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Interactive Comment

# *Interactive comment on* "NGRIP $CH_4$ 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.

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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.

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.

Page 4660, line 24 "air from ice samples"

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).

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



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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.

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

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.

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.

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



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some qualification. (May also be relevant to 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.

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.

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).

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. Page 4674-5. Your r-IPD calculations suggest a significant role for boreal wetlands, but they are not mentioned here.

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 $CO_2$ increases, but there seems to be no convincing case that the overall shape of the jumps (ie the mu values) are related to $CO_2$. 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 $CO_2$, 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 $CO_2$, but $CO_2$ does not show a long trend, it just shows a jump around 70 ka. I think this section needs rethinking.

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.

Fig. 1: "Methane…in grey". Nothing on the published plot is grey: do you mean different shades of blue?

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