given that it describes a key feature of the GNOM. Running such diagnostics in more traditional GCMs is not a trivial task, but here, owing to the matrix and steady-state representation, these diagnostics are easily set up and take roughly 10 seconds to run on a modern laptop.

Referee #1 is certainly entitled to their opinion that a Lagrangian model would be suited to such a study, but here again, we disagree. It would actually be quite difficult for Lagrangian models (i.e., particle-tracking models) to investigate the question of ε_{Nd} conservativeness because they would not capture the effects of eddy–diffusive mixing, which is *very important* for quantifying conservativeness. Dissolved tracers can diffuse and mix in a way that particle-tracking Lagrangian models can't represent directly.

Referee #1 also fails to characterize what makes our inversion "wobbly" and shortening a manuscript for the sake of shortening a manuscript seems ill-guided.

No changes have been made in response to this portion of the comment.

Line 314: Arsouze and others are making the same first order assumption

Agreed.

In response, we have added a citation of Arsouze et al. (2009):
"We follow, e.g., Bacon and Anderson (1982), Siddall et al. (2008), and Arsouze et al. (2009), and assume that dissolved and scavenged Nd are exchanged via a first-order kinetic reaction"

Line 350: Lagarde et al recently published a well documented section of particulate REE (in the North Atlantic) that could be considered here too (Biogeosciences, 2020)

It seems like the Lagarde et al. paper mostly focuses on scavenging onto biological particles (e.g., biogenic silica), but they do discuss scavenging onto Fe-Mn (hydr)oxides. We have therefore added this citation to the sentence mentioning Fe-Mn (hydr)oxides, since our model already incorporates biogenic particles such as silica, which are known to scavenge Nd.

We have added this citation to L353.

Line 414: I would suggest to precise that the authors are tackling the dissolved Nd parameters (in the title for example)

OK. We have edited this section title to specify that we are focused on dissolved Nd.

"2.4.2 Dissolved neodymium and εNd data"

Referee #2

Below are Referee #2 comments reproduced in grey italic and our point-by-point replies in black. Line numbers in our response refer to the revised manuscript with changes accepted.

Dear Editor, dear authors,
After reading the authors' reply I believe my disagreement with the authors is about the model philosophy, and what does this paper want to achieve.
In the initial draft, the manuscript seems not only a model description paper, but at the same time, the authors' affirmative languages make it look like a thorough study of the modern ocean Nd cycle. As stated in my previous comments, I have little criticism on this manuscript as a model description paper, which I welcome STRONGLY (except for some problems with the optimization approach). My criticism is mainly about this manuscript as a "proper" geochemical study of the ocean Nd cycle.
The authors insisted in the reply that this manuscript is mainly a model description paper, and the results are "test examples" that will be redone with better care later, and thus largely deferred my criticism on the Nd cycle to future studies. If so, they should make is clear that the results should NOT be considered "final" as they themselves believe so. My worry is that, these test results will confuse readers and lead to incorrect citation without proper warning.
The authors are trying to purse a "truly global" parameterization of every aspect of the Nd cycle. This is highly commendable and ambitious, and it is also the ultimate goal of all scientists studying Nd. But my opinion remains that this is NOT achievable in practice at the moment, because of sever data limitation and poor mechanistic understanding. My model philosophy is thus conservative: the modeler should follow measured data whenever they are available even if imperfect, rather than use sweeping global generalizations and optimizations that appear superior but has no mechanistic foundation. This is clearly my bias as a data scientist which I acknowledge fully.

We would like to thank the reviewer for their comments and their favorable view of our paper as a model description paper.

In our latest revision of the manuscript, we have included more language throughout that clarifies the manuscript goals (to describe a new model of the marine Nd cycle) and reiterates that we are not presenting final parameter values (which we hope to do in a future science-focused paper).

We also appreciate the reviewer's perspective about implementing global models of the Nd cycle despite yet incomplete data coverage. We feel that it is still a worthwhile endeavor at this point, but we also hope that this model will only become more useful and accurate in the future as additional data are published.

Referee #3 (Brian Haley)

Below are Referee #3 comments reproduced in grey italic and our point-by-point replies in black. Line numbers in our response refer to the revised manuscript with changes accepted.

I appreciate the efforts taken by the authors in response to the (many) comments made in review. I can fully endorse publication of this work at this time.
*(However, I just want to stress to the authors that (1) the comments on the Sm/Nd ratio difference between ALL rocks is as critical as how old they are for eNd. Yes, basalts are *really* different, and this their eNd is so different. But the age of a rock is only a part of the causality of a given eNd. Also (2) hydrothermalism is a sink, and does not add to nor modify water column eNd (also see work by Chavagnac) - at least not directly. But then i dont know how your model deals with the precipitate once it forms...)*

We thank Referee #3 for their detailed initial review, which substantially improved the manuscript. In future iterations of this model, we hope to include a more detailed (and accurate) description of neodymium cycling at hydrothermal vents.

The Acknowledgements section now includes this sentence:
"The authors would like to thank the editor, Brian Haley, and two other anonymous reviewers for their helpful comments, which substantially improved this paper."