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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Methane at Svalbard and over the European Arctic Ocean" *by* Stephen M. Platt et al.

Anonymous Referee #1

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Summary:

This is an excellent and interesting study. It presents both long term surface data (relating to atmospheric methane climatology and interannual trends and variability in the European Arctic Ocean region) and relatively long-term case study data collected from a research vessel to investigate sea-to-atmosphere fluxes linking meteorology and water-column processes to dissolved methane resulting from geological seeps and sea-bed methane hydrates. Dispersion model approaches are used to interpret the ocean-atmosphere flux (diagnosing an interesting transient hotspot which has been investigated) and there are insights into methane hydrate analysis methods and carbon-isotopic composition and hence thermogenic and biogenic sources of methane contributing to hydrate formation. The role of wetland, anthropogenic and

ocean fluxes are also investigated seasonally using measured data, emissions inventories and FLEXPART modelling to interpret the changing roles of each source and how this manifests in the measured data. In that respect, the study actually covers a lot of ground for a single paper.

In a wider context, this study helps to inform a part of the very active global methane debate, focusing on the Arctic region and oceanic sources to atmosphere and their origins in the sea-bed and water column. The paper is very well written and the narrative is clear, despite the multiple topics that are discussed. The data used are high precision and world-class, linked to international standards with sound calibration practices. The data are analysed and presented rigorously and methods are clear and world-leading. Figures are good quality. The article also offers guidance on new approaches to lab measurement of hydrates. While I have recommended minor corrections here, they are very minor indeed and I recommend publication in ACP, where it will be received well by the readership of this journal. As a result, the specific comments in this review are relatively brief.

Specific comments:

1/ The abstract perhaps does not capture all of the important content/conclusions of the paper. It does not refer to the new methane hydrate measurement method or the new understanding of the hydrate isotopic/tracer characterization that followed from that part of the work. Can this be summarized and added? It is useful summary guidance and insight.

2/ P.2 line 13. The introduction summarizes the global methane debate from the point of view of sources only. The debate on sinks should perhaps be very quickly acknowledged. However, this particular line phrases a conclusion which indicates "another source than fossil fuel emissions as an explanation for recent CH4 increases". I know what the authors are trying to say here but this phrase keeps popping up in the global methane debate and it is not accurate insofar as it is self-contained, and can even be very misleading to, and misused by, those with an agenda. Without fossil fuel (thermogenic) emissions in general, global methane concentrations would be declining. It is true that isotopic data etc all point to a growing proportion of biogenic emissions in the total budget recently but both source types are broadly agreed to be rising in their respective emissions - just at diverging rates (since 2006). This sentence, like so many in other papers, leads the reader to conclude that only biogenic emissions explain the recent rise - this is simply not true. All sources contribute to the rise, just in different proportions. And as I have said above, without thermogenic emissions, methane would be declining. So, this sentence is wrong, misleading and a disservice to the policy implications of the wider global methane debate. It must stop. Yes - biogenic sources are increasing, but no - they are not solely responsible for the recent rise in burden. The causes of the rise are agreed to be more convoluted than this simple conclusion suggests. Please could the authors improve the accuracy of this contextual statement in their intro?

3/ P.10, line 18: The conclusion/statement that seafloor venting is a very small influence on climate change is a bit of an extrapolation. The calculated flux is for the seep region in the European Arctic Ocean but the conclusion made here reads as though this applies generally for the planet. Seeps in other areas may be much larger - we don't know this yet. We cannot yet say if seafloor venting globally is insignificant or not. This region's contribution may well be insignificant (over the duration of this study) but the conclusion made needs to reflect the limitation of its scope.

Technical corrections: P.3 line 7: Change to "Ch4 fluxes (to atmosphere) were below..."

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