Interactive comment on “High-resolution interpolar difference of atmospheric methane around the Last Glacial Maximum” by M. Baumgartner et al.

Answers to L. Mitchell (Referee)

GENERAL COMMENTS
This paper is an excellent addition to the scientific literature and is within the scope of Biogeosciences. It presents new data that leads to an important revision of our understanding of the methane interpolar difference (IPD) during the last glacial termination.
The authors have in general presented a very high quality discussion of the IPD, but I do have some comments/suggestions about a number of topics including the specifics of their modeling choices, their logic behind the connection between speleothem records and their IPD record, and a few other topics. I applaud the authors for their hard work to produce this record and look forward to seeing a revised manuscript.

Thank you very much for your detailed and constructive review comments, which have substantially improved the presentation of the material.

The two most important changes we applied to produce the revised version of the manuscript are the following:

- *Additionally to the already discussed two-box model run, we also performed a three-box model run (model from Chappellaz et al.,(1997)) to estimate the methane source distribution of the past. This allows to differentiate between boreal and tropical methane sources and to compare our two-box model run with an accepted model used in studies about the interpolar difference of methane.*
- *We changed the structure of the discussion part to point out more clearly the stability of the relative interpolar difference of methane (overall stability, with notable exceptions).*

The specific comments are answered in detail below.

SPECIFIC COMMENTS

Page 5473; line 4. The rIPD in 2010 is about 7% and later in the paper (pgs 5487, 5492) this is referred to as the "present day anthropogenicly modified rIPD (7 %)". However, if you look at the rIPD from the northernmost and southernmost NOAA sites for the whole modern record you can see that the rIPD was much higher (~8.5%) in the 1980s and has decreased since then. I calculate that the average rIPD over the whole modern record is 7.6%. I think this would be a better figure to report than just 2010 which is actually one of the lower points in the modern record. Perhaps it would be good to also report the standard deviation (0.5%) on this as well, just to give readers a reference as to the amount that the rIPD can change on an interannual basis. I would encourage the authors to verify my calculations.

Thank you for this very good suggestion. We used the NOAA stations ALT (Alert, Canada) and SPO (South Pole) and agree with your calculated numbers. We changed the sentence to: 'The higher CH4 emissions in the Northern Hemisphere compared to the Southern Hemisphere induce an interpolar concentration difference (IPD) which today is (under the anthropogenic influence) about 134+\-7 ppbv (7.6+\-0.5%) averaged over the years from 1985 to 2010 (Dlugokencky et al., 2011), where the uncertainty is the 1sigma standard deviation over the selected time span.'

Page 5473; line 29. Levine et al (2011) does not find that the effects of BVOCs are negligible. They find that the effect of BVOCs is counteracted by the effects of temperature and that the net change after accounting for the effects of temperature change and BVOCs is negligible.

We apologise for this incorrect statement. We replaced it with (see suggestion by other reviewer, J. Levine): 'the effect of changes in BVOC emissions to be compensated by the effects of changes in air temperatures on humidities and gas-phase chemical kinetics'. We further added: 'Levine et al., (2012) confirm this statement also for DO events, and suggest that the concentration changes in CH4 are mainly source driven.'

Page 5474; line 21-22. Technically this statement ": : :thus represents about the maximum resolution possible." Is correct, but it is misleading since higher resolution and/or higher precision measurements would reduce the uncertainties of the IPD. My opinion is that this statement should just be removed because it does not add anything.

We deleted: 'and thus represents about the maximum resolution possible'.

Page 5475; line 7. This is not an entirely circular argument because atmospheric smoothing and firn air smoothing would cause the fast and strong variations in CH4 to be smoothed out. If a true atmospheric variation in the IPD lasted for long enough to actually be visible in the ice core record you would need to invoke some extreme scenarios such as shutting down NH sources for multiple years, possibly a decade. This is such an extremely unlikely scenario that it can be ignored based on Occam's razor.

We deleted: '…which is a circular argument.' We further added the already cited reference Buiron et al., (2011) to the statement about the IPD/synchronisation problem.

Page 5476; Line 3. While the effects of gravitational depletion may not affect the IPD very much, they do have an impact on the absolute concentration values. Since this is a known effect and the authors state that it is a simple correction, I see no reason NOT to make the correction. If the correction is not made, it becomes cumbersome for other workers to compare their data to this study and thus limits the utility of their data. It could also increase the confusion among people who are trying to use their data but do not have specialized knowledge about all the specific corrections that ice core records need.

