Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2016-53-RC2, 2017

Earth System Dynamics Discussions

Interactive comment on "The importance of terrestrial weathering changes in multimillennial recovery of the global carbon cycle: a two-dimensional perspective" *by* Marc-Olivier Brault et al.

Anonymous Referee #2

Received and published: 3 February 2017

This paper describes a terrestrial weathering scheme for the UVic model that takes into account the spatial variation of climate and rock types to calculate weathering rates. The reaction of the model to future warming is accessed. The study is one of only a few that look at the effect of using a 2D weathering scheme in climate models. The model seems appropriate for this study and the paper is appropriate for Earth System Dynamics. Overall I think the paper is interesting, well written and should be published after some relatively minor revisions.

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General Comments:

A major weakness, in studies of long term weathering, lies in our poor understanding of the processes involved. While we may be able to roughly estimate current rates of weathering, future weathering rates are poorly constrained. The paper concludes that the effect of changes in weathering is small, on timescales less than 1000 years (consistent with other studies), and that changes in vegetation have a greater effect than changes in temperature and runoff. How robust are these conclusions? Perhaps it is impossible to know. This is not a criticism of the authors or the paper but an inevitable issue when modelling poorly understood processes. While the authors do recognize that the results are quite uncertain, it would be useful if they could at least try to quantify this uncertainty?

Specific Comments:

Page 9, Line 20: Given that you mention the GEOCLIM model is one of the few other models that have attempted a 2D weathering approach, it would be useful to briefly describe the differences between your implementation and what was done in GEOCLIM. You do provide some details with how 2D weathering was done in GENIE but not in the GEOCLIM (FOAM -LPJ) model.

Page 11, Line 22: It is actually the "net" sedimentation rate of $CaCO_3$, which is the sedimentation rate minus the dissolution rate. You might want to call this the net burial rate to avoid confusion.

Page 14, Lines 8-9: If I am reading this correctly, it seems odd to me that you would have one scheme for steady state weathering, which depends only on runoff, area of a rock type and a constant weathering rate multiplier, while changes in weathering also depend on terrestrial biological production, a different form of runoff and temperature. Would it not be possible to derive global average steady state weathering for each rock type, using global average runoff (presumably this is what is done in a 0D model), and then apply different steady state weathering rates spatially, depending on deviations of

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NPP, runoff and temperature from their global average values? From your description, I am assuming it is not done this way. Is there a reason not to? It seems more consistent to me. This would be a test of the robustness of how weathering rates change with (spatial) changes in climate. If you could still generate reasonable weathering rates spatially, it would help validate your parameterizations. This may be one of the only tests you can do. While it is not critical to change this, perhaps you could discuss this possibility.

Page 15, lines 14-17: In the version of the UVic model without interactive weathering, the global weathering rate of CaCO3 during the spin-up is set to be equal to the global net sediment accumulation rate of CaCO3 which ensures carbon conservation. The model spins up to a steady state within about 10,000 years. How was the spin-up done with the interactive weathering model? What is the net sediment accumulation rate compared to the overall weathering rate? It seems that either the global average weathering rate or the global average biological production that leads to net sedimentation of CaCO3 should be adjusted so that they are equal under a specified level of CO2. If not, then you will have drift in ocean carbon and alkalinity. If you are not doing this, how much drift in DIC and alkalinity do you see after your spin-up?

Page 21, line 1-2: It might be clearer if you say: “immediately balanced by an equivalent outgassing of carbon from the ocean”. When you use “uptake” it sounds like you mean uptake by the ocean not the atmosphere. I realize you say uptake “from” the ocean so what you are saying is right - I just think it would be clearer to use the term outgassing.

Page 21, Lines 2-8: I am not sure I follow this. I would expect little delay - if you draw down a unit of CO2 and send it to the ocean as DIC, the ocean should outgas a unit of CO2 again, but if there is a significant delay in terms of alkalinity (as you suggest - although why is unclear), why not take CO2 from the atmosphere, put it in the ocean and let CO2 outgas naturally? Although I do not think this is necessary, it does seem more logical. Am I missing something?

