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Interactive comment on "Multi-proxy fingerprint of Heinrich event 4 in Greenland ice core records" *by* M. Guillevic et al.

Anonymous Referee #1

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This paper presents a multi-proxy dataset across a set of Dansgaard-Oeschger (D-O) events. A particular novelty is that it presents the first high resolution 17O-excess data across a series of D-O events. The authors attempt to make the case that they can discern different substages within GS9, and they suggest that one of these stages is associated with iceberg discharges. If this were correct, it would allow a clearer definition of how Heinrich Events fit into the DO paradigm.

Firstly, I would like to congratulate the authors on producing the 17O and new 18Oatm data that are the basis for the paper: both are impressive datasets. However the way the authors use the data requires a chain of guesswork and circumstantial evidence that is not sufficient to support the bold claims they make, not least in the title of the paper.

The first issue is with the way they interpret some of the data themselves. 17O data can be interpreted through the use of climate models. However, as with deuterium-excess, it is already obvious that there is no simple explanation for any change, as the very vague interpretation in the last paragraph of section 3.1 indicates. I realise this is not simple as we do not yet have a full understanding of what factors may control 17O-excess. However, as an example of the confusion in this interpretation, on page 1188 at line 11 the authors describe scenarios that "are in agreement with a southward shift of the vapour source. . ..as suggested by the high d-excess", but at line 26 a southward shift is required to cause low 17O-excess but constant d-excess. As a result, the interpretations that follow are highly speculative and not certain enough to support the broad inferences in the paper.

Perhaps even more worrying is the interpretation of MSA as a sea ice proxy: in support of this the authors cite a recent review (Abram et al 2013) and a paper by O'Dwyer that showed a weak but statistically significant negative correlation (less sea ice = more MSA) at Svalbard. However the O'Dwyer paper specifically pointed out that the only work (and to my knowledge still the only work) in Greenland had suggested a positive correlation with sea ice (though this is very tentative in the Legrand paper cited). The Abram review cited the same papers and certainly did not support the use of a negative correlation for Greenland. I agree that it is plausible that MSA might be a sea ice proxy in Greenland (just as it is plausible, but completely unproven for Greenland, that sea salt, which would tell a different story, may be a sea ice proxy). However this is no basis for the strong conclusions that follow; on the showing of the only published work we are aware of, the conclusion would actually be the opposite. I don't advocate the opposite conclusion, and other factors have to be taken into account (such as the effect of lower accumulation rates on concentration, of higher dust on MSA preservation, and of changing transport paths and source regions). However, it does mean that the strong interpretation in terms of sea ice is simply not currently supportable. (As an aside I wonder why the authors used GISP2 MSA, when MSA data were certainly collected at NEEM).

The second issue is simply with the way the data are described, particularly the 17O data on which the story is hung. The description in the first paragraph of section 3.1 is very leading for the reader, essentially stating that 17O is high in GI and low in GS – except that the relationship breaks down for half the record!! If one looks at Fig 3 the relationship is really not that obvious: I'd like to know what the correlation coefficient between 18O and 17O excess actually is across the whole record. Having made this rather wishful statement, the authors then try to interpret the departures from it, in particular picking out more detailed changes in GS9. Clearly if the initial GI/GS difference is not really robust, then the discussion of departures from it becomes difficult. I can see that there are interesting signals there, but so there are in GS8, where I could also argue for 3 stages and yet no-one proposes an HE there.

Thirdly, while I am happy to agree that HE occur only after GS have taken hold, the link from the authors's substage 2 to the iceberg discharges is completely guesswork. I can see nothing (other than that the iceberg discharges appear to be in mid-stadial) to support this. Section 3.3 seems very speculative.

Finally the attempt to implicate HE5 as having a similar structure, in the absence of the 17O data, is unconvincing. As stated above I think we could find 3 phases in any section of the core if one subdivides carefully enough. In this case the classification seems to be largely on the basis of MSA, which shows numerous changes as large as the ones separating the notional GS13-1,2,3 and yet these are not commented on.

In summary I think this paper is a long way from being suitable for publication in CP. The new data are fascinating and hard-won. They deserve to be published. It would be reasonable to discuss the changes within GS9 but this needs to be done with much less certainty about their significance, and the link to the iceberg discharges must be considered very tentative (indeed probably this can be no more than a suggestion, certainly not the basis for the title). I am afraid this will leave a paper that is little more than a data presentation and with a very different focus, but it's really hard to see that the present analysis is supportable. I will suggest major revisions, but I admit that

I have doubts that even a major revision can produce a publishable paper with the present focus (ie on Heinrich events).

Detailed comments:

Page 1180, line 3. "coincide" is a bad word, as the whole point of the paper is that the HE and the stadial don't coincide (implying causality), rather the HE happen to occur during some GS.

Page 1180, line 10: infirmed is not correct English. "disproven" would be a possible replacement.

The abstract will need to be toned down. The three phases are nowhere near as obvious as the paper suggests, and the link to the HE is tenuous.

Page 1183, line 1-2. "stricto senso" is not a phrase I know, do you mean "strictu senso"?

Page 1185, para 1. It would be worth here clarifying that you use the GS numbering as recommended by INTIMATE and as developed by various authors, but that this is different to the C-numbering of cold phases sometimes used in the marine community (as in Rousseau et al 2006, numbering is one different). Given that this paper is aimed partly at marine people, you need to make this clear.

Section 3.1. A better description than the one given in item (i) is needed. Indeed the reasons why 17O excess deviates so much from del18O and deuterium-excess across the entire record might be a better focus for the paper.

Page 1189 – as explained already the use of MSA as a sea ice indicator in Greenland is not supported by the references shown, and is probably not at this point supportable. Again a much more complex discussion will be needed if this is to be used in this paper at all.

Page 1190, line 10. In discussing the effects of oxidation on methane between GI and

GS, you might want to cite [Levine, J. G., et al (2012), Controls on the tropospheric oxidizing capacity during an idealized Dansgaard-Oeschger event, and their implications for the rapid rises in atmospheric methane during the last glacial period, Geophys. Res. Lett., 39, L12805, doi:10.1029/2012gl051866.]

Section 3.4. I think it would be better to leave this discussion of HE5 out altogether. It is not convincing.

I did not check all refs, but Eynaud et al 2009 has a strange bug in the author list (F., ni, . . .). I also assume the odd page numbers at the end of each ref are an artefact of the editing and will need to be removed in future versions.

Fig 4: You use the notation GI and GS in the text so better to use that on the figure, rather tha DO (for GI) and GS.

Fig. 6 is really hard to follow because you have made no attempt at synchronisation. What the reader is trying to do is see if the IRD is synchronous between sites to judge the importance of your observation that the onset of cold is not synchronous. However this is really hard to do on this presentation. I understand the reluctance to make a synchronisation if it is not supported by data but perhaps you could at least draw some dashed vertical lines depicting key times you consider synchronous, to help the reader follow the text.

Interactive comment on Clim. Past Discuss., 10, 1179, 2014.