

## ***Interactive comment on “The origin of methane in the East Siberian Arctic Shelf unraveled with triple isotope analysis” by Célia J. Sapart et al.***

### **Anonymous Referee #1**

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The manuscript (ms) by Sapart et al. presents valuable information on the origin of methane in the East Siberian Arctic Shelf (ESAS), long been discussed based on assumptions and speculations. I like that this data set represents results of triple-isotope analysis. To my knowledge, this is first presentation of such a kind collected in the marine Arctic. This fact itself makes this ms novel. Besides, the data are from the potentially globally important region, because a predominant fraction of submarine permafrost is there; the ESAS was suggested to incorporate much of organic carbon and, probably, hydrocarbon stocks and hydrates in the sediments beneath permafrost. It was also shown to be warming due to natural warming as well as global warming. I believe this new data would be of great interest to scientists working in different disciplines and areas of the Arctic: to geologists, bio-geochemists, oceanographers, atmospheric scientists, climatologists and climate scientists.

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Authors report methane concentrations and triple isotope data analyzed on gas extracted from sediment (four long cores down to ~53m depth) and water sampled over the ESAS from 2007 to 2013. Samples were taken from different areas of the ESAS, which represent different state of subsea permafrost (frozen to thaw) extending from the coastal zone to the outer shelf. The manuscript is clearly written. The approach is valid and the applied techniques are appropriate – I have no questions regarding triple isotope measurements accomplished by the authors in the best European laboratories. I can't completely agree with the authors that that the predominant presence of methane is not of thermogenic/natural gas origin, but resultant from microbial methane formation using as primary substrate glacial water and old carbon preserved in the subsea permafrost or below, but I can share the logic of the authors of this ms, which is mostly resultant from limitations possessed by currently available methods of triple-isotope data interpretation. It is interesting that most of methane in the ESAS sediments is of Pleistocene age or older – in my understanding, this should mean that submarine permafrost is somehow permeable for gases. I am not satisfied with explanations presented by the authors regarding the contribution of super-modern radiocarbon in methane – anthropogenic sources would have been explaining this fact plausibly if it has been methane from the water column, not from the sediment cores. I understand that it must be very complicated to come up with more realistic explanations, but the authors should be working further on it.

I can raise several questions regarding this work. Specifically: - why did not authors consider results published by Franke and Cramer (2005) and Bussmann et al., (2013), who presented clearly thermogenic signature of methane sampled in the same area? -Why did not authors collected gas from bubbles included in the sea ice (reported by Shakhova et al., 2010) to measure triple-isotope signature of methane? Would not it give a kind of integral isotope signature of methane potentially approaching the atmosphere? -Why did not authors consider release of super-modern methane found further off-shelf (lines 327) by groundwater transported from the land (lines 327-329)? - How would the authors explain enrichment of  $\delta^{13}\text{C}$  by  $<50$  per mil if residence time

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of shelf water is only few months?

Many other questions could be raised, but I realize that this manuscript is based on multi-year work in the harsh Arctic environment. It is clear to me, that one paper, even incorporating that extensive data set, cannot answer all scientific questions regarding the complex, and previously unstudied, Arctic marine methane cycle. I appreciate that the authors have been accumulating data for a long period trying to cover as much aspects of this novel topic as possible. I also understand limitations possessed by current state of isotope biogeochemistry, which make it difficult to interpret isotope data collected in actual environmental conditions where methane of different origin, age, from different sources could be contributing differently in different areas – it differs so much from all idealized conceptions used to interpret the data. I suggest that these questions would be addressed in further work on this topic and the current ms would be taken as a baseline, relative to which results of further investigations in this area could be evaluated. At this point, I recommend that this paper is published as is.

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