Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-159-RC2, 2020

Climate of the Past
Discussions

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

Luke Skinner (Referee)

luke00@esc.cam.ac.uk

Received and published: 27 February 2020

This is a highly thought provoking, and a substantial, study that tackles a long standing and important puzzle. Arguably, if one wanted an illustration of just how much we have yet to resolve regarding global carbon cycle change on glacial-interglacial and millennial timescales, one need look no further than an attempted explanation of past atmospheric carbon isotope (radiocarbon and d13C) variability. This study demands some stamina of the reader; however the effort is rewarded. The study clarifies a number of fundamental principles concerning radiocarbon cycling and the controls on atmospheric radiocarbon across a range of equilibration/waiting times, and sheds new light on the role of sedimentary fluxes in setting atmospheric radiocarbon activity. Although the latter point is not completely new (e.g. Kohler et al., 2006), and the basic principles of radiocarbon inventory balance are well established, I do not know of any previous systematic exploration of the effects of variable sedimentary outputs on atmospheric radiocarbon.

Probably the main drawback of this work is that a full sensitivity study of the sediment cycle is not provided, and that only sedimentary *feedbacks* on the simulated ocean dynamical/gas exchange changes are explored. We therefore do not get a full 'map' of the expected impacts of variable sediment fluxes (e.g. including reconstructed changes in sedimentary organic carbon and carbonate fluxes over the last glacial cycle). It is natural, and I think a strength of the study, that after mulling over this manuscript a whole host of additional model scenarios come to mind, including e.g. different forcing functions (e.g. scaling tuned parameters to atmospheric CO2 instead of benthic d18O), or different deconvolutions (e.g. of carbonate/POC export, instead of radiocarbon production rates). However, it would not be reasonable to suggest such alternatives as revisions, and therefore I would suggest that this study is ready for publication in Climate of the Past, perhaps subject to technical revisions, in order to give the authors a chance to make small changes in light of comments they receive. Ultimately, i think that the effort required of the reader by this manuscript would be even more greatly rewarded if, by way of conclusion, the authors included a more specific list of the observations/tests that could help to finally resolve the 'atmospheric D14C puzzle', in light of this study's findings.

Below I provide a list of specific comments that I think might be useful to the authors in revising their manuscript for publication.

1. Line 11: I feel that the term 'mystery interval' has become current without having a particularly clear meaning; it seems to be used to refer to a chronozone, for which there already is a name (Heinrich Stadial 1, etc...). Furthermore, the 'mysterious' part of the interval seems to be perceived differently by different people; is it the atmospheric ra-

diocarbon decline, the proposed lack of marine radiocarbon activity increase, the entire ‘mystery’ of deglaciation? Not everyone shares the same notions regarding such ‘mysteries’, particularly regarding the marine radiocarbon inventory change, against which the term ‘mystery interval’ seems to have been directed. I would like to stick my neck out and suggest that this term has served its purpose in stimulating interest in a topic, and no longer serves a purpose for clear communication of a specific idea. I would therefore propose that the authors refer to other more clearly established chronozone designations, or even dates if these are trusted sufficiently.

2. Line 18: I think the word “more” can be dispensed with, here and elsewhere. One wonders: more than what?

3. Line 40: here and throughout the manuscript I was not sure whether “millennial-scale” was a helpful designation, as it made me think of variability associated with Dansgaard-Oeschger events. Perhaps the term, or another such as “short term” etc..., can be defined clearly when first used?

4. Line 60: same thoughts as above regarding the term “mystery interval”; if it is coincident with HS1, then we should use that term instead I think. At the time of the Broecker and Barker (2007) study there was proposed to be a lack of evidence for a radiocarbon depleted ocean interior at the LGM, and a subsequent increase in its radiocarbon activity; however, this is arguably no longer the case.

5. Line 67: probably best to be more specific, e.g. “...used only high accumulation sites, and square barrel gravity cores with minimal sediment disturbance..”

