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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Using δ^{13}C-CH_4 and δD-CH_4 to constrain Arctic methane emissions" *by* Nicola J. Warwick et al.

Anonymous Referee #2

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General

The manuscript 'Using d13C-CH4 and dD-CH4 to constrain Arctic methane emissions' by Warwick and co-workers presents a study of atmospheric CH4 and its isotopes (d13C and dD) in the Arctic. Model simulations of all three components together with observations are used in a qualitative way to draw conclusions on the main arctic emission sources and their seasonal behavior. The analysis applies state-of-the-art modeling techniques and the methods and results are generally presented with a clear language and structure. The work clearly adds an important piece of information entangling the contributions of different methane sources and will probably help to further improve process models that are of paramount importance to understand future climate-emission feedbacks in the Arctic. I only have a few minor comments that can be addressed in the revised text but will, most likely, not require any major changes in

the analysis.

Major comments

One possibly more important remark has to do with the chosen wetland emissions in the presented simulations. Why was the rather old Feng et al. dataset chosen as a reference? As can be seen in Figure 1 the LPJ-Bern emissions already follow the suggested delay in summer-time emissions. Related to this: What is the influence of the spatial distribution of the chosen wetland emissions. Could the suggested shift in emissions also stem from an erroneous distribution in space rather than in time? How different is the spatial distribution of LPJ-Bern as compared to Feng et al?

Minor comments

P1, l18/19: Clarify if by inventories you are referring to purely anthropogenic emissions here.

P1, l26: What is the status of the Nisbet et al. publication? If not yet published another reference is needed here.

P2, l33: 'In this study': Does this still refer to Berchet et al. or the current study?

P3, l6/7: The given reference is rather old. Please give some newer references and a total amount of emissions here. See for example Kirschke et al. 2013 for some numbers and additional references.

P3, l8: The additional stratospheric sink by Cl and O1D should be mentioned here as well, although it probably adds little to the seasonality.

P3, l22-24: You should mention the work by Rigby et al. here as well, who already ran a CH4 isotope model (both d13C and dD) to evaluate the benefits of atmospheric isotopic observations: Rigby et al. 2012, JGR, VOL. 117, D12312, doi:10.1029/2011JD017384.

P3, l30f: In figures 5, 8 and 10 more than these 4 sites are used for comparison. Please mention which other sites are used in the figures.

P4, l16-18: For which period are these values given?

P5, l2: Which functional relationship was actually used to calculate the OH reaction rate coefficient? Reference or equation.

P5, l9f: To put the importance of these other reactions into perspective, could you give an average lifetime of CH4 wrt to stratospheric and MBL reactions as well?

P5, Table S2: Table S2 should be integrated in the main paper, since it is an essential information for the study. However, in the table it should be clarified which source is taken from which reference and how seasonality is considered.

P5, l24f: Which influence may the spin-up still have on the results. The spin-up basically leads to an initial state in which sources and sinks are in equilibrium. Is this adequate for the study period or could it be important to start from a background that is not in perfect equilibrium (due to steadily increasing emissions in reality).

P6, l22: 'with the seasonal cycle'. Does this refer to the observed seasonal cycle?

P9, l2f: Fig. 1 should be mentioned here again, which shows the emission profiles of Fung and Fung delayed.

P9, l6: 'forward': To me this is confusing. I would call it shifting the seasonal cycle backward.

p10, l1-3: Berchet et al. 2016 clearly showed a strong seasonal cycle for emissions from ESAS, with a summertime peak in the order of what was suggested by Shakhova et al. 2012 for the whole year (see Fig. 5 in Berchet et al. 2016), i.e. 10-15 Tg/yr. In contrast, Berchet et al. suggest close to zero emissions in winter. How could this seasonality, that was not considered in the current model analysis, change the drawn conclusions concerning the impossibility to accommodate the ESAS flux as suggested by Shakhova et al.?

Section 6.2: Please comment on the good agreement of the delayed temporal devel-

opment used in your sensitivity run and that simulated by LPJ-Bern (as seen in Fig. 1). Does their model version already include possible CH4 emissions after freezing of the top soil?

P11, l21-23: This thought should be given some more discussion. Would it still be possible to accommodate a 50 % reduction in high-latitude anthropogenic emissions (as compared with the BASE run) within the range given by previous studies (e.g. Kirschke et al.).

Figure 2: Should be part of the supplement. It is not essential to the study, but 'only' demonstrates that the applied model seems to perform reasonably well in terms of OH degradation.

Figures 6 and 9: Why is there no observed dD for Ny-Alesund? If it was not observed there, it should be mentioned somewhere in the text.

Adding a table summarizing all model runs (BASE and sensitivities) and their specific settings would be nice. All the sensitivities should be given an abbreviation/name (not done in all cases) in this table and in the text so that it is easier to quickly identify what the specifics of a certain run are.

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