We thank the anonymous referee #1 for her/his careful review of our paper. We appreciate the useful comments and believe the input will improve the manuscript. The original referee comments are copied in black, and the author's response to the comments are given in red italics. We add paragraphs from original discussion paper in blue italics and our modifications in green italics.

Summary of manuscript

First of all, I would like to congratulate Ji-Woong Yang et al. for the excellent work they put into producing a high-resolution record of CH_4 mole fractions as well as their interpretation of the data. So far, the early Holocene is underrepresented in high-resolution CH_4 reconstructions and this paper will be a valuable addition to the literature. I hope the following comments will be helpful and I look forward to reading the revised version of the manuscript.

Ji-Woong Yang et al. reconstruct the CH_4 variability of the Early Holocene, from 11.6 to 8.5 ka before 1950, using a melt-refreeze extraction system coupled to a GC-FID analyser, which was newly developed at Seoul National University (SNU). The new method is very briefly described in this paper and not yet published. The authors show that the SNU data are in good agreement with two existing benchmark records from the WAIS divide ice core, where the latter two were measured using *i*) a similar technique (WAIS members 2015) and *ii*) a sample gas stream derived from a continuously melted ice core, analysed by an optical instrument (Rosen et al., 2015).

Ji-Woong Yang et al. observe millennial CH_4 minima besides the 8.2 ka event, which have not been identified in previous studies. The authors relate these CH_4 minima to events in other geological records that indicate climate variability in the low and high latitudes of the Northern Hemisphere. These records include: $\delta^{18}O$-H_2O (NGRIP), ice rafted debris, ^{10}Be, reflectance of Cariaco Basin sediments, $\delta^{18}O$-$CaCO_3$ (speleothems) and $\Delta\varepsilon$. The authors show convincingly that the millennial CH_4 variability correlates with millennial variations in $\delta^{18}O$-H_2O (NGRIP)/Greenland temperature, which is a new and interesting finding. The authors furthermore discuss the relation of CH_4 with the other records and suggest that Northern Hemispheric cooling and a concomitant southward shift of the ITCZ created a teleconnection pattern of reduced intensities of Asian and Indian monsoons. Thereby, the authors identified changes in CH_4 emissions from tropical wetlands as the most likely cause of the millennial CH_4 minima. The authors claim that this mechanism cannot explain the CH_4 minima around 10.2 ka alone.

In a next section, the authors review how the variability in external forcing during may cause an "El Nino-like" climate. They discuss some relation in the climate system but how this is hypothetically related to their CH_4 data remains unclear. The authors conclude this discussion cannot be developed further as there is no ENSO reconstruction for the Early Holocene. The purpose of this section is not entirely clear to me, also in the light of a range of existing publications on ENSO reconstructions (e.g. Z. Liu et al., 2014, Nature, Vol. 515, p. 550-553)

In order to investigate the CH_4 record further, the authors calculate the inter-polar difference in CH_4 (IPD) using the presented SNU and previously published NEEM data. The calculated IPD record is close to the range of previously published estimates, but exhibits interesting features on millennial time scales. The authors suggest a high IPD at the onset of the Holocene, which then decreases between 11.1 and 10.7 ka and increases again between 10.7 and 9.9 ka to previous levels. They furthermore use a previously published box model to separate hemispheric and tropical CH_4 emissions. The authors discuss the variability of tropical and boreal source fractions over time in the

light of other studies and conclude that the IPD increase between 10.7 and 9.9 ka is a result from Northern Hemispheric warming/thawing and expansion of boreal wetlands. This is in convincingly good agreement with previous reconstructions of increased CH_4 emissions from boreal wetlands.

The study of Ji-Woong Yang et al. is a valuable contribution and in the scope of Climate of the Past. I would recommend the publication of this manuscript but think that major revisions are required.

General comments:

1) Ice core: The authors could provide more complete information on the ice core and related logistics. For example, it is not explicitly mentioned that the samples were shipped from the USA to South Korea (just names of institutions). Neither did the authors mention the year the ice core was retrieved nor how long it was stored prior to analysis. Do the authors think that storage time affects the analysis? Do the authors think that extended storage time could help with the handling of samples from the brittle ice zone? The authors did not specifically address that their samples were from the brittle ice region. However, I would strongly recommend to raise this point and how it might or might not have affected the data.

More paragraphs dedicated to sample logistics and ice core will be added based on details below:

- *Ice core and sample logistics: Siple Dome A (SDMA) deep ice core was drilled from 1997 to 1999 on the Siple Coast, West Antarctica (81.65S, 148.81W; 621m elevation) (Taylor et al., 2004). The Siple Dome ice core samples were selected and cut at National Ice Core Lab (NICL), Denver in January to February 2013. The samples were packed in isothermal foam boxes with numerous eutectic gels and shipped to South Korea via expedited air freight. A temperature logger was enclosed within the isothermal box to track the temperature change during the logistics, and it showed that the temperature was maintained below -25°C during the transit. Then the Siple Dome ice samples were stored in a walk-in freezer of Seoul National University (SNU) that maintained below -20°C, and the CH_4 analysis was carried out from autumn of 2013 to spring of 2014.*
- *Brittle zone: Siple Dome ice was reported to be severely fractured below 400 m depth (e.g., Gow and Meese, 2007). Hence, during the ice sample preparation at NICL, the samples were carefully selected from unbroken part of the ice cores. Replicate measurements demonstrate a good integrity and reproducibility of the adjacent Siple Dome ice samples. In addition, comparison between the Siple Dome CH_4 records from OSU and SNU shows a good agreement within analytical uncertainty and no systematic drift, which ensures again lack of contamination in ice from the brittle zone. Further, comparison of CH_4 data from various ice cores with the Siple Dome records (from both OSU and SNU) show good agreement (below Figure R1). Therefore, it seems unlikely that the brittle zone ice affected our results.*

2) The analytical system: Even though the authors are preparing another manuscript on the analytical method, a more detailed description of the analytical system would be helpful. For example, the authors describe their melt-refreeze method as “traditional”, though, I am in doubt that most readers have a melt-refreeze technique in their lab, if they work in a lab at all. I find the term “traditional” misleading, as it might be taken as a support for the performance of the method.

We agree with the suggestion. We will delete the expression "traditional" and add a more detailed description of the analytical method as follows:

The air occluded in ice was extracted by melting and refreeze process under vacuum. Gas extraction line was evacuated to under detection limit of 0.1 mTorr, and our system leak check was done daily before analysis. Ice samples were prepared in a walk-in freezer in the morning of each experiment day, and trimmed the outermost >2 mm to eliminate possible contamination by ambient air during the storage. Then the samples were moved to the laboratory and placed in glass sample containers. The sample containers (sample flask hereafter) were custom-made glass flasks welded to stainless steel flange, and attached to the vacuum line with a copper gasket. The sample flasks were partially submerged into a chilled ethanol bath during ice insert and attaching to the line for preventing temperature increase by laboratory air. After that all flasks were evacuated at least 40 minutes, the ice samples were melted by submerging the sample flasks in a warm water bath. Melting process was usually completed within 30 minutes. The sample flasks were then submerged in the cold ethanol bath chilled to ~-82℃ more than 1 hour to refreeze. During the refreezing, we carried out GC pre-running (20 injections) and daily calibration that normally took 90 minutes. The ethanol temperature rises up to ~-55°C just after submerging the flasks, and it was chilled to below -65°C before expansion of the air in the flasks. The extracted air in the head space was expanded into a gas chromatograph (GC) equipped with a flame ionization detector (FID) to measure CH_4 mixing ratio. The detailed configuration of the vacuum line and GC is described in another paper that is currently in preparation (Yang et al., manuscript in preparation).

The inter-tank calibration using four working standard air cylinders (395.50, 721.31, 895.03, and 1384.91 ppb CH_4) that were calibrated and manufactured by NOAA GMD CCG in NOAA04 scale (Dlugokencky et al., 2005) shows good linearity of our GC system. The daily GC calibration curve was determined by measurements of a working standard having the closest CH_4 mixing ratio of expected value from the samples; in this study we used 721.31 ppb CH_4 standard for samples of the early Holocene. To account for system condition change throughout experiments (i.e., influence by water vapor), we calibrated with a standard air 6 times before and after sample measurements.