If the authors feel that their estimate of gravitational depletion is not sufficiently accurate to make the correction, then this should be explained in detail.
Gravitational correction was never corrected for in previous studies (e.g., Buiron et al., (2011); Stenni et al., (2011); Loulergue et al., (2008); Schilt et al., (2010); EPICA community members (2006); Huber et al., (2006); Spahni et al., (2005); Flückiger et al., (2004); Flückiger et al., (2002)), at least not in studies from the Bern lab (and neither in the collaboration works with LGGE Grenoble). We thus prefer not to correct the data for gravitational depletion, to remain consistent with the above listed datasets (especially with previously published NGRIP and EDML data). We added the above listed references to the manuscript.
Further, there are always offsets between different labs (at the moment we observe a 6ppbv offset between the lab of Grenoble and Bern (Spahni et al., 2005). This study shows that the offset can even be 30 ppbv (GRIP measurements). Further, the measurements are made on an internal Bern scale which is 1% higher compared to the NOAA scale (see comment below).

Also, as a smaller point, I don't understand how they calculated the combined IPD error on line 1 (0.5 +/- 0.7 ppb). I see that 2.9-2.4 = 0.5, but if you take the max and min uncertainties you should get 0.5 +/- 1.0 ppb, right?
The error of the gravitational depletion was calculated according to Gaussian error propagation \sqrt(0.6^2+0.4^2).

Also, they do not attempt to correct for the effects of solubility or even mention this. Since they are using a wet extraction technique they should address this topic as in Mitchell et al (2011). Again, if their results are to be compared with other labs, or with data obtained from a dry extraction technique, they should correct for this.
Before and during the measurement series we perform extensive series of measurements with air-free ice with standard gas to check for a solubility effect. We inserted the following paragraph on this topic:
'In contrast to Mitchell et al.,(2011), who observe a loss of CH4 due to solubility effects during the wet extraction process, blank measurements with air-free ice and standard gas show a concentration independent contamination (Chappellaz et al., 1997) in the order of 10 ppbv depending on the particular glass container. For each of the glass extraction containers, we determine a separate correction value, which is subtracted from each measurement on natural ice.'

As a final point about the methods, I did not see anywhere that they reported which methane calibration scale they were using. I'm assuming that it is the NOAA04, but it should be explicitly stated in any case.
We use an internal calibration scale. Measurements in 2012 of three NOAA standard gases (370.6 ppbv, 559.2 ppbv, 788.2 ppbv) show that the internal Bern scale is about 0.7% above the NOAA scale. In 2006, the same NOAA standard gases were 1.3% higher on the internal Bern scale compared to the indicated NOAA values.
We inserted: 'Intercalibration measurements with NOAA standard gases show that the Bern CH4 concentrations are in the order of 1% higher compared to measurements performed on the NOAA scale.'

Page 5482; line 1. Perhaps I'm misunderstanding this, but it says "The output of the firn model is shifted to lower concentration until the smallest root mean square difference to the measured EDML data is achieved." Do the authors actually mean they reduced the linear scaling factor for the INPUT to the firn air model until the root mean square difference was minimized? I'm not sure what is meant by shifting the firn model. Also, as a syntax correction, it should be "concentrations" not "concentration".

We apologise for this unclear formulation. As you assumed correctly, we varied the linear scaling factor for the input to the firn air model until the root mean square difference was minimised (same procedure as at the NGRIP site). Since we use the linearly scaled NGRIP data as the input for the model at the EDML site, and due to the IPD, the mean concentration value of these NGRIP data is higher compared to the mean concentration value of the EDML data. This concentration offset has to be corrected for (was meant by 'shifting') before the fit to the EDML data is made.
We replaced the lines1-3 (page 5482) with: 'The smallest root mean square difference between the output of the firn model and the measured EDML data is obtained by varying the linear scaling factor (1.48 [1.35,1.62] for DO event 3 and 1.11 [1.02,1.21] for DO event 4) and the offset between the NGRIP and EDML data, which has been assumed to be constant over the entire DO event.'

