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Comment

Interactive comment on “Burial-nutrient feedbacks amplify the sensitivity of carbon dioxide to changes in organic matter remineralisation” by R. Roth et al.

Anonymous Referee #1

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The manuscript by Roth et al. describes the impact on atmospheric CO₂, ¹³C and nutrients distribution of a deepening of the Particulate Organic Matter (POM) remineralization depth.

100 kyr long sensitivity experiments are performed with an Earth System Model of Intermediate complexity, which also includes a sediment model. It has been previously suggested that a deepening of POM remineralization during glacial times could have played a role in decreasing atmospheric CO₂ (Matsumoto 2007, Matsumoto et al. 2007, Kwon et al. 2009, Menviel et al. 2012 and Chikamoto et al. 2012). This study is thus relevant to understand glacial/interglacial changes in the carbon cycle and I recommend its publication in ESD with minor revisions.

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1) Introduction:

In general I find the Introduction a little messy with some inaccuracy and redundancies. Some specific examples include:

a) the latest references have been chosen to illustrate the hypotheses brought forward to discuss glacial changes in pCO₂ whereas I think that it might make more sense to cite the original reference. For example, even if Volker and Kolher 2013 paper is very interesting I think it makes more sense to cite Toggweiler et al. 2006.

b) Please do not only quote the abstract of Kwon et al. 2009, but instead it might more relevant to say that a 24m increase in the e-folding depth of the POM remineralization leads to a 10ppmv decrease. The 27 ppmv is obtained when export production is kept constant.

c) the impact of changes in viscosity on the speed of temperature (Taucher et al. 2014) is discussed both on lines 14 and 28 of p475.

d) A little more information on Menviel et al. 2012 study in the introduction would be relevant. For example that a progressive increase of the POM and DOM remineralization depth over the glaciation led to a 31 ppmv pCO₂ decrease, while the increase back to initial condition during the deglaciation led to a 21 ppmv increase, thus suggesting that ocean-sediment interactions could play a role in amplifying the pCO₂ decrease and thus motivated the present study.

e) At the end of the introduction L14-18, I am not sure I agree with “previous estimates ... underestimate the long term feedback”. I would suggest to rephrase that sentence as either long previous studies could not study that feedback due to their model/experiment set up or they did point out that on timescales greater than 10kyr ocean sediment interactions could amplify the signal.

2) Experiments:

This section needs some revision. The “closed system” set up might need some more

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explanation: usually in a closed system the riverine input balances the burial of organic matter and/or CaCO_3 . Apparently here another method is used, indicating that the sediment model has been disabled, therefore there is no burial.

Also I don't see how that is similar to "Kwon et al. 2009" as they use in their experiments "surface nutrient restoring" or "constant export".

L 2, p483. Please rephrase as "Our discussion mainly focuses on . . ."

L.5-6, p483, please rephrase.

3) lcalc:

Experiments in which lcalc is changed are not described at all in the experiments section. It is as cryptic in most of section 3.2. as it is not mentioned in the text by how much lcalc is changed or any specificity. One has to wait until the end of paragraph 3.2. to get an example on what has been changed. Figure 10, where the lcalc results are shown is only discussed in the following paragraph.

4) EOF:

EOF are a useful statistical method to highlight modes of variations and is widely used in oceanography, meteorology. . . Here the authors spend some time explaining the method in section 2 and mention their EOF a little bit everywhere (abstract, introduction, section 3.1).

While I have no doubt that EOF can be very useful I am a little bit more skeptical about their use here. Moreover because the 2nd EOF explains only 0.27% of the variance. Technically one would say that the 2nd EOF is not significant. A deepening of the POM remineralization leads to a DIC increase at depth, while the alkalinity might be little affected at first. This thus induces a decrease in deep $[\text{CO}_3]$ and eventually dissolution of CaCO_3 . Deep $[\text{CO}_3]$ thus increases again. SO basically it seems that the 2nd EOF is showing this initial deep $[\text{CO}_3]$ decrease. If the only thing you want to show by using the EOF is that deep $[\text{CO}_3]$ first decreases before increasing, there might be an easier

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and cleaner way to do that than using the EOF. I would thus suggest taking out all that is related to the EOF in the paper.

For example I think that the “closed system” experiments provide all you need to make the point. I would first suggest to add the evolution of POM export, calcite export. . . for the closed system in Figure 3. Then you could add in the text that the experiments in the “open system” follow the ones of the “closed system” for a few thousand years after which ocean-sediment interactions start to play a significant role. Another way would be to make Hovmoller diagram as the one shown in Fig8b.

5) C13:

L2, p 491, I would avoid “isotopically enriched calcite”. And I doubt this has a significant effect on deep d13C. Additionally the changes in d13C simulated are fairly small. I am quite surprised by the fact that the simulations do not reach an equilibrium after 200ky.

6) Conclusions: L 21, p497: Deep Atlantic Cd/Ca content was increased during the LGM indicating a greater deep [PO4] content.

L25p 497: “ a 25m increase in the e folding. . .”

There are some typos throughout the text. Some examples: L3, p479 : “calcifier” iof “calcifer”.

L 28, p 481 “so” is missing.

L16, p483 “induced”.

L 16, p 488 “ As a result”.

L8, p 495, “the experiments”.

L 14, p 495 “fixed”.

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