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

Answers to E. W. Wolff (Referee)

GENERAL COMMENTS
This paper makes use of an extensive new set of methane measurements on the NGRIP ice core. Data have been released in several tranches over the last decade: this paper adds a significant number of additional measurements (doubling the existing number) and completes the record at reasonable resolution. It attempts to correct for offsets between different datasets. It then uses what is the first complete and systematic such dataset to compare with estimates of Greenland temperature (in terms of value and timing), and to estimate interpolar differences along the core.

Overall I find this a very valuable paper. Firstly, the complete methane dataset will be a crucial resource for research, and I trust it will be available online (at the data centres as well as in the supplement to the paper). The attempts to rationalise the datasets released so far are also important: I think this could be made rather clearer, but I am very happy to see this done. Then the inferences about sensitivity, timing and interpolar differences lead to an interesting and valuable discussion. While I feel it could be tightened up (and perhaps shortened) in places (see below), in general, this is an appropriate use of the data, and the conclusions are worthwhile and set in a good data and modelling context. One critical issue is that the paper relies very heavily on the temperature estimates made in the parallel Kindler et al paper: the current paper cannot be accepted until the data in Kindler are also validated through acceptance – I assume the editors will check this. While the paper will need some revision, I expect this to be quite modest, and it will then be suitable for publication in CP.

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
Page 4659, line 16-18. This is rather loosely expressed. Better would be "and that therefore the warmings recorded in Greenland are mirrored by changes in a larger portion of the northern hemisphere". While I think there are other data that suggest a hemispheric influence (not necessarily warming), the methane data alone don't necessarily point to the whole hemisphere, nor just to warming rather than wetting.
Changed.

Page 4660, line 24 "air from ice samples"
Changed.

Page 4661, line 2. Sorry to be picky, but I assume that is 1050 and 408 ppbv of CH4 (you also just mentioned N2, O2 and Ar in the same standard gas).
Clarified.

Page 4661, general. Please could you clarify the explanation here. This page refers to 163 new measurements at LGGE. But on the next page it refers to the same number of measurements made in 2001-2, which hardly makes them new! To clarify this you should refer in the first description of the LGGE data to the year of measurement and to Table 1. If these are previously unpublished then there should be the same level of analytical information as for the Bern data (ie something about reproducibility and standards). Finally I am quite confused about the offset: you mention a 12.2 ppbv offset increased from 6 ppbv, but now you use 26.5 ppbv. Please just try to set this all out as clearly and systematically as possible.
The 163 measurement from LGGE were measured in the years 2001-2, where 'new' was used as a synonym for 'unpublished'. A reference to table 1 at the first appearance of the LGGE data is not appropriate, since table 1 lists the offset calculations and therefore belongs to the next paragraph. The information about standards and reproducibility of the LGGE data was given on page 4661, lines 22-25. To make it clearer, we have rewritten lines 19-24: 'At Laboratoire de Glaciologie et Géophysique de l'Environnement (LGGE) in Grenoble, from where we use a total of 163 previously unpublished NGRIP measurements (made in the years 2001–2002), the same technique as in Bern was used but with a standard gas with 499 ppbv of CH4 (Chappellaz et al., 1997). The LGGE measurements have been performed in parallel to 103 measurements on the Vostok core covering the same climatic period (Bender et al., 2006), which is crucial for a reliable determination of the small interpolar concentration difference (see section 3.4)'. Regarding offset corrections: the 12.2 ppbv are the offset we would expect when we would analyse today one sample at Bern and the same at Grenoble. The 26.5 ppbv offset results from a comparison of Bern today minus Grenoble around 2001-2002 (as you mentioned, these measurements are hardly new, but they were previously unpublished). The reason of this high discrepancy remains unclear, one possibility is the application of a different blank correction at that time. We have substantially modified and expanded the paragraph about offsets so that it should be easier to understand now.

Table 1. The values of n should not have a decimal point!
Done.