6. Line 72, last sentence of the paragraph: I don’t mean to suggest that ther is anyh I gotta incorrect about this sentence, but I found this to be an odd way of phrasing things. To me there is one question, “why was atmospheric radiocarbon activity so high during the last glacial (including well after the Laschamp excursion)”, which entails a subsidiary question, “how much did production changes contribute to this elevated atmospheric radiocarbon activity”.

7. Line 95: I would say that the time required for ocean ventilation is not “up to”, but rather “over” 1000yrs. Perhaps Primeau (2005) can be referenced for this. 8. Line 100: multi-millennial timescales?

9. Line 106: Andrey Ganopolski would disagree (see Ganopolski et al., CP, 2017). Perhaps this statement should be modified to say that it is currently not possible to do so without the use of any parameterisations of key processes, or something more specific?

10. Line 113: this sentence seems to suggest that the main proposals for explaining glacial-interglacial CO2 involve exchanges with the solid earth, but this is not really true. Arguably, as has been sketched out many times before, including in a recent review (Galbraith 2020), the “ingredients” for glacial-interglacial CO2 change are well accounted for, it is their ‘calibration’ and organisation within an orbital pacing framework that remains elusive.

11. Line 118: in idealised settings..

12. Line 122: here and throughout the manuscript it would be best to suffix D14Catm, so that we know what reservoir is referred to.

13. Line 125: is it not more accurate to state that the production rate is inferred from an atmospheric radiocarbon budget, combined with a range of hypothetical radiocarbon and carbon cycle scenarios?

14. Line 159: air-sea equilibration times are very different, which is potentially important...

15. Line 169: perhaps Stuiver et al. 1978 should be referenced.

16. Line 170: would it be clearer to state that DI14C is simulated, separately from DIC?

17. Line 189: I wonder if this is not a major part of the whole problem with simulating atmospheric radiocarbon in the past? If the modern (pre-industrial) state is in fact far

Printer-friendly version

Discussion paper

from equilibrium then this would mean that production rates are all miscalibrated. Why not explore the possibility that production rates are higher than required for equilibrium, e.g. due to ongoing equilibration of sedimentation following the deglaciation and early Holocene? It seems to me that the very conclusions of this study require that this be explored as a possibility. More specifically, and perhaps I am not getting this right.. we might expect that, following the expansion of the terrestrial biosphere during the Holocene (and the removal of carbon from the atmosphere-ocean system, causing a slow reduction of 'young' carbonate sediment output from the ocean), the radiocarbon inventory of the ocean and atmosphere should be on a slow disequilibrium downward trend, so that a higher radiocarbon production would be needed to get today's radiocarbon activity as an equilibrium state. Is that correct? Or is it the opposite? In any event, one has a sneaking suspicion that this sort of thing might be important here.

18. Line 220: "...levels, given available 14C production scenarios."

19. Line 244: Why was benthic d18O chosen? It is a smooth, slow function that lags behind most of the climatic processes that were important for the carbon cycle. Although it might seem circular, I don't think it is any more ad hoc to scale these parameters to atmospheric CO2 instead.. having rapid jumps in HS1 and the YD, and a faster change than benthic d18O, might help with getting the deglacial CO2 change 'right' (for parameterised reasons).

20. Line 254: the cited study is based entirely on the 'plateau tuning' approach, which may be questioned. Perhaps best to also cite Skinner et al. (2017) who showed that the LGM ocean was 'older' pretty conclusively with a range of other data.

21. Line 283: It seems crucially important to me that the 10Be and 36Cl flux records from the ice cores are NOT consistent with the final age scale that they are all placed on. As far as I can tell from Adolphi et al. (2018), the ice core data were converted to fluxes based on each ice core's individual age scale, and then they were all placed on the GICC05 age-scale, whereas Channell et al. (2018) argued that this age scale

Printer-friendly version

Discussion paper

implies very different fluxes. Surely the ice core cosmogenic nuclide data ALL need to be placed on the same age scale and THEN the fluxes should be calculated and 'stacked'. I think this is a really crucial thing, and I am really confused as to why the specialists working with these isotope records take a different approach that surely produces incorrect fluxes. A basic test I would propose is: are the individual ice core flux records consistent with the accumulation rates that are implied for each ice core by the GICC05 age scale? If not, they need to be corrected, surely. I suspect this will only make matters worse for reconciling everything, but it is still important to consider carefully.