Page 5482; equations. Many of these variables are undefined in the text. What is mo? What is co? M? S?
We define now m0, c0, M, S, and Omega in the text. Further, we defined a new factor m within equation 4. This factor converts concentration into mass (see also comment on m0/c0 below).*

Page 5483-5484. The parameterization of the tex component of their model is not sufficiently justified. They obtain a value of tex of 2 years and discuss that the reason for such a long interhemispheric transport term is that they do not lower the concentration of their NH record to represent the entire NH. I have reservations about this approach for the following reasons: 1.) All previous studies that I have seen have estimated an interhemispheric mixing time of ~1 year. This includes Fung et al (1991), Chappellaz et al (1997), Kahlil and Rasmussen (1983), and more recently in Cunnold et al (2002) (there are probably many others too). All of these studies tune their transport with tracers such as 85Kr, SF6, and CCl3F. An interhemispheric mixing time of ~1 year is a broadly accepted value and to use a value of 2 years is not sufficiently justified in the paper, nor is their tuning procedure documented. 2.) Sowers (2010) lowered the cn, but so did Chappellaz et al (1997) (who is not referenced in this part of the paper). I would say this is the "accepted" method of modeling the IPD and I would expect justification for why this method is wrong or inferior if the authors wanted to use a different method.
I do not expect that these model parameter choices will change the magnitude of the modeled methane emissions much or at all. However, it strikes me that they are using model parameters that are inconsistent with previous models and I would like to either see additional justification for their approach or just use a previously published model.
We absolutely do not want to say that the accepted method with three-box model is inferior or wrong. Both models (three-box/two-box) have their advantages and disadvantages.
We added a small section at the end of chapter 4, which addresses the question about two box model / three box model. Further, there is an additional plot which shows the calculated sources from the two-box model and also from an alternative run with the three-box model from Chappellaz et al., (1997). We also plotted the fractions of the boreal, tropical, and southern sources of the three-box model in Figure 6b. We further removed the estimate of the total source strength from Figure 6b, since the total source strength is now visible in the additional plot.

The two-box model was initialised on today's concentration (1985-1987) of the northern NOAA station (BRW at 71°N, cn0=1778 ppbv) and the southern NOAA station (SPO, cs0=1620 ppbv). The total emissions of 490 TgCH4/yr are from Fung et al.,(1991), and are split between Northern (sn0=371.1TgCH4/yr) and Southern Hemisphere (ss0=118.9TgCH4/yr). This initialisation leads immediately (no model tuning) to tau=10.1 yr and tex=2 yr. Concerning the initialisation on a very high northern latitude CH4 concentration we stated: 'This essentially implies that this exchange time is representing the time needed for CH4 to sustain the measured interpolar and

not a mean interhemispheric concentration difference.' (We moved lines 5-15 (page 5484) to page 5483 (line 1), to give more weight to this statement)
We could also have lowered the northern concentration by 26% of the interpolar difference (Sowers (2010)). This would lead to tau=9.9yr and tex=1.4yr which is closer to the order of tex=1yr (and actually very close to the estimate (1.3 +/- 0.1 years) from the SF6 study by Geller et al.,(1997)). The calculated sources would change less than 1 TgCH4/year for this scenario.
To be consistent with the modern calculation from the introduction part (see first comment) we changed the northern NOAA station from BRW (71°N) to ALT (82°N). At ALT the CH4 concentration appears to be slightly smaller, but it is the most northern station. This changes the parameters slightly to tau=10.0 yr and tex=1.8 yr.

We further added an alternative initialisation according to the source distribution from a more recent study by Spahni et al., (2011):
'An alternative initialisation with a source strength estimate (548 Tg/yr) and a source distribution estimate for the year 2004 from a more recent CH_4 budget modeling study (Spahni et al., 2011) yields tau=9.5yr and tex=1.7yr, which compares well with the above initialisation.'

Another question I have about their model is they say the ratio mo/co = 1.45 Tg/ppbv (pg 5483 line 2) and they use this to convert concentration to mass. First of all I could not find this figure in Steele et al (1992) and second of all this is much different than that reported by Etheridge et al (1998) which used 2.767 Tg/ppb (from Fung et al (1991)) and Mitchell et al (2011) which used 2.844 Tg/ppb. Perhaps this is just representing ½ the values used in Etheridge et al (1998) and Mitchell et al (2011), but this should be explained more clearly and should have a better citation (my apologies if Steele et al (1992) do report this, but I couldn't find it).
Steele et al (1992) do report this. This reference to convert concentration to mass was also used for the three-box model studies (Chappellaz et al (1997), Dällenbach et al.,(2000)). Steele et al (1992) equal 4800 Tg CH4 to 1650 ppbv, which results in a conversion factor of 2.91 Tg/ppbv. m0/c0 represents one half of this value, as you correctly assumed. The factor ½ results from the volume of one hemisphere, which is ½ of the total atmospheric volume. The conversion from concentration to mass is now described more clearly in the manuscript.

