Review on

Mysteriously high $\Delta^{14}C$ of the glacial atmosphere: Influence of ^{14}C production and carbon cycle changes

by A Dinauer et al

submitted to Climate of the Past, article reference: cp-2019-159

Date: May 20, 2020

I think the authors have done a great job in their response to the reviews and in their submitted revision.

Below are some replies to their response on the reviewers, which might need some fine tuning:

 Reviewer 1, fundamental points on weathering and sedimentation. I understand that you take the fluxes that leave to the ocean to the sediment as those which need to be put back in the ocean as weathering, sidestepping the atmosphere, as you said. Therefore, we find a closed loop of ocean-lithosphere interaction in Figure 2.

The given stoichiometric ratios of Si:Alk and C:Alk for the given equations however do not make sense to me, since the atmosphere from which CO₂ comes from, is ignored. For example, for carbonate weathering it is said C:Alk is 1:2, but the input into the ocean is 2 HCO₃⁻, thus 2 C and 2 Alk, which I would consider as 1:1 ratio in C:Alk. Or do I have to understand these given ratios, that you indeed put the losses in the deep ocean (here CaCO₃ or CO₃²⁻ with a C:Alk of 1:2) as such in the weathering fluxes to the surface ocean? If so, it would imply that you do not put HCO₃⁻ as weathering fluxes in the ocean, but CO₃²⁻, which needs different wording, (e.g. compensation fluxes for sedimentary loss). In this respect in lines 205-206 and 868-869, there is something misleading: You mention there is an alkalinity input of 34.37 Tmol HCO₃⁻/yr. However, the input of HCO₃⁻ is a change in both alkalinity and DIC, so it is not clear to me if this fluxes is already included in (or in addition to) the DIC flux of 0.46 GtC/yr (which equals 38.33 TmolC/yr). However, maybe all is correct in a condensed sense which implicitly assumes the effect of the sidestepped atmosphere, but it is maybe worth to rethink these statement. Since this discussion is only contained in the rebuttal, but not in the revised draft, it would probably not change the draft, but a clarification might nevertheless be helpful. Maybe you then also reconsider, that some of what is said here might be added to the draft (e.g. the Appendix with the model description).

- 2. Reviewer 1, minor point 1: It is not enough to mention in the caption of Fig 1, that the considered 14C half-life of 5700 years has been accounted for, this also needs to be stated in the main text, when Intcal13 and Hulu Cave 14C are introduced.
- 3. Reviewer 1, minor point 5: Considering to cite the underlying ice core papers (suggested by the reviewer) was referring to the papers with the data, not with the spline method, as done now by citing Enting (1987). In Köhler et al. (2017) the data availability section ends with "When using these data, please consider citing the original publications from which the data underlying this compilation have been taken.", which would imply for the CO₂ spline of the last 55 kyr something like the following: The spline combines raw data from Talos Dome, Siple Dome, WAIS Divide, EPICA Dome C, EPICA Dronning Maud Land and Law Dome each on the most recent age scale, e.g. AICC2012, GICC05, WD2014. The relevant ice core data papers underlying the spline (Ahn et al., 2012; Ahn and Brook, 2014; Bauska et al., 2015; Bereiter et al., 2012; Lüthi et al., 2010; MacFarling-Meure et al., 2006; Marcott et al., 2014; Monnin et al., 2001, 2004; Rubino et al., 2013) and those related to the age models (Buizert et al., 2015; Sigl et al., 2016; Veres et al., 2013) are those which have been suggested to be mentioned by the reviewer initially.

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