

Interactive comment on “Millennial and sub-millennial scale climatic variations recorded in polar ice cores over the last glacial period” by E. Capron et al.

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This manuscript presents important new data on the pattern of rapid climate change during MIS5 and should be published in CP, with some minor modifications. The authors document a new type of behaviour in the Greenland ice core records, consisting of an abrupt warming near the end of a long gradual decline, termed a “rebound event”. They also demonstrate that the linear relationship, found by earlier work in MIS3, between amplitude and duration, does not seem to hold in the longer DO events of MIS5. These findings will generate new thinking about the nature of the bipolar seesaw and this amply justifies publication. There are a number of small fine-tunings needed, de-

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tailed below, having mostly to do with the English.

On pg 145, line 29, a warming of 14 C is cited at GISP2 for GIS19. This does not square with my memory, which has it at 11 C at GISP2. Please check this for accuracy. Also, a citation is needed here. I believe it was Grachev. Or did you mean to write GRIP, rather than GISP2? In either case, why not mention all three sites – that would strengthen your argument that there is heterogeneity from site to site in the amplitudes of the warmings.

Furthermore, you state that there is a continentality effect at NGRIP. Please explain this, as it seems to be relevant to the paper’s topic. Also please back up this assertion with references. What about the effect of near-surface atmospheric inversion layers? Is it possible that the differences between GRIP (16 C) and GISP2 (11 C) amplitudes at GIS19, which are only 27 km apart, can be explained by differences in boundary layer stability? GISP2 is on a slope, and so has persistent katabatic downslope flow, whereas GRIP does not. This may explain the larger temperature change at GRIP. If true, then these changes are not real free-troposphere changes, but rather are due to local micrometeorology. In this case it would be inappropriate to draw conclusions about the temporal slope from gas isotope data. Of course, precipitation d18O would not be sensitive to these near-surface inversions, as the precipitating clouds are above this layer.

On pg 146, line 7, you state that “Only one study in Antarctica so far has used the gas fractionation paleothermometry method.”. This is not quite accurate. Taylor et al. (2004) used d15N and d40Ar to infer a 6 C (3 C to 9 C) abrupt warming at Siple Dome, 22 ka. The magnitude of the d18O_{ice} shift was 4 per mil, making it consistent with the figure of “within 20% of the classical interpretation...”. This result suffered from gas loss, so it was far from an ideal reconstruction. Nonetheless you should mention it here.

The discussion on page 152 is difficult to read, and quite speculative. It should be

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tightened up, and made much shorter, by only saying those things that really add new scientific understanding. Similar problematic statements occur on page 153. Line 5, for example, states that the “rebound at the end of the GIS should be explained by an enhancement of the AMOC through perturbation in the salinity budget.” This is speculative and does not make a convincing case. Did you mean to say, “could be explained...”?

Pg 138, line 16 “This description originates mainly from THE DO events occurring over Marine Isotopic Stage 3 . . . , which BENEFIT from a robust chronology.”

Pg 138 line 21 “This global CHARACTERISTIC . . .”

Pg 139 line 29 “. . . a time period of great interest because it represents an INTERMEDIATE stage..” (an intermediary is a person who acts as a mediator between two other people)

Pg 140 line 13 “. . . a longer pacing than the approximate 1.5 THOUSAND YEAR (hereafter kyr) DO event frequency suggested by Grootes and Stuiver. . .”

Pg 140 line 19 “. . . which faces the South Atlantic ocean. . .” This is a bit awkward in construction. Perhaps eliminate this phrase and insert it somewhere else. Generally it is good style to use a comma before a phrase beginning with “which” to let the reader know that it is a subordinate clause. In this case there are so many references that it might be too dense if a comma was used, so perhaps it is better to eliminate the phrase altogether, to make the sentence more readable.

Pg 141 line 1 “Hereafter, we..” This should be “In this paper we..”

Pg 142 line 25 “using such ASSOCIATIONS,..”

Pg 142 line 27 “. . . derived from the Hulu CAVE RECORD between . . .” you should cite Wang et al. 2001 or whichever appropriate reference for the Hulu cave dating.

Pg 143 line 17 “provides A FIRM BASIS to..”

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Pg 143 line 27 “. . . has to be INTERPRETED CAREFULLY.”

Pg 144 line 14 “By contrast, available information SUGGESTS that..”

Pg 145 line 3 “due to gravitational SETTLING and thermal diffusion”

Pg 145 line 12 “. . . QUANTIFIED..”

Pg 145, line 19 There is some word missing, and your meaning is not clear. “. . . obliquity, ice sheet..” Do you mean obliquity and ice sheets? Or obliquity via ice sheets? Obliquity or ice sheets?

Pg 146, line 22 “. . . corrected FOR elevation..”

Pg 147, line 2 “. . . corrected FOR sea water isotopic composition..”

Pg 148, line 1 “. . . a ‘squared’ structure” This is likely to confuse your readers. Perhaps it would be clearer if you wrote “. . . a ‘square wave’ structure..”

Pg 148, line 7 “Finally, a stable phase is observed WITH A DURATION OF 500 yr..”

Pg 148, line 11 “. . . PROBABLY includes LOWER-LATITUDE COUNTERPARTS..”

Pg 148, line 12 “. . . drop in CH4 concentration OVER 150-200 yr.”

Pg 149, line 12 “Grachev et al.” is misspelled

Pg 149 line 17 “Grachev et al.” is misspelled again

Pg 151 line 12 It would be wise to restate this as “Here we present several possible mechanisms for..”

Pg 151, line 26 “. . . would NECESSITATE high resolution..”

Pg 154, line 6. “A linear relationship with a constant slope fails to reproduce the AIM amplitude vs. duration for the long GS which supports the thermal bipolar seesaw concept.” What I think you’re trying to say here is that “A linear relationship with a constant slope fails to reproduce the observed AIM amplitude vs. duration for the long

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GS, which is in conflict with the thermal bipolar seesaw concept.” In any case this sentence needs clarification.

Pg 154, line 7 “..predict that for A long period..”

Pg 154, line 12 “HERE we make A sensitivity test..”

Pg 162, line 32 Grachev is misspelled again.

Pg 178, figure caption. Grachev is misspelled again.

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