A final question/comment about the model. A particular set of transport terms was used for both glacial and interglacial time periods. I would like to see some discussion about how changing atmospheric transport may have affected the rIPD on glacial-interglacial timescales. This could presumably be a very important factor in interpreting the records and could be the reason for the very slight differences between glacial-interglacial differences in the rIPD. It might be unrealistic or unhelpful to try to model any of these changes with a simple two box model, but I would like to see a qualitative discussion about what the literature suggests atmospheric circulation changes to be (if there is any literature on this). If there is no literature on it then the authors should point out the need for future work on this topic.
Thank you for this helpful suggestion. We extended the discussion on Figure 5:
'Similar sensitivity experiments by Brook et al., (2000) with the three-box model show that for a fixed rIPD the boreal source increases and the tropical source decreases with decreasing tex (faster interhemispheric mixing). In the two-box model used in this study, assuming a fixed rIPD, sn would increase and ss would decrease with decreasing tex (faster interhemispheric mixing). Conversely, assuming a fixed source distribution, a decrease in tex (faster interhemispheric mixing) would result in a decrease in the rIPD. Glacial/interglacial changes in tex could thus have affected the rIPD. If the interhemispheric mixing was faster in the glacial times, this would imply that the northern source was stronger than estimated in this study. Further work is required to constrain the changes in tex on glacial/interglacial time scales.'

Page 5485; line 2-4. This sentence indicates that boreal emissions control the isotopic composition of CH4, but this is not exactly true. Boreal wetland and tropical wetland emissions have a similar d13C signature. Also, this sentence seems to have too many ideas in it and should be rewritten for clarity.

The introduction about d13CH4 in the discussion part is now as follows (see also additional comment by J. Levine):

'The isotopic composition of CH4 is influenced by the relative strengths of the different CH4 sources with different isotopic signatures. However, also changes in the isotopic signature of individual methane sources over time (Schaefer and Whiticar, 2008) or changes in the relative strengths of different methane sinks (e.g. oxidation by atomic chlorine or the hydroxyl radical) showing greater/lesser preference for removing 12CH4 over 13CH4 can influence delta13CH4 (e.g. Allan et al., 2001; Levine et al., 2011).'

Page 5485; line 14. The cs/cs,ref curve in plot 6 looks pink to me, not red. Labeling it as red threw me off because the "pink" curve looks more red to me than the red curve.

We changed 'red' to 'light red curve in the background'. And we inserted 'EDML' just before 'CH4 concentration' for more clarity what data set is to be expected.

Page 5485; lines 18-21. While the rIPD mean values have the trends that the authors indicate, the uncertainties are large enough to make this type of differentiation a bit suspect. For example, the rIPD estimates between 24-28ka are all not statistically different from the reference time period. The estimate at 21ka is lower, but DO2 is higher. So, is there really a robust argument that "the rIPD tends to be lower" than the reference time period from 21-28ka? Similarly, of the 4 rIPD estimates between 11-21ka, 3 of them are not statistically different than the reference time period, so is the statement that the "rIPD tends to be higher" robust? Perhaps what is needed here is just a little more verbiage acknowledging that while the rIPD mean values are higher or lower, the estimates are not statistically different from the reference time period.

We agree that this concept of low/high rIPD states was inadequate. We followed the suggestion by J. Levine and now stress a relatively constant rIPD with notable exceptions. We changed the titles of the sections 5.1.1 to 'rIPD around the LGM' and 5.1.3 to 'rIPD during Termination 1' and integrated section 5.1.2 to section 5.1.1.

We replaces lines 18-21 with:

'To second order, we identify notable exceptions in the overall stability of the rIPD. The most outstanding feature is the low rIPD in the interval 21.9-21.2 kyr BP after DO event 2. Both the decrease in the rIPD from DO event 2 to this interval and the subsequent increase in the rIPD from this interval to the interval 20.4-17.8 kyr BP are statistically significant. The same is true for the increase in the rIPD from the interval 27.0-24.4 kyr BP to the DO event 2. In the following we discuss in detail the time span around the LGM, the Termination 1 (T1), and the DO events.'

