

Interactive comment on “The consolidated European synthesis of CH₄ and N₂O emissions for EU27 and UK: 1990–2018” by Ana Maria Roxana Petrescu et al.

Anonymous Referee #2

Received and published: 10 February 2021

The authors provide the most comprehensive assessment of CH₄ and N₂O emissions for EU27 and UK during 1990–2008 using top down (emission inventory, ecosystem modeling) and bottom up (inverse modeling) approaches. The results from the synthesis with uncertainties are then compared with European NGHGI data in 2019. This is a strong paper with valuable information to the scientific community and certainly the one that resonates with the readers of ESSD. I also commend the authors approach to use dozens of datasets to provide the most comprehensive assessment of these fluxes (its never easy to do that). The manuscript is well written (although the text can be trimmed significantly), logically organized and clearly presented. Below are my comments which probably would be helpful for improving the manuscript.

1. The results are presented as individual CH₄ and N₂O fluxes (figures 1-9). It would be great to have these figures together particularly for both gases as it will increase the readability especially when comparing the CH₄ and N₂O fluxes for each region. Currently, its hard to look at these figures of CH₄ and N₂O fluxes separately and derive any conclusion or recommendation on the dominant control of CH₄ on N₂O fluxes for different regions. 2. I am left wondering about why only CH₄ and N₂O emissions are included in this study. Since the study uses ground based observation, ecosystem modeling and inverse modeling, there should also be availability of data related to CO₂ fluxes and that would provide a big picture of the net GHG for EU27 and UK. Adding CO₂ into the current CH₄ and N₂O fluxes would be valuable not only to identify regions that are GHG sources/sinks but also to carry out large scale mitigation effort depending on the dominant control of individual GHG at the given location. 3. Datasets: Since this is a data paper, I only see the aggregated data provided in Fig 1-9. I strongly suggest the authors to provide these data at pixel level so that it can be meaningful and useful to other colleagues working on CH₄ and N₂O fluxes for the UK and EU27 region. I am also wondering whether appropriate approach has been made to use data from all the papers that the authors have cited and whether there has been an agreement on making the data open source through this paper. 4. I have also seen unexpected citation approach (for example: line 160-165). The authors cite Yuanzhi Yao as personal communication for 66% of the N₂O emitted from rivers are considered anthropogenic. This needs an appropriate citation with 66% of what, and how what is the contribution of the rivers toward total N₂O fluxes (I do believe it should be relatively small compared to fluxes from agricultural lands). 5. Uncertainty: I am still not convinced about how the uncertainty was assessed since the data came from different sources. For example, if the authors are using ecosystem models, is it appropriate to use the standard deviation to determine uncertainty in N₂O and CH₄ fluxes. I do believe that model uncertainty comes from parameter use, the model structure and uncertainty in input datasets. However, the authors have not tried to address this issue in the manuscript. 6. Tables: While there are many details on the datasets used to estimate N₂O and

[Printer-friendly version](#)[Discussion paper](#)

CH₄, there is no any tables that shows the emissions from different sector when these datasets are compiled together. I strongly suggest the authors to provide the top-down and bottom up N₂O and CH₄ fluxes in one table with different sources (agriculture, natural vegetation, wetlands etc). 7. Table 1,2 and 3 all can go in supplementary material. These tables are just taking too much space in the manuscript and given that Table 3 is adopted from some other paper, I do not think it should be in the main content. 8. Seasonal flux estimates: Currently, the manuscript estimates CH₄ and N₂O fluxes at annual time scale and completely ignore the fact that understanding seasonal dynamics of these fluxes are important and useful for climate mitigation efforts. At least, there should be an acknowledgement on why seasonal fluxes were not estimated.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-367>, 2020.

[Printer-friendly version](#)[Discussion paper](#)