I think the presentation of the analytical performance needs to be developed further. The authors describe their system as similar to Mitchell et al., (2011). To my understanding, the system of Mitchell et al., (2011) is the benchmark GC-FID system in the community, with an estimate for measurement uncertainty of ±2.8 ppb, based on the pooled standard deviation of replicate samples that were measured with extended periods of time between the analysis of each sample pair. That is, the uncertainty estimate of Mitchell et al., (2011) can be understood as "worst case scenario". The method of Yang et al. is presented with an uncertainty of ±1.0 ppb, which is determined by the pooled standard deviation of 8 replicate measurements. While I am more than happy to be convinced that a newly developed method is superior in performance to an existing method, I feel strongly that this claim has to be proven. I think a more detailed description of the method to determine the uncertainty estimate is required, especially because the method of Yang et al. is supposed to be by far superior than that of Mitchell et al., (2011). In the light of a total number of 295 samples measured for this study, the authors need to describe why they chose these specific 8 replicate samples to determine the analytical uncertainty and why they did not chose other samples.

We will add more words to better clarify the methods as we described below.

"Duplicate measurements for 8 depths show ±1.0 ppb precision (1 sigma; pooled standard deviation)" will be more explained. Our CH_4 data are presented by averaging the results of duplicate sample analysis from the same depths. In order to estimate our data precision, we reanalyzed duplicate samples from the adjacent ices (~10 cm depth difference) at 8 depth intervals 8-80 days after the first analysis (see Table R1). The depth intervals were randomly chosen. The pooled standard deviation of the average of duplicates from first and second measurements was ±1.0 ppb (mean absolute difference of 1.9 ppb).

Table R1. Summary of the first (original) and second (replicate) measurements from the depths used for system reproducibility test.

Depth (m)	*1st measurements*		*2nd measurements*		*Difference*	
	CH_4 (ppb)	Date (dd/mm/yy)	CH_4 (ppb)	Date (dd/mm/yy)	$1^{st} - 2^{nd}$ (ppb)	Time (days)
523.15	*631.8±0.1*	*27/01/14*	*632.7±1.6*	*24/02/14*	*-0.9*	*29*
530.95	*663.0±2.2*	*03/02/14*	*664.7±1.6*	*24/02/14*	*-1.7*	*22*
558.295	*676.7±2.1*	*14/03/14*	*679.3±4.5*	*02/04/14*	*-2.6*	*20*
559.85	*682.0±5.8*	*03/02/14*	*684.7±1.9*	*26/03/14*	*-2.7*	*52*
561.15	*685.0±0.8*	*14/03/14*	*683.7±3.1*	*02/04/14*	*1.3*	*20*
562.407	*682.8±0.8*	*26/03/14*	*684.3±1.5*	*02/04/14*	*-1.5*	*8*
575.913	*676.5±0.2*	*07/02/14*	*679.3±3.8*	*28/03/14*	*-2.8*	*50*
578.15	*676.2±4.3*	*04/02/14*	*674.9±2.3*	*24/04/14*	*1.3*	*80*

We note that Mitchell et al. (2011) used different solubility correction methods. Applying the solubility correction method described in Mitchell et al. (2011) yields pooled standard deviation of the average of duplicates at adjusted samples of 1.4 ppb. One of the main differences of our analytical system compared to the OSU one is that, while in OSU the blank test was done once in several days and the systematic offset was interpolated between the blank days (Mitchell et al., 2011), we measured at least 3-4 blank ices every day. By doing this, the systematic offset can be quantified more precisely that accounts for daily changing conditions of instrument.

Furthermore, the method includes several corrections, including blank (determined with bubble-free ice, 5-15 ppb), gravitational fractionation (1.97±0.15 ppb) and correction for dissolved CH_4 (range should be specified). These corrections include uncertainties that should be represented in the uncertainty budget of the method. Furthermore, a blank of 5-15 ppb is enormous, both in absolute values as well as in the range, especially when the total performance is stated with ±1.0 ppb. For comparison, the method of Mitchell et al., (2011) has a blank of 1.1±0.5 ppb, while a blank of 5-14 ppb was interpreted as indicator of leakage. I think this issues need to be clearly demonstrated so that superior performance can be claimed.

The blank offset, which is calculated from the mean of the 3-4 blank results, reflects any errors by contaminants, leaks, or different GC conditions. The exact cause of the blank offset is not clear, but

the important thing is that the 4 blank results agree well each other, yielding the intra-day standard error of the mean of 2.0±1.0 ppb. This implies that the daily offsets of the 10 sample flasks are rather systematic although the inter-day blank correction varies 5-14 ppb. The robustness of our final results was proven by reanalysis of duplicates at adjacent depths (< 10 cm) 8-80 days after the first analysis. The absolute difference of mean of duplicates between the time intervals was 1.9 ppb on average (see the table above).

System leakage is unlikely. We carried out system leak check as a daily routine before starting gas extraction. If any pressure increase in 10^{-4} Torr scale for 30 seconds is detected, the experiment was stopped to figure out the leakage.

Further issues that could be clarified include the uncertainty of the standard air (available for each NOAA04 cylinder), how the linearity of the system is controlled (additional air standards covering the analytical CH_4 range?) and what reason the authors base their decision on to subtract 3 ppb from the OSU data instead of adding 3 ppb to the SNU data or to take the 3 ppb as temporal signal? If the measurement uncertainty claimed by both institutes is realistic, both data should be on the NOAA04 scale and therefore be in close agreement. I think this is in the order of expected disagreement, but I feel the manuscript is stronger if these issues are clearly addressed.

We appreciate Referee #1 for his pin-point review. We compared the Siple Dome CH_4 data measured at OSU (Ahn et al., 2014) to the WAIS Divide CH_4 records to see which one is more reasonable between adding 3 ppb to SNU data and subtracting from OSU data. To do this, the WAIS Divide CH_4 data points were linearly interpolated to the ages of the Siple Dome CH_4 records. The comparison of the mean CH_4 difference between the two data set shows that the OSU Siple Dome data itself fit better than the 3 ppb subtracted data, which were previously used for the early Holocene CH_4 composite in our discussion paper.

It seems that the 3 ppb offset is due to different correction for gas solubility effect. Unlikely to Mitchell et al. (2011), we calculated the expected gas enrichment by assuming that equilibrium state is achieved between air-water during the melting process. In OSU, the gas solubility effect is empirically estimated by measuring CH_4 mixing ratio from the air that is re-dissolved after melt-refreeze process (Mitchell et al., 2011). The directly-measured OSU correction method seems more realistic, but the uncertainty is large (~10%) due to small amount of air. After applying the OSU solubility correction scheme, the average difference between SNU and OSU reduces to less than 1 ppb. Therefore, for the better fitted composite record, we applied the OSU correction method to SNU data set.

3) CH_4 data: The authors mention 295 measurements. However, Figure 1 seems to show a much smaller number. If the displayed measurements are averages of duplicates or if other measurements are not displayed in Figure 1, the number would have to be revised.

The number will be corrected as below. Basically we made duplicates for all depths, and the average of the replicates was used for the data.

"We measured 295 samples from 156 depth intervals ranging from 518.87 to 718.83 m of Siple Dome ice core (including 8 replicates and 13 rejections, all duplicated), covering from 8.36 to 20.25 kyr BP after synchronizing to GICC05 scale. Analytical uncertainty of each point is deduced from standard error of the mean of duplicate samples. Figure 1 presents only the data points during the early Holocene period (119 depths, 518.87 – 623.38 m, 8.36 – 11.71 kyr BP) that we discuss in this study

Data older than 11.71 kyr BP have lower time resolution and are unevenly spaced, thus those records have not been used in this paper."

Otherwise, please clarify. How long is the overlapping period between OSU and SNU data? Maybe Figure 1 could show this in a detailed Figure?

We will add an enlarged figure in Figure 1 (See Figure R1). The overlapping period is from ~8.4 to ~9.1 ka. To make the SDMA CH_4 composite, we used the OSU data from 7.7 to 8.5 ka, due to higher mean temporal resolution of the OSU data.

Figure 1 has a reference in the captions (Brook and Sowers, 2016) that is new to me and that I cannot find in the reference list.

Reference will be modified.

The authors may or may not consider to show data from previous publications (e.g. Brook et al., 2000, Flueckiger et al., 2002) to highlight the superiority of their temporal resolution.

Figure 1 will be modified as Figure R1 that includes the data from Brook et al. (2000) and Flueckiger et al. (2002) (see below).

