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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of the theoretical limit in instrumental detectability of Arctic methane sources using ^{13}C atmospheric signal" *by* Thibaud Thonat et al.

Anonymous Referee #1

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In this study, an atmospheric transport model is used to quantify the accuracy that is required for continuous measurements of d13C-CH4 in the Arctic. Isotopic measurements provide important process specific information about sources and sinks, which has proven very useful in global studies of methane using flask measurements from the global monitoring network. In recent years, instruments are becoming available for continuous measurements of methane isotopes. Their application is still limited, but has been demonstrated to be useful for regional networks. So far, however, they have yet not been deployed in the Arctic zone, but this could be a very promising application. This study quantifies the amplitude of the isotopic signals that can be expected, which is a useful contribution. I am less convinced about the approach to quantify detectabil-

ity, as will be discussed further below. Some suggestions are made to help improve that part, and widen the application area to further strengthen the significance of this work.

GENERAL COMMENTS

In my opinion, the scientific value of being able to detect methane emissions from wetlands in the Arctic is limited. We know that those emissions exist, and that they are important. More interesting is to be able to improve their quantification. For that, detection is not a sufficient requirement. The detection of regional trends would add significant understanding, but for that the requirements will be different. The question is not only about single measurement precision, but also the minimum number of measurement sites needed. This also brings in the dimension of data averaging, reducing the requirements depending in the statistics of the errors, the measurement frequency, and the temporal resolution that is needed. The conditions that are used to define 'detectability' in this study are not well motivated. Since the required measurement performance will depend on the details of the scientific questions that the measurement should help to answer, however, I think that to quantify the expected amplitude of variation is a more important outcome. It is possible to turn this into requirements, but then the purpose should be more clearly defined, and the inevitable limitations should be discussed as well.

An important distinction is found between remote, and regionally to locally influenced stations. Since the signal amplitudes differ between those sites, so will the measurement requirements. Yet the abstract and conclusion sections generalize the requirements to a single set. It should be made clearer what kind of sites are addressed by the numbers that are listed (rather than just a statement that the requirements will vary between sites). More useful would be to distinguish between applications. For some applications the requirements may be less stringent, especially if a larger number of cheaper sensors are deployed.

Over land, the amplitude of the signal will depend strongly on the altitude of the air inlet, and therefore the model level that is sampled. The altitudes in Table 1 probably refer more to the local orography than the height of the measurements with respect to the ground. There is a potential for increasing the significance of this work by adding the vertical dimension. What is the implication for required accuracy of towers and aircraft measurements?

SPECIFIC QUESTIONS

page 3, line 140: Although not long-term, the benefit of high frequency measurements was convincingly demonstrated by Roeckmann et al (acp, 2016).

page 4, line 218: It seems that the detectability of biomass burning could be influenced by the use of monthly average emissions, since in reality they may vary strongly with time.

page 4, line 224: GLOGOS

page 5, line 255: The d13C value of natural gas from West Siberia is known to be highly depleted (see e.g. Tarasova et al, 10.1007/s10874-010-9157-y)

page 7, line 366: 'However, they are excluded from our analysis . . .' But later the threshold detectability is defined from the source making the largest contribution to the signal. Shouldn't this signal include variations due to the background (it they overwhelm the regional sources this should limit the detectability)

page 8, line 441: Wouldn't the fact that the most significant sources all lead to methane depletion limit detectability. How do you distinguish one depleted source from another? It occurs to me that the definition of detectability ought to take differences in signatures into account, rather than only single process contributions.

Table 4: Is the year dependence of the thresholds important enough to restrict it to the year 2012?

Figure 3: What do the triplets of numbers at each site represent?

Figure 5: This shows that for a median wetland signature, the threshold of 0.5 per mil listed in the abstract would yield no single day of measurements. This seems to suggest that 0.5 is a too relaxed requirement.

TECHNICAL CORRECTIONS

Page 2, line 63: carbon dioxide

page 4, line 235: ERA-Interim reanalysis

Table 1: 'Range' i.o. 'Variant'

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