Clim. Past Discuss., 6, C133–C144, 2010
www.clim-past-discuss.net/6/C133/2010/

Climate of the Past Discussions

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.

E. Capron et al.

emilie.capron@lsce.ipsl.fr

Received and published: 5 May 2010

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-

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.

»We could not find a reference from Grachev stating that the warming at the onset of GIS 19 is around 11°C. As far as we know only the δ15N measurements over this period have been published by Grachev et al. (2009). Actually, we base this statement on the study detailed in chapter 5 of the phD thesis of Landais (2004) as written in the manuscript. In this study, Amaelle Landais used exactly the same method to determine the amplitude of the warming at the onset of GIS 19 at GISP2 and NorthGRIP, i.e. the combined measurements of d15N and d40Ar and firnification and heat diffusion model by Goujon et al. (2003) forced by different surface temperature scenarios as described in Landais et al. (EPSL, 2004). The δ15N and δ40Ar were measured at GISP2 by A. Grachev (δ40Ar not published) and by A. Landais at NorthGRIP (Landais et al., GRL, 2004). So, by using exactly the same paleothermometry method, we can clearly state that the amplitude of the temperature change is smaller in GISP2 (14°C with this method) than in NorthGRIP at that time. This method may still be biased even if we tried to best take into account the uncertainties in Landais et al. (EPSL, 2004). So, the reason why we do not wish to include GRIP and the study of Lang et al. (1999) is that they used a completely different method to infer a 16°C temperature change: they used δ15N data only and tried to best fit the observed Δdepth between change in δ18Oice and change in d15N using the firnification model of Schwander et al. (1997) and changing the temperature scenarios used as inputs. They did not try to reproduce the δ15N change as in the studies of Severinghaus et al. (1998), Landais et al. (2004; 2004), Huber et al. (2006) or Kobashi et al. (2007) Finally, this paragraph was only a short note (we even wonder if this is needed) to emphasize that we should not take the amplitude of the temperature changes in NorthGRIP over the onset of the different GIS as a reference for the whole Greenland so that we prefer not to expand this part.

We propose this new paragraph:

Note that the amplitude of temperature change at the onset of the different GIS in NorthGRIP should not be used as a quantitative reference for Greenland climate. As an example, NorthGRIP and GISP2, only 325 km apart, present different temperature changes at the onset of GIS 19: 16 °C and 14 °C respectively (Landais et al., 2004a ; Landais, 2004). These regional differences are probably due to a more continental climate at NorthGRIP as has been suggested by the comparison of GRIP and NorthGRIP δ18Oice curves (NorthGRIP c. m., 2004): the difference in δ18Oice between NorthGRIP and GISP2 is highly correlated with past continental ice volume reconstructions thus suggesting that larger ice sheets enhance the remoteness of NorthGRIP from low latitudes air masses while Summit (GRIP / GISP2) is less affected by such continentality effect. Regional differences in moisture origin during the current interglacial period have also been identified in deuterium excess profiles (Masson-Delmotte et al., 2005b). Finally changes in ice sheet topography can also generate regional elevation changes impacting regional temperature at ice core sites (Vinther et al., 2009). However, these regional differences modulate the regional expressions of climate variability and do not prevent us to use the NorthGRIP δ18Oice profile to qualitatively characterise the relative amplitudes and shapes (Johnsen et al., 2001; NorthGRIP c.m., 2004).

-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.

» The continentality effect with important effect of ice sheet extension in glacial time has been first suggested by NorthGRIP c. m. (2004)through the resemblance between the sea level curve and the difference in δ18Oice between NorthGRIP and GRIP. This is now stated in the new paragraph (see above). Then, as for the effect suggested by Jeff Severinghaus, it is indeed very interesting but we prefer to restrain comparing GRIP and GISP2 temperature changes over DO event 19. Indeed, as explained above, the methods used are very different between Lang et al. (1999) (GRIP, 16°C) and Landais (2004) (GISP2, 14°C) so that the discussion of such difference is perhaps not sound.

-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 d18Oice 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.