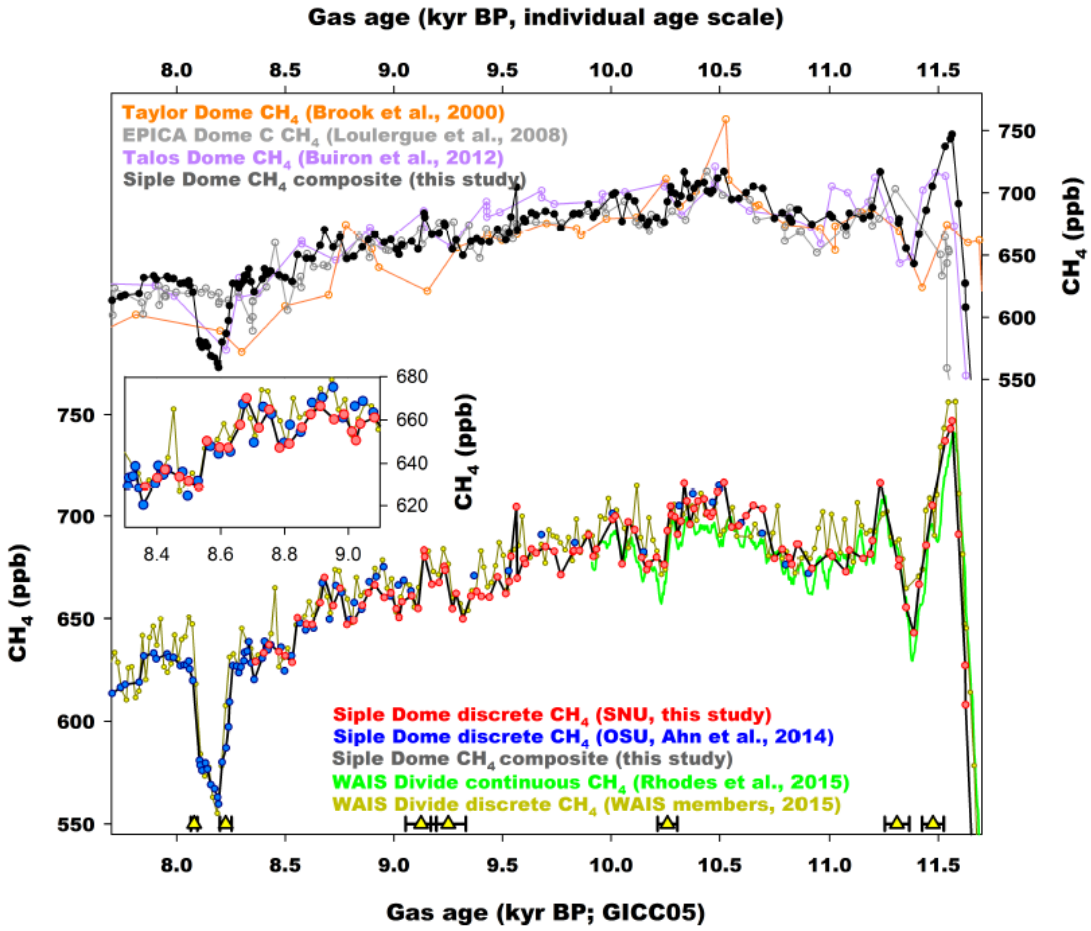

Figure R1. CH_4 reconstructions during the early Holocene. Top: new high-resolution Siple Dome CH_4 data (black, this study) compared with previous records from Taylor Dome (orange, Brook et al., 2000), EPICA Dome C (grey, Loulergue et al., 2008), and Talos Dome (purple, Buiron et al., 2012).

Bottom: Siple Dome CH_4 records measured in OSU (blue, Ahn et al., 2014) and SNU (red, this study). Siple Dome composite (black line) is plotted with WAIS Divide discrete (dark yellow, WAIS members, 2015) and continuous technique (green, Rhodes et al., 2015). Inset: Enlarged plot showing overlapped interval between OSU and SNU Siple Dome data. Note that this figure may be subject to change.

4) IPD: IPD is a powerful concept, but very one has to be very careful in its reconstruction and interpretation. The authors mention the potential for ill-calculated IPDs based on errors in the gas age scale of the CH_4 records. Therefore, the authors developed a tool to synchronize the CH_4 records, which I think is a very good approach! However, it is not clear to me why the authors chose to calculate the IPD based on the Siple Dome data? My concerns have several reasons:

i) The authors state themselves that the histories of gas enclosure is more similar between NEEM and WAIS. The authors state that, based on just this fact, the amplitude of CH_4 variations is 10-20 ppb larger in the WAIS than in the Siple Dome record. Therefore, I understand that both the NEEM and the Siple Dome CH_4 records are altered by physical processes during gas enclosure that are different for each core. I understand that a dampened amplitude in the Siple Dome CH_4 record would create a IPD variation. Ideally, gas enclosure effects should be identical in both records so that they would cancel. Since the WAIS record is in that sense more similar to the NEEM record than the Siple Dome record is, I would suggest to use the WAIS record for the IPD reconstruction.

ii) The WAIS record is of even higher temporal resolution than the Siple Dome record. I would expect that the IPD reconstruction based on NEEM and WAIS would be more robust.

iii) The comparison of the CH_4 records from Siple Dome and WAIS in Figure 1 shows two periods (~10.5 and ~10.7 ka) where Siple Dome CH_4 exceeds WAIS CH_4 by up to ~20 ppb. During this period, the smoothed CH_4 variations (Figure 2) also show an ice core specific pattern of disagreement. Around 11 ka, the pattern of agreement is different. Here, the continuous CH_4 record from WAIS seems to agree better with CH_4 from Siple Dome, while the GC-FID record from WAIS contains a number of samples that exceed the former records by ~20 ppb. The difference between the records during these times exceeds the stated measurement uncertainty by far. It is also important that the difference seems to be in the order of the presented IPD variability.

All of the above mentioned reasons directly impact on the IPD reconstructions. The choice of the authors to use their Siple Dome data for the IPD reconstruction is justified and understandable. However, I fear that the interpretation is sensitive to the choice of CH_4 record so that this choice could impact on the IPD result for the above mentioned reasons. Therefore, I suggest to calculate the IPD also using both WAIS records as three independent sets of IPD reconstructions as a sensitivity test. This will make the interpretation of reconstructed IPDs more robust and will furthermore give valuable insights into the IPD technique on high temporal resolution records for future studies. Because this concerns one of the main outcomes of this manuscript, I would consider this essential.

We appreciate the Referee #1 for her/his useful and reasonable comment on IPD reconstruction. Calculating IPD from different ice core data would draw more objective conclusion. We will provide alternative IPD reconstructions from various records (NEEM discrete, NEEM continuous, WAIS discrete, WAIS continuous, and Siple Dome discrete data) to test robustness of early Holocene IPD trend.

As mentioned in comment iii) above, the Siple- and WAIS ice core records do not agree at some period, and this offset could lead erroneous IPD change. However, here we consider Siple data show more reliable result by following reasons. First, it was shown that SNU Siple Dome discrete data (this study) have a good agreement with OSU Siple Dome discrete data (Ahn et al., 2014) during the early Holocene interval, while PSU (Penn State University) WAIS discrete data that covers most of the early Holocene period show offset up to ~9.9 ppb to OSU data (Rhodes et al., 2015). Second, PSU WAIS discrete data show a pooled standard deviation (for depth-adjacent samples) of 7.3 ppb (1sigma), which is larger than SNU Siple data. Thus we will weigh our interpretation on IPDs calculated with Siple data than those from WAIS discrete record. In addition, NEEM continuous- and discrete data do not agree well in some intervals (Figure R2), even though NEEM continuous data were calibrated against to discrete measurements carried out at OSU (Chappellaz et al., 2013). Further, the NEEM continuous record is not exactly "continuous", that may introduce uncertainty into synchronization. Hence, here we will regard the NEEM discrete, Siple Dome discrete, and WAIS Divide continuous records as more reliable ones than the others during the early Holocene period (Black and Green curves in Figure R2).

Resulting IPD curve from NEEM discrete and WAIS continuous (Green) shows long-term increase from the onset of Holocene to ~9.9 kyr BP. This indicates that contribution from boreal sources increased during ~11.5 to 9.9 kyr BP, which is consistent with increase of northern extratropical temperatures and thermokarst lake CH_4 emissions. The revised 3-box model results (Table S1) show elevated emission from N-box and slight decline in T-box. However, since a small IPD increase during ~10.8-11.2 kyr BP observed in Siple Dome IPD is not supported by alternative IPDs, it remains unclear the short-term IPD change during this time period. Therefore, we will modify descriptions and findings on early Holocene IPD trend. Also we address the inconsistency of ice core records for the period older than ~10.3 kyr BP, which makes the alternative IPDs different from each other.