Page 5486; lines 9-12. This idea that an ITCZ shift may change the volumes of the NH/SH boxes is interesting, but the implications for the IPD are not clearly spelled out. The authors indicate how it might affect the concentration in the boxes, but not how it might affect the IPD. If there is a significant difference then presumably they can model it to quantify it.

We found a way to model the volume changes connected to the ITCZ shifts and describe it now more quantitatively as you suggested. The paragraph is now as follows:

'Second, a southward shift in the ITCZ would increase the volume of the northern box at the expense of the southern box. A 1° southward shift would change the volumes in the northern and southern box by about 2% in opposite directions. For 5° the volume change of a box is 9% and for 10° it is 17%. If we assume that the interhemisheric mixing time of a box is proportional to its volume, the two-box model simulates the volume changes caused by the ITCZ shifts by

using different mixing times texn and texs for the northern and southern box. Assume now that the ITCZ was exactly at the equator during the glacial reference interval (20.4-17.8kyrBP). If we now take the northern and southern emission strengths of the glacial reference interval from Table 2 (s_n =76:5Tg/yr, s_s =35:3Tg/yr) and vary the volumes of the northern and southern box, respectively, we find an alternative way to explain the rIPD variations e.g. of theglacial neighbour intervals. We need a 8.5° southward shift of the ITCZ to explain the rIPD in the interval 21.9-21.2 kyrBP. Analogous a southward shift of 4° is needed for the interval 27.0-24.4 kyrBP and a northward shift of 3.5° for DO event 2.'

Page 5486; lines 17. The reference to Singarayer et al (2011) is inaccurate. The ITCZ is not mentioned once in the main paper and in the supplemental material it only mentions that the ITCZ is correlated with 65N summer insolation when NH ice sheets are large and 30N during interglacials. In my view Singarayer et al (2011) find that subtle changes in the regional forcing of all methane regions combine to cause the 0-5ka increase in emissions.
In their abstract, Singarayer et al (2011) state: 'Our analyses indicate that the late Holocene increase results from natural changes in the Earth's orbital configuration, with enhanced emissions in the Southern Hemisphere tropics linked to precession-induced modification of seasonal precipitation.'
And at the end of their paper:
'We conclude that the late Holocene increase in methane can be primarily ascribed to increasing emissions from the Southern Hemisphere tropics.'
Although not mentioned in the text (as you remarked correctly), the southward shift of the ITCZ might contribute to the southward migration of methane sources from 5kyrBP to 0kyrBP (southward migration is visible in the Figure 3b of Singarayer et al., (2011)).
However, we removed the word 'ITCZ' from the sentence and now say: 'Along the same line Singarayer et al (2011) explain the increase in the CH4 concentration during the Holocene, which started at 5 kyrBP, with increased emissions from the southern low latitudes due to precession-induced modification of seasonal precipitation.' Further we replaced 'southward shift of the ITCZ' by 'southward migration of the CH4 sources' on line 21 (page 5486).

Page 5486; line 21-23 & many other places. The terminology used here to refer to the Holocene is slightly confusing. Chappellaz et al (1997) have 4 time periods in the Holocene: 0.25-1ka, 2.5-5ka, 5-7ka, and 9.5-11.5ka. Throughout much of this paper the authors refer to the time period 0.25-1ka as "Preindustrial Holocene" but this is inaccurate because presumably the "Preindustrial Holocene" is any time period between 0.25-11.5ka. This should be changed to either "Late Holocene" or "Late Preindustrial Holocene". Further, the mid Holocene is typically referred to as ~5ka, but this is right between the two time periods of 2.5-5ka and 5-7ka. I encourage the authors to just define what they mean by "Mid Holocene" and stick to that definition within the paper.
We added the exact time periods (e.g. 2.5-5ka) when necessary.

Page 5486; line 28. In the comparisons with the South American speleothem records, I would suggest that the authors consider the record from this paper:
Wang, X. F., A. S. Auler, R. L. Edwards, H. Cheng, E. Ito, Y. J. Wang, X. G. Kong, and M. Solheid (2007), Millennial-scale precipitation changes in southern Brazil over the past 90,000 years, Geophys. Res. Let., 34(23), 5.
This record is continuous over the CH4 IPD time period considered in this paper and shows much tighter anti-correlation with the Asian speleothems. Perhaps some of the discussion in the paper would change based on looking at this speleothem, but for now I will continue to review the IPD record compared only to the Kanner et al (2012) speleothem record as the authors have written the paper.

