Response to reviewers

The authors have properly addressed all the comments I had concerning the first submission, and considered the corrections I suggested. The reorganization of the manuscript, mostly following the comments of reviewer #2, is relevant. So far, I don't have any more request on this revised version, save a few minor corrections. I do have a last broad question on the model's behavior, but I don't consider it requires to modify the current manuscript.

I was startled, looking at Fig. 7 (former Fig. 6) to see a complete latitude flip in the sensitivity of runoff to the "omega" exponent. As far as I understand, the only changes that were made in the MEBM code were to replace "q" by "q/rh" in eq. 8, and to reformulate the gross moist stability from "h(0) - h(x) + cst" to "1.06*h(0) - h(x)". Are those two changes enough to explain the difference? We're talking about a 5% versus a 25% of runoff increase when reducing "omega" in Polarslice experiment. The author's justification is that the ratio E0/P was closer to 1 at high latitude than in the tropics, in the former version of the model, but is now closer to 1 in the tropics. This is not improbable per se, but it reveals quite a high sensitivity of the model to these parameterizations.

Thanks for pointing this out. We ran some sensitivity tests and couldn’t reproduce the change. Ultimately, we traced it back to an error in the first submission that was fixed when we re-ran our simulations for the revisions. In the first submission, we accidentally plotted the results for a mid-latitude belt rather than a polar belt. This is why omega was closer to one in “polarslice” (actually mid-latitude slice) world and now is closer in “tropicslice”. We re-ran polarslice with the old code and got results very similar to the new code, as expected. We have also gone back and confirmed that the rest of our plots match the stated geography.

Minor corrections:

The modifications of Fig. 4 ("% of global discharge 320 ppmv", weathering units, and indication "for each equal-area latitudinal grid cell" in the caption) are welcome, but should also be made on Supp. Fig. 4.

Thanks, these changes have been made.

Fig. 7's caption needs to be updated: the "The slower and larger magnitude climate response" is now of Tropicslice, comparred to Polarslice.

Thanks, changed.

Similarly, Supp. Fig. 3's caption needs to be updated: both configuration now have similar runoff sensitivity to temperature, and Polarslice has a larger weathering sensitivity to pCO_2.

Thanks, changed.

Line 223: The Damköhler weathering coefficient has the dimension of a runoff (unlike the Damköhler number, that is dimensionless).

Correct, thanks for catching this. Changed.

Line 312 and in Supp. Table 3: the values of [Ca], [Mg] and [SO_4] are in mol/m3, not in mol/L

Thanks, values have been changed to mol/L in both places.

The parameter "theta" (Clausius-Clapeyron coefficient) is still referred as "alpha" in Supp. Table 1.

Thanks, fixed.

The reference temperature "T0" in Supp. Table 2 is probably 14°C, not 14 K.

Indeed! Thanks.

I am pleased that Kukla and colleagues paid much attention to the reviewer's comments. They addressed all comments and questions, and adjusted the text where appropriate. They corrected bugs in the model code in response to two reviewer comments and re-ran all experiments.

The authors also added a section including some sensitivity experiments to address my main comment of the first review. I thank the authors for this. However, I am a little disappointed by this section because it is not a comprehensive sensitivity analysis and therefore does not really address what I had in mind with the comment: "Many processes are not explicitly represented in the model and are thus highly parameterized and a large source of uncertainty. Therefore, a comprehensive sensitivity analysis is needed to quantify this uncertainty, identify the most sensitive parameters and adequately understand how the model works."

The new results only show that interacting effects between model parameters exist (which is to be expected) without quantifying, for instance, which parameters have more significant interacting effects than others, or to which model parameters model results are most sensitive.

I appreciate that "the model is a patchwork of existing frameworks that we stitched together, and previous work has already addressed the sensitivity of these individual components." However, from coupling different modules together, modelers have experienced that a coupled, interactive system can react differently than individual components alone. I also understand that the model behavior depends on the boundary conditions. Therefore the author's approach of simulating a low and a high pCO2 world using a modern continental configuration (Section 4.3) is a good way to provide some general guidance but could be extended.

