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Earth System Science Data Discussions

Interactive comment on "Gridded maps of geological methane emissions and their isotopic signature" *by* Giuseppe Etiope et al.

Anonymous Referee #3

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Review of the paper by G. Etiope et al. entitled "Gridded maps of geological methane emissions and their isotopic signature"

General comments :

This paper proposes the first gridded map of (natural) geological methane emissions for four categories, together with an estimate of their isotopic signatures, and ratios methane/ethane and methane/propane. These products are of primary importance to help closing the global methane budget, and especially to be used as a prior description in atmospheric inversions. A very large amount of work has been done to produce this map, based on the long experience and recognized expertise of the first author and there is no doubt that these emissions gridded description will be widely used.

Balancing a bit this large interest for paper's products is the content of this discussion paper. The structure of the paper is satisfying but I have several general concerns and many specific ones listed below:

1/ Abstract and introduction needs attention (see specific comments)

2/ You quote 20 self publications (Etiope or Etiope et al). It seems a bit too much regarding the total number of references and I recommend to keep only the main ones. Also, some recent relevant references are missing such as Petrenko 2017 (downward revision of geological source of methane), and Thornton 2017 (downward revision of ESAS methane emissions by a factor of about 8), ... Please limit self-citation and quote more of the recent literature. I strongly suggest also to include in section 8.1 a short discussion about these recent papers and the implication for your work : you downward estimate of 37 Tg/yr is smaller than the previous 50 Tg/yr, but still well above the Petrenko suggested value of 15 Tg/yr.

3/ The methodology section needs attention. It has to explain more in detail how the actual flux measurements or the statistical approaches are used to build the flux estimation or to point more precisely to sections into the supplementary.

4/ You have to explain more clearly at the beginning that some part may be missing in the gridded map and that it means a possible underestimation of global emissions. I am not convinced by the extrapolation made by the authors to complement the gridded estimate as it mostly rely on very rough estimates of the missing part (some additional areas emitting might be there and there, Arbitrary 50% flux, ...). In this sense column 3 of table 3 is a bit strange to me as roughly estimated whereas you spend a lot of time and energy to properly provide gridded estimates of column 2. This extrapolation has to be presented much more carefully and not put at the same level than the gridded estimate. Also the sentence about some of the emission not being an update/improvement, mentioned several in the paper, is a bit strange to me and should be rephrased. In fine, I would just indicate in the conclusion that the gridded product

will/may be revised regularly, upward or downward, when more data become available

5/ An uncertainty estimate has to be given for emissions of all categories (and reported in table 5), as for MS and isotopic signatures. This is critical for consistency of the paper and usage in atmospheric inversion. Although it might not be easy, the authors are the best choice we have to make such estimates, which else will be made by inverse modellers who probably know much less on the specific topic.

6/ All along the text & tables : please harmonize the number of significant digits in the numbers provided. Considering the uncertainties I am not sure that 3.87 Tg/yr is relevant for instance for OS and I suggest to at least use 3.9 Tg/yr or possibly 4 Tg/yr. No more than 1 digit after the comma in any case.

The paper will definitely be a major (evolving) piece to improve the global methane budget and should be improved after these general comments and the specific ones below are addressed.

Specific comments :

Abstract : "representativeness for many sources" suggested : and their isotopic signatures

Abstract : "This gap is particularly wide for geological CH4 seepage, i.e., the natural degassing of hydrocarbons from the Earth's crust. While geological seepage is widely considered the second most important natural CH4 source after wetlands, it has been mostly neglected in top-down CH4 budget studies, partly given the lack of detailed a priori gridded emission maps". This sentence is polemical and should be removed from the abstract which should reflect the work done. Considering the estimates of the CH4 emissions from geological seepage in the literature and in this paper, and the uncertain estimates from inland water systems, it is difficulet to say robustly that geological source is the 2nd. I would say a major source. And the lack of interest is true for past budgets but recent ones (e.g. Saunois et al., 2016) account for this source.

P2 l5 : I suggest to update the ref to Saunois et al., 2016 and 558 MtCH4/yr

P2 l7 : "emission inventories" and process-based models

P2 l8-9 : TD and BU show strong disagreement only or natural sources, please precise.

P2 l 9-12. The sentence has several problems. Schwietzke et al 2016 is not 3D inverse modelling but box modelling. The improvement brought by recent 3D modelling is arguable the recent study mentioned actually enlarge the range of emission estimates and needs to be further reproduced to pretend to get closer to the truth than other studies. I would rephrase to point that the usages of updated inventories of isotopic signatures has brought new constraints for the global methane budget. In any case, please rephrase.

P2 l28 : geological degassing is today recognised as the second most important natural CH4 source after wetlands : see remark from the abstract. Also, the recent Petrenko paper should be quoted here (and commented later in the paper) as it proposes a downsizing of geological emissions to 15 Mt/yr at maximum.

P2 l 29-30 : it is a bit unfair to quote specific papers when the highly visible synthesis from IPCC or GCP mention geological emissions in thei budget (e.g. Saunois et al., 2016). Please rephrase.