22. Line 355: note again that this conflicts with the premise that the modern state is at equilibrium!

23. Line 448: my intuition tells me that air-sea has exchange may have a small effect, but depending on the circulation state. Is it not possible that changes in air-sea exchange might combine non-linearly with particular changes in the circulation geometry?

24. Line 460: Although I see why the authors try to wiggle free from resolving the deglacial CO2 problem, I think is it entirely possible to set it aside, and I also think it is basically not true that the study deals only with the glacial portion of the record. It is the glacial versus interglacial amplitude of atmospheric D14C that is of concern, and therefore the change across the deglaciation is entirely relevant! In fact, as suggested below, I would propose provocatively that this study shows that atmospheric radiocarbon can be explained reasonably well up until the deglaciation, and that it is the modern radiocarbon activity that defies explanation. I wonder what the authors think of this contention.

25. Line 537, the discussion of simulated B-Atm values: why do the authors not refer at all to published data for comparison? The compilation of Skinner et al. (2017) estimated, with the available data, that the global average ageing of the ocean at the

LGM was ‘only’ ~689 14C years. This is relevant here, and indeed it would suggest that all of the model scenarios produce rather extreme outcomes as compared to available data.

26. Line 545: I think it is worth specifying in what ways these indirect methods are also potentially inaccurate, due to different processes affecting e.g. oxygen and radiocarbon.

27. Line 567: ..is a dedicated ‘control knob’, in the model.

28. Line 605: viewed as tentative, perhaps. The viewing is not tentative; the results are.

29. Line 676: is it worth stating by how much this polar bias would have to be in order to reconcile everything? Is that magnitude reasonable?

30. Line 703: in this paragraph the realism of the implied sea ice changes is discussed, but again no mention is made of what existing marine radiocarbon data imply. These are really important constraints to mention, surely.

31. Line 726: I couldn’t help but feel that the conclusion of the study might be more hard hitting if we had a more specific ‘shopping list’ of things that could help to resolve this puzzle. For example, constraining the global marine radiocarbon inventory change across the deglaciation, estimating any gradient in cosmogenic nuclide production across latitudes (i.e. polar bias, perhaps from tropical ice cores?), estimates of global carbonate/POC export rates (which already exist incidentally; Cartapanis et al., 2016; 2018), etc...

32. Table 1: it would be helpful to specify here which simulations have active sediments included. Incidentally, why was the rain ratio changed in one simulartion?

33. Fig 3, caption: I think it is more mathematically correct to state <100m and >1500m, no?

34. Fig 7, caption, line 1203: I think it would be helpful to state "...using the mean reconstructed palaeointensity.."

35. Fig 8: shouldn't all the simulated D14Catm traces start at the same value and end at different values? Although this might look nasty, it suggests a different outlook in my view. Incidentally, the outputs in plots c and d are obvious candidates for comparison with existing data (e.g. Skinner et al., 2019, 20176), perhaps for a future study if not this one.

36. Fig 9: this is a fascinating figure, though I find it slightly problematic. First, what is the rationale for normalizing to the average D14Catm value 0-50ka? I think that plots a and b should be replaced with normalization to the final 'modern' value, and that plots c and d should be extended up to the present. The latter is surely important, as it shows how we (well, you!) can do a pretty good job at simulating the amplitude of D14Catm change in the glacial when tweaking all the model's knobs, but that we can't subsequently get the deglacial change to the modern value, just as we can't quite get the deglacial change in CO2. I feel this must be significant... I wonder what the authors think.

37. Figure 10 and 11: I would suggest including a narrow plot at the base of each of these showing the offsets between simulated and observed values over time.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-159, 2020.