We thank the reviewer for this constructive and detailed review. Below we respond to each point in blue text and propose altered text in italics.

This paper will be a valuable addition to the literature on the isotopic composition of methane in the past and the Holocene methane cycle. The use of data sets from both hemispheres is very important, as the heuristic modeling in Figure 2 shows. This point could in fact be emphasized a bit more. I do not have any major concerns about the manuscript but do have several smaller issues to bring up that might help improve the clarity of the presentation.

Introduction in general: The introduction is slightly long, and while there is nothing wrong with it some of the sentences and paragraphs could be shortened.

We went through the introduction carefully and besides some minor changes did not change much. We think that all the information given is required to put the study in perspective and therefore we are not able to shorten it considerably.

Page 2, line 5: “Low pass” filter may be misinterpreted by those not familiar with ice cores, I suggest being more specific about what you mean here.

We will change the text to be more specific:
Only polar ice cores can provide this information as they represent a direct, albeit through the bubble enclosure process in the firn low-pass filtered archive of trapped air and, therefore, offer the possibility to investigate the atmospheric composition of the past.

Line 17: Sowers 2010 is probably not the first reference for the bowl shape. Chappellaz et al., 1997?

We will change the citation to *Blunier et al., 1995* instead of Sowers 2010

Line 36: normally elements are not capitalized (deuterium rather than Deuterium).

Yes, we will change that.

Line 40: Leaves instead of leave.

Yes, we will change that.

Line 42: In instead of on.

Yes, we will change that.

Page 3, line 10-14: The sink discussion is a bit cursory particularly since there are papers in the literature about changing sink terms influencing the Holocene budget. For example, Kaplan et al., 2006, GBC. I suggest expanding this section to provide a more detailed defense of keeping the sink strength constant.

We will change this paragraph:
As mentioned above, changes in the sinks leave an imprint on the isotopic composition of CH4 in the atmosphere as well. However, while minor changes in the relative importance of the sink processes cannot be ruled out, in general, atmospheric chemistry models point to small changes in total lifetime (Kaplan et al., 2006; Levine et al., 2011a) and we also assume the relative contribution of individual sinks to be rather constant over the Holocene when relatively stable climate conditions prevailed. Therefore, the discussion of this study centres around CH4 source processes, hence emissions of different CH4 source categories.

Page 4, line 30-31: It is not clear to me what “local average density” means in this sentence. Apparently the local average density is constant but the data are unequally distributed and this sounds contradictory.

We will change the text as follows:
The down-sampled data are still unequally distributed, but on average the data density is similar over the whole time period studied.

Page 5, lines 14-18: It is not clear here if step 2 in the synchronization actually does anything. Is it necessary to have this as a separate component? It reads sort of awkwardly.

Yes, we did not find any additional tie points when we tried to align the different datasets within the two hemispheres. However, we think it is important to mention that we could not find any incongruities with the given data resolutions.

Section 2.4 Heading and elsewhere. The term deconvolution is used here, and I think in general this is probably fine, though some would associate it with signal processing, whereas I believe the technique used here would also be called inversion, a term also used in the paper.

We will change the term deconvolution to inversion throughout the manuscript.

Page 7, line 11. A minor point perhaps but is the ppb – burden conversion here consistent with the latest concentration scales?

The ppb-burden conversion is the same as in the 4 box model described in Fischer et al., 2008 and in Baumgartner et al., 2012. This allows a direct comparison between the different model results. By using more recent estimates of the atmospheric CH_4 burden and the mean CH_4 concentration we end up with a slightly different conversion factor (e.g. using numbers by Dalsøren et al., 2016 we get $m^* = 2.6\ to\ 2.7\ Tg/ppb$ instead of $m^* = 2.9\ Tg/ppb$). However, since the focus of this study is on relative changes and not on absolute values this does not affect our conclusions.
We will change the text as follows:
The parameter m^*=m_0/c_0 translates atmospheric concentrations (ppb) to total atmospheric inventories (in Tg) using the mean atmospheric concentration value c_0=1650 ppb of the year 1987. Here we use the corresponding global CH4 burden m_0=4800 Tg by Steele et al. (1992) that was also used in previous ice core studies (Baumgartner et al., 2012; Fischer et al., 2008). Note that this value is 7-10% higher than more recent estimates (Dalsøren et al., 2016) and accordingly the derived absolute emissions may be 7-10% overestimated. However, we focus our interpretation on relative emission changes, which are not affected by this scaling factor.