P3 l34 – figure 1 : References to other sources should be updated to the Saunois et al budget (GCP 2nd budget) instead of Kirschke et al. (GCP 1st budget). Please precise that figure 1 reflects literature and not the results of this paper.

P5 l12-13 : what does it mean ? How can you know there is a seep of you cannot locate it ? Please precise and rephrase.

P5 l 22 : Why not documented ? please provide a reason.

Section 4.1 : How can you be sure that oil&gas seeps are not double counted in anthropogenic inventories as possibly located close to fossil fuel exploitation facilities ? It

is important to mention this somewhere in the paper and possibly discuss it as double counting is one clue to explain why bottom-up and top-down studies are not consistent for natural methane emissions.

P5 l 30 : “few tens” : can’t you be more precise ? it is important to have a more precise idea of the fraction compared to the total number.

P5 l30-39 : the methodology should be a bit more detailed here (the supplementary does not bring much more on this). How did you use the direct measurements to calibrate ? How did you attribute a measurement to a type of seeps ? how many types did you use ?

P5 l 38 : how do you account for miniseepages ? please provide ref or explanation.

Section 4.2.3 big emitters. What fraction of these big emitters has been directly obvserved ? It would be important to mention as they are not so numerous and a strategy to refine the estimate would be to measure them all (if not done yet). Please precise here.

Section 4.5.1 : This section needs attention l15-16, if you do not do this work to update or improve estimates, why doing it so ? I am pushing a bit what you write but please rephrase. L28-29 : where does the 30% and 50% come from ? The 50% looks like a bit arbitrary ? is this 100% error reflected in column 3 of table3, moving from 3.8 to 8.1 Tg/yr for OS ? It is not clear to me why producing a gridded map if it cannot be used directly for global scale and needs re-assessment of emissions. Please clarify this section and the meaning of column 3 of table 3.

Section 4.6 : It is strange to me that you do not provide an uncertainty attached to emissions and signature in this section as in 6.6 for MS. “Order of magnitude” means a factor of 10 uncertainty. Does it means that OS emissions range from 0 to 38 Tg/yr ? Please be more precise in this section of possible or explain whay you cannot provide a range or a sigma for uncertainties.

P9 l 27: there is no section 5.5.1.

Section 5.5 : The total of 20Tg/yr has been highly controversial in the past years and recent papers related to ESAS largely reduced emission estimates (Berchet 2016, Thornton 2017). I would not present this number as a target to reach in the text. Lines 15 to 20 are highly arbitrary and should be identified as so. Why 5 to 10 Tg/yr ? These extrapolations should be taken with caution to me and mentioned as so. Again do these estimate refer to column 3 of table 3 (5-12 Tg/yr, where text mentions 7-12) ?

Section 8.1: This section has to be enriched to reflect a more complete spectrum of estimates than the ones provided by the co-author of this paper. At least the estimate from the recent Petrenko 2017 paper is important because it lowers to at maximum 15 MT/yr the total global value of geological emissions. Also, the 14C constraint on total 14C free methane from Lassey 2007 could be quoted. These elements should be quoted and discussed briefly in this section.

Section 5.6 : same remark as for OS : can you provide an uncertainty number for emissions (sigma or range) as in 6.6 for MS ? Section 6 : Even more critical than with OS emissions, the possible double counting with anthropogenic emissions should be addressed. How can we be sure that this diffuse source is not part of the oil&gas estimates of inventories ? OS are precisely located so the risk may be smaller than for diffuse MS. But for diffuse sources in the middle of oil&gas fields it seems more tricky. Please at least mention/discuss this in the text as a cause of uncertainty in section 6.6

P15 l29 : what is the impact of the 4 km choice on the emission estimate ?

P15 l 37 : “few cases” : please provide a more precise number if possible.

P16 l 7 Again this sentence is unclear to me. Please rephrase

P16 l25 : “It is known . . .” : any reference to justify this ? Any explanation ? please provide a reference or explanation

Section 7.6 : as for other categories please provide a number (sigma/range) for the

uncertainty on GM emissions as in 6.6

P17 l34 : again please clarify this sentence.

Table 5 : As already mentioned, please provide an uncertainty estimate for emissions from OS, SS and GM and fill it in table 5, column 3.

P18 l27 : is there a risk that some SH emissions are forgotten because of les knowledge of the terrain ?

P19 l4-6 : The 20 Tg/y value previously widely used for SS has been revised downward by several studies at least because of ESAS region (Berchet 2016, Thornton 2017). As already noticed, one should stop giving the idea that this value is kind of a target to reach, as suggested here and in the corresponding paragraph of the text (see previous comment). The reference given here (Kvenvolden et al. 2001) seems a bit old regarding the past years activity on these emissions. Can the author provide a more recent reference and rephrase according to this remark ?

P19 l31-33 : if no description of geological is given in an inverse modelling exercise, all the flux is spread on other distribution, possibly for onshore emissions, but with no guaranty, on the anthropogenic fossil emissions. So the term low bias should be rephrased (while the high bias is possibly correct for anthropogenic fossil). Please rephrase.

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