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Climate of the Past Discussions

Interactive comment on "Rapid changes in ice core gas records – Part 2: Understanding the rapid rise in atmospheric CO_2 at the onset of the Bølling/Allerød" *by* P. Köhler et al.

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We have answered all the reviewers comments in details in our individual responses in the interactive discussion. Here, we summarised how and why we finally revised the paper accordingly. Thus, this response letter is brief and focuses on the performed improvements, while more details on the discussion are found in our individual replies.

1. Title and companion paper: The companion paper is due to missing reviews still in discussion. We therefore decided to make our paper here independent from the companion paper by including its main message, which is of relevance here, in the Appendix B. This Appendix B deduces the synchronisation error of 200

yr at the onset of the Bølling/Allerød between the two ice core EPICA Dome C and NGRIP. The synchronisation of ice cores and age correction is therefore explained now in more details in Appendix B, as also requested by point 3 of reviewer 2. We furthermore revised the title towards the more independent

Abrupt rise in atmospheric CO_2 at the onset of the Bølling/Allerød: in-situ ice core data versus true atmospheric signal and its implications

which now also implies the stand-alone character of the paper. Please note, that the companion paper is much more general than what we compile in Appendix B.

2. General layout of the paper: We revised throughout the draft, that we follow here a hypothesis, which might (or might not) be true. This was done as a consequence of the discussion on the accuracy of the gas age distribution, from which we learnt, that our approach might indeed be realistic and correct, but we can due to the limited data availability not prove that our approach is the only valid one. However, we like to emphasise that our paper contains two main findings: (1) The true atmospheric CO_2 during the onset of the B/A is different from the in-situ ice core data in EDC. The details of our findings here depend on the gas age distribution and the discussion on its reliability which is now condensed in Appendix A. (2) We interpret this large and abrupt rise in atmospheric CO_2. It can be understood in what we called the "shelf flooding hypothesis" by the impact of meltwater pulse 1A on the carbon cycle. Finding (2) depends on the timing of events, and might be proven right or wrong by future synchronisation efforts. However, even if evidences against this causal relationship between sea level rise and abrupt rise in atmospheric CO_2 would be brought up, finding (1) is still valid and needs to be explained somehow.

3. Methods: Following points 1 and 4 of reviewer 2 and the intense discussion on a test on the chosen function and width of the gas age distribution we condensed

this test in Appendix A and briefly summarised the main findings in the last paragraph of the methods section. This test and the online discussion clarified the draft and we are now even more confident on the usability of our approach, because finally the test using CH_4 data supported our so far pure-theoretical approach. However, we have to appreciate that getting to the final test results was a long way and the interactive discussion was not always as clear as it could have been.

4. Methods: Following point 2 of reviewer 2 we extended and explained why we used a log-normal function and not a Green function to describe the gas age distribution probability density function and that thus other metrics are used here than in similar papers.

5. Methods, carbon cycle modelling: Following point 2 of reviewer 1 we clarified why we think that a change in the AMOC is precisely not important in our experiments here and what the impact on the study would be if performed with an AMOC in the on mode.

6. Methods, carbon cycle modelling: Following point 5 of reviewer 2 we extended how C was injected into the atmosphere and on what evidences the length of the chosen injection time windows was based on (based on rapid climate change as observed on the NGRIP ice core for that period of time).

7. Results: We revised the structure of the results and discussion section to sharpen the paper. Section 3.1 is split in two parts: The first part "3.1 Determine the size of the carbon injection" contains the deconvolution of the size of the C injection necessary to explain the observed CO_2 in EPICA Dome C. It thus contains as main finding only that the original peak was larger than measured in the ice core data, but no explanation is given here. This is the main finding (1) laid out in point 2 above. The second part "3.2 Fingerprint analysis and process detection — the shelf flooding hypothesis" includes interpretation based also in and the long

discussion on the shelf flooding hypothesis (main finding (2) in point 2 above). Our arguments that changes in CO_2 at 14.6 kyr BP have to be caused by processes not connected with the bipolar seesaw is widely extended mainly based on proxy-data evidences that the AMOC state in Heinrich stadial 1 was similar to other Heinrich stadials and that the AMOC resumption here was not exceptional, since similar amplification characteristics can be found for other D/Os. Finally, the last part of the results section is renamed to "3.3 The impact of shelf flooding on the carbon cycle". This now also includes a discussion that shelf flooding might increase the marine biological pump, as suggested by point 1 of reviewer 1.

8. Results: We clarified and highlighted that already the evidences from the other ice cores (Taylor and Siple Dome) indicated that CO_2 in EPICA Dome C was different from the atmospheric CO_2 record and that therefore our whole approach is based on solid wide-spread evidences.

9. Results: We did not extend our results/discussion section on the findings of Siddall et al. (2010) QSR 29, 410-423, as suggested by point 3 of reviewer 1. Although it is true that there seemed to be some similarities between our and their study, this is only the case at the first glance. Siddall et al. (2010) find what they call a "bipolar switch", meaning that sea level change is more related to Antarctic temperature in MIS 3, but more related to Greenland temperature during Termination I. We also find a switch in the relation of CO_2 to either northern or southern high latitude temperature: CO_2 is related to Antarctic temperature during MIS 3, but more related to Greenland temperature during the onset of the Bølling/Allerød. However, for the rest of Termination I CO_2 follows very clearly Antarctic, not Greenland temperature. We therefore find, that the similarity between both studies is very weak and might lead more to a confusion of the reader than to further insights if included and discussed in our study.

10. Throughout the text: All spotted typos were corrected and minor comments were

incorporated in an overall revision of the language of the text.

Interactive comment on Clim. Past Discuss., 6, 1473, 2010.