Page 4663 and Table 1: Why do you use this old age scale and not GICC05modelext, which has been the basis for most recent papers, including the transfer to NEEM, and for the AICC2012 exercise in Veres et al? Given that this is GICC05modelext is likely to be the standard for Greenland for some years to come, it could be both confusing and annoying that you have used something different. I appreciate that it is probably a lot of work to change this now, but a minimum would be that you set out very clearly in the text that

you have done this, specifically mentioning the alternative age scale. I note that you do give a column headed "AICC" in the supplement, and I assume this is the AICC2012 age. You need to add some lines explaining this (including a citation to Veres et al) in the readme: in particular this should point out that the AICC2012 age scale is identical to GICC05 in the upper 60 ka. Without these additional explanations your dataset will only cause confusion.
The gas age scale we use was calculated by Kindler et al., 2013 (Climate of the Past Discussions) based on the ice age scale ss09seabm, which was the only age scale for NGRIP spanning the whole period from 120 to 10 ka before the publication of AICC2012 (Veres et al. 2013, Clim. Past). Kindler et al. 2013 did not choose GICC05 (or GICC05modelext) because it does not provide accumulation rate estimates for ages older than 60ka (accumulation rate is needed to calculate a gas age scale). The problem of AICC2012 is that it apparently does not take into account all of the CH4 tiepoints of our new NGRIP record (although we made available the unpublished data set to the community for CH4 tie point identification, but we were probably too late). If the EDML, TALDICE, and NGRIP CH4 data are displayed on the AICC2012 gas age scale, we can observe that some DO event CH4 transitions of NGRIP are out of phase compared to the Antarctic counterparts. The error can be as large as 500-1000 years (most apparently for DO18 and 22, but also DO 2, 5, 6, 7). We therefore conclude that for the gas age of NGRIP AICC2012 unfortunately needs revisions. AICC2012 in its current form is therefore not suitable to produce Figures like Fig. 7a. It would even cause more confusion if we would show NGRIP on the AICC2012 age scale and synchronise the TALDICE and EDML CH4 data to the "NGRIP AICC2012" age scale. We therefore would like to continue with the ss09seabm age scale, which is very close to the Kindler et al. 2013 paper. We have included on page 4671, line 6: 'Note that our new NGRIP CH4 data reveal that the AICC2012 (Veres et al., 2013) gas age scale for NGRIP is inconsistent with the CH4 data of EDML and TALDICE for several DO event transitions. We thus do not make use of the AICC2012 but use our own CH4 synchronisation.' We inserted the same statement at the end of the caption of Fig. 7.

Page 4663, line 24. I agree you can mainly just refer to Kindler, but can you clarify for the reader that the 15N temperature estimate takes account of both the gravitational and thermal fractionation. This is important to know later when you look at the phasing of temperature and methane.
Clarified.

Page 4666, line 16. For the thermal fractionation, I agree that the use of 15N precludes any uncertainty in delage. Is this also true true for the gravitational fractionation? On a warming, the accumulation rate increases, which will only slowly change the firn thickness as the new snow has to reach the firn-ice transition before the full effect is achieved: this leads to some uncertainty unless you think your firn model is perfect. So, this then takes me back to an earlier question: are you able to define most of the rapid warmings with a thermal jump? If so, then this sentence is OK; if not it may need some qualification. (May also be relevant to page 4668).
Yes, for all of the DO events, a pronounced thermal jump is visible in the model output of Kindler et al., (2013). The influence of the gravitational component is discussed on page 4668.

Page 4666, line 21: "equal probabilities". I accept that this is the easy thing to do, but it is clearly not correct that there is an equal probability, because you have y-axis information you don't use. If we take Fig 4 as an example, then the gradient between the two red circled points is very high: this means that it is very likely the 15N started to rise early in the interval between them (if it had been late it would not have risen so far by the time of the next datapoint). The same applies to the methane in the same plot. I think one could

use a more sophisticated statistics to define the start of the jump better, but failing that, you should mention that the assumption of equal probabilities is crude.
If we understand correctly, you suggest assuming a maximum rate of increase possible in the ice core records to better define the latest point of the start of the increase. To do so would require to include firn modelling both for CH4 and d15N. Especially for d15N to assess a maximum increase rate would be rather complex and might vary from one DO event to the other, and the approach would lose its current transparency. We prefer to keep it as simple as possible at this point. We added: 'the most conservative assumption of' between 'with' and 'equal probabilities' on line 21, page 4666.

