Comments on Berchet et al for ACPD (ms # doi:10.5194/acpd-15-25477-2015)

This manuscript has earlier been rejected from an AGU journal and the current version here submitted to ACPD is only superficially edited relative to the earlier rejected version.

There is a lack of recognition concerning the limitations of their own approach and the huge uncertainty overall of these systems. I remain convinced about the value of atmospheric inversions, not the least as one approach to estimate current methane emission from the East Siberian Arctic Ocean, yet this study fails to provide the type of firmly supported and important insights that would be required for a journal like ACP. The "new" submission to ACPD has now been studied. Below, a number of key considerations are detailed that at present hinders this study from constituting a valuable contribution, yet are meant to support the authors in their coming efforts.

1. The database for the top-down evaluation

The authors keep insisting that there are >20 pan-arctic observatories able to address the current issue of atmospheric methane fluxes from the subsea system on the East Siberian Arctic Shelf (ESAS). The study then limits itself to the big five – Zeppelin, Pallas, Alert, Barrow and Tiksi, yet a closer scrutiny shows that the actual data for the claimed year-round 2012 atmospheric observations is even smaller. They acknowledge that ZEP and PAL are minimally influenced by CH_4 emissions from the ESAS due to their distant locations; and there is apparently no methane data for ZEP-2012 (see fig. 2 and 4). While far from sufficiently acknowledged at key locations (e.g., Fig. 1 and 4 captions), the BRW 2012 record is completely absent for the key summer-fall period, thus severly limiting its value (see e.g., fig. 4).

The interior ms text further acknowledge that "*ESAS methane releases cannot be obtained from TIK alone…*". It must also be noted that the TIK methane program is not yet published anywhere, preventing thorough scrutiny, incl. suitability of local site/meteorology.

Taken together, this leaves us with Alert as the one key site to "nail down" the ESAS methane emissions, located some 2500 km away with complicated and non-ideally constained high-Arctic meteorology. This by itself severely undermines the reliability of the definite conclusions articulated in this submission.

2. On skills of a model

The authors claim to achieve good agreement between their top-down estimates and summer fluxes reported by Shakhova et al., while winter fluxes did not show agreement as they seem over estimated by Shakhova et al. Let's give it a closer look. It is stated that CHIMERE demonstrates a very good skill in winter in representing the atmospheric methane mole fraction variability at high latitude sites (they report correlation r=0.89). However, in winter, contributions of 2 of the 4 simulated sources (terrestrial emissions and fire-inducted emissions) are equal to zero while anthropogenic emissions are still very poorly constrained. The anthropogenic emission inventory presents in a single number the annual emissions from the whole country. How did they distributed these emissions over the Russian north, if there is a single numbers that goes to the entire territory without apportioning either particular territories or attributed to particular seasons? Thus, their model works best (with r=0.89), when two sources do not contribute at all and the most uncertain one contributes "mysterically"? Is this situation called good model skill?

3. On the implications of "higher resolution"

Insisting on "higher resolution" modeling, they actually replace high resolution, in the true meaning of higher accuracy, with high model resolution, in this work only meaning a smaller model grid. There cannot be any high accuracy achieved by using the same data sets as global models use by only reducing the size of the grid – the amount of data on methane remains the same any way! No improved meteorology can help to make up for lack of data on the modeled tracer itself (methane).

4. The "all seats taken" argument: why is disagreement btw modelled and observed CH4 mixing ratios purely "blamed" to ESAS bottom-up estimate as opposed to bottom-up estimates of other sources and boundary conditions implicit in the model?

This is a key aspect of the whole study approach. The ms only pay lip service to the huge uncertainties of both the global and the terrestrial+marine Arctic methane numbers (e.g., Ciais et al., 2013).

Given the massive field observational programs that apparently are underlying the Russian estimates of the bottom-up ESAS methane emissions, in comparison with the database for bottom-up terrestrial wetland emissions in northeast Siberia, may it not be possible that the terrestrial wetland component is overestimated? That component, and all other flux vectors but ESAS, are here taken as "given" and untouchable. This approach is flawed and biased. The same is likely true not only for the Arctic scale but also for the global scale,where limited number of actual observations today serve as the base for the strongly held paradigm that tropical wetland CH4 emissions (where much lower conc is found than over the Eastern Arctic Ocean) is the globally-dominant natural source.