Overall, I had hoped for a more elaborate quantitative analysis of some largely conceptual/abstract parameters (i.e., without explicit links to physics, biology, or chemistry) in order to provide a better understanding of the model's considerable parameter uncertainty. Methods for quantitative sensitivity analysis exist (see, e.g., the Elementary Effects Tests in the publications I provided in the first round of reviews) and could be quickly adopted, especially when considering the low computational costs of the model.

However, if the authors and the editor decide that the current sensitivity analysis is sufficient for convincing readers of the conceptual model framework, I will not stand in the way of publishing the manuscript (once the minor comments below have been addressed). As I said before, the authors did a really good job in setting up this new Earth system model framework which fills the gap between conceptual box models and Earth system models of Intermediate Complexity.

Specific comments (only minor comments):

Line 188: kice: technically you need to distinguish between kice and kland with kland=1 always (or in a similar manner). Otherwise, it looks like you are changing the size of the effective runoff also over ice-free areas.

We added a piecewise equation (eq. 11) to formalize the text saying that kice=1 over ice-free land, and modify the text to clarify that we test the sensitivity of kice over glaciated regions (holding kice over land constant).

Lines: 264 "and carbonate [C]sil,eq 2 times greater" I find this difficult to understand - please rephrase.

Thanks, we revised the text to clarify that max equilibrium carbonate concentrations are two times greater than silicate.

Line 413+414: I think this should be equation 10, right? There is no kice in equation 9.

Thanks for catching this. We changed to equation 10 (and also refer to the added eq. 11 for completeness).

line 415+416: "For example, in Figure 3B, kice is zero, which is why the solid lines go to zero

at glaciated latitudes."

Please delete this comparison to a different model setup (Fig. 3 uses cat-eye geography and different pCO2). It is confusing that you suddenly refer to a different configuration to explain the kice parameter. If you want to give more general information on kice – around line 190 would be a better place.

Thanks, deleted.

Line 416: "Here, we test the model response to varying kice from 0 to 1 (in all other simulations, kice is set to 0)."

This sounds like you change kice "from 0 to 1". Do you mean varying kice between 0 and 1?

Yes, we replaced "from 0 to 1" with "between 0 and 1".

3 Model Experiments:

Third set of experiments – halving of volcanic flux for different kice values

I misunderstood the experiment setup in the 1st version – two things were unclear from the text (to me):

1) that the initial state was ice-free

2) that kice is kept constant during the experiments. I thought kice was changed from the default value

simultaneously with halving volcanic outgassing.

I only understood this from reading your response – in particular the last part (lines 512 - 517). It would be good to include this information to “3 Model Experiments” as it describes the experiment setup and not the results. (Also thanks for including the iceline subfigure to Fig. 6.)

Thanks, we see how this was confusing and have clarified the model setup in section 3. The text now includes “Starting from an ice-free climate for each of the five glaciated kice values,… . The different kice values have no impact on climate until the decrease in volcanism is sufficient for glaciers to form.” (lines 422-424).

Section 4.2: I still think this Section is a little low on content and could be improved. But that might be a personal preference and I am okay if the authors decide to keep the section as it is.

We appreciate the point, though this section is primarily meant to show the model captures a basic perturbation. We don’t want it to be lengthy.

Section 4.3: Please define ΔT in Fig. 5 (is it ΔT of global mean surface temperature or does it take spatial differences into account)?

ΔT is global mean surface temperature change relative to the expected change if variables added linearly. Changes in surface temperature are not spatially uniform, so ΔT is sensitive to regional effects. We clarify that ΔT refers to global mean surface temperature in the caption of fig. 5.

Figure 6: I was still confused why “Ice sheet growth limits runoff”. I think the note you added to the caption of Fig. 6 (“Note that (D) refers to terrestrial runoff relevant for weathering (which accounts for changes k ice).”) is very important. So Fig. 6D is not “Global mean runoff” but “Global mean effective runoff”? If yes, I do understand Fig. 6 and please change the y-axis label accordingly.

It is effective runoff, we’ve changed the y-axis and caption to reflect this.