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

Answers to Anonymous Referee

GENERAL COMMENTS
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.

Thank you very much for your constructive review comments. It improved the quality of the manuscript and reduced the length of the discussion part.

The two most important changes we applied for the revised version of the manuscript concern the discussion chapter 4:

- *On the suggestion of the reviewers, we provided a new figure which shows the methane to NGRIP temperature sensitivity as a function of age together with NGRIP methane and temperature amplitudes. We further subtracted the normalised northern summer insolation from the sensitivity to reveal remaining features.*
- *We removed the discussion about the influence of carbon dioxide on methane emissions (section 4.3, including Figs. 8 and 9) since concerns were raised about this section by all of the three reviewers. While the comparison of baseline trends should be investigated in the future, it does not concern the main topic of the paper. The CO2 effect on the methane to NGRIP temperature sensitivity is hardly visible after subtracting insolation and is thus speculative and certainly does not deserve a full section of discussion.*

In the following, the specific comments are answered point by point.

DETAILED COMMENTS

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

It is unlikely that those values are measurement artefacts, since we measured these outliers twice, as we always do for outliers. While we do not perfectly confirm the large values in the second measurement, these are also elevated relative to the concentration baseline of DO events 21, 23, and 24, and hence we cannot conclude that these are outliers. It could be very fast real atmospheric variation (although there are no counterparts in the high-resolution NEEM record from Chappellaz et al., 2013, but there are still some data gaps in this record, e.g. during DO event 21), but it could also be in situ production within a local ice layer. Recent studies have shown that in situ production can occur also for methane in Greenland ice either due to surface melting processes or due to bacterial activity within the ice (Rhodes et al., 2013; NEEM community members 2013). During the warm DO events 21, 23, and 24, surface melting can certainly not be excluded. However, we know that the rate of change of atmospheric CH4 during these DO events is possible within the limits of the smoothing process during gas enclosure. Even higher rates of change have been observed during DO events 16/17, which are paralleled in temperature at a similar rate deduced from an ice signal that is recorded in a different depth interval.

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.

We inserted 'local Greenland' just before 'temperature' on page 4657, line 9.

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"

Done.

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

We additionally cited Capron et al. (2010a), McManus et al. (2004), and Piotrowski et al. (2004).

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.

We prefer to present this material in the main text in order to retain emphasis on this important section and not relegate is to a rarely read 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.

In terms of greenhouse gas concentration methane is a global parameter. But CH4 is also an excellent proxy for Greenland temperature and hence we would not agree with the comment that apples and oranges are compared by determining mu. But the referee is correct that the ultimate goal is not the sensitivity to Greenland temperature, however, a high-resolution record representative of northern hemisphere temperature over the entire last glacial cycle does not exist. Moreover Hopcroft et al also compare in their model study the methane sensitivity to Greenland temperature, thus our sensitivity represents the only viable heuristic approach to the problem.

Page 4666, line 11: "in" should be "at"

Done.

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?

Here is a short explanation on how the pdf is constructed: Taking the example of DO event 8 we have lag_min=-41 yr and lag_max=109 yr (see table 3). This yields a total time interval of delta_time=150 yr. The probability for this time interval is set to P=1/delta_time=0.007. In the following, for all possible lags (-500...500 yr, see Fig. 5) the probabilities are summed up according to groups (red: significant lag, black: insignificant lag; brown: all events). At the end, the area of each of the three curves is normalised to 1.

We are not sure what you mean by your second question. Probably you refer to the brown curve as a result from the red and the black curve. The brown curve for all events is constructed independently from the red and the black ones.

We inserted on page 4668, at the end of line 2: 'The pdfs are calculated as the sum (normalised to one) of the probabilities of all DO events of a group (red, black, brown), where the probability of an individual event is the inverse of the full uncertainty range of the lag.'

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.

We moved lines 20-29 on page 4669 forward to line 10. After this paragraph, we continue with: 'For the stadials, we calculate the IPD as a mean...'

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.

Sea level does not have to change rapidly to influence CH4 amplitudes. Since CH4 jumps occur only in coincidence with interstadial northern warming, it depends on the state of the sea level at the moment of the rapid warming. A higher sea level might prevent water run-off from coastal low lands and thus extend the total wetland area, which may lead to a higher CH4 response.

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 "8-15C". Is the conclusion that from 0-8C the response is from low latitudes?

The '8-15°C' was a typing error (see comment of second reviewer, Hinrich Schaefer), it should be '5-15°C', i.e. the whole range of observed Greenland temperature. Insolation dominance of mu does not automatically follow from the insolation dominance of CH4. The range of temperature variation is large enough so that you can easily simulate a mu which does not carry the insolation signal.

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.

See answer above.

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

CO_2 concentration and suggest to shorten this discussion to a minimum and remove figure 8.

We agree that the influence of CO_2 might be strongest for the CH_4 baseline. We discussed it in Fig.9 and in the second part of section 4.3. Although the CO_2 dip around 65 kyr BP is not visible in CH_4, the MIS3-2 concentration baselines of CO_2 and CH_4 are quite similar, which suggests a direct fertilisation and climate feedback influence of CO_2 on CH_4. However, in agreement with the suggestions of the other two reviewers, we decided to delete the whole section 4.3 (including Figs. 8 and 9). The discussion about baselines is not really relevant for the paper and the effect of CO_2 on mu might be indeed very minor, i.e. section 4.3. was much to long to discuss minor effects only.