

Interactive comment on “CH₄ and N₂O fluctuations during the penultimate deglaciation” by Loïc Schmidely et al.

Luke Skinner (Editor)

luke00@esc.cam.ac.uk

Received and published: 9 February 2021

Dear Loic, if I may,

The review/discussion phase for your manuscript has now ended, and you are invited to respond to the review comments. I would like to add an editorial perspective, in case this helps to guide your response, and make the process more efficient.

Based on the review comments that have been received, I see there to be two major issues that must be addressed in a substantially revised version of your manuscript, before it can be considered for publication.

The first, emphasised primarily but not exclusively by Reviewer 2, is that your manuscript serves as presentation of an updated methodology, and yet appears to

[Printer-friendly version](#)

[Discussion paper](#)



fall short of what one might expect for a ‘method paper’. As suggested by Reviewer 2, there are ways to do this without interrupting the flow of a paper that also seeks to present novel ideas and interpretations on palaeoclimate events and processes; however, I would suggest that one of these is to include a dedicated section that is clearly signposted after the introduction (e.g. “In section 2, we describe a revised method. . . Readers who are less concerned with the details of the methodology are invited to skip to section 3, where we discuss. . . etc. . .”). I think the preference for a dedicated section versus an Appendix should be premised on the length of the text required, and whether or not the manuscript seeks to serve as a viable reference for the updated methodology. To me, this seems inevitable, because the new method has not been described elsewhere. A side note here, is the comment of Reviewer 1, that having developed and presented an updated methodology, a lack of confidence in the accuracy of the resulting data is suggested by the arbitrary correction that is subsequently applied – this deserves some discussion and context in a revised manuscript version.

The second major issue, emphasised primarily by Reviewer 1 this time, is that the main findings presented (the existence of centennial/millennial events within the HS11 ‘complex’, and thus across TII, are not in themselves entirely new, and that some aspects of the interpretations appear to be somewhat confused (e.g. the concept of Heinrich stadials, HS, merging into ‘non-Heinrich, DO-, stadials’ – opening up a terminological can of worms that is not resolved at all). My own feeling is that some confusion does indeed arise from the manipulation of concepts and terminology (generally from previous studies), while becoming somewhat detached from relevant observations (e.g. the end of a ‘Heinrich event’ would need to be defined in terms of ice-rafted debris deposition, etc. . .). In this respect, I find that Reviewer 1’s comments have some traction, and should not be cast off lightly. My own suggestion is that the critique or Reviewer 1 could be addressed through a greater attention to relevant studies/datasets (for comparison), and perhaps also some effort placing key records on consistent age-scales etc. . . so as to show clearly the associations between e.g. AMOC, ocean oxygenation, North Atlantic ice-rafted debris deposition, terrestrial climate, sub-tropical hydrological

[Printer-friendly version](#)[Discussion paper](#)

cycle, and polar ice-core archives, so as to make an observationally supported proposal for the mechanisms behind the events that you identify in your CH₄ and N₂O records. Here, I would like to emphasise that the key point made in your manuscript, regarding the existence of at least two different ‘types’ of sub-millennial event across TII and within the ‘HS11 complex’ (e.g. Tzedakis et al., NComms 2018), is sufficiently interesting and important to deserve being placed on a more robust observational footing.

In summary, it seems to me that the manuscript is to some extent caught in a position of tension, between serving as a ‘method paper’, and serving as a new investigation into sub-millennial variability across HS11 and TII. Furthermore, some work appears to be needed for the manuscript to meet either (or both) of these candidate goals. I therefore invite you to please consider preparing a significantly revised manuscript version that you believe addresses the issues raised by both reviewers, and that will likely be reviewed by a third and independent expert with access to the first set of comments. I further suggest that you explicitly take into account the detailed comments of the two reviewers in any revised manuscript. I sincerely hope that you will see this, as I do, as an opportunity to revise your study in such a way as to make it significantly more impactful. I believe that with some further analysis, and redrafting of figures, this manuscript could indeed serve as a key reference that highlights the importance and possible significance of late glacial/deglacial variability across TII.

Sincerely, Luke Skinner

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-131>, 2020.

Printer-friendly version

Discussion paper