Table S1. 3-box source distribution model results of tropical (green, T) and boreal (red, N) boxes and boreal emission fraction (N/(T+N+S)) compared with previous results. Errors denote 95% confidence interval.

Ref.	N box	T box	N/(N+T+S) ratio
(ka)	(Tg yr^{-1})		(%)
Brook et al., 2000 (9.5-11.5 ka)	64 ± 5	123 ± 8	32 ± 3
Chappellaz et al., 1997 (9.5-11.5 ka)	66 ± 8	120 ± 9	33 ± 3
This study (9.5-11.5 ~~10.8~~ ka)	66 ± 4 ~~65 ± 2~~	120 ± 4 ~~122 ± 4~~	33 ± 2 ~~32 ± 1~~
This study (11.5 ka)	57 ± 6	119 ± 11	30 ± 4
This study (9.9 ka)	70 ± 4 ~~74 ± 2~~	115 ± 4 ~~110 ± 3~~	35 ± 2 ~~37 ± 1~~

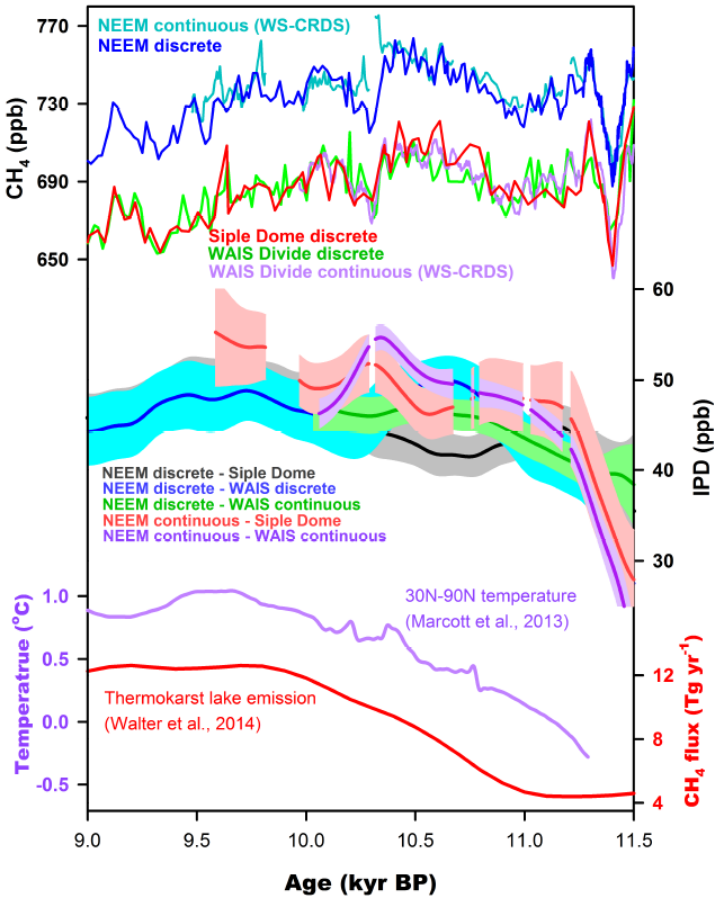

Figure R2. Inter-polar difference (IPD) reconstructions. Top: high resolution CH_4 records from Greenland and Antarctica, synchronized to NEEM gas age scale by Monte Carlo procedure. Middle: Millennial-scale IPD trends derived from various pairs of data set. Shaded area indicates 95% significance interval. Bottom: Proxy-based temperature reconstruction for northern mid to high latitude and boreal CH_4 emission from northern thermokarst lakes. Note that this figure may be subject to change.

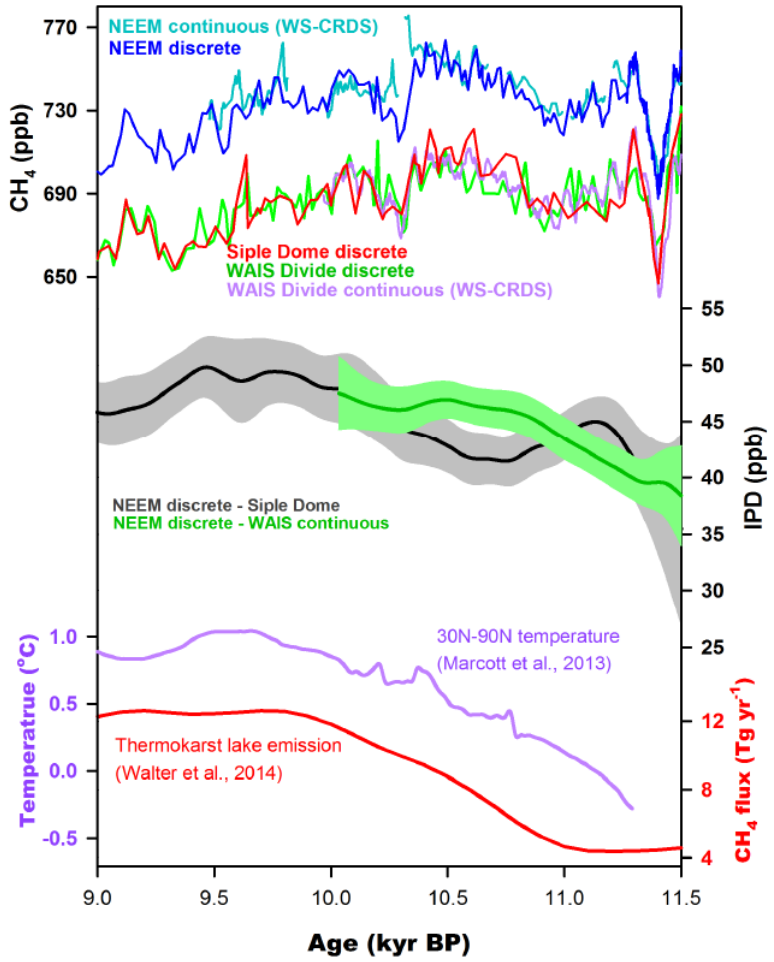

Figure R3. Same as Figure R2, but shown are IPDs using NEEM discrete, Siple discrete, and WAIS Divide continuous data. Note that this figure may be subject to change.

5) Consideration of significant publications: *i*) The authors claim that no correlation between CH_4 and tropical monsoon signals has been reported on shorter time scales, however, I feel this is not accurate. Both Cruz et al., (2005, Nature, Vol, 434, p. 63-65) and Sperlich et al., (2015, Global Biogeochemical Cycles, 29) have related CH_4 and $\delta^{13}C$-CH_4 records to South American speleothem records, respectively. Both publications would principally support the interpretation of this study. *ii*) The authors discuss that their IPD reconstruction suggests increasing boreal source fractions during the early Holocene and support their finding with studies on boreal wetland dynamics. However, their finding of increased boreal source fractions is in line with the interpretation of $\delta^{13}C$-CH_4 data by Fischer et al., (2008) and Sowers, (2010). Again, both publications would principally support the interpretation of this study while Sowers (2010) had the same finding for the early Holocene previously. *iii*) The authors stated that there is currently no ENSO reconstruction for the early Holocene, even though a range of them exist (e.g. Z. Liu et al., 2014, Nature, Vol. 515, p. 550-553, Clement et al., 1999, Paleoceanography, Vol. 14, p. 441-456).