Thank you for this very good suggestion. We replaced the record from Kanner et al., (2012) with the record from Wang et al., (2007).

Page 5487; line 7-8. The uncertainties associated with the IPD and the rIPD show that the level at 21ka is not statistically different from the levels at 26ka, DO3 or DO4, so I disagree that the levels at 21ka can be labeled the "glacial maximum in the CH4 cycle".
We deleted this sentence.

Page 5487; line 14-28. There are no simple to interpret trends in either of the speleothem records around 21ka (for example Hulu does not have any trend at 21ka, but changes quite a lot between 15-20ka). My feeling is that since the IPD and rIPD records presented here are indicative of long time periods and are not a continuous record it is not especially helpful to discuss variability that is much shorter than the IPD time periods. Also, the authors note that we should be careful in comparing speleothems and ice cores due to chronology synchronization issues, but I would further argue that there is no simple relationship that could relate speleothems to ice core methane records on timescales shorter than millennial. This is in part because there is still not a widely accepted interpretation of what exactly speleothems represent (amount effect or seasonal effect). I have found this paper to be very illuminating:
Clemens, S. C., W. L. Prell, and Y. B. Sun (2010), Orbital-scale timing and mechanisms driving Late Pleistocene Indo-Asian summer monsoons: Reinterpreting cave speleothem delta(18)O, Paleoceanography, 25.
Thank you for this interesting reference on the interpretation of speleothem records. We agree with you that it is not especially helpful to discuss variability that is much shorter than the IPD time periods. Thus, we deleted lines 14-18 (page 5487). We further added: 'Due to synchronisation uncertainties between ice core and speleothem records and only weak variations in the speleothem signals during this time period as well as uncertainties in the interpretation of speleothem records (Clemens et al., (2010)), we do not attempt to interpret any trends in view of changes in monsoon strength.'

Page 5488; lines 3-5. The millennial scale speleothem variations (including Wang et al (2007) above) during the BA/YD are a little clearer than during the LGM and I think this section could benefit from a discussion about this topic (despite the caveats of the speleothems listed above).
We added: 'It is also notable that the monsoon records from the northern and southern hemispheres show a pronounced anti-correlation during the BA-YD-Holocene transition. The southward displacement of wet conditions might contribute to the slightly lower rIPD during the YD.'

Page 5488; lines 11-23. All 3 of these arguments have issues which should be addressed. 1.) The authors state that they need an INCREASED sn, but then state that the Afro-Asian drought actually caused a DECREASE in sn (lower latitude NH is still within the NH). This is a limitation of their present modeling framework which only has 2 boxes. If you only have 2 boxes then it is not technically possible to distinguish between NH tropical and NH boreal source changes. To do this the authors would need to follow the modeling framework presented, for example, in Chappellaz et al (1997) which used a 3 box model. However, the drought also affects the tropical and SH portions of Africa which is consistent with a slightly decreased ss in the record, but the authors do not comment on this. 2.) It is unclear to me how a change in d13C might indicate a change in the boreal source. In Fischer et al (2008) they argue that the increase in concentration must be biogenic and that the increasing IPD indicates that it must be boreal, but the d13C by itself cannot distinguish boreal from tropical sources since they are both wetland

sources and have a similar d13C signature. In fact, perhaps an interesting thing to point out is how this study contradicts the findings in Fischer et al (2008). In Fischer et al (2008) they state "Considering the 50% reduction of atmospheric CH4 concentrations and the lack of an interhemispheric gradient in the LGM, a reduction of boreal wetland emissions is more likely." This study shows that there WAS an interhemispheric gradient in the LGM and so the increase in biogenic sources between the LGM and the BA must have occurred in both the boreal regions AND in the tropics. This is an important distinction and one of the main points from section 3.3, 5.1, the abstract and the conclusions but is not discussed here. 3.) It is not clear to me that benthic d18O records should have a 1 to 1 relationship with boreal ice coverage and/or the northward migration of permafrost. Perhaps this is true but I would like to see a reference making this connection in this argument.

