

Interactive comment on “Mysteriously high $\Delta^{14}\text{C}$ of the glacial atmosphere: Influence of ^{14}C production and carbon cycle changes” by Ashley Dinauer et al.

Anonymous Referee #1

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This paper describes simulation experiments with the Bern3D model in order to understand past levels of atmospheric $\Delta^{14}\text{C}$ reconstructed in the international effort IntCal13, especially how can be explained that the reconstructed values over the last 50 kyr went up to about 500-600‰. The conclusions made suggest that the consideration of sedimentation in the model is crucial for the understanding of ^{14}C , and that reconstructed ^{14}C production rates based on either geomagnetic field strength or ^{10}Be and ^{36}Cl from ice cores are not sufficient to produce with the Bern3D model atmospheric $\Delta^{14}\text{C}$ that is in reasonable agreement with reconstructions. The authors therefore suggest, that alternatively ^{14}C production rates based on their model output might be taken in the future, leaving the ultimate question why reconstructions and model results differ for

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future efforts.

The study is timely and well set, and results are explained very well. I nevertheless have a few comments, which I ask to consider in a revision, which are partially fundamental. I believe in a necessary revision following especially my fundamental remarks below some of the more general remarks (especially in the abstract, introduction and conclusions) need to tone down and give also some more room for the shortcomings of the study itself (e.g. constant weathering rates, potentially missing silicate weathering, potentially missing volcanic CO₂ outgassing).

Fundamental remarks:

1. Weathering vs sedimentation: It is said that when sedimentation is included in the model the atmospheric $\Delta^{14}\text{C}$ strongly decreases in comparison to an atmosphere-ocean model version only. I believe this is naming the wrong process. My understanding of the model description is, that weathering is the process that brings ¹⁴C-free C into the system, so it is carbonate weathering, that is fundamental for the ¹⁴C cycle. It is clear that once weathering input (of alkalinity and DIC to the ocean) is considered also sedimentation as sink needs to be implemented (otherwise the carbon cycle would run away with an ocean accumulating alkalinity, and subsequent changes to atmospheric CO₂ levels), but sedimentation is not the important process here that changes $\Delta^{14}\text{C}$. This might then also lead to a different name of the model configuration now called OCN-SED.
2. No details on weathering are given, but since it is said, that ¹⁴C-free C is entered via weathering I have to assume, this implies carbonate weathering. However, it need to be clarified (and maybe corrected?), that in carbonate weathering, 50% of the carbon that enters the ocean as weathering product (bicarbonate ion, HCO₃⁻, which changes DIC and alkalinity in the ocean) comes from rocks

(^{14}C -free), and 50% has its origin in atmospheric CO_2 with its atmospheric ^{14}C -signature. For silicate weathering, also bringing HCO_3^- to the ocean, 100% of the carbon has its origin in the atmosphere. Is silicate weathering considered? For details see, for example, Colbourn et al. (2013). Without checking on recent updates, I believe both silicate and carbonate weathering contributed about a similar amount of HCO_3^- input into the ocean. At least in a study some years ago (Hartmann et al., 2009) in present day weathering the CO_2 consumption is twice as big in silicate than in carbonate weathering, but since in carbonate weathering 50% of the C has its origin from rocks, both processes should contribute about the same. Since weathering is the relevant process for this paper more details on its implementation in the model should be included. From the Appendix I understood, that weathering rates are constant in time, but please give their numbers, which would be especially of interest to other modellers doing similar things. Also consider in a discussion, that missing temporal changes in weathering rate might be one reason why reconstructed $\Delta^{14}\text{C}$ (and CO_2) is not met with simulations. Having found, that the input of ^{14}C -free carbon to the system is so important for an understanding of $\Delta^{14}\text{C}$ brings me also to the question if ^{14}C -free CO_2 outgassing from volcanos is considered, which might have similar effects on ^{14}C . I understand that this has been investigated previously with the Bern3D model (Roth and Joos, 2012), but with focus on ^{13}C . Maybe some more insights from previous simulations are possible here, at least in a discussion. At least please mention the applied CO_2 volcanic outgassing rates. Note, that there is a fundamental, analytical derived solution from the steady state assumption on volcanic CO_2 input being 50% of the CO_2 consumption by silicate weathering, which is of relevance for times longer than 100 kyr (briefly mentioned in Munhoven and François (1996) or in depth discussed on pages 80-81 of Munhoven (1997), <http://www.astro.ulg.ac.be/~munhoven/en/PhDIndex.html>). For shorter periods such as the last 50 kyr considered here, differences from this numbers are certainly possible, but this relationship gives a rough guideline, and

