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

Anonymous Referee #1

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This manuscript describes a new protocol for the measurement of CH4 and N2O concentrations on small samples of ice. The new method is applied to samples from the EDC ice core and enables new composite records to be developed covering the penultimate deglacial period (Termination 2).

I'm sorry to say that I was rather underwhelmed by this paper. I had expected to see exciting new records with new insights and an opportunity to learn more about T2. Indeed, there are a couple of novel features reported but nothing very exciting. In all I suspect this paper will add to a growing body of studies dealing with T2 that will ultimately (but not yet) lead to an increase in understanding.

The authors describe a new analytical approach, an important step forward which deserves to be documented. However, the authors point to a significant offset with mea-

surements made by earlier methods and end up correcting their new data in an ad hoc fashion (minimising the difference between the various datasets). This operation implies that the authors have little confidence in the absolute values of their results, and this is obviously alarming. Is there really no way to produce a standard that can be used to cross calibrate between techniques?

The data themselves are interesting but offer little new insight beyond increasing temporal resolution of older records from the same ice core. The new composite record of N2O does improve on the older record but the CH4 record merely confirms that details which previously were suspected are actually real.

Some confusing nomenclature is developed here that leads to ambiguity and a loss of logic. For example, the authors distinguish 'late stadial' from 'intra-stadial' variability, which is fine on the face of it but becomes confused when they discuss the 134ka event, which occurs within a stadial event (HS11) but is apparently not an 'intra-stadial' event (somewhat of an oxymoron?). This confusion comes from the fact that the authors are using a non-specific term (intra-stadial) to define a specific mode of variability that was described in a paper(s) by Rhodes et al. (2015) and was previously argued to be related to strengthening of southern hemisphere monsoon systems and a southward shift of the ITCZ (as opposed to a northward shift, which might be expected with the abrupt transitions from stadial to interstadial state). Perhaps the authors need to find an alternative (more descriptive) name for these 'type' of event.

More confusion occurs with the discussion of the 130.5ka 'event', which the authors suggest might represent a transition from an HS event to a D-O stadial. This description doesn't make much sense to me I'm afraid. A Heinrich-stadial (HS) has been defined as a stadial that contains a Heinrich event (HE). Thus the label HS11 implies that this cold interval contains the Heinrich event HE11. It makes no sense to imply that HS11 can change to a regular stadial (that does not contain an HE) once Heinrich Event 11 has ended (if that is what happens). I suppose you could argue that HE11 (Heinrich Event 11) ended before the end of HS11 but HS11 does not become a regular stadial once the HE is over. I realise that the present authors took this idea from an earlier paper by Landais et al., (2013) but it makes no more sense in that paper.

Perhaps I am missing something obvious but the authors' discussion and figures describe an event at 128ka which I believe occurred between 128.6 to 128.7ka, in which case they should perhaps round up to 129ka rather than down to 128ka. Rounding to the nearest kyr and adding a tilde would be fine in the text (~129ka) but this is not something to do in a figure. In figures 2 and 3, the authors indicate a transition from stadial to interglacial conditions at 128ka that does not align with the rapid increase in CH4 that would more commonly be interpreted to indicate the end of a stadial period (cf end YD as shown in their Fig. 2).

My final major gripe concerns the authors' discussion and interpretation of their results. Most of the interpretation follows previous studies (e.g. rapid CH4 rise coincides with AMOC recovery at end of a stadial period) but where there is something new to discuss (e.g. the mysterious 134ka event) the authors provide a very slim argument for a major conclusion within their abstract (that this event was analogous to a D-O warming of MIS 3).

Below I outline some more comments in order of their appearance in the ms.

Abstract Line 5/6: "These features occurred in concert with reinvigorations of the Atlantic Meridional Overturning Circulation (AMOC) and northward shifts of the Intertropical Convergence Zone." Implies that the authors have identified at least 2 instances of this feature, which turns out to be a single instance (which was previously known about) and an additional instance that is substantially unsupported.

Line 8: (editorial) The authors' use of ka and ka BP needs attention; ka is fine on its own as shorthand for kyr ago or kyr BP (avoid ka ago or ka BP).

Line 8 and throughout: "...are assimilated to...". The meaning here is not clear – please check use of English throughout.

Main text Line 45: 'late stadial' – perhaps better as 'late HS'?

Line 46: "This mode of variability has been evidenced for the HS during the last glacial period..." Please check English.

Line 66-75: Much of this intro section feels more like discussion.

Results section: Much of the text here is unnecessary and feels verbose; you don't need to describe every inflection and quantify every rate - you have figures for this!

Line 129: "Abrupt CH4 rises are identified at ~134 and ~128 ka BP" Again, if rounding is to be used then rounding up from 128.6/7 to 129 ka would be more conventional.

Discussion Line 154: But the CH4 point at 139.9ka is from a previous study yes? Need to make this clearer.

Line 210: "...remarkably coeval...". Nothing remarkable here, perhaps approximately coeval.

Line 229: "Overall, the 130.5-ka event fits into the framework of the late stadial events (Schilt et al., 2013, 2014; Fischer et al., 2019) and can be viewed as an analogue of the late HS1 rise during TI." I am not convinced by this; the rise in N2O ~130.5ka is very similar in magnitude and rate as that ~129ka and yet the 'late stadial' rises documented for T1 and MIS 3 (Schilt et al., 2013) are significantly slower than the rather abrupt rise that accompanies the transition to interstadial conditions. Given that the 130.5ka rise is also separated from the 'end-stadial' rise by ~1000 years, which is also not noted for events during T1 or MIS 3 I think there is room for questioning the validity of this analogue.

Lines 235-284: I agree that something interesting happened ~134ka and that the CH4 and N2O evidence presented here, together with the speleo records and monsoon reconstruction reported by Nilsson-Kerr et al. (2019), suggest that something atmospheric was involved. The logical next step is to invoke a change in AMOC but there is no firm evidence for this. The authors argue (reasonably) that the age models employed for ODP 1063 by Bohm et al. (2015) and Deaney et al. (2017) are uncertain enough to allow significant wiggle room but the change in eNd that would involve (up to 2 epsilon units) is nearly half of that associated with the hypothesised AMOC resumption ~129ka. If eNd is taken at face value as a deep-water circulation tracer (which is not entirely free of problems) then why do we not find more evidence of such a large event ~134ka throughout the North Atlantic? Furthermore, the authors state that the timescale of CH4/N2O change across this event 'precludes' an oceanic source – so why call on an oceanic mechanism? Perhaps the authors need to soften their argument that the 134ka event is really such a good analogue for a D-O event (see earlier comment on Lines 5/6 of abstract).

The figures and fonts on these are painfully small.

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