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Geoscientific Model Development Discussions

Interactive comment on "Inclusion of a suite of weathering tracers in the cGENIE Earth System Model – muffin release v.0.9.10" *by* Markus Adloff et al.

Anonymous Referee #1

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I read this paper with great interest- cGENIE is already a hugely important model that has been central to a vast number of advances in understanding our Earth's climate, and the controls that influence it. As silicate weathering is an important component of climate regulation, it makes a lot of sense to incorporate these weathering-sensitive isotope systems into the model- and in doing so it opens all sorts of possibilities: for example to reverse model past events where these isotope records are published and available. As such this model is a really important and exciting advance in the field. There is no question that a model description paper such as this is suitable for this journal, and the material is clearly significant enough and important enough to warrant publication. I have a number of suggestions, however, that I think should be addressed

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in order to improve the model and its presentation in this manuscript.

There are a couple of issues with the model that I am not sure about- in particular with how Os is being handled. For instance, there is no Os input from Corg weathering, but output into POC and COrg burial. This to me seems a bit of a shame- partly the system seems underparameterised, but more importantly also, it really limits the model's potential in evaluating some important hypotheses in Earth history regarding weathering changes and its effect on the carbon cycle. For instance, one cannot test to what extent Os isotopes were reflecting just the exhumation of new lithologies high in Os (see Myrow et al. 2015, EPSL), vs. weathering changes, because one cannot change the lithological map. See also things worth testing with such a spatially detailed and complex model in papers by Zhang & Planavsky (Am. J. Sci., 2019) and Jagoutz et al. (2016, PNAS). It seems to me reading section 3.1 that although there is no explicit representation of organic matter weathering in ROKGEM, and sure carbonate-vs-silicate rock would not be a very good bounding line for modelling Os, there is a representation of shale lithologies in ROKGEM. Given the importance of shale in Os weathering fluxes, couldn't the model at least try to represent lithology in a more mechanistic way that would allow better flexibility in terms of what could be modelled? It seems a bit of a cop-out/missed opportunity to just scale to continental runoff, when the system is so sensitive to the lithology of the Earth surface..

Also with Os, I was disappointed by the decision taken in Section 4.2.2 to ignore data-model mismatch on the basis that it 'should be the basis of a separate study'.. isn't the whole point of this paper to be the paper that presents a working model? If there is a fundamental process affecting Os distribution that is missing in the model such that it can't replicate the modern, shouldn't that tell us that it isn't ready to apply to the past? It isn't really an issue of only being interested in basin-scale patterns if there is a missing, possibly unknown, process somewhere that could have been far more prominent in the past and thus render the whole Os representation inaccurate. There is some mention on Page 24, Line 17 of it being due to Os binding to organic matter in the water column

in low O2. This is confusing, because in Section 3.5 there is a rough parameterization of this process – can't the model just include an [O2]-dependent sink here? Given the propensity of low O2 regions during OAEs, mass extinctions, etc. where this model might be used, isn't getting this process right of utmost importance?

Secondly, the presentation of the data/model fits could be improved in places:

- In Fig. 1, what are the grey shaded regions, and how are they derived? This is not explained. And do the means and shaded regions take into account the uncertainty on each data point, in some sort of Monte Carlo fashion? It might be advisable- for example with Li where some of the extreme values have reasonably high uncertainty, and so should not be weighted in the same way when calculating the mean.

- I appreciate that the authors should not be expected to critically examine the methodologies and structural sources of uncertainty in all these data publications. However, there might be some simple ways to help the reader ascertain which of these data should be considered more reliable as estimates of the seawater isotope composition or element concentration.. For instance, in recognition of the advances in mass spectrometry in the past decades that result in superior analytical accuracy and precision relative to some of these pioneering but now perhaps less trusted estimates, might it be reasonable to colour-code the measured data by year published? Or if that is too busy, one could decide on an arbitrary cut-off date (e.g. 2000) and draw the data points before that date as slightly lighter than the more recent, and likely more reliable, estimates? Of course, chronology is an imperfect metric of reliability, but it is likely to be at least indicative (I noticed that the datasets that are most inclined to diverge from the model are often the older studies; e.g. Angino 1966, measured via atomic absorption spectroscopy). It's easier to evaluate the model's performance if the reader has a sense which datapoints might be most likely to be reliable.

- In Fig. 2, the yellow Calcium arrows are very difficult to see at times.. could use a different colour- sky blue?