Page 4672. I don't understand the discussion in the first para of this page. Firstly you don't show us the evidence that the NGRIP temperatures are not influenced by orbital parameters. However the next sentence then does not follow at all: if the NGRIP temperatures are not orbital, but the methane is (as many modelling studies make us expect) then the ratio of methane to NGRIP temperature should have the orbital signal in it, as it does. I just don't follow the discussion therefore – please explain.
Following the suggestion from the second reviewer, Hinrich Schaefer, we included an additional figure for the revised version of the manuscript. It shows the CH4 and temperature amplitudes as well as insolation as a function of age. The figure shows that the NGRIP temperature amplitudes of the DO events are not influenced by orbital parameters.
As to answer to your second comment: We agree with your argument for the case where the NGRIP amplitudes would be rather constant from one DO event to the other. However, the variability in NGRIP temperature amplitudes is rather large (5-15°C). By randomly choosing a temperature amplitude for each DO event you can easily find a case where the ratio of methane to NGRIP temperature amplitudes does not have an orbital signal in it, although the CH4 amplitude still has.
We replaced lines 5-13 on page 4672 by: 'However, CH4 increases also correlate with temperature increases (R=0.36), which themselves are not influenced by orbital parameters (R=-0.03). This temperature effect thus obscures the orbital signal in the CH4 amplitudes (R=0.66). It is the sensitivity mu, which seems more directly linked to the insolation (R=0.72).'
Related to this subject we inserted on page 4665: 'Figure 3 shows that CH4 amplitudes tend to increase with increasing amplitude in temperature. For example, if we take the average of ΔCH4 for $\Delta T < 10°C$ it is substantially smaller than the average of CH4 for $\Delta T > 10°C$. Apparently, on the average, a higher NGRIP temperature amplitude is favourable for CH4 emissions.'

Page 4673: there is a lot of emphasis in this discussion on temperature when surely precipitation should be very critical in determining the strength of emissions from wetlands (or at least in modulating the area we describe as wetlands).
We agree that precipitation is the most important factor. Precipitation is discussed in the paragraph before on page 4673. To give more weight to that, we replaced the last sentence of this paragraph (lines 10-11 on page 4673) by: 'Variations in the amount of precipitation (e.g. monsoon strength) from one DO event to the other are thus very likely to drive the observed non-linear variations in mu via total wetland area and length of emission seasons.' We further replaced on line 22: 'could also be introduced' by 'could be supported'. Lines 25-29 have been removed based on a comment by the second reviewer (Hinrich Schaefer).

Page 4674: I find this link to rapid sea level change interesting but am not convinced by what you show. If your mechanism was correct then the SL change would give a brief methane pulse, so one would expect an overshoot only at these particular DO events. It's hard to tell in the figures but I am not sure they are any different from the others.
You are right, this was rather speculative. Maybe it is more the inland ground water level, which is lifted as a response to sea level rise, and which could increase total wetland extent. Anyway,

the discussion on sea level has been shortened and moved to the paragraph where minor effects on the sensitivity are discussed (see also comments by second reviewer, Hinrich Schaefer).

Page 4674-5. Your r-IPD calculations suggest a significant role for boreal wetlands, but they are not mentioned here.
We are not sure if we understand your comment correctly. The boreal source and the rIPD reconstruction are mentioned on lines 27-29 on page 4674. The rIPD reconstruction appears to be inconsistent with a continuous decrease of the boreal source strength during the last glacial. However, after having subtracted insolation from the sensitivity, we do not really see a place for the discussion on boreal wetlands, although one might see a very small remaining decreasing trend from 90 kyr to 20 kyr BP.

Section 4.3 is long and discursive without being very convincing. Fig 8 really just looks like a cloud of points. I think you may be able to make a case that the slow CH4 rises may be linked to CO2 increases, but there seems to be no convincing case that the overall shape of the jumps (ie the mu values) are related to CO2. In particular you have already "explained" the shape of the mu values by insolation, so while you can discuss whether there is also an effect from CO2, you need to do that on data that have already been normalised for other effects such as insolation. I guess you are implying that the slope of the interstadial mu in Fig 7c (itself not very convincing) is related to CO2, but CO2 does not show a long trend, it just shows a jump around 70 ka. I think this section needs rethinking.
Following your suggestion and the one from Hinrich Schaefer we removed section 4.3 as well as Figs. 8 and 9 from the manuscript. We also corrected mu for insolation, which is shown in a new plot we created for the revised version of the manuscript. Although the residuals might be sensitive to age scale uncertainties, it is indeed hard to identify a remaining trend after subtracting insolation. Thank you for your comment on the long term trend of CO2, your argument is plausible and on the second view we agree that there is effectively no clear long term trend in CO2.

Page 4680. I also find that the discussion in this section is drifting, and not very convincing. The paper will be more powerful if you present the data and discuss the major and clear features, without too much speculation on ideas that the data do not clearly support.
We followed your suggestion and removed this paragraph, on the benefit of a more concise paper.

Fig. 1: "Methane ... in grey". Nothing on the published plot is grey: do you mean different shades of blue?
The symbols of the old data are now in black and should be easier to distinguish from the new data.