Authors are asked to consider why global atmosphere methane models fail to reproduce observed pole-to-pole gradients in mixing ratios (also relative to that of other gases). Decreasing tropical emissions and increasing Arctic emissions, in the model description of emissions would make this fit better. All of this affects the baseline/boundary condition of the model run in the current submission.

The highest atmospheric mixing ratios of methane are found over the eastern Arctic – how can methane from the tropics (of lower mixng ratios) build up this phenomenon? It goes against basic entropy. Arguments of different mixing heights do not work as then all other passive gases would also be "compressed"; the opposite is observed. Arguments of strong anthropogenic methane import to the eastern Arctic does not gibe with this feature existing also before the Anthropocene. It is time to reconsider basic paradigm/assumptions of methane emissions and their model construct. Until that is done, any “new” sources will just be ill-treated as “overfilling the cup”.

5. The insistence on the notion of large emissions from terrestrial coastal wetlands in northeast Siberia

The pre-conceived notion of sizeable emission from the very shallow and rather dormant surface soil of NE Siberia, competing with ESAS coastal emissions (also terr wetlands, buried beneath a much warmer ocean – a thermal regime more prone to thawing) seems to lack scientific basis. The authors respond that they like to “*reinforce their assertions*” but offer no observational basis for their bias.

As authors ought to be aware of, this Oyagosski Yar region is subjected to intense erosion of coastline and subsea permafrost (these are not what is meant by terrestrial wetlands) – for a start, look at Günther et al (2013 Biogeosciences),Vonk et al (2012 Nature) and Shakhova et al. 2010 (JGR).

Olefeldt et al. (2012) recently collated information to show that the pan-Arctic terrestrial methane emissions from an area of >20 million m2 is covered by 300 sites (i.e., >65 thousand km2 per site). In contrast, I gather from the work reported by Shakhova et al (2010) that their emission estimates from the 2 mill km2 ESAS area is based on at-sea measurements at about 1000 sites, giving 2 thousand km2 per station). Furthermore, the NE Siberian wetlands, particularly deprieved in N. Yakutia with continuous permafrost, covers only <20% of the Siberian wetlands and <10% of the ESAS area, and are covered only by very few actual flux observations.

6. On the transport of methane from ESAS to Alert

Some of the key underpinning assumptions articulated in Locatelli et al (2013) – a co-author of the current ms – is not met and remain to be addressed.

Furthermore, is the model accounting for uptake of methane from the atmospheric boundary layer to the ocean water/snow/ice during its 2000-3000 km long transport (much longer if not transported on a straight line between ESAS and Alert)?

Given the much lower methane concentrations in AO interior surface waters, snow/ice and the very long passage, there is ample time for this fugacity gradient to take its toll on the BL methane levels and deliver a lower mixing ratio once the air parcel finally arrives to Alert. How is this addressed in the employed model?

There are many more questions and aspects that the current state of this manuscript is raising than what is reasonable to put down in this review.

For instance, if existing atmospheric observatories are so well placed to provide the type of clear onstraints and conclusions claimed in this submission, why is there such urgent and prioritized efforts going into establishing major new atmospheric observatories around the ESAS, such as at Ambarchik/Kolyma mouth (MPI) and Bolshevik Island of Severnaya Zemlya (AARI and FMI)? These ESAS bordering observatories will be really well placed to address methane emissions from the ocean in the near future.

Why is the western part of Laptev Sea not marked as part of the ESAS source area in Fig 1, and the source area called Laptev Sea instead of ESAS (incl the vast East Siberian Sea) at many locations? What source area is really considered?

No matter if today's methane emissions from the ESAS are 4, 8 16, or 32 Tg/yr, that is still a minor portion of global methane emissions into an uncertain global methane budget. This is very likely hard to rule out given the large uncertainties of almost all natural methane sources (Ciais et al., 2013), if not taking the rest of them for granted and instead subjecting all sources to equal scrutiny.

Any study of this sort ought to recognize that the bigger question is how this system, hosting many 100s Pg C-CH4 in potentially vulnerable resevoirs may develop in the coming decades-centuries. Top-down atmos observations of mixing ratios will be able to record such changing trajectories but deeper geophysical/geochemical investigations will be needed to provide the system understanding to make such predictions. This is a useful context and the current approach is one piece to our common puzzle. The present submission is not a suitable contribution but the general approach will be a useful window in the future.