

## ***Interactive comment on “Using ship-borne observations of methane isotopic ratio in the Arctic Ocean to understand methane sources in the Arctic” by Antoine Berchet et al.***

### **Anonymous Referee #2**

Received and published: 5 November 2019

Understanding Arctic methane is of great importance because it is region where there is a large volume of frozen carbon that is highly susceptible to global warming. There are few continuous monitoring stations that monitor changes in Arctic methane emissions, fewer still measuring methane isotope ratios. This paper describes an Arctic ship-borne measurement campaign that measures total CH<sub>4</sub> and isotope ratios. These data are used in an atmospheric transport model and a simple inversion system to characterise the variability of methane source signatures from Arctic source types. Usually, isotope data are used in an inversion to improve the attribution of emission estimates, an approach that assumes the isotopic source signatures are known. Here, the authors assume the methane emissions are known and instead solve for isotopic source

C1

signatures from the Arctic region. They find there is variability in the source signatures and therefore it is not appropriate to assume constant emissions source signatures for Arctic methane. The work described in the manuscript will make a useful contribution to the literature by further understanding methane isotopic ratios and their importance to climate change in the Arctic. This paper is within the scope of ACP and presents a novel dataset in an interesting way. I support the work being published in ACP after the authors have addressed my comments and queries below, which I believe will make their work clearer to understand.

In general, I found the methodology could be clearer and some of the decisions in the data analysis approach could be justified (in some cases) and justified more thoroughly (in other cases). Without such additional information I would find it difficult to reproduce their results given their dataset.

In particular, in section 2.3, I would like to see the inversion approach described in more detail. It should be made completely clear what they are solving for in their state vector and which parameters go into the inversion. The approach of the inversion is also unclear to this reviewer. This could be rectified by detailing the inversion method, and then laying out the relevant equations (perhaps in the supplementary material). In section 2.2, it sounds as though only OH is included as a sink, which ignores other potentially important sinks (such as the soil sink, Cl radical and stratospheric loss). Similarly, these sinks seemingly aren't accounted for in the optimisation, despite the fact that they also have larger uncertainties that vary in space and time. I am not suggesting the authors solve for them but they should at least acknowledge these other sinks for completeness. In section 2.2, it seems that only wetlands and anthropogenic emissions are used in the model, but again that is not clear from reading the manuscript. I suggest to address this explicitly in the main text or summarize the information in a table that lays out all the sources used in the model.

Building on this, it would be useful if the decisions that went into the setup were justified more clearly. For example, the authors describe that they assume emissions and

C2

atmospheric transport from the model are suitable due to how they match observations, but this is not rigorously tested. It might be possible to first solve for emissions and then for source signature (making a two-step inversion), or to perform sensitivity tests on their assumptions by, for example, perturbing model transport. Similarly, the lack of sinks other than OH, and the choice of which sources that are included, are not justified. The latter should be quite simple to rectify with a short explanation and some literature examples, or by laying out a table of the current understanding of methane source signatures.

Some minor corrections include spelling and grammar errors. Some sentences do not make sense, e.g. the first sentence in the abstract. In addition, some further details of the how the model works could go into the supplementary information section. Figure 2 could be made clearer – it's a little hard to discern the observation line from the model results in the top panel. Maybe a separate panel with total model and observations would solve this. Also, the introduction would be stronger if it laid out current understanding of Arctic methane source types, thereby justifying the article by highlighting our current lack of understanding.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-595>, 2019.