

## ***Interactive comment on “Atmospheric constraints on the methane emissions from the East Siberian Shelf” by A. Berchet et al.***

### **Anonymous Referee #2**

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Anonymous Referee #2 of the paper: “Atmospheric constraints on the methane emissions from the East Siberian Shelf” by Berchet et al, 2015.

This paper is focused on the understanding of the methane budget in the Arctic, and particularly the understanding of the ocean-atmosphere interactions and fluxes from East Siberian Arctic shelf and the impact of this on larger scale. With the increase in CH<sub>4</sub> over the last 10 years, also confirmed by a strong increase from 2013-2014, understanding of the CH<sub>4</sub> budget is essential, and insight into emissions from Arctic sources vulnerable to climate change is of particular importance.

The paper is of significant importance and highly relevant, contributing to reducing uncertainty in the understanding of the CH<sub>4</sub> budget in the Arctic, and importantly, providing a top down flux estimate of CH<sub>4</sub> from East Siberian Arctic shelf (ESAS). ESAS

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is an area where large emissions are reported. This work seeks to constrain the flux of CH<sub>4</sub> from this region providing an upper flux range by combining the observations of CH<sub>4</sub> mixing ratios in the Arctic, with modelling of CH<sub>4</sub>. The top down approach used in the present paper employing emission estimates and forward modelling, utilising the observations in the region to constrain the highest possible emission are a very useful. This complements the bottom up approaches of flux estimates from the region. Furthermore, the top down approach presented indicates that an additional Arctic source (from ESAS or others) is not likely of the strength of 8 Tg CH<sub>4</sub> yr<sup>-1</sup>. No sources of this strength seem to be lacking to describe the observed Arctic CH<sub>4</sub> levels.

The topic is very well suited for publication in ACP, and publication is recommended. Before finalising the work, and to make the paper ready for publication, some revisions, clarifications and central issues need to be addressed. Additionally, it is an advantage if the language, particular in the introduction, is improved.

Generally, the documentation and presentation of the results need to be “one step up” to be ready for publication in a journal like ACP.

Specific revisions that need to be addressed:

1. When employing top down approaches to constrain emissions, the density of sites are important. This is particularly true for inverse modelling methods, but also the forward modelling as used in this work. There are few sites with quality assured CH<sub>4</sub> measurements with high time resolution in the Arctic. Hence, it is surprising that the authors do not include the CH<sub>4</sub> observations from the Zeppelin observatory in the study. In particular since Pallas is not located in the core of the domain influenced by ESAS emissions (Figure 1), and the Zeppelin site experiences influence from ESAS (see, Figure 1), and isotopic data from Zeppelin are used to confirm CH<sub>4</sub> dominating Arctic sources at this sites. Harmonised observations of CH<sub>4</sub> with high time resolution (1 h) is available from this site over the full year investigated (2012). Figure 1 indicates that Zeppelin will experience an influence of ESAS of ca 30-50 ppb (hard to see accu-

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rately from the figure), and the site will contribute to constraining the model simulations and fluxes, hence these data should be addressed to have a stronger fundament for the constrain of the flux estimate.

2. Page 25478 - line 8: "summer 2010" was this the year with isotopic data? According to section "3.1 Summer isotopic observations in the Arctic" this does not seem to be the case. The selection and description of years used throughout the manuscript are confusing, please clarify and check this thoroughly.

3. Page 25478 – line 25: I propose to rephrase the sentence. The expression "... the regional methane budget still has significant uncertainties, depending on the method used for its assessment" can be misinterpreted, indicating that it is the uncertainty that depends on the choice of method, not the estimates. At this stage, I believe that no methods are proven better than others and the uncertainty is rising more from the lack of agreement between the approaches, I assume that was the meaning as well.

4. Page 25478 – line 26: remove "the" before "one side"

5. Page 25479 – line 11: Carbone -> carbon

6. Page 25482- line 3: Please double check the use of years; it is confusing a few places. E.g. What is the relevance of mentioning 2010? The isotopic data is from 2008-2009, the other period is 2012, as it seems. Or?

7. Page 25483-line 20/21 ->: The use of the emission inventories in section 2.3 is adequate and appropriate, and this is essential studying the influence of an additional source. However, if I understand this correctly, emission for 2010 from EDGAR is used, and not 2012. The reason for this (which is obvious, but nevertheless) should be clearly stated, and also the potential impact of this approximation. Furthermore, it is not clear which year the LPJ modelled wetland emissions are used, and also not the GFED (fire emissions). This should also be clearly stated. My interpretation is that all emissions are for 2010, and the metrological conditions and input for 2012. Is this correct? I

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suggest to add a figure showing the monthly variations of the emissions throughout the year + total emissions to make this clearer, also with information of years. This can potentially be in the supplementary. Figure 5 is nice and useful, and this additional Figure suggested can be referred to in the context of this.

8. Page 25386 - line 14: The rationale behind using afternoon averaged mixing ratios is not clear, bearing in mind the lifetime of methane, and also the long distance to local sources with high diurnal variation. Furthermore, is the OH reaction included? This is not clear from the description, please add. In any case, using afternoon mixing ratios is probably not influencing the results, but the set up do not seem tuned to Arctic studies, and this might be addressed in later potential studies.

9. Page 25489 – line 11: "Figure 5 shows..." Should it be Fig. 5 or Figure 5? This is not consistent throughout the text.

10. Figure 4 (Page 25500): It would be interesting to see the difference between the simulations and observations more clearly. I propose an additional figure or table, potentially in supplementary. I suggest to add a figure (with bars?) showing the seasonal or monthly mean of observations, together with model simulations with and without ESAS. Preferably, also with standard deviations included to visualise the simulations and the observations better (or in a table).

11. Related to this: Thank you for Table 1 (Page 25496). I miss more comments to this table and the correlation coefficients, and particularly the low correlation with Tiksi in the reference simulation during summer. Also a comment to the simulation after adding ESAS emissions would be very interesting.

12. Page 25491 – Line 15: I would not recommend the use of commercial cavity ring down spectrometry for isotopic measurements at Arctic sites yet, as the precision is still, unfortunately, too low. I suggest to modify this statement. The sites are normally far from many of the sources and the dilution is high resulting in low variability over the year. Hence, very high precision is necessary.

13. I suggest to use either “methane” or “CH<sub>4</sub>”, but consistently

14. Numbering of Figures in “Supplementary” is wrong. It is confusing referring to figure S7, S8, S9 etc. in the manuscript, as they are nowhere.

15. I think it might be relevant to refer to Shakhova et al, 2015:  
<http://rsta.royalsocietypublishing.org/content/373/2052/20140451>

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25477, 2015.

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