1) First, the increase in sn is needed because the concentration as well as the rIPD increases from the interval 20.4-17.8 kyrBP to the interval 17.8-14.7 kyrBP. We use the Afro-Asian drought as an argument for why this increase in sn could be boreal (because we can not distinguish between boreal and NH low latitude sources with the two-box model output, as you remarked correctly). Second, there is no decrease in ss, ss remains at a constant level (Table 2, page 5500). We guess that your interpretation was based on Figure 6b, where sn and ss are plotted as a fraction of stot, which is a bit tricky but is more closely related to the rIPD.

The three-box model run shows a strong increase in the boreal source and a slight increase in the tropical source. Unfortunately, the three-box model can not distinguish between NH and SH tropical emissions. If we believe in the constancy of ss from the two-box model, then we could attribute the increase of the tropical source to the NH tropics.

2) We changed the second point about isotopic composition and compared it more closely with the interpretation from Fischer et al., (2008) and say, what the consequences of our new data are.

3) Since the relationship between benthic d18O and boreal ice coverage is indeed not straightforward and this is only a minor part of the paper, we decided to delete the benthic d18O record from the discussion.

Page 5489; lines 12-26. The detailed discussion of fine scale features of the speleothems is interesting, but I do not think it adds anything to this paper. As I mentioned before, I would be cautious about interpreting the SH monsoon strength or the NH monsoon strength based on a single speleothem (Clemens et al (2010)).

Thank you for this comment; we deleted lines 12-26 (page 5489). We replaced it by: 'As described in section 5.1.1, we hypothesise that the source redistribution within lower latitudes and the changes in the size of the northern and southern hemispheric box connected to shifts in the ITCZ also contribute to the subtle variations in the rIPD during DO events.'

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Page 5490; line 3. It is not clear to me why the authors talk about the RATIO of Northern to Southern summer insolation (Ins/Iss) instead of just talking about the NH insolation? In figure 6 and 7 they just plot the NH insolation, not the ratio. They should either change the text to refer to just NH insolation or add the Ins/Iss curve to figures 6 & 7.

We changed the text and refer now just to NH insolation.

The same was done in the conclusion part (page 5491, lines 25-26).

Page 5490; line 10. First, I actually can't tell when the maximum in Ins/Iss is because it is not plotted. Presumably the authors are talking about NH summer insolation maximum, but that is not until ~9ka (not ~14ka as they mention in the text) and the maximum in the rIPD is at 16ka, not 14ka! I actually don't know why the authors mention 14ka at all here.

The 14ka was the age of the BA2 interval, interpreting this as the maximum rIPD because the interval at 16ka has a rather large uncertainty. We apologize for this crude formulation. We changed this paragraph substantially, now it reads:
'Further, with the exception of DO event 2, there is a clear decreasing trend in the rIPD from 30-20 kyr BP in line with northern summer insolation. Less clear and again with an exception, the same could be true during the Holocene from 10-0 kyr BP. The increasing northern summer insolation from 20-10 kyr BP, however, has no clear counterpart in the rIPD due to the stability of the rIPD during T1.'

Page 5490. The author's interpretation is not consistent with the data here. On lines 11-12 they argue for a long term trend (ignoring the data point that they don't like at 4ka) when the data error bars all intersect the reference value, so the trend cannot be statistically different from zero (relative to the reference). On lines 12-14 they do the same thing for the time period from 30-20ka and again have the same problem, all the data is not statistically different from the reference value except for DO2 (which they don't want to include in their trend) and the one data point at 21ka. To me the more important finding should be exactly the opposite of what they are claiming: there are NO statistically significant trends associated with orbital scale changes in insolation, contrary to the Singarayer et al (2011) model. This would be clearer if the authors had plotted their error bars on figure 7 and included DO2 in their time series, or not plotted a line at all, or the authors should exclude ALL the DO events from their time series which would make sense because Singarayer et al. (2011) did not claim to simulate millennial scale events. If they did this it might be clear that the Singarayer et al (2011) estimate of the rIPD for 20-30ka is too low. This would be an important finding to report because modeling studies can use a relatively constant rIPD as a modeling target, and the variations in the Singarayer et al (2011) record may be too large.
Thank you for your useful suggestions. We plotted the rIPD error bars on figure 7, removed the line and the special treatment for DO event 2. We also included the rIPD value for DO event 8 from Brook et al., (2000), which was already visible in Figure 3. We decided not to remove all DO events, since from the Singarayer et al., (2011) study it is not clear whether the stadials or the interstadials should be removed (They are not able to model millennial scale variations, but they partly model interstadial methane concentrations levels).
Concerning the points we do not like: On lines 16-17 (page 5490) we stated: 'Further, the DO event 2 and the Late Holocene are clearly outstanding, which points to superimposed processes on shorter time scales'.
Despite the overall stability of the rIPD, we should respect the weak variations observed in the record (some changes are statistically significant). In other words, we should not discard any variations in the rIPD based on the fact that the variations appear to be much smaller than previously reported.
Anyway, we substantially rewrote section 5.1.5 and think it is more straightforward now. The section is concluded with the following statement (see suggestion by J. Levine):
'In summary, our data neither support nor fully rule out a possible long-term influence of northern summer insolation on the rIPD. The limited range of our data set combined with the weak variation and the superimposed processes on millennial time scales do not allow for any conclusive remarks on this topic. High-resolution records produced in the way presented here from both poles, and over the whole last glacial cycle, are needed to address this question. The importance of the rIPD as a constraint for models is a strong motivation for future high-resolution measurements.'