» We agree that we forgot in the submitted version to cite this reference. We modify the sentence in the new version of the paper as follow:

Few studies have used so far the gas fractionation paleothermometry method on Antarctic records (Caillon et al., 2001; Taylor et al., 2004). Based on δ15N and δ40Ar data performed on the Vostok ice core, Caillon et al. (2001) estimate a temperature change at the MIS 5d/5c transition consistent within 20 % of the classical interpretation of water stable isotope fluctuations.

-The discussion on page 152 is difficult to read, and quite speculative. It should be tightened up, and made much shorter, by only saying those things that really add new scientific understanding.

» We agree that some parts of the discussion are speculative and that listing the different factors that can impact on the sub-millennial scale variability without sometimes any convincing arguments lead to a text not clear and difficult to read. For this reason, we have decided to shorten and simplify the discussion: first, we have removed the paragraph on the CO2 hypothesis since we do not bring any interesting conclusions on that point. Moreover, we make several others modifications since this part of the paper was also asked to be modified by the anonymous reviewer. Find below the new discussion as it is written in the new version of the paper:

The detailed analysis of the long GIS of MIS 5 provides evidence for sub-millennial scale variations during these phases. During GIS 21 and GIS 23, we depict a specific structure composed of a precursor-type warming event leading the GIS and a "rebound-type" abrupt event before the GIS abruptly ends. Such a structure is recurrent during MIS 3 at shorter timescales and Figure 6 displays a linear relationship between the durations of the "rebound-type event" and of the preceding GIS regular cooling. Inspired by the factors previously proposed for explaining the classical DO variability, we present here some of the possible mechanisms for favouring these additional sub-millennial scale features: (i) ice sheet size controlling iceberg discharges (MacAyeal, 1993) and the North Atlantic hydrological cycle (Eisenman et al, 2009), (ii) 65°N insolation affecting temperature, seasonality, hydrological cycle and ice sheet growth in the high latitudes (e.g. Gallée et al., 1992; Crucifix and Loutre, 2002; Khodri et al., 2003; Flückiger et al., 2004). Note that these influences may also be enhanced through feedbacks. In particular, sea ice extent variations are often given as trigger (Wang and Mysak, 2006) or amplifiers (Li et al., 2005) of abrupt warming events. We first discuss the link between the occurrence of the sub-millennial variability and the ice sheet volume. The length of the GIS displayed on Figure 6 appears to be related to the mean sea level with the long GIS 23 and 21 being associated with the highest

sea level while GIS 11, 12, 14 and 16 are associated with lower sea level during MIS 3 (Figure 1). Such a link between the GIS length and sea level is expected from a simple Binge-Purge mechanism (MacAyeal 1993): largest ice-sheets are expected to be easier to destabilize. However, such a Binge-Purge mechanism is unlikely to explain the existence of sub-millennial scale climatic events during sequences of events 21-24 and 15-17 since they occurs during relative ice sheet volume minima (Bintanja et al., 2005). A more plausible mechanism for these precursor events would be that the smaller ice sheets as observed during MIS 5 (equivalent to sea level of about 20 to 60 m above present sea level; Bintanja et al., 2005) are more vulnerable than large ice sheets observed during MIS 2-3-4 (sea level between -60 and -120 m; Bintanja et al., 2005) to local radiative perturbations. If so, a strong 65°N summer insolation would lead to intermittent freshwater outputs and trigger fast changes in the AMOC intensity. The influence of the Milankovitch insolation forcing on the sub-millennial variability can also be explored (Figure 5). During MIS 5, the GIS 21 precursor-type event and GIS 24 are both in phase with two relative maxima in summertime insolation at 65°N while the GIS 23 precursor-type event occurs during a relative strong 65°N insolation and lags the preceding insolation maximum only delays by $\sim$2.5 kyrs (Figure 5). During MIS 3, we again observe that precursor-type events GIS 14 and 16 are associated with secondary insolation maxima. On the contrary, GIS 11 and 12 are not preceded by a precursor and occur at a time without a marked anomaly in 65°N summer insolation. Our data therefore suggest a link between high 65°N insolation and the presence of a sub-millennial scale climatic variability in addition to the GS-GIS succession. This hypothesis also applies to the last deglaciation. Indeed, centennial-scale variations in the NorthGRIP δ18Oice profile are superimposed to the BØlling-AllerØd warm phase followed by the Younger-Dryas cooling (Björck et al., 1998) while the 65°N insolation during those events is equivalent to the one observed during the sequence of events 15-17. Finally, rebound-type events tend to be associated with long GIS intervals characterized by a slow cooling. We speculate that the rebound at the end of the GIS could be explained by an enhancement of the AMOC. Indeed, a progressive cooling could increase sea ice formation and reduce precipitation amount/runoff, increasing salinity in the North Atlantic region.

