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Climate of the Past Discussions

Interactive comment on "Spatial gradients of temperature, accumulation and δ^{18}O-ice in Greenland over a series of Dansgaard-Oeschger events" *by* M. Guillevic et al.

Anonymous Referee #2

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This is a very well written and organized paper addressing an important topic with relevant existing and new data. I have comments, below, that I think need to be addressed, but are hopefully relatively easy to deal with. A main concern is whether the temperature changes at the different sites are truly statistically different, something I think could be addressed with some additional statistical analysis.

Page 5212, Lines 5 and 6. I don't think that Blunier and Brook claimed that all DO events are associated with AMOC changes. In any event I do not think there I strong oceanic evidence for such a statement. It might be true, but I do not think there is ironclad evidence that it is so.

Page 52,12, Lines 7 and 8. Similarly, I don't think that the ID of counterpart Antarctic warmings necessarily proves AMOC involvement, and I think our community should be a little skeptical that there are actually counterpart warmings for each DC cooling – is this really robustly demonstrated?

Page 5212, Lines 20-23. We may be surprised by higher resolution CO2 data. I suggest saying that CO2 appears not to be in play based on existing data.

Page 5213, Lines 12-14. Are there any data to support this: "Unlike Central Greenland where snow falls year round, NW Greenland precipitation is simulated to occur predominantly in summer (Steen-Larsen et al., 2011; Sjolte et al., 2011; Persson et al., 2011)."

Page 5213, Lines 23 and 24. Can you be more specific about what "conditions" means?

Page 5216, line 7. Is accuracy the term desired here?

Page 5216, line 7. Small point, but saying that the uncertainty is 1439 a seems to imply a 1 a precision, which is probably not appropriate.

Page 5216, line 7. Lines 20-25. Will all of these match points be published in this paper and made available?

Page 5217, line 7. Another small thing, layer counting in GISP2 only goes to 40 ka if I recall correctly (I think it was actually blended with an d18Oatm based time scale between 40 and 50 ka.

Page 5221, Line 22. I think you should explain a bit more about Ca being a dust proxy. As written only ice core scientists "in the know" will know what you mean.

Section 3.1. If we accept that the right way to fit the data is to reduce accumulation rate, then what about the dust issue, do you think it is not important?

Page 5225, Lines 6-10. Given the larger uncertainties in NGRIP temperature reconstruction, what is the probability that all of the DO8 results actually show the same

temperature change? I think that a more rigorous statistical analysis should be possible.

Page 5225, Lines 1-5 I think a bit more explanation of how the offset of dust and ions tells you about snowfall seasonality is needed. Page 5220, line 10-15. Based on the discussion in prior paragraphs it seems necessary to conclude that both kink height and some thickness change could be involved, not just kink height. Also, what is the justification for saying that the needed thickness changes are unrealistic? It seems a bit circular to trust ice sheet models for that. And what about margin location changes, don't they enter in to this?

Interactive comment on Clim. Past Discuss., 8, 5209, 2012.