We checked the suggested publications and take them into consideration for further developing discussions.

i) We will modify the statement as below:

"However, no direct correlation between CH_4 and tropical monsoon signals has been reported for the early Holocene. Abrupt increases in stalagmite $\delta^{18}O$ record from subtropical Brazil (Botuvera Cave) are found to be coincided with rapid increase in CH_4 during the last glacial period and Younger Dryas events (Cruz et al., 2005). This indicates that the monsoon precipitation in subtropical South America was reduced when atmospheric CH_4 increased, which implies northward migration of monsoon rain belt and increasing CH_4 emissions in tropics (Cruz et al., 2005). A recent study using $\delta^{13}C$-CH_4 analysis from Greenland ice cores reported that the sharp CH_4 increase at Greenland Interstadial 21.2 (~85 kyr BP, ~150-year duration) is concurrent with intensifying precipitation in Amazonian and Asian wetlands (Sperlich et al., 2015)."

ii) *We will cite the suggested publications and modified the relevant paragraph as below:*

"The IPD estimates from Siple Dome and NEEM discrete CH_4 records are generally consistent with the previous results from CH_4 isotopic ratio analysis. Fischer et al. (2008) found that increasing boreal source contribution is required to explain the decreasing $\delta^{13}C$-CH_4 during the Younger Dryas - Preboreal Holocene transition. Sowers (2010) disclosed the CH_4 isotopic ratio covering the entire Holocene that displays a gradual decrease of $\delta^{13}C$-CH_4 by ~2 ‰ from 10.5 to 4 kyr BP, which they attributed it to progressive expansion of NH high latitude sources. Using an isotopic mass balance model, they proposed an additional emission of 19.6 Tg yr-1 from boreal thermokarst lakes to explain such amount of $d^{13}C$-CH_4 depletion. According to $\delta^{13}C$-CH_4 data in Sowers (2010), we observe ~0.8 ‰ decrease in $\delta^{13}C$-CH_4 from 11.3 to 9.0 kyr BP. It corresponds to ~8.3 Tg yr^{-1} increase of the Arctic lake emissions, which agrees with our three-box model results and thermokarst lake emission model result by Water Anthony et al. (2014) (see below). The boreal source expansion during the early Holocene is further supported by proxy-based temperature reconstructions that indicate a gradual warming in northern high latitude region (30N-90N) until ~9.6 ka, while tropical temperature remains stable (Marcott et al., 2013). The climate warming in northern high latitudes caused ice sheet retreat (e.g., Dyke, 2004) and enhanced CH_4 emission from boreal permafrost by forming new wetlands in mid- to high latitudes (e.g., Gorham et al., 2007; Yu et al., 2013) and accelerating microbial decomposition of organic materials (e.g., Christensen et al., 2004; Schuur et al., 2015). Thermokarst lakes created by thawing ice wedges and ground ices in Alaskan- and Siberian permafrost are suggested as a source of CH_4 (e.g., Walter et al., 2006, 2007; Brosius et al., 2012). Indeed, the increased boreal CH_4 emission of 9.7 ± 3.5 (1σ) Tg yr^{-1} during 11.5 – 9.0 kyr BP is in similar order of the CH_4 release of 8.2 Tg yr^{-1} from thermokarst lake reported by Walter Anthony et al. (2014). However, it should be noted that the CH_4 release estimates from the thermokarst lakes are based on present-day CH_4 flux measurements in Siberian- and Alaskan lakes and that 9 Tg yr^{-1} is a small change in the budget that could be driven by conventional northern CH_4 emission. A recent study also argued a possibility of underestimation of such CH_4 emission measurements (Wik et al., 2016)."

iii) *We appreciated the suggestion, but unfortunately, the mentioned publications (Liu et al., 2014; Clement et al., 1999) are dealing with climate modelling result, not proxy-based reconstructions. Instead, we will cite Kirby et al. (2010) for PDO, and Moy et al. (2002) and Rodbell et al. (1999) for Holocene ENSO reconstructions. According to Holocene ENSO activity reconstructions by Moy et al. (2002), weak- or no ENSO event was recorded during the early Holocene until around 7 kyr BP. Nevertheless, it shows a slight (up to 3 events per 100 years)*

increase in warm ENSO event numbers during 10.4-10.1 kyr BP, where abrupt CH_4 drop is observed without significant changes in ITCZ and NH monsoon intensities (Figure R4). Recent studies have shown that warm ENSO event leads drying in low latitude wetlands (e.g., Dai and Wigley, 2000; Lyon and Barnston, 2005), however it is currently uncertain if this magnitude of change in ENSO frequency caused significant drying in wetlands and CH_4 emissions. In the other hand, by using sediment grain size analysis, Kirby et al. (2010) found PDO-related drying intervals in North America during 9.5 – 9.1, 8.9 – 8.6, and 8.3 – 7.8 kyr BP. The overlap of CH_4 minima at around 8.2 and 9.3 kyr BP is compelling because Mitchell et al. (2011) have reported a significant positive correlation between CH_4 and PDO variability for the late Holocene.

6) Chosen data filtering technique: I suggest to provide more information why a 250-year window width was used. What is the effect of other window widths on the data you use and on your resulting interpretation? The authors state on p5L2-5 that the 250 year window was also used in other studies. However, that is not necessarily a satisfying justification. The window width should be carefully chosen in dependence on the time scales you are investigating.

Power spectrum (REDFIT, Schulze and Mudelsee, 2002) of Siple Dome CH_4 data indicates moderate (over 90% significance level) powers at ~1340, 401, 309 and 96-year period. Considering ~42 years of gas age distribution of Siple Dome (Ahn et al., 2014), it is not reliable to study centennial scale variability. Thus we applied 250-year window to smooth out any high frequency component having shorter period than 309 years. We will add above details to that paragraph.

7) Sometimes, I have trouble to understand the point the authors intend to make, e.g. p1, 12–18; p5, 18–29; p6, 25–p7, 5; p10, 6–12, p22, 1-2. Please consider re-wording.

We will thoroughly revise the mentioned sentences.

8) I have difficulties to follow the discussion and the display of CH_4 and other records. For example, the authors discuss why there is agreement/disagreement between some records within the uncertainty of the age model. However, I find it tough to see this in Figure 2. For example, the CH_4 minima are highlighted with yellow bars. During 9.4 and 10.2 ka, the yellow bars include both local minima and local maxima of the other records, e.g. of the Cariaco Basin record, of $\Delta\varepsilon$, of Dongge Cave. Other local extrema, e.g. in the Cariaco Basin record have no correspondence in CH_4 during 8.5-8.7 ka or 10.5 ka, which is not mentioned at all. Therefore, I feel this discussion needs to be further developed to provide more guidance to the reader. Is ^{10}Be really important here? Sometimes it seems to correlate, other times it is anti-correlated. Could figure clarity increase without it?

Not every single variation of the Cariaco Basin reflectance record corresponds to abrupt CH_4 change, because the Cariaco Basin record could also have local signal. What we wanted to show in this Figure is that abrupt cooling occurred around Greenland changes tropical rain belts and hence CH_4 emission. ^{10}Be and IRD proxies were included to discuss trigger of abrupt Greenland cooling, but data resolution and dating uncertainty ($\pm$100 to 150 years, Bond et al., 2001) prevent us from drawing rigorous conclusion. By taking into consideration with the comment #12 (Figure R4), we will modify the Figure 2 and relevant discussions.

