Overview:

The manuscript “Using $\delta^{13}C$-CH_4 and δD-CH_4 to constrain Arctic methane emissions” by Warwick et al. describes the results of a modelling study of Arctic wetland and hydrate emissions, in which the simulated concentrations of CH4, along with the associated $\delta^{13}C$-CH_4 and δD-CH_4 ratios, are compared to observations made at a number of high-latitude Northern Hemisphere measurement sites. The latitudinal gradient of these isotopologues is also assessed in comparison to observations. Finally, in an attempt to improve our current understanding of methane emissions from the Arctic, the effect of changes made to the wetland and hydrates emission inventories in the region is investigated.

Overall the manuscript is very well written, with few technical corrections necessary. The figures are generally quite clear and well chosen, although some small alterations are necessary for a couple of them. The methods used in this manuscript provide a neat way of assessing the accuracy of some of the current methane inventories used in atmospheric models, and the improvement in the comparisons with observations after the seasonal cycle of the wetland emissions is altered is striking. Using the three isotope ratios of methane as a ‘triple check’ on the seasonal cycle of the emissions works well and provides extra clues as to the timing and magnitude of emissions in the region. Finally the examination of the magnitude of hydrate emissions in the Arctic, whilst brief, does indicate that some recent estimates of emissions from this source may be too large.

My main reservation is that the conclusions drawn are dependent on a single (fairly old) wetland inventory, and there is no discussion on the impact that this fact might have on results. Is the relative geographical distribution of wetland emissions important for your conclusions to be substantiated? See general comments for more details.

I recommend this manuscript for publication after these revisions have been carried out.

Comments:

Page 1, lines 20-29: These paragraphs could use some extra references. You describe the recent changes in the methane growth rate without referring to any sources for this information (‘2007… rapid methane increase’,’growth was strongest in the tropics’, etc.), and there is also no reference for the assertion that fossil fuel changes could play a role in the global growth rate or that Arctic emissions are poorly quantified.

Page 5, lines 5-9: In this work, you have used observations averaged over 2005-2009 and model meteorology for 2009 only, but in order to show that the OH fields used in the study are to some extent accurate, you show comparisons with MCF concentrations at one site for the year 2011. To be consistent with the meteorology used for the later figures, can you show 2009 concentrations here? MCF measurements should also be available at Alert, Canada. Does the model also capture the seasonal cycle that far north?

Page 5, line 16: My main reservation with this study is related to the emissions inventories used. The Fung wetland inventory is now 25 years old, and whilst it generally does a good job, I think it is worth at least discussing the idea that the distribution of emissions in this inventory may not be correct. Since all of your observation sites are located in the US and Europe, are the observed seasonal cycles sensitive to the significant emissions from Siberia, or is the cycle only of the local emissions important?
Ideally, you'd carry out a supplementary model-run in which an alternative wetland scenario is used. The Bousquet (2011) inversion inventory, for example, assimilated observations of CH_4 made throughout the Arctic, and would likely, therefore, be able to capture the seasonal cycle of Arctic CH_4 well. However, according to Figure 1, it does not show the same delayed seasonal cycle and large magnitude of autumnal emissions required in your FUNG_DEL cycle in order to capture the seasonal cycle of CH_4. Also, as far I can tell, it has not been compared to observations of methane isotopologues before, and doing so may back up your conclusions that significant emissions deeper into the autumn are necessary.
Related to this, I note that you used the GFEDv2 biomass burning inventory. Version 4 of this inventory is now available, and any changes to the impact that the heavier $\delta^{13}C\text{-}CH_4$ has at these locations might affect your conclusions. However, I accept that the relative contribution of biomass burning emissions compared to wetland emissions at these latitudes is probably very small and therefore unlikely to have an effect unless emissions are local to the measurements.
If further simulations are not possible, I think a discussion of the effect of your choices on your results should be included in the results section.

Page 5, line 16: This is the first mention of the BASE scenario. You should make explicit that here, 'BASE' refers to the control experiment that uses the emissions described in the previous section, rather than some model set-up.

Page 6, line 6-10: This paragraph needs a little more detail. You have not previously described the locations of those measurements made further south than Cold Bay (perhaps they could also be included in Figure 4?). You say that the gradient in δD-CH_4 is captured, and also that it is underestimated in the NH mid latitudes. Can you explain more clearly? It looks to me that perhaps the δD-CH_4 is mostly captured quite well as far to 50S, but that using the South Pole value as a baseline is shifting the model away from the observations. Perhaps it's the SH gradient that isn't captured, rather than the NH gradient?

Page 8, line 12: It's a shame that there are no δD-CH_4 ratios included here for completeness, but since the changes to the wetland emissions in this section of the study don't improve simulated CH_4 or δC^{13}-CH_4 concentrations, I understand the reluctance to carry out the runs.

Page 8, line 18: The name "WETLD_X2" is a little misleading, as emissions have been increased only by 50%. Can you change this name?

Page 11, line 15: Are the model lines here full zonal means across all longitudes? If so, is there any impact on the comparisons at the sites in the Arctic if you compare only at the measurement locations? I think the plot would be too busy if you included these comparisons within it, but you could mention it in the text if there is any effect.

Figure 3: I think that this plot could be a little clearer. Can you include the locations of the measurement sites here (or in Figure 4)? Is this an annual mean or is it the peak summertime emissions? Can you differentiate between regions where wetland emissions are zero and where they're just smaller than the lowest value in your colourbar?
Perhaps you could include a similar second panel showing the standard deviation of the emissions, or the month during which emissions peak (or at least mention it in the text)? i.e. do emissions peak in July everywhere in the Artic, or does it vary by region?

Figure 10: Can you differentiate the lines more clearly in this plot? The difference between the dash, dot, dot-dash and dot-dot-dash lines is not obvious enough in a plot of this size (especially as they only deviate in a small subsection of latitudes).

Technical corrections:

Page 1, line 11 and throughout: I find the use of the term ‘coloured’ throughout the manuscript to describe the different tracers a bit odd, although I accept that it can be a difficult idea to describe well. I’d suggest changing to the term ‘tagged’ or similar for clarity.

Page 2, line 3: “to-date” -> “to date” (no hyphen)

Page 12, line 29: “May-time emissions” -> “May emissions”/“emissions than predicted in May”

Page 13, line 13: “currently lacking” -> “currently-lacking”