After re-reading the abstract and conclusions, it seems that those parts of the paper stress a relatively constant rIPD and this portion of the paper is inconsistent with the abstract and conclusion.

Indeed the new data show a relatively constant rIPD. In particular, the new rIPD data show less variation than previously reported. However, some variations are still statistically significant. The fact that the variations in the rIPD are much smaller than previously reported does not justify to neglect the smaller variations observed in this study. Thus, we do not agree that the section about the long-term trend is inconsistent with the rest of the paper. However, it has been substantially modified and should be easier to follow now.

Page 5491-5492. Some of the conclusions may change based on the above comments, particularly the last part about the monsoons. The Abstract is similar. I'm sure the authors will do a good job updating the abstract and conclusions based on the above comments.
We deleted: '…coincident with decreasing southern and increasing northern summer monsoon strength' (page 5492, lines 1-2) from the conclusion, based on your comments on the speleothem records.

We replaced lines 15-17 (page 5492) of the conclusions with the following sentence:
'We hypothesise that latitudinal shifts in the ITCZ and the monsoon system contribute, either by dislocation of the CH4 source regions or, in case of the ITCZ, also by changing the atmospheric volumes of the northern and southern hemispheres, to the subtle variations in the rIPD on glacial/interglacial as well as on millenial time scales.'
Lines 20-24 (page 5472) from the abstract are replaced by the same sentence.

We added to the conclusion (as you suggested in an above comment): 'In agreement with Brook et al., (2000) we conclude that the increase in the CH4 concentrations over Termination 1 is established by an increase in the boreal and the tropical source by approximately the same factor.'
Similar in the abstract: 'We thus find that the boreal and tropical methane sources increased by approximately the same factor during Termination 1.'

Page 5505 Figure 5. I think the labels inside the figure area (tao = 10.1 yr and tex = 2 yr) are supposed to be switched between the upper and lower panel.
The grey lines indicate the values of tau and tex used in the model:
'While one parameter is varied, the other is set to tau=10.1yr and tex=2 yr (grey lines), respectively. The model is run at these values which have their origin in the initialisation with a present-day source distribution from Fung et al., (1991).'
We thus see no reason to switch the labels.

Page 5508 Figure 7. Refer to the comment above. The error bars should be plotted. Also, DO2 should be included in the time series, or all the DO events should be excluded from the time series, or there should be no line at all.
The error bars are now plotted, DO2 is included and the line is removed.

TECHNICAL CORRECTIONS
Page 5473; lines 12-15. The sentence which starts with "Wetland CH4 production: : :" is confusing because it is relating the factors which affect methane emissions and the correlation of satellite observations to temperature and precipitation. These two things (factors controlling emissions and observed correlations with emissions) should be in separate sentences for clarity.
Done.

Page 5487; lines 2-3. The sentence starting with "In line the: : :" is confusing and should be re-written for clarity.
Due to changes in the discussion part, this sentence has been deleted.

Page 5487; line 25. Also on Page 5492 line 5. Modern emissions are 2.5 times the Late Preindustrial Holocene, not "twice as large". This could be changed to "more than twice as large" but it would be nice to just explicitly say 2.5 times as large.
Changed to 2.5 times as large.