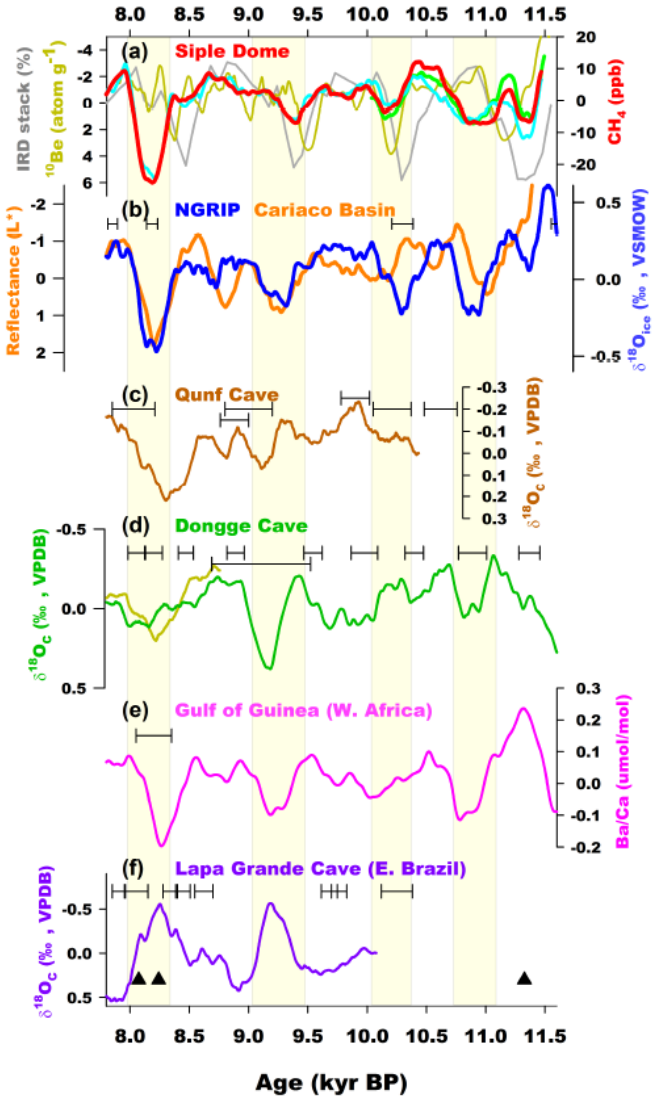

Figure R4. Millennial scale climate variability. All proxies present here were smoothed by 250-year running average and detrended by high-pass filter with 1/1800-year window. (a) Siple Dome CH_4 (red, this study), Greenland ^{10}Be (dark yellow, Finkel and Nishizumii, 1997), North Atlantic IRD stack (grey, Bond et al., 2001). Also shown are WAIS Divide CH_4 data by discrete (cyan, Buizert et al., 2015) and continuous (yellow green, Rhodes et al., 2015) technique. (b) NGRIP stable water isotope ratio (blue, Rasmussen et al., 2006) and Cariaco Basin reflectance (orange, Deplazes et al., 2013). (c) Qunf Cave speleothem oxygen isotope (Fleitmann et al., 2007). (d) Dongge Cave speleothem oxygen isotope (green, Dykoski et al., 2005; dark yellow, Wang et al., 2005). (e) Gulf of Guinea planktonic Ba/Ca ratio (Weldeab et al., 2007). (f) Lapa Grande Cave speleothem oxygen isotope (purple, Strikis et al., 2011). Age tie-points used to adjust Siple Dome and WAIS Divide CH_4 data to GICC05 scale are marked in black triangles. This figure may be subject to change.

9) Figure 2: Presented is CH_4 anomaly. I don't see an advantage of anomaly over CH_4 mole fractions. Also, how is anomaly of 0 defined for each record?

We refer "anomaly" in Figure 2 as detrended time series after filtered with highpass window. We will use "detrended data" instead of "anomaly" to clarify it.

10) Structure: I would suggest to avoid three levels, e.g. 3.1.1 and 3.1.2 but make 3. Millennial scale variability, 4. Latitudinal distribution…. to keep structure with max. two levels.

We will simplify the structure in two levels.

11) In many places of the manuscript, the author review literature, e.g. on pattern of climate teleconnections, for which they allow extensive text sections. While I think it is important to review in such detail, I feel the authors could improve their discussion of how they think this is linked/relevant to their CH_4 interpretation. A good example for this is the entire section 3.1.3. I would like to encourage the authors to consider this throughout the entire manuscript, even though this probably means either adding to, or re-writing many sections of the manuscript.

We appreciate Referee #2 for pointing out this aspect and we agree with it. We will insert below paragraph and re-structure the 3.1.2. section. Moreover, other sections will be thoroughly revised and re-written.

"From the previous publications it can be inferred that low solar activity might be a cause of tropical rainfall change via abrupt cooling in North Atlantic and southward migration of ITCZ (e.g., Broccoli et al., 2006; Renssen et al., 2006; Jiang et al., 2015). Meanwhile, shifting to El Nino-like SST condition was suggested as another mechanism that changes tropical rainfall pattern (Marchitto et al., 2010). According to modern atmospheric observation, El Niño condition leads drying in low latitude wetlands in Africa, Asia, and America (e.g., Dai and Wigley, 2000; Lyon and Barnston, 2005), which reduces tropical CH_4 emissions. Thus we can hypothesize that both the ITCZ migration and El Niño-like SST change affected the tropical surface hydrology and CH_4 emission."

12) Understanding the variability in tropical wetlands is crucial for the understanding of CH_4 source regions and tropical CH_4 fractions. (The same rule applies for boreal wetlands.) The authors fully acknowledge this throughout the manuscript. However, I note that the authors exclusively focus their interpretation on Asian/Indian monsoon systems. It has been described previously that the African monsoon system and wetland extension changed tremendously throughout the Holocene (e.g. Sahara region etc). There are also several publications on South American monsoon systems besides the Cariaco Basin reflectance, which I understand the authors only use as proxy for ITCZ migration, but not for their interpretation of hydrological changes in South American wetlands. Including further records (e.g. Cruz 2005) might allow for a more comprehensive evaluation of hydrological changes. Based on $\delta^{13}C$-CH_4 data, the South American monsoon system has recently been suggested to be a controlling factor in rapid CH_4 changes leading up to DO21 (Sperlich 2015). I would strongly recommend to either include a complete representation of tropical wetlands or to discuss why you think monsoon systems other than the Asian/Indian are not relevant.

As mentioned by the Referee #1, it is possible that the monsoon system of other regions could play a role in CH_4 change. However, considering that mean position of ITCZ was moved to northward than glacial condition (e.g., Deplazes et al., 2013), the rainfall and CH_4 emission of Asian/Indian monsoon regions should be stronger than southern hemisphere monsoon regions, for example, South America and Africa. This makes boundary condition different from those arguing that Southern Hemisphere emission leads abrupt CH_4 increase during Heinrich Stadial 1, 2, 4, and 5 (Rhodes et al., 2015) or DO21 (Sperlich et al., 2015). Both studies are dealing with abrupt CH_4 change under glacial condition. In the meanwhile, one of main conclusion of our paper is that abrupt cooling in Greenland lead ITCZ mean position change and tropical CH_4 emission. Therefore, it seems sufficient to support the idea with the Asian/Indian monsoon records and Cariaco Basin reflectance data.

However, for the interested readers it would be good to insert additional discussions and/or supplement figure on changing other monsoon systems at the same time. Lapa Grande Cave (Eastern

Brazil, 14°25'22"S) records demonstrate clear $\delta^{18}O$ depletion at 8.2 and 9.2 kyr BP (Strikis et al., 2011), indicating that ITCZ rain belts temporarily migrated southward and induced wet condition over eastern Amazonia region. This evidence agrees well with Cariaco Basin rainfall reconstructions and does not go back beyond ~10.2 kyr BP, therefore we thought that adding the South American monsoon proxy would not improve our conclusions. The monsoon record of northern Peru (El Condor and Cueva del diamante, Cheng et al., 2013) and southern Brazil (Botuvera, 27°13'24"S, Cruz et al., 2005) are not highly resolved enough to see abrupt changes. Otherwise, the reconstructions of West African monsoon (Gulf of Guinea, Ba/Ca ratio of planktonic foraminifera, Weldeab et al., 2007) and Indian monsoon entire early Holocene (Qunf Cave, Fleitmann et al., 2007) track well the millennial scale CH_4 minima. Furthermore, Australian-Indonesian monsoon rainfall records (not shown) from Borneo (Partin et al., 2007) and Liang Luar (Griffiths et al., 2009) do not show clear evidence of abrupt change that coincides with Greenland cooling and CH_4 drop. This may reflect that the rainfall in tropical western Pacific region was affected by both northern- and southern hemispheric climate change (Griffiths et al., 2009; Partin et al., 2007).

13) Data availability: I understand that Copernicus has developed a new policy for authors to provide either descriptions on data access, or to provide the data through international data-bases or supplementary information. Copernicus requires a dedicated section that describes this in detail, which the authors might want to consider during their revisions.

Thanks for this information. We will describe how to access our new data in a dedicated section of revised manuscript.

Specific comments

The manuscript may be subject to considerable re-writing. Therefore, the specific comments will not include comments on grammar, wording or writing that might be subject to change. Since I am not a native English language speaker myself, I am not sure to what extend my comments would help to make it better or worse. Please understand suggested re-formulations as suggestions, only.

p1L1: Understanding processes controlling atmospheric methane

The sentence will be changed as below:

"Understanding ~~the atmospheric methane (CH_4) change~~ processes controlling atmospheric methane (CH_4) mixing ratio is crucial to predict and mitigate the future climate change.

p2L2: reference Daniau et al., is it possible to provide a reference on palaeo-fire or a reference that is more specific on pyrogenic gas emissions?

Carbon monoxide (CO) and ethane (C_2H_6) can be used as paleo-fire indicator, but currently there is no available CO and C_2H_6 reconstruction covering the early Holocene.

p2L5: sink strength and light availability? e.g. polar winter

We will add more details on methane sink process, especially on photochemical oxidation of CH_4.

p2L16: Lisiecki and Raymo 2005, though this reference is not on CH_4, general I think a reference on CH_4 and Northern Hemisphere temperature would be useful here

We will change the citation as below and added the reference:

(e.g., Brook et al., 2000; Chappellaz et al., 1993; Huber et al., 2006; Loulergue et al., 2008).

