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Climate of the Past
Discussions

Interactive comment on "CH_4 and N_2O fluctuations during the penultimate deglaciation" *by* Loïc Schmidely et al.

Anonymous Referee #2

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Schmidely et al. provide new high-resolution CH4 and N2O datasets over the penultimate deglaciation from the EDC ice core, and based on their data suggest an interpretation of the last deglaciation that has more structure and richness that the "canonical" interpretation of just a long AMOC shutdown over H11 from 136 to 128 ka BP (Cheng et al., 2009; Clark et al., 2020). While the interpretation is somewhat speculative at times, I for one believe that they are mostly correct. It is always exciting to see that the climate is more complicated than we thought at first – it always pays off to evaluate ice core records at the highest possible resolution. I think the manuscript is suitable for publication after addressing some issues. In particular, the new analytical setup (a major achievement by itself) deserves some more attention, as well as the observed (and surprisingly large) offset with existing records.

(1) The 134 ka DO event

I am most excited about the discovery of a minor DO event early in the deglacial sequence (134 ka BP). When first looking at their data, I assumed the 134 ka CH4 features was the TII equivalent of the 16.2 ka Heinrich 1 CH4 feature identified by Rhodes et al. (2015). However, based on the Chinese speleothem record, as well as the size of the feature, Schmidely et al. convincingly argue that it is in fact a small DO event.

Personally, I think the authors should present this 134ka feature as an analog of DO2 – which likewise has an extremely short duration, and a (very!) small CH4 peak. The two have a similar orbital configuration, with very small NH summer insolation.

The small magnitude of the CH4 peak seems in line with the small NH insolation at 134 ka. (Baumgartner et al., 2014).

The authors struggle to explain the short duration of the event relative to the Bolling, but I think this is not necessarily hard. It has been shown (Buizert & Schmittner, 2015) that the duration of DO events scales strongly with Antarctic temperature (and following more recent work, presumably also with mean ocean temperature). The 134 ka DO event occurred much earlier in the TII deglacial sequence than the Bolling event in the TI sequence. Therefore, during the 134ka DO event global CO2 and Antarctic temperature were both lower than they were during the Bolling. These factors would predict a much shorter DO event at 134 ka than at 14.7 ka, as is indeed the observation of the authors.

The idea that meltwater "squelched" the DO event seems unlikely to be the complete explanation by itself, given that the Bolling was likewise accompanied by MWP-1A. Such a MWP is indeed perhaps the result of suddenly warming the NH via AMOC invigoration, thereby melting ice at the NH high-latitudes. However, Buizert and Schmittner (2015) show that under colder Southern Ocean background conditions (as during the 134 ka event), the AMOC is much more susceptible to meltwater perturbation. This may explain why the Bolling survived MWP-1A, and yet the 134ka event did not survive

MWP-2B.

(2) The analytical setup

The new setup is a major part of the paper, and has not been documented elsewhere. The line uses some new methodologies that have not previously been used for CH4/N2O analysis. It thus seems necessary to provide more details on this aspect of the paper. The authors should provide a schematic drawing of the line, and discuss its operation in more detail. For example, how is the sample calibration done, and how often? What is the precision of the new line from replicates? Do you calculate concentration using the air concentration from the thermal conductivity detector? What carrier gas is used? What is the “line offset”? How many samples can you analyze? Do you apply a solubility correction? Does the line produce total air content data?

Most importantly, the authors should perform a more thorough investigation of the large offset of the new setup with old data. The authors choose to correct the new data, implying they trust the old method better. What could cause such an offset? It would need to be understood if the setup is to be applied to periods of time where we do not have a reliable CH4 record already that can be used for offset correction – during such periods we need to be able to rely on the internal calibration of the line.

Perhaps all this material could be place in an appendix as to not affect the flow of the paper. This would be a valuable place of reference for future work that uses this setup.