Page 21, Lines 13-16: Why, if your weathering fluxes are steady, would it take so long to each equilibrium? Why is there any difference in equilibration time, compared to the default model? It is the slow reduction in CO2 that causes long equilibration with silicate weathering (given a CO2 perturbation), but if CO2 is fixed, why does this take so long? I am not sure I understand this. Is it really just that weathering does not equal sediment burial and any drift makes it look as if it is taking longer to equilibrate? If this is the case, then it will only reach "equilibrium" when you allow CO2 to change (changing the climate and thus weathering), and that would take a long time.

Page 22, Line 2: "Table 1" should be "Table 2"?

Page 24, Line 23: is the difference in CO2 between A0 and A2 really 164 ppmv at year 12000? It does not look like it in figure 3a – closer to zero maybe. Do you mean the 0D vs 2D was different by 164 ppmv? That would not be too surprising and not really comparable to Meissner et al. 2012.

Page 25, Line 10: "indifferent" seems a bit anthropomorphic - more poetic though.

Page 25, Lines 13-14: What do you mean here? The reaction of all the reservoirs seem different to me (and not unexpectedly). The land reservoir is behaving a bit like the atmosphere and the sediment a bit like the ocean. The land and sediment are certainly not behaving any more "differently" than the ocean and the atmosphere. If you mean the land and sediment react differently to large or extended emissions - that is not clear either. I must admit the sediment reaction does seem a bit odd. I would have expected the carbon content of sediment to decrease at some point (see next comment).

Page 25: Lines 18-20: It is not really clear that you are really plotting the change in CaCO3 mass in figure 3b. I suspect this is the change in total buried mass. This may be mislabeled in the model output. The buried mass of CaCO3 should be reducing for at least some of this time period but total mass (which includes clay) may well keep increasing. The reason I suspect this is the case, is that the change in the total mass

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Discussion paper

of CaCO3 should just be the integral of the difference between the accumulation and dissolution rates (or the integral of net burial). If you look at figure 6b or 8b, the change in dissolution rate is more than double the change in accumulation rate and the percent of CaCO3 in the pore layer heads lower after year 3000. This should mean that you have a negative change in total CaCO3 (which is what you would expect with carbonate compensation) and yet we do not see this in the change in buried mass. Even if the total buried mass of CaCO3 is still always increasing (burial rate is still positive), the slope must be decreasing after year 3000 since the change in dissolution is much greater than the change in accumulation. This is not obvious in the figure. I think your plots of CaCO3 buried mass need to be checked and possibly corrected.

Page 26, Lines 4-8: The pattern of warming looks pretty standard to me. Extra warming at the poles from polar amplification, more warming over land than ocean, more warming where vegetation has increased (including the tropics). Why does this need explanation? Why would static wind fields necessarily trap more heat in the tropics? Changes in wind fields could cause more divergence or more convergence of heat in the tropics. Atmospheric reorganization does not guarantee increased heat transport out of the tropics. Much of the heat transport in the UVic model is diffusive and that certainly does not trap heat in the tropics.

Page 26, Lines 20-22: Are you suggesting tropical forest die off in SE Asia and that they eventually grow back (by 12000 when NPP is similar again to PI)? Do other tropical forests show die off?

Page 27, Lines 2-3: What do you mean here? Do you mean statistically correlated? Can you state the strength of the correlations? Given that you have defined weathering to be basically linearly dependent on NPP but exponentially dependent on runoff, does comparing correlations (at least linear ones) mean anything?

Page 27, Lines 7-9: The significant changes in weathering near Kazakhstan and Zambia are curious. Nothing stands out in rock type, temperature, NPP or runoff that would cause these large changes in weathering. Is this somehow an odd overlap of many factors? Something related to your complex NPP parameterization? Can you nail this down a bit?

Page 28, Lines 1-4: This leaves me wondering how robust these results are? Are these differences in dependences real or just due to uncertain parameter choices?

Page 28, Lines 9-15: I agree this is interesting but it would be more interesting if you knew why. Can this not be diagnosed from model output? Your explanation here is confusing. Is there a large decrease in vegetation NPP in A0 that would not affect B1 (or B2)? Is this from the drop off of NPP in SE Asia after year 3000 (as in Figure 4b)?