Huber, C., Leuenberger, M., Spahni, R., Fluckiger, J., Schwander, J., Stocker, T. F., Johnsen, S., Landais, A., and Jouzel, J.: Isotope calibrated Greenland temperature record over Marine Isotope Stage 3 and its relationship to CH_4, Earth Planet. Sci. Lett., 243, 504-519, 2006.

p2L18: See comment 5) in general comments

See Author Comment to general comment #5.

p2L18-19: too weak. The correlation between CH_4 and NH temperature (δ^{18}O-ice) is well established

It is true for longer time scales. The CH_4 – $\delta^{18}O_{ice}$ of Greenland has yet been revealed in shorter time scales during the early Holocene period.

p3L15: here and everywhere else: It is recommended to restrict the use of "concentrations" to public debate but to use "mole fraction" or "mixing ratio" in scientific literature (WMO, recommendations of GGMT experts)

This will be updated as suggested.

p3L17: here and everywhere else, δ^nX, the δ is supposed to be *italicised*, same for Δ Δ (Coplen 2011, DOI: 10.1002/rcm.5129)

This will be updated as suggested.

p3L27: there is still some ice left in Greenland

We will change the sentence as below:

"~~We note~~ It should be noted that environmental boundary conditions of the early Holocene were not identical to those of the late Holocene.~~, given that the global sea level rise continued throughout the early Holocene as the last sections of the northern hemisphere glacial ice sheets melted.~~ The global sea level was rising throughout the early Holocene and there is still some ice left in Greenland."

p3L30: ensure stated, used and displayed sample numbers agree, give age interval with depth

Please refer to our response to General Comment #3.

p3L32: (NICL, city, state, country), (SNU, city, state, country)

This will be updated as below:

"… National Ice Core Laboratory (NICL, Denver, Colorado, USA), and shipped to Seoul National University (SNU, Seoul, South Korea) …"

p4L3: "are described in…", referring to a paper that is currently in preparation as XYZ et al., (in prep.) seems strange as it is not useful to look it up. "A manuscript that describes the method in detail is currently in preparation."

We will change the sentence as below:

"~~The basic principles of gas extraction and CH_4 analysis are described in Yang et al. (in preparation)~~ Further details of analytical method is described in another manuscript that is currently in preparation."

p4L7: was the standard air added before or after the bubble-free ice was melted?

The standard air was injected before melting the bubble-free ice. We revised the wording for clarity.

p4L7: "traditional" melt-refreeze seems misleading to me, traditional can be left out

We will delete the word "traditional".

p4L7-17: see point 2) in general comments

See Author Comment to general comment #2.

p4L17: provide reference how you calculated gravitational fractionation

Relevant citation and reference will be added: Craig et al., 1988

Craig, H., Horibe, Y., and Sowers, T.: Gravitational separation of gases and isotopes in polar ice caps, Science, 242, 1675-1678, 1988.

p4L19: provide reference for GICC05 time scale

Reference will be added: Rasmussen et al. (2006)

p4L25: "one of the high resolution data sets" sounds vague and strange to me, where do you draw the line between high and low resolution? The temporal resolution of your data is higher than some but lower than both WAIS records. "It has the currently second/third highest temporal resolution of Antarctic CH_4 records covering the early Holocene."

The sentence will be revised as below:

"Our new Siple Dome CH_4 ~~record is the one of the high-resolution data set~~ data has the currently third highest temporal resolution of Antarctic CH_4 records covering ~~from 11.6 to 8.5 ka, apart from the WAIS Divide records (Rhodes et al., 2015; WAIS Divide members, 2015)~~ the early Holocene after the WAIS Divide continuous (~2 years, Rhodes et al., 2015) and discrete (~20 years, WAIS Divide members, 2015) records."

p4L26: develop a more complete representation of analytical uncertainty

Please refer to our response to general comment #2, as well as our response to the Referee #2.

p4L30: describe the overlapping interval and describe why you think the OSU record should be adjusted to match the SNU data. Why not the other way around, why not accepted as real difference? Both should be on NOAA04?

We attribute the SNU-OSU offset to different correction method for solubility effect. After applying revised solubility correction used in OSU (Mitchell et al., 2011), the mean offset reduces 0.6 ppb which lies well within analytical uncertainty of both institutes.

p5L3: this comparison example only makes sense if you look at variations on similar time scales. Otherwise the argument that you use the same filter as has been applied for the WAIS record is not valuable, but could be misleading.

Here we hesitate to remove the comparison. As we describe in this sentence and Figure 2, to ensure robustness of millennial scale variability from other Antarctic ice core record is important. We used filtered data because both data set have different time resolution, and they show different fluctuations in short time scales as shown in Figure 1. We will revise the wording not to mislead.

p5L14: add references that show anthropogenic signal in LPIH CH_4, e.g. Ferretti 2005, Mischler 2009, Sapart 2012)

We will change the sentence with relevant citations as below:

"Mitchell et al. (2011) found no significant correlation with Greenland climate in multi-decadal time scale during the late pre-industrial Holocene (LPIH), possibly due to growing anthropogenic emissions (e.g., Ferretti et al., 2005; Mischler et al., 2009; Mitchell et al., 2013; Sapart et al., 2012)."

p5L16: "even though this conclusion is less robust as there are no age tie-points…"

The sentence will be modified as suggested:

P5L14-18: "In contrast, we observe a moderate positive correlation between the Siple Dome CH_4 and NGRIP $\delta^{18}O_{ice}$ during the early Holocene, which implies that natural CH_4 budget is closely connected with Greenland climate in millennial timescales, even though this conclusion is less robust as there is no age tie-point between the 8.2 ka episode and the Preboreal oscillation (Fig. S1)."

p5L21-22: shift this sentence to after the following sentence to keep logical flow from NH to tropics

We agree with the suggestion. The paragraph will be changes as suggested.

p5L25-29: describe the meaning for CH_4, develop the discussion towards CH_4, what does a ITCZ shift mean for South American CH_4 source regions?

South American monsoon proxy shows concurrent intensification at similar timings of ~8.2 and 9.3 ka CH_4 drop (Strikis et al., 2011), where precipitations in Cariaco Basin and other NH monsoon regions decreased. For older period it is difficult to draw robust conclusion due to lack of high resolution data at the timing of preparing this paper. The southward ITCZ migration may lead reduction in NH wetland emission and enhanced in SH. However, given the orbital parameters that show maximum summer insolation in NH while minimum in SH during the early Holocene, it can be inferred that contribution of SH wetland emission was relatively low and cancelled by reduction of NH emission. We will update the paragraph as below:

"~~Moreover~~ The ^{18}O enrichments of Asian (Dongge) and Indian (Hoti and Qunf) cave stalagmites occurred at similar timing with abrupt cooling in Greenland, which indicate the reduction of monsoonal rainfall at northern tropical wetlands. The speleothem records from Chinese and Oman caves seem to lag by ~100-200 years after the CH_4 change at ~9.3 ka, but this lies within chronological uncertainties of ~200-400 years at around 9.0 ka (Dykoski et al., 2005; Fleitmann et al., 2007). Moreover, sediment Ba/Ca ratio from Gulf of Guinea demonstrates concurrent decrease of west African monsoon (Weldeab et al., 2007). These evidences indicate that precipitation over the major wetland area was reduced and in turn it would lower the wetland CH_4 emissions in NH. In the

meanwhile, an inverse relationship is observed from the Eastern Brazilian speleothem data (Lapa Grande Cave, Strikis et al., 2011) that demonstrate the increasing of precipitation at the time of abrupt CH_4 drop occurred as a result of southward migration of ITCZ. Considering the orbital parameter that shows maximum summer insolation in NH while minimum in SH during the early Holocene, it can be inferred that contribution of SH wetland emission was relatively low and cancelled by reduction of NH emission."

p6L4: there are other monsoon systems that Asian/Indian that should be considered

See our response to general comment #12.

p6L10: the monsoon intensity change. (delete Asian, include other monsoon systems)

We will change the sentence as below:

"Given the weak reduction of precipitation over the Asian, Indian, and African monsoon regions (Figure R4), ~~This~~ it may imply CH_4 ~~reduction~~ drop was controlled by other processes than the ~~Asian~~ monsoon intensity change."

p6L20: even though the Cariaco Basin record is shown, it is presented only as indicator for ITCZ migration, without direct connection to CH_4. I feel the assumed passiveness of South American CH_4 source regions during the study period might not be a natural assumption and should be explained.