(3) Line-by-line comments:

L8 and L9: What does “assimilate” mean in this context? Please clarify

L13: why compare it to the Bolling? Why not DO2 – based on background climate (insolation, CO2, mean ocean temp, etc) that may be a closer analog. Compared to DO2 it looks quite normal.

L34: What is the width of the gas age distribution using the recent estimate of Epifanio et al. (2020) who related it to Delta-age?

Page 2: The distinction between millennial and centennial CH4 events is somewhat arbitrary and perhaps even incorrect. Several of the DO events last only a few hundred years, and some of the H-event CH4 changes persist > 1000 years (like H4 and H5). Maybe just call them DO and H-event CH4 features? That would be much clearer.

L61: “believed to” should be “hypothesized to”

L73 is “though to be” driven exclusively by . . .

L78: maybe give a one-sentence description of the IRMS setup for N2O istopes

L91: acknowledge this is also the approach used by the Japanese lab at NIPR (Oyabu et al., 2020)

Section 2: More details are needed here, as outlined above.

L102: This is a very large offset – surely much greater than the specified precision of either line. Can you clarify? The approach is quite cavalier. An explanation of the offset is needed. Is this due to the new method? That is implied by the correction approach. The comparison in Fig. 1 implies that the offset may be concentration-dependent (both CH4 and N2O). Can you show a scatter plot of old vs. new (found with the spline method), with a linear fit? It appears it may not be a constant offset as applied.

L147: hidden in the previously published?

L152: could you add subheadings to the discussion? This is just one long block of text now. Maybe separate out the discussion of the 130.5 ka event and the 134 ka event, for example. There may be other subheadings that can be added. This would help structure the discussion better.

L155: what does “tentative approach” mean?

L155: How do you define the width? Please be specific. Some common metrics are the second moment of the distribution (or variance, or spectral width) or the FWHM (full width at half maximum). Just be specific how you define the width.

L185: the 128 ka may as well be an analogue of the Bolling transition that ended HS1.

L189: It looks like the 128ka event in CH4 is more similar to the Bolling transition in terms of magnitude and timing.

L190: I doubt the difference in magnitude is related to smoothing, since the step in CH4 lasts for millennia, so that both sites reach the new value.

L214-216: this explanation seems a little tentative. The abrupt features are superimposed on the long-term precession-driven d18Oatm signal. Why would the earlier trend not just represent the orbital signal, for example? If abrupt transitions in the hydroclimate were involved, one would expect to see it in the Chines speleothems. Severinghaus et al. 2009 use the \Delta \epsilon_land to interpret d18Oatm, which is a more meaningful analysis.

L218: It seems a bit of a stretch to interpret the low-res Pa/Th data with such age uncertainty in this way. I think the authors need to exercise a bit more caution.

L226: Following Landais et al. (2013), we propose that. . ..

L242: where does the 70 ppb number come from? Based on the estimated degree of smoothing, what do you think the true magnitude of the DO event was like?

L248-249: I agree with this conclusion. Note that during the H1 CH4 event Chinese speleothems get more positive (and more negative for the 134 ka event).

L255: The magnitude of CH4 rises during DO events is modulated by the NH insolation signal. Based on that, can you place the 134 DO event into context for us? Are we expecting a large or big CH4 DO signal at 134 ka based on insolation?

L261: strengthening of the ASIAN monsoon system. . .. (probably SH tropics show opposite)

L285-292: the quenching of the emergent DO event by a MWP pulse does not explain the difference to the Bolling, because the Bolling was coincident with MWP 1A, which

seemed not to hurt it!

L302: As noted above, I think the short duration of the event is fully in line with expectations from DO events of the last glacial cycle, most notably DO2.

Fig 2 caption: what is the statement about AMOC vigor based on? And why are the transitions not aligned with those in the CH4 data? I think it would make more sense to align the boxes using your data. Also, shouldn't the newly discovered DO event have a red background (interstadial)?

Fig 4, right panel: why not show the d18O_atm here also? Would be very helpful. Can you show insolation somewhere, as well as Antarctic temperature? The slowdown in warming at 130.5 ka can be pointed out that way.

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