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- might explain long-term drifts in the C cycle, if not obeyed. Taken together, I have the impression, that no silicate weathering, and also no volcanic outgassing of CO₂ is considered here, which would indicate according to this theory no drift in the system, but also the missing of two important processes. If so, I am not saying, these should be implemented in the revision, but it needs to be stated clearly if and how they are (not) included. How does your weathering flux compare to others, e.g. Fig 7 in Brovkin et al. (2012) or Colbourn et al. (2013)?
3. Earlier simulation studies have shown, that to get the ¹⁴C cycle right, one needs to have the C cycle right as well. Köhler et al. (2006) has shown that previous studies (Beck et al., 2001; Hughen et al., 2004) focusing only on ¹⁴C, but showing no simulated CO₂, they therefore have very likely some deficits. For atmospheric Δ¹⁴C especially the air-sea gas exchange is important, which depends similarly on the gas exchange velocity (k_w , which is considered here in sensitivity experiments), but also on the CO₂ gradient between atmosphere and surface ocean. This implies that whenever simulated CO₂ differs from reconstructions there will also be an offset in simulated Δ¹⁴C from data. In a recent simulation effort for IntCal20 (the successor of IntCal13) the marine surface Δ¹⁴C has been simulated (Heaton et al., submitted). There, the importance of time-dependent changes in CO₂ has been as important for the simulated surface ocean Δ¹⁴C as that of climate change (temperature change, ocean circulation change etc), which via gas exchange would also feedback to atmospheric Δ¹⁴C. This is unpublished so far, but since it is submitted and will probably be available in due time I nevertheless mention it here.
 4. The coauthor Florian Adolphi is also coauthor of the now submitted IntCal20 effort (updating the atmospheric Δ¹⁴C record), (Reimer et al., submitted) and should therefore be aware of the large changes which occur between IntCal13 and IntCal20, namely the amplitude of the Δ¹⁴C maxima around 40 kyr increases in IntCal20 towards the Hulu Cave numbers. Maybe this should be briefly discussed

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in an outlook.

Minor issues in chronological order:

1. The decay constant of ^{14}C used here is based on a half-life of ^{14}C of 5700 yr (here) but of 5730 yr in Intcal13 (and IntCal20) which produced the atmospheric $\Delta^{14}\text{C}$ record. If you consider the decay of ^{14}C over 40 kyr (the time of the maximum in atm $\Delta^{14}\text{C}$) with either 5700 vs 5730 yr you get a 2.5% smaller number when based on 5700 yr, although the half-life time differed only by 0.5%. This difference is small when compared to the difference of IntCal13 and the Hule Cave data, but should nevertheless be mentioned.
2. line 173: Please be specific, how ^{14}C is fractionation corrected.
3. line 180-181: Weathering is prescribed as constant input of DIC, but no ^{14}C . See fundamental comment above, but no matter where the C of weathering comes from the input in the ocean should be a HCO_3^- , changing both DIC and alkalinity in the ocean. I hope this is only a too simplified description here, but has implemented correctly in the model. Please revise.
4. line 184: For the preindustrial spin-up CO_2 is fixed to 278.05 ppm. Why this number, would not 278 ppm do the same job? Is this OCMIP protocol? Also: What would be the internally calculated preindustrial CO_2 ? By prescribing CO_2 concentration during spin-up C is added or extracted from the simulated system, which might be a potential source of bias. How long is the spin-up time?
5. line 229: Consider citing the underlying ice core paper, from which the greenhouse gases splines provided by Köhler et al (2017) have been calculated.

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6. line 232: Global benthic $\delta^{18}\text{O}$ is not only a global ice volume proxy, but has also a considerable contribution from deep ocean temperature, see papers of the van de Wal group from Utrecht University on the deconvolution (e.g. Bintanja and van de Wal, 2008). Taken this knowledge into consideration, would this change your approach how sea level is changing? You might also discuss how different sea level reconstructions vary, e.g. see three different sea level reconstructions in Fig 1f of Hasenclever et al. (2017), and what this uncertainty in sea level might introduce into your approach.
7. lines 223-236: Model description says that greenhouse gas radiative forcing has been taken from data, thus I assume that CO_2 seen by the carbon cycle is never prescribed, but always model-internally calculated. Please state this explicitly (or the correct version of this sentence, if this was not the case). However, you might also consider one scenario in which CO_2 is prescribed for the C cycle from data (similar as in Butzin et al., 2017, in which atmospheric CO_2 and $\Delta^{14}\text{C}$ has been prescribed by data), since this would bring your simulated C cycle as close to observations as probably possible, which might further reduce the bias in ^{14}C (see also fundamental remarks above).
8. Obtained surface reservoir ages (Fig 8c) might be compared with data and other models, e.g. see Butzin et al. (2017); Skinner et al. (2019). Benthic-atmospheric offsets (Fig 8d) might be compared for the LGM with the data compilation of Skinner et al. (2017). Note, surface reservoir ages might vary a lot as function of latitude, so this Fig 8c needs more information on averaging; even better: might be revised and thus restricted to sea ice-free areas only.
9. Please state somewhere the absolute (PI) values of those parameters which are changed in your sensitivity experiments, maybe in Table 1?
10. Table 2: ^{14}C production rates is given in relative units, relative to what? Probably preindustrial state. I also do not remember if the ^{14}C production rate in absolute

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numbers is once given in the manuscript for preindustrial state, please insert somewhere.

11. Fig 4 captions does not need a description of the different colors of the lines, since a legend is given in the figures themselves.

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