Clim. Past Discuss., 9, C1927–C1933, 2013
www.clim-past-discuss.net/9/C1927/2013/

Climate of the Past Discussions

Interactive comment on "NGRIP CH_4 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.

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Received and published: 4 September 2013

Baumgartner et al. present new measurements of past atmospheric methane (CH4) concentration from the NGRIP ice core. The new data set is combined with older published and unpublished data to provide a complete high resolution record of CH4 atmospheric evolution over the entire last glacial period. This time series is analysed together with nitrogen isotopes (δ15N), a temperature record for the drill site derived from the latter, as well as pCH4 and pCO2 records from Antarctica. The study derives the climate sensitivity of CH4 and uses established approaches to study the inter-polar gradient in pCH4 and the lead/lag relationship with local temperature. The main

contribution of the study is to provide a complete picture for these parameters over the course of the last glacial that gives interesting insights into geographical patterns of CH4 emissions under varying climatic conditions. This is a good contribution to our understanding of the methane cycle and suitable for publication in CoP. There are some minor points that should be addressed as detailed below. In addition, I think that the discussion of CO2 effects needs a real overhaul. I think that the authors' support for a CO2-CH4 link is based on selective use of evidence, such as not quantifying the potential signal in the defined climate sensitivity mu and examining only specific time windows. The CO2-CH4 link should be analysed in a robust manner or left out altogether. In contrast, the findings on trends in inter-polar concentration gradient and lead/lag relationships with temperature make this study a valuable contribution.

Detailed comments:

Abstract: the main findings of the study could be spelled out more clearly, while currently there is strong emphasis on reporting specific numbers of individual results. For example, the identification of CH4 lags for periods with low increase rates and implied geographical source shifts are worth mentioning here. Another solid finding is the evolution of rIPD throughout the last glacial.

Page 4659, lines 26-27: A possible reference would be: Mariotti, A. Atmospheric nitrogen is a reliable standard for natural 15N abundance measurements. Nature 303, 685–687 (1983).

Methods: there should be a reference to the data table in supplementary material. Also, as the lead-lag determinations use actual d15N data a methods description for those analyses must be presented. This can take the form of a reference and a quick summary, but e.g. the reproducibility should be mentioned so that the reader can evaluate the choice of 1-sigma exceeded for the start of a T-increase.

Page 4661, lines 8 and 9: completely air free ice is notoriously hard to produce. If the authors have evidence that their blank ice indeed contains zero CH4 than this would be a good place to mention it. Otherwise, it should be pointed out that residual CH4 from the blank ice would lead to an overestimation of the blank correction, which may affect comparability with measurements from other labs, such as LGGE.

Page 4661, lines 26 and following: the authors could point out that the re-measurements themselves allow for quantification of CH4 increases during D-Os (if I read Fig. 1 correctly), thus reducing uncertainty from splicing data sets.

Page 4662, lines 1 and following; as well as supplementary table: there needs to be a complete documentation on analytical precision. It may be fair to apply the same precision to all Bern measurements as is done in the supplementary table, but this should be stated and the concerned data sets listed. Yet, Schilt et al. quote Flueckiger et al. (2004) for their methods, implying a sigma value of 10 ppbv. For LGGE data, it must be clear what precision is assigned to individual data sets and why. Most of the LGGE data were published quoting 1 sigma values between 8 and 11 ppbv. The unpublished LGGE data of the present study were measured during the same time periods as the latter, so a similar precision is likely. There seems to be some confusion on the use of standard deviation and standard error as 1-sigma. This becomes relevant for assigning lead-lag times, where the start of an increase is defined as 3-sigma exceeded. Are the lead-lag findings robust in light of differing precision of the various data sets?

Page 4664, lines 20 & 21: this would be a good place to say explicitly why it is useful to not just compare CH4-variability magnitudes between D-O events but to scale them to temperature.

Page 4666, lines 8-10: this deserves a reference or two.

Page 4667, lines 8-11: here it is important for the reader to have the information on d15N analytical methods.

Page 4671, lines 14 and following: I suggest moving this technical information to the start of the section and discussing the results afterwards (although I appreciate the

note on a conservative interpretation of the results).

Page 4672, lines 4 & 5: please provide a reference or show a plot of the relevant temperature and insolation data. It would be helpful to mention that Greenland temperature variations are not just uncorrelated with insolation at 30 degN but also at 65 degN (Flueckiger et al., 2004).

Page 4672, lines 5-13: as a side note, should the range quoted in line 9 be “5-15 degC”? More importantly, I don’t see the value of the experiment performed here. Instead of randomly swapping temperature values between events, a more sensible test would be to correlate just the CH4 magnitude for each event with insolation. If the authors’ claim in lines 11-13 was true, then mu (sensitivity) should be fairly constant for all events and temperature would be orbitally controlled after all.

Section 4.2: this section lays out the fundamentals for influences on mu. However, there is also a lot of discussion on possible factors controlling the variability that is rather speculative (which is okay) and is completely disconnected to the later explanations for the same observations given in section 4.4. (e.g., low mu results from a stronger source displacement). I recommend that the discussion on mu-variability be presented in one coherent section. A possible option may be to restrict 4.2. to the basics and refer to 4.4. for a complete discussion of mu-variability.