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: : :"?

» We made some modifications in the text on this part following also comments from the anonymous reviewer:

Finally, rebound-type events tend to be associated with long GIS intervals characterized by a slow cooling. We speculate that the rebound at the end of the GIS could be explained by an enhancement of the AMOC. Indeed, a progressive cooling could increase sea ice formation and reduce precipitation amount/runoff, increasing salinity in the North Atlantic region.

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

»Done

-Pg 138 line 21 "This global CHARACTERISTIC : : :"

»Done

-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)

»Done

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

»Done

-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.

» We modify the structure of the sentences to make the text more readeable:

We also use here an ice core drilled within the European Project for Ice Coring in Antarctica (EPICA) in the interior of Dronning Maud Land (hereafter, denoted EDML, 75°S, 0°E, 2892 m above sea level, present accumulation rate of 6.4 cm w.e.yr-1). It represents a South Atlantic counterpart to the Greenland records (EPICA c. m., 2006)and provides a resolution of ∼30 yrs during MIS 3 and of ∼60 yrs during MIS 5 that makes the EDML core particularly suitable for studying millennial scale climatic variations in Antarctica.

-Pg 141 line 1 "Hereafter, we.." This should be "In this paper we.."

»Done

-Pg 142 line 25 "using such ASSOCIATIONS,.."

»Done

-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.

»Done

-Pg 143 line 17 "provides A FIRM BASIS to.."

»Done

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

»Done

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

»Done

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

»Done

-Pg 145 line 12 “..QUANTIFIED..”

»Done

-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?

»Done ; we modify the sentence as follow :

Here, we do not find any systematic relationship between the evolution of the temporal slope and the long term evolution of components such as ice sheet volume or orbital parameters.

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

»Done

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

»Done

-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..”

»Done

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

»Done

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

»Done

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

»Done

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

»Done

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

»Done

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

»Done

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

»Done

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

» We agree that our sentence was not clear and needed to be re-written because the idea behind is of first importance. What we meant to say is that what we observe supports the thermal bipolar seesaw concept of Stocker and Johnsen (2003) but not the simplification made for MIS3 where it sounds that events can fit on a linear fit. Indeed looking at the simulated evolution of TS as a function of TN according to the equation given by Stocker and Johnsen (Figure 7), it is predicted that for long GS duration, the Antarctic warming rate decreases and eventually the warming ceases meaning that despite long shutdown of the AMOC, there is a limit in the capacity of the South to accumulate heat. We have rewritten the sentence as follow:

This shows that for extraordinarily long stadial durations the linear relationship between the stadial duration and the accompanying Antarctic warming amplitude is not longer valid. This feature is indeed expected from the bipolar seesaw concept (Stocker and Johnsen, 2003; EPICA c. m., 2006). Stocker and Johnsen (2003) predict that for long period of reduced AMOC (equivalent to GS duration in their model) a new equilibrium is reached and the Antarctic warming would eventually end. This type of situation could be relevant for the long DO/AIM 21, while DO/AIM events during MIS 3 may be too short for an equilibrium to be reached.

-Pg 154, line 7 "…predict that for A long period.."

»Done

-Pg 154, line 12 "HERE we make A sensitivity test.."

»Done

-Pg 162, line 32 Grachev is misspelled again.

»Done

-Pg 178, figure caption. Grachev is misspelled again.

»Done

We thank you for your review.

Interactive comment on Clim. Past Discuss., 6, 135, 2010.