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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Detectability of Arctic methane sources at six sites performing continuous atmospheric measurements" *by* Thibaud Thonat et al.

Anonymous Referee #2

Received and published: 16 May 2017

This paper describes an Arctic version of the model CHIMERE, which has been used with tagged emissions of methane to diagnose the influence of different sources on 6 observational sites.

For me, the most interesting new result in this paper is the freshwater lakes inventory work, as this is a non-negligible source of methane that many models neglect. I think it would be good to make this clearer in the abstract. I think it would also be worth pulling out some figures to quantify how important the lakes are in the abstract, eg freshwater lakes account for 11-26% of the signal at your sites. I would also suggest that it would be useful for potential readers if this were reflected in the title of the paper as well, if you agree that this is the most important aspect of the paper.

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Discussion paper

Another interesting finding was that a later wetland seasonal cycle seemed to agree best with the observations. I think this is of interest as (a) we have many different wetlands emissions inventories and we want to know which is best to use in models, and (b) this agrees with recent observations from Zona and modelling from Warwick. So I think this would be good to highlight in the abstract.

Section 3.1.3 line 423, and line 678: When describing the seasonal cycle of methane in the Arctic, I would expect there to be lower methane in summer because of the presence of OH, compared to in the darkness of winter. In my mind, this outweighs the higher emissions of methane from wetlands in summer. I would see that as the main driver of the seasonal cycle over the whole Arctic, with any deviations from this attributed to some local influence eg from nearby wetland emissions. I am not sure I would attribute the seasonal cycle to transport from outside of the domain unless you had evidence to back this up. Even if you do have those numbers, isn't it the fact that the OH influence is acting in the midlatitudes too, so ultimately the transport into the boundary is related to the OH seasonal cycle anyway? I suggest that this section is revisited, with the OH seasonal cycle in mind.

Specific minor points:

Use methane or CH4 consistently throughout manuscript. Same with American/British spelling eg analyzes/analyses, vapor/vapour. Also, does Pole need a capital letter?

Line 58: There were two recent OH sink papers in PNAS, by Rigby et al and Turner et al. Maybe worth referencing here too. Dalsoren 2016 reference contains a typo.

The submitted Poulter reference is mentioned a few times. Unless this is published first, perhaps a good idea to mention the project name, so people might be able to look it up a bit easier.

Line 132: I think it should be "of emissions" not "on emissions"

Line 148: methane and Arctic should be the other way around

Line 192: please explain why you only use the background data here

Line 215: do you really mean forecasts, or do you mean analyses?

Line 218: Define LMDz

Line 241: define FAO

Line 281: Perhaps worth stating the resolution in km here as well.

Section 3.3: does bLake4Me stand for anything?

Line 560: I suggest adding “(black dots)” after “A positive value”, as the colours confused me at first.

Line 587: the numbers here are confusing. I would say “ The bias is improved from -6.4 to -6.0 ppb over the year”

Line 618/fig 10a: setting the sink to be a positive value is confusing. Consider changing this, or explaining it a little to make less confusing.

Line 701: Warwick at al 2016 also supports a delayed seasonal cycle in wetland emissions.

Fig 6 and 7: the quality when I printed these is not good. There are fuzzy areas, and it’s hard to see the boundary conditions and the observations.

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