Page 29, Lines 9-12: This is the bit that I find really confusing. If the percent of $CaCO_3$ in the pore layer is decreasing, then likely so should the total. This change in the total amount of $CaCO_3$ should be checked against the integral of the net accumulation of $CaCO_3$ (accumulation minus dissolution) at the sediment surface (see comment Page 25: Lines 18-20).

Page 29: Line 15: Should both "were"s be "are"s - given that we are not talking about figures in the past tense?

Page 29, Lines 20-22: I am assuming that you mean the pattern of weathering between B1 and B2 would look the same but the magnitude would be a bit higher in B1 (given that globally increased CO_2 would increase weathering everywhere). Is that correct?

Page 31, Lines 20-22 and Page 32, Lines 1-2: I agree it must be deep alkalinity changes that are helping preserve the sediments and that both silicate and carbonate weathering are helping with this (comparing C1 or C2 to C3). Presumably silicate weathering (C2) is more effective at preserving sediments because it is also reducing DIC (compared to C1) and thus pCO_2 and acidity. Would you agree?

Discussion: This section seems a bit speculative without more references.

Page 32, Lines 15,17: It seems to me that your parameterization of NPP is only quasi linear and runoff is exponential, so I am not sure you can say that it varies linearly.

Page 33, Line 1: Do you mean “rely heavily on the ratio of NPP or runoff and their pre-industrial values” rather than “the ratio between initial weathering and initial NPP/runoff”?

Page 33, Lines 12-23: There is no doubt that the UVic model has a simplified atmosphere and this may limit a number of potential feedbacks. What is not clear, is how important these feedbacks are in the context of this paper. Expansion of atmospheric cells and poleward shifts in wind patterns with warming are, to a certain extent, captured by parameterizations in the model. It is not clear that the UVic model is overestimating tropical temperatures 1000 years into the future (as you also suggest in Section 3.1). What evidence do you have for this? Perhaps a more general statement about climate model uncertainty at these time scales would be more appropriate.

Page 34, Lines 1-2: Reference?

Page 34, Lines 5-19: Do you really think that sea level rise will inundate enough weathering active areas to make much of a difference. I am skeptical. Do you have any references?

Page 34, Line 20-21: The statement that “There has been an extensive discussion in recent years” is just begging for a reference.

Page 35, Lines 1-4: What processes? Reference?

Page 35, Lines 5-11: References?

Page 36, Lines 12-15: Again, I wonder how robust this is considering the uncertainty in the parameterizations.

Table 2: The caption mentions “pulses” even though I think few people would consider emissions spread over 1000 years to be a “pulse” (as in A2). I would just remove the references to pulses. Maybe something like: "The emission total is the total amount of

carbon emitted after year 2000, while the emission period represents the time span of emissions; the emission total is divided equally among the number of time steps during the emission period."

Figure 1: The caption for Figure 1 suggests that there are 3 panels (a), (b) and (c). Where is panel 1c? I think what is labeled as Figure 1b is really referred to as Figure 1c in the caption and the original Figure 1b was dropped. Is that correct? It seems reasonable not to include the original Figure 1b but the caption for Figure 1 should then be corrected.

Figure 2: The caption for Figure 2 refers to Figures 1b and Figures 1c. Figure 1c does not exist and it either needs to be included or the caption needs to be corrected.

Figure 3: In the caption for Figure 3b, maybe change "budgets" to anomalies or differences from pre-industrial. Budget is a bit ambiguous. Why do the lines for A0* and A1* stop before 12000 for DIC in Figure 3c but not the other panels?

Figures 3, 6 and 8: This is more a matter of personal style but I find it a bit distracting having the vertical axes labels switching from left to right on your line plots. I would think figures would be more compact and thus could be made larger if the axis were all on the left. The other thing that is a bit distracting is the change in horizontal scale which adds artificial breaks in the slopes of the lines. Would it be possible to show a small break in the line when you change scales just to emphasize that the slope is not really continuous? I wonder if this many scale changes are really necessary? The first vertical dashed line does not even seem to indicate a scale break and the last line is superfluous.

Figure 6: Again, in the caption for (b) maybe change "budget" to anomaly or difference from pre-industrial.

Figure 8: Change the caption for (b) as for Figure 6.

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