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- I can see how these plots ranking measurements in terms of values is sensible and meaningful for isotope systems where the residence time in seawater is vastly more than the timescales of circulation.. But in the case of Os, where there is a chance of some regional differences, it doesn't make so much sense. Because if the spread in the data is as shown but actually most of the data is all from one region, say, like the North Atlantic, but the model has every grid point in the ocean, you're comparing apples and oranges. The model extremes might reflect spatial differences, but the data extremes might reflect unreliable measurements but from a limited geographic range. But the plot makes it seem like the comparison should be meaningful. Something more like Figs. A3 or C1 would likely be more useful, but then at the same time these figures are not as helpful as they could be. In Fig. A3 it looks like Os concentrations and isotopes are totally homogenous everywhere in the model. But in Fig. 5 there are clearly divergent values that suggests Os in seawater is not homogenous. So which way is it? In Fig. A3 also there are few enough measured profiles that you could give them different symbols for different ocean basins, and a colour that scales with latitude?

- A lot of the figures are very very small and awkward to read. For instance Fig. A1.. this is a lot of panels of different sizes and shapes, about different things, and thrown together in one figure- some with superimposed labels in a jarring serif font positioned in an odd way, some without labels, etc.. Why not just make them separate supplementary figures for each isotope system - there is no limit on numbers I would guess, and then you could have plots that people can read properly.

- Shouldn't one of the plots in Figure 5 be concentration?

- In Fig. 7 the light and dark lines are hard to tell apart- particularly the orange, purple and green. Can't one be dotted and one be dashed, as well?

I find the examination of the weathering response to CO2 is very interesting, but also very surprising. If the model suggests that an instant release of as much as 5,000 Pg of C instantaneous results in barely any change in d7Li, but the PETM, where a similar

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order of pCO2 was released saw a 3-4 ‰ change in d7Li (Pogge von Strandmann P, Jones M, Schmidt D & Murphy M (2019) Goldschmidt Abstracts, 2019 2682), then what gives? Is the model undersensitive, or missing a flux? Or is the Li isotope excursion at the PETM caused by something else? It would be good to discuss this, because otherwise it is impossible to tell how much of it is due to missing fluxes/reactions, or to know what they might be.

In Tables 1-4, values for various parameters and estimates are given, with references for the source in each case. However, from Table 5 on, when talking about model default parameters, there is no such detail. It can be hard then for the reader to know which values are chosen for a good scientific reason, and which are chosen just based on not wanting the model to crash. It would be good to say where these numbers came from. Also it seems like a disproportionately high number of parameters are set to zero- is this because these are the choices made in the chosen scenario only, or are these a lot of parameters that are theoretically included but that people should not use because they may crash the model?

Page 28, Line 26: Would it be hard or computationally-expensive to just scale the Ca2+ source at the seafloor to the bottom water temperature that is simulated?

Section 3.1: I know there are descriptions in the ROKGEM paper, but a cursory explanation of how source rock lithologies are programmed into the model would be helpful in this paper, so the reader doesn't have to read a whole other paper to understand this one.

Section 3.7: Should strontium sulphate delivery by Acantharians matter and be considered?

Page 2, Line 5: mantle spelt wrong.

Page 6, Line 13: Perhaps Dellinger et al. (2015, GCA) might be a good paper to include as a citation here?

Page 9, Line 9: I would say it's due to analytical inaccuracy, technically, rather than the uncertainty bounds on the values.

Page 12, line 21: Would it be possible to give the user a knob to turn to partition more or less hydrothermal flux in one ocean basin vs. another at least? Thinking of the Cretaceous with restricted seaways- Os may be heterogenous, and centres of hydrothermal activity also.

Page 13, Line 19: Why only Li in authigenic carbonates, but not others like Sr?

Page 14, Line 12: buried spelt wrong. Also a reference for the statement would be good.

Page 18, Table 13: I get that there is no fractionation of Os isotopes parameterized, but this description in this table suggests that the 187/188Os signature of inputs is set to 0, which seems to be wrong..?

Page 19, Lines 21-22: Why is this needed to balance the Ca cycle? And what is the natural process this is supposed to mimic? Can you give a reference?

Page 24, Line 15: not sure 'against' is the right word to use here.

Page 26, Line 1: no date for the Hall reference

Page 26, Line 11-12: Say 'another' indication that this paper is wrong.. but wasn't the first point of discussion earlier in the paper that these values could be wrong talking about Angino and Billings (1996), not Angino et al.?

Fig. A1: Never heard of the Indic Ocean! Also see my main gripe. Aesthetics in these plots could also be a lot nicer- it's very default python. For instance the colour palette for the data series or the data point symbols could be used to convey information about ocean basin, or study, or something.. The Os isotope panel for example is really hard to read and not nice on the eyes.

Fig. A2-A5: Many of these figures really don't print well. The various greys and blacks

are often very hard to tell apart when printed- the FLUXES vs TUNED for example. I would using some colours to help with this. Fig A2 has a mixture of [] and () in axis titles. Numbers on the axes are unnecessarily small.

Fig C1: These datapoints are far too small! Impossible to judge the colour of the points. Ditto the axis labels and legend entries.. There is a missing colour scale label for the bottom left panel, and the X longitude axis is completely cut off for the bottom two panels.

Acknowledgments: For some reason starts with 'Furthermore'.. Are there some sentences missing/lost before?

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