Page 4674, lines 14 & 15: this would be true for salt marshes. I guess the hypothesis is based on the concept that higher sea level raises groundwater levels in coastal lowlands and so creates new wetland areas that are not subject to marine flooding and salt water intrusion.

Page 4674, lines 22 & 23: In Fig. 7d the time markers for D-O events are placed on the summer insolation curve and not on the spring curve, despite the fact that the evolution in mu seems to correlate with the latter. When placing the time markers on the spring curve one sees that there are simply no data for mu during the minima at 100 and 120 ka BP. Therefore, I think that the whole section (starting page 4674, line 16) on the co-evolution, or perceived lack thereof, between mu and insolation is missing the point. Insolation seems to explain pretty much the complete observed variability in mu. This has implications for the discussion of the CO2-CH4 link (see comments below). It is still valuable to discuss potential other factors such as shut-down of boreal sources, but it should be clear that this is done only for completeness of the argument and not because there is need for additional controls on mu.

Page 4675, lines 17-19: my reading of Singarayer et al. (2011) is that the one third difference can be attributed to CO2 fertilisation alone, as opposed to CO2 climate forcing as well (compare runs “All” and “All_FixCO2” in their Fig. 3). If the effect really is that high, would it not be apparent when comparing mu, CO2 and insolation throughout the last glacial?

Page 4675, lines 19 and following: the modern-day maximum CO2 sensitivity may be lower than at the LGM when low pCO2 may become limiting for NPP and small changes could have a greater impact (a point made on page 4676, lines 19-21). However, for a robust assessment whether there is a CO2-CH4 link one should use the minimum postulated sensitivity and test whether a corresponding signal can be detected in mu after accounting for insolation changes.

Page 4676, lines 4 and following: I find the concept of correlating pCH4 and pCO2 as illustrated in Fig. 8 throughout the last glacial period problematic. Yes, the correlation coefficients are impressive, but what would we get when correlating pCH4 with parameters like pN2O or dust flux, despite the complete lack of a causal link? Evidence that I would find convincing is a trend in mu during the last glacial or maybe a step change in mu at the time of the MIS5-4 transition. Neither is discernible in Fig. 3. I am worried that Fig. 8 shows a co-evolution that has no information on causality and is therefore misleading. If the derived sensitivities would be clearly below the Melton et al. values I would accept this as a negative result, but as is I strongly recommend omitting the whole paragraph and Fig. 8.

Page 4676, lines 21-25: in fact, if pCO2 has any discernible influence on CH4, how would this NOT be expressed in mu? Further, there is no evidence for slowly decreasing mu values (see Fig. 3), while the slowly decreasing maximum values can be explained by insolation alone. At least, the authors never attempt to show otherwise. There should be a discussion of the fact that Fig. 3 shows no trend in mu during the glacial, despite a steady decrease in pCO2. This also rules out a climate effect of CO2 (and other GHGs) on mu. For a robust test of the CO2-CH4 hypothesis, mu should be normalised for insolation and the correlation between residuals and pCO2 investigated. This would allow for an independent test of the findings by Singarayer et al. and Melton et al. If the authors proceed with a discussion of the CO2-CH4 link I see this as the minimum requirement.

Page 4676, lines 26 and following: this whole argument of correlated trends of pCH4 and pCO2 during and between D-O events is based on selective use of evidence. Is there an objective criterion to present the chosen time windows but not others? What is the relative occurrence (or duration) of times with co-correlation of pCO2 and pCH4 compared to times without? An illustrative example of the flawed argument is the time period before D-O 17, as shown in Fig. 9. Fig 7 shows that the pCO2 rise starts already 64 ka BP, which means that pCH4 stayed flat for another $\sim$3ka. Yet the authors use the event as support for a co-evolution of pCO2 and pCH4. This is not a robust test of the hypothesis; for a convincing argument one needs to demonstrate a pattern that is generally valid and a plausible explanation for exceptions. This passage is a real weakness in the paper and should be either rectified or taken out.

Section 4.4.: as mentioned before, parts of this discussion are relevant for explaining variability in mu.

Page 4680, lines 19-22: it would be helpful to explain the direction of these changes and how that aligns with the observed CH4 variability.

Fig. 1: I find it very hard to distinguish between old and new data in the current colour

scheme. It may also be helpful to plot the data from individual existing studies separately in order to show potential biases (or demonstrate that the applied corrections are correct).

Fig. 3: this is an important figure for the discussion of trends over time, specifically concerning insolation or potential CO_2 driven changes. It would be very useful to provide visual guidance on the ages of the data points. One suggestion would be to use a colour scheme for the plotted points that reflects their age (so that one could easily see if, e.g., warmer colours group in one sector).

Fig. 7: it would be helpful to include the NGRIP temperature reconstruction here. I have also argued above that the time markers for D-O warmings should be placed on the spring insolation curve rather than the summer one.

Fig. 8: as explained above, I recommend omitting this figure.

Fig. 9: as detailed above, I don't see a place for this figure. However, if the authors can make a convincing argument why it should be used, please note that the different x and y-axis scales in the bottom row make it impossible to see the alleged similarity of changes.

Page 4667, lines 2-4: please check grammar of this sentence.

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