Response to Referee #1 (Dr. David Archer)

We express our gratitude to Dr. David Archer for his useful comments. Our response to the reviewer's comments and the corresponding revision are described in detail and separately below. The numbers of pages, lines, equations, tables and figures are those in the revised manuscript unless otherwise described.

Comment 1:

"The paper could be much more interesting with the addition of some additional sensitivity experiments. The authors cite the literature on the 14-C distribution in CaCO3 in the equatorial Pacific, which gets older as the %CaCO3 goes down, the opposite of what you would expect if the CaCO3 dissolved homogeneously – a low %CaCO3 would imply a short residence time and a low age. Somehow shells become "armored" from dissolution if they survive 1rly stage. I have attempted to replicate this by using multiple phases of CaCO3 with varying dissolution kinetics or solubilities, but I never managed to reproduce the trend in the observations. I think this observation is mirrored in the 14-C age distribution of mollusk shells; it seems to be a general thing. Perhaps attacking this problem is for a future study, while this is just a model development paper, but it doesn't seem like another sensitivity plot or two would add too much baggage to the paper."

Response:

We agree with the reviewer that including the potential model application to the 14-C age problem would make the paper more interesting. As suggested by the reviewer, we conducted a sensitivity analysis for the rain fraction of fine and coarse species considered in the experiment in Section 3.2.3. In these experiments we adopt method 3 instead of method 2 for tracking 14-C age. We vary water depth (from 3.7 to 4.1 km) and total sediment rain (from the default value of 12 μmol $CaCO_3$ cm^{-2} yr^{-1}, to 6 and 3 μmol $CaCO_3$ cm^{-2} yr^{-1}, with fixed $OM/CaCO_3$ and clay/$CaCO_3$ ratios, Fig. 16).

We found that the trend of increasing ^{14}C age with lower $CaCO_3$ wt% can be simulated for a coarse $CaCO_3$ species by increasing the rain fraction of the fine species (Fig. 16a). On the other hand, an opposite trend is found for fine species (Fig. 16b). The above aging effects for coarse and fine species are enabled because the fine species dissolves faster and increasing the rain fraction of the fine species leads to a longer residence time if total rain and water depth are fixed. Bulk ^{14}C age and $CaCO_3$ wt% shows a trend that is a combination of the opposing trends for the fine and coarse species (Fig. 16c). Therefore, whether bulk ^{14}C age decreases or increases with $CaCO_3$ wt% depends on the contribution of fine vs. coarse species along the trajectory for the fine rain fraction at a fixed sediment rain and water depth.

While we could not reproduce the observation perfectly, introducing the fine and coarse species can show the ^{14}C age vs. $CaCO_3$ wt% trend which is not possible when considering only bulk phases, thus supporting the utility of IMP for a better interpretation of proxy signals.

Changes in manuscript (Page numbers/Line numbers):
We added a sensitivity analysis described above as Section 3.3 (P20/L601-P22/L646) and Fig. 16 (P44).

Comment 2:
"I had some questions as I was reading, points of confusion. For the Fickian diffusion, does the rate taper off exponentially with depth or is it an abrupt cutoff? What is the difference between Ficking diffusion and homogeneous mixing?"

Response:
Fickian mixing is a 'local' mixing, where particle translocations occur only between adjacent layers. Homogeneous mixing is introduced as one example of 'non-local' mixing, where particles can be exchanged between remote layers.

   We did not implement the biodiffusion coefficient as a function of depth, although tapering off the coefficient with depth might be more realistic (e.g., Ridgwell, 2001, Glacial-interglacial perturbations in the global carbon cycle, PhD thesis). The simplified parameterization for the biodiffusion coefficient still serves our purpose, that is, to illustrate the effects of variation in bio-mixing styles on signal distortion.

Changes in manuscript (Page numbers/Line numbers):
We added an illustration of modified transition matrices $K_{\theta,ij}$ as Fig. 1 (P29) and more explanations on the difference between local and non-local mixing referring to Fig. 1 (P8/L229-P9/L241).

   We clarified that the biodiffusion coefficient for Fickian mixing does not change with depth (P8/L210-211).

Comment 3:
"line 45: clarify what you mean; I would have thought that Fickian diffusion is random mixing. In that section it might also be worth mentioning that some models use uniform mixing down to an abrupt cutoff, while others use an exponential dropoff in mixing rate."

Response:
Please see our response to comment 2 by the reviewer where we address the issue mostly.

   The point of this paper is to illustrate the model's capability to simulate the effects of changes in bio-mixing style on proxy signals, rather than those of changing the parameterization of Fickian mixing. So we mentioned the parameterization of Fickian mixing only in the later section (Section

2.2.2).