Equation 3: Although one could calculate rx, out of interest to the reader could you give the value for ITCZ at 5 N to make it clear how much difference it makes?

OK, we will include *(r_n=0.46, r_s=0.54 for φ_ITCZ=5°N).*

Page 7, line 5: Is little r defined anywhere near equation 1?

The ratio of the box volume r_x is defined in Eq. 3.

Page 7, line 23: Should these sink terms be constant if the boxes are not the same size?

The sink processes are quantified by the fluxes s_OH, s_strat, s_soil and s_Cl (in Tg CH_4 per year). According to where the different processes are thought to take place they are distributed over the two hemispheres. The size of the hemispheres (r_x) becomes important when the hemispheric lifetime (and sink fractionations) are calculated (Eq. 4). We kept the box size hence also the relative sink contributions constant in time in our standard run.

Page 9, line 6: Indicate if time is left to right or right to left in Figure 2.

By omitting the direction of the time flow we tried to indicate that the artificial time series could principally be interpreted in both directions. But we will add arrows indicating that time is running from right to left (to be consistent with all other figures) to avoid confusion.

Line 23: There may be a better word than “compensate” here, the emissions are required to have a certain time history by the assumptions made.

We will change the text to:
At the same time the emissions in the isotopically lighter CH4 mixed from the northern into the southern box requires the southern box emissions to become more enriched in the heavy isotope (isotopically “heavier”, equivalent to higher δ-values) over time.

Line 42-43: This seems repetitive.

Yes, maybe this is slightly repetitive. But we think it is important to point it out clearly that although the chosen ranges are based on the Holocene data, the explained changes are not realistic.

Page 11, paragraph 2: The data are interpreted in terms of 2 sigma uncertainty but 1 sigma is plotted. This gets confusing because from the figures there are interesting deviations in the results but the text tells us they are not to be interpreted that way.

Yes, we will add 2 sigma uncertainty ranges in Figure 3.

Line 16-20: The last two sentences of this paragraph could be reworded. It is first stated that there is a remarkably strong constraint and then stated that the uncertainty does not allow robust conclusions. These two statements seem inconsistent. I think I know what is meant here but it could be made clearer to the reader.

We will change the wording in this paragraph to clarify:
The absence of a long-term trend in δD-Ex over the Holocene with the significant changes in the atmospheric [CH4] is remarkable and a strong constraint on the average Holocene CH4 budget. However, the large uncertainty in δD-Ex does not allow us to make robust conclusions about millennial variations in the hydrogen isotopic signature of CH4 emissions.

Section 3.1: I may be missing something here. I understand that SF6 in the modern atmosphere can help determine mixing time. I presume that this calculation must by definition choose a value for the mean ITCZ position. One can, I guess, calculate different sets of mixing times for different assumed ITCZ positions given the SF6 data. Is this what what was done for the sensitivity studies? If I have this correct, is this a full exploration of the possibility of changes in atmospheric mixing? Why not just vary the mixing term independently? Is there an assumption here that it only changes because of changes in ITCZ position? What about atmospheric dynamics, could that change?

Yes, our SF6 calibration approach is based on modern values and thus represents todays atmospheric dynamics. Thus the mixing time derived from the SF6 calibration is a modern value and we keep this constant over time as the (tropical) climate conditions are rather stable over the Holocene except for slight shifts in the ITCZ position.

Figure 3: It is clear enough that red and blue in the right hand panels are for N and S, though this is not actually labeled explicitly in the legend.

In the figure caption we will add:
Red colours refer to northern hemisphere records, blue colours to southern hemisphere records throughout the manuscript.