Considering the orbital configurations that show maximum summer insolation in NH while minimum in SH, contribution of SH wetland emission had to be reduced during the early Holocene.

p6L23-p7L5: describe relevance for CH_4, what is the CH_4 controlling process chain? A sentence that says "the proxies show this and that which could explain the increase/decrease in CH_4 during time period XY".

We will explain more details on proxies and its relevance to CH_4 change. Adding following sentence at the end of the paragraph will be more explained: "Above evidences indicate that the early Holocene CH_4 minima were triggered by anomalous low solar activity, but future study is warranted to draw more conclusive result."

The entire paragraph will be revised as below:

"Bond et al. (1997) reported four large ice-rafted debris (IRD) drifts occurred at ~8.1, 9.4, 10.3 and 11.1 ka caused by surface cooling of North Atlantic Ocean. They found that the ocean surface cooling and the IRD events are closely related to cooling over the Greenland. Figure 2 shows that each IRD event (maxima in hematite stained grain) occurred concurrently with minima of NGRIP $\delta^{18}O_{ice}$ record within age uncertainty. Then the Greenland cooling leads southward shift of ITCZ and in turn it changes wetland CH_4 emission. Bond et al. (2001) found that IRD maxima during the Holocene coincide with solar activity minima. The authors suggested that solar forcing could affect the climate change around the North Atlantic Ocean (and Greenland), through amplification by changes in sea ice and/or deep water formation. However, the forcing mechanism of solar activity on the North Atlantic and global climate is not well understood during the early Holocene. Renssen et al. (2006) suggested that low solar activity (in terms of total solar irradiance) can induce sea-ice expansion

around the Nordic Seas and weakening of deep water formation and cooling in North Atlantic region. Nevertheless, the anti-correlation between solar forcing and sea-ice expansion (and hence deep water formation weakening) is not strong during the early Holocene due to relatively warm climate conditions. Jiang et al. (2015) also found a negative correlation between North Atlantic SST and solar forcing proxies (^{14}C and ^{10}Be), which is statistically significant for the last 4000 years, while the correlation disappeared during the mid- and early Holocene. They hypothesized that climate sensitivity to solar forcing is high for cooler climate. Above evidences indicate that the early Holocene CH_4 minima were triggered by anomalous low solar activity, but future study is warranted to draw more conclusive result."

p7L8-9: you could add Cruz et al., 2005 to the reference list, as they discussed the interplay of solar radiation, monsoon intensity and CH_4 mole fractions

Here we hesitate to cite Cruz et al. (2005) here because the time scale they are dealing with is quite different.

p7L16-17: see above comment regarding references on ENSO variability during Holocene period

Please refer to our response to general comment #5. We will add further discussions on relationship with PDO- and ENSO proxies for the early Holocene interval.

p7L23: provide temporal resolution of NEEM record, 1 sample in how many years?

We will provide information on temporal resolution of each record as below:

"The NEEM CH_4 record is chosen as a reference because the mean time resolution is ~~higher than our data set~~~11 years, which is higher than Siple Dome data (~26 years) during the early Holocene."

p7L24: consider IPD reconstructions with CH_4 records from WAIS

We will provide alternative IPD reconstructions from more reliable records; NEEM discrete, WAIS continuous, and Siple Dome discrete data. Also refer to our response to Referee #2.

p7L25-30: NICE approach!

p8L2: …show an increase by XYZ ppb from…

We will add the magnitude of increment as follows:

"Our results show an increase by about 8 ppb from ~10.7 ka to ~9.9 ka, which was not previously reported."

p8L4: …in both hemispheres during…

We will change the sentence as suggested:

"Considering the long-term decreasing trend of CH_4 mixing ratio in both ~~poles~~ hemispheres during the early Holocene, the increasing IPD implies that the amount of boreal emission reduction should have been less than that of low latitude emissions."

p8L6: …from both hemispheres and…

This sentence will be modified as below:

"Given the new high resolution CH_4 records from both ~~poles~~ hemispheres and IPD, we ran a simple 3-box CH_4 source distribution model to quantify how much the boreal and tropical source strengths were changed."

p8L8-9: extra-tropical latitudes (30N or 30S is not high latitude, rather extra-tropical)

This will be modified.

"Briefly, the model contains 3 boxes; northern ~~high~~ extra-tropical latitude (30-90°N, N-box), tropical (30°S-30°N, T-box), and southern ~~high~~ extra-tropical latitude boxes (30-90°S, S-box). CH_4

concentrations in 3 boxes (in Tg box^{-1}) were determined from CH_4 mixing ratio of Antarctica and Greenland."

p8L13-14: develop description of model assumptions and impacts, e.g. what life times did you assume and why? did you tune life times to match previous flux estimates?

We used the identical parameters described in Chappellaz et al. (1997) and Brook et al. (2000). Previous estimates are averaged values throughout the early Holocene, so that it is difficult to compare and match our higher resolution IPD to the previous results.

p8L15-16: quantify and discuss flux estimates, otherwise meaningless

The flux estimation will be given.

p8L18: in tropical emissions by XYZ Tg. (quantify)

Amount of tropical emission change will be given with its proper uncertainty.

p8L25-30: increased CH_4 emissions from boreal wetlands were previously suggested by Sowers 2010, that agreement should be acknowledged

Please refer to our response to General Comment #5.

p8L29: explain "conventional" northern CH_4 emissions

We will change the phrase to "…small compared to current understanding of boreal emission…".

p9L1: the isotope records are already published and need to be acknowledged (Sowers, 2010). these isotope data are available and should be added to the figures of this manuscript.

We will add "high resolution" into the sentence as "…as well as high resolution CH_4 isotope ratio data for the younger time period". Also we will add the citation.

p9L10-15: therefore, IPD should be calculated with WAIS records as well

See our response above and Figure R2 and R3.

p9L30: there is also no explanation for the drop in IPD if I am not mistaken?

The slight decrease in IPD between ~10.3 and 10.8 ka is not observed in the alternative IPDs. Although it is argued above that Siple Dome CH_4 is more reliable data than others, we cannot rule out the possibility that the Siple data deteriorate during this period. At present it is difficult to say the drop in our IPD represents the real change or not.

p10L5-6: why can the 10.2 ka event not be explained by low latitude hydrology, but the other events can? I feel this is a section where great care has to be taken to prevent from over interpretation. There is no quantitative estimate for low latitude emissions during other events, i am not convinced that the presented records allow for a partial explanation of the CH_4 minima and that there is only a missing bit. I would recommend to re-formulate. Even if the revised IPD reconstruction supports the current discussion, this might seem as two results are made to fit together. I feel this can be toned down and still be strong a conclusion.

The 10.2 ka event does not bring corresponding abrupt change in Cariaco Basin record, Dongge Cave speleothem data, and Siple Dome $\Delta\varepsilon_{LAND}$. Thus we speculated that CH_4 drop at 10.2 ka was caused by source reduction in boreal regions and/or Southern Hemisphere. We will add further discussions on this and rephrase the sentence.

Assessing the latitudinal source change from IPD is not reliable, given each ice core gas record has had own characteristic smoothing process at firn, because it could lead erroneous result for abrupt changing intervals such as 10.2 ka event.

p10L6-12: I am not sure I understand this section

We will re-formulate the sentences to better clarity.

p10L11: the quantification 20-40 ppb is mentioned for the first time here. The conclusions cannot include information that have not been presented earlier in the manuscript. I am not sure how you quantify ppb changes? Is that from the box model?

The amplitude of each millennial scale CH_4 drop is quantified by CH_4 anomaly curve shown in Figure 2. We will add this into earlier part of manuscript.

p16L19: I didn't check all references, but Sowers 2010 is not correct. This is "Atmospheric methane isotope records covering the Holocene period, Quaternary Science Reviews 29, 213-221, 2010". The title/journal name in your references refers to his 2006 paper

The reference will be modified.

p18L4: add reference to the list or check reference

We will check the reference list.

p18F1: show overlapping period, show minor ticks on both axes

Please see our response to the General Comment #1.

p19F2: define 0 ppb in anomaly or show CH_4 mole fractions, check width of yellow bars, can be confusingly wide

The Figure will be updated.

p20F3: add IPD with WAIS data

Please see our response above, and Figure R2 and R3.

p22T1: caption is confusing to me, also what is this table supposed to add? how can this agreement be explained, life time?

The table and caption will be updated with revised IPD reconstructions.

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