Changes in manuscript (Page numbers/Line numbers):
Please see our changes in manuscript in response to comment 2 by the reviewer.

Comment 4:
"line 110. Kudos for coding the model up in multiple languages! But why python, when Julia seems just as elegant and flexible and also lots faster?"

Response:
Julia is a younger language than Python and thus we assumed that Python user population could be larger than Julia user population. Python is slow but as in other languages it can call a module created from a Fortran code. We include such Python (plus Fortran) usage option and a readme file on our code repository (iMP/Python/readme_Python_Fortran.txt) instructs how to create a Python module from the Fortran code and call it from a Python script. Julia is definitely one of candidates which will be used in the future release of IMP.

Changes in manuscript:
No change was made in response to the comment.

Comment 4:
"line 225. How can the initial condition have vanishinly small concentrations of all of the solid phases? Don't they have to sum to fill the solid volume implied by the time independent porosity? (On further reading I understood that this is an initial state for an iteration, which by the time it converges will have solved the problem. However, maybe a sentence here would help clarify.)"

Response:
As stated in the parenthesis by the reviewer, small solid concentrations deficient for solid space prescribed by porosity are allowed only as an initial state of an experiment. Later time-integration fills up the initial void space and once filled there is no void solid space or expansion of solids compared to the prescribed solid space by porosity.

Changes in manuscript (Page numbers/Line numbers):
We revised manuscript to avoid potential confusion (P10/L275-276).

Comment 5:

"line 260. "time implicit method". It took a bit of digging to figure out if the model is time dependent or steady-state? This was a clue in the text but it didn't specify whether it applied to solid and dissolved species or what. I figured it out from the figures, but it would have been useful to state it more explicitly earlier on. (And on that, why bother with time dependence for the solutes? It must slow things down a lot.)"

Response:

The model is time dependent, as we stated earlier e.g., in lines 87-88. We agree that it would be better to clarify that time-dependent calculation was made for all species in the relevant sentence.

Although including the time dependent simulations slows the calculation, the applicability of the model increases. For example, one will probably need the time dependent simulations even for solutes when considering a diagenesis including deep reactions involving methane cycling (e.g., Archer, 2007, Biogeosciences 4, 521) although not considered in the simulations presented in this paper.

Changes in manuscript (Page numbers/Line numbers):
We revised the relevant sentence to be clearer that the model conducts time-dependent simulations (P11/L319).

Comment 6:

"Equations 23-24. These are succinct descriptions of the matrices, but they are not very transparent as far as explaining what the mixing models do. Why does homogeneous mixing use P rather than D? (On subsequent rereading there is an extensive discussion on the formulation of homogeneous mixing, but a bit of summary here would be helpful.)"

Response:
We agree that description of transition matrices was not very transparent in the previous manuscript.

Changes in manuscript (Page numbers/Line numbers):
We added the detailed description of transition matrices to Section 2.2.2 (P8/L229-P9/L241) and heat maps of transition matrices as Fig. 1 (P29).

Comment 7:

"Would it be possible to make some kind of visualization of the transportation matrix, a heat map of some sort that would show how the mixing mechanisms differ?"

Response:

We agree to provide heat maps of transition matrices to facilitate comparison between different styles of bio-mixing.

Changes in manuscript (Page numbers/Line numbers):

The heat maps of transition matrices were added as Fig. 1 (P29).

Comment 8:

"line 500. It would be interesting to integrate how much excess CaCO3 dissolution occurred due to the change in solubility (water depth) – how the buffering strength of the sea floor depends on the mixing model."

Response:

Bio-mixing affects $CaCO_3$ dissolution to a given destabilization (a water-depth increase), as can be inferred from simulated wt% $CaCO_3$ record (Fig. 13). This indicates that buffering strength would change with bio-mixing style. However, because we enforce dissolution in an idealized way, i.e., not in a realistic way in the experiments in Section 3.2.2, we avoid providing exact values of $CaCO_3$ dissolution fluxes.

Changes in manuscript (Page numbers/Line numbers):

We added description of the potential changes of buffering strength with bio-mixing styles (P19/L570-572).

Comment 9:

"line 515. Do the smaller particles have higher surface to volume, and also less mass, so they dissolve more quickly for those reasons also? It would be useful to add differences in kinetics or solubility here, and separate out the different effects."

Response:

Thermodynamic differences between fine and coarse species can be implementable in IMP as stated in lines 34-35 but not included in any experiments conducted in this paper. Kinetic differences between fine and coarse species are considered in the experiment in Section 3.2.3 as stated in lines 578-584.

Changes in manuscript (Page numbers/Line numbers):

No change was made in response to the comment.