

Interactive comment on “NGRIP CH₄ concentration from 120 to 10 kyr before present and its relation to a $\delta^{15}\text{N}$ temperature reconstruction from the same ice core” by M. Baumgartner et al.

Anonymous Referee #3

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Baumgartner et al, present an impressive number of new methane mixing ratio measurements from the NGRIP ice core. They further make the effort of homogenizing previous datasets to the latest set of measurements. They thus make sure that no offsets exist in the data they present measured over a decade in two laboratories. The presentation of the data is followed by an exhaustive discussion about changes in the methane main source over orbital and millennial time scales. The data is certainly of excellent quality and deserves publication. The discussion, however, is lengthy, not to the point and needs revision. Some of the arguments put out to support hypotheses are based on data picking. The manuscript is too long and therefore hard to read.

Specific remarks

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Figure 1: There are some outliers within DO 21, 23, and 24. Are those values real or potentially a measurement artifact?

Page 4657, abstract: To prevent misunderstandings it is important to state already in the abstract that the temperature is not a global mean but Greenland site temperature.

Page 4657, lines 16-19: to be more clear what “between DO events 18 and 19” means I suggest to write “the stadial between DO events 18 and 19”

Page 4659, line 11: Stocker and Johnson present a conceptual model please chose another data based reference for the AMOC change during DO events.

Page 4660, Methods: This section is very important but only of interest for a limited number of readers and I suggest shortening it in the text and transferring it to the appendix.

4662, Results: Mu is an interesting parameter but it compares apples and oranges. While methane is a global parameter the temperature is local. Therefore a perfect match is not expected. This discussion can be shortened. I doubt that mu is the interesting parameter for model simulations.

Page 4666, line 11: “in” should be “at”

Page 4667, and figure 5: How is the pdf constructed? This looks like a bootstrap. Is it not simply the overlap of two normal distributions?

Page 4669, Interpolar difference: State at the beginning that you will not look at interstadials due to resolution and different smoothing but only calculate means for the stadial periods.

Discussion:

Page 4674, line 10: Sea level rises slowly the methane increase is fast. I do not see how the two things are supposed to be connected with respect to DO events.

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Page 4672, line 5-13: I don't understand why it is surprising that mu is linked to northern low-latitude insolation. CH4 is and therefore is mu. Also I do not understand the argument "8-15°C". Is the conclusion that from 0-8°C the response is from low latitudes?

Page 4674, lines 8-12: If sealevel should contribute to the DO methane amplitude the inundation, which I agree can be fast, should coincide with the rapid warming which would be rather surprising.

Pages 4675-4677: It seems rather odd that the CH4 production should be dependent on CO2. Substrate is produced in the wetland and it is produced immediately. I also doubt that the substrate is the limiting factor for CH4 production. I thought that it had been shown that the CO2 fertilization effect wears off relatively quickly after a concentration raise. The entire discussion about CO2 and CH4 seems rather odd. If CO2 had an effect on CH4 then I would expect a correlation between CO2 and the baseline (CH4 - DO events). E.g. I would expect a dip in the methane record around 65 kyr BP. I do not find that. I see little support for the hypothesized dependence of methane emissions on the CO2 concentration and suggest to shorten this discussion to a minimum and remove figure 8.

Interactive comment on Clim. Past Discuss., 9, 4655, 2013.

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