Review of Monteil et al.: The regional EUROpean atmospheric transport inversion COMparison, EUROCOM: first results on European wide terrestrial carbon fluxes for the period 2006-2015

General comments: the authors presented inverse biogenic fluxes estimates in Europe for over ten years (2006-2015) using the regional inversion technique as opposed to global inversions. These flux estimates were done under the protocol of using the same fossil fuel emission and a common database of in-situ CO2 observations, while the transport models, inversion approaches, the choices of observation and prior biogenic fluxes differed. The authors concluded that at the continental scale, the European ecosystems are a relatively small sink (-0.21±0.2 PgC/year), consistent with the results demonstrated by the global inversions in the previous studies. This conclusion is quite out of my expectation and quite different than my experience. I have a few comments below that may potentially change the conclusions and maybe improve the results.

The manuscript is generally well written. The analysis is well presented. The authors provided a reasonable interpretation of their results and are definitely aware of the caveats of the study.

However, after reviewing the submission guidelines of ACP and GMD. I found this MS is a better fit for a GMD publication. I am listing what I found below for the authors' reference.

> ACP research articles:
> "…Research articles must include substantial advances and general implications for the scientific understanding of atmospheric chemistry and physics. Manuscripts that report substantial new measurement results, but where the implications for atmospheric chemistry and physics are less developed, may be considered for publication as measurement reports (see below)…."
>
> GMD aims and scope:
> "…model experiment descriptions, including experimental details and project protocols;…"

This work has collected the inversion results from different groups without a well-designed protocol. It is very challenging to fully understand the cause of the spread of the ensemble and the underlying uncertainties, which limits the scientific advances of this work. The results overall match the previous studies which were drawn from the global inversions. There are a few technical aspects that could be improved, and I will list them as follows. I have quite a lot of faith that the author should deliver better inverse estimates if those technical improvements are implemented.

Given that it is an important paper for the EUROCOM project and can potentially provide some insight for future experiments, it is worth a publication. However, it's not clear to me how the authors handle boundary conditions and the related uncertainty in the inversions. The authors should have better clarifications on this aspect before re-submission.

For an ACP publication, however, I would recommend the following improvements to start with.

The authors built the results on top of a set of ensemble inversion results. The only protocol, for now, is to the same fossil fuel emission and a common database of in-situ CO2 observations. To understand what caused the large spread of the ensemble, fixing at least one of the model components is required. That's said, the authors should at least have one set of results using the same transport model, same prior fluxes, same boundary conditions, or the same observations. without the common setup, it limits the depth of the scientific understanding of this work.

The regional inverse results in the MS do not appear to have better constraints than the global inversions. Technically, regional inversions can be driven by mesoscale transport. All of the experiments in the MS were driven with reanalysis or forecast data at ~101 to 102 km, and most of the meteorological forcing data do not have TKE. I suspect that these limitations are the main reasons that lead to unexpected equivalent results to the global inversions. I strongly recommend the working groups to use the mesoscale model output as the meteorological forcing for future experiments.

As I mentioned before, the boundary conditions need to be stated in more detail.

The authors mentioned that the locations of some observations are very challenging for transport models in section 4.1. It may be a good idea to remove those challenging sites before inversions to avoid large transport errors. A detailed model-data mismatch would be more appreciated and help to improve the inverse flux estimates.

<u>Specific comments:</u>

1. Line 25, spell "NBP" out.
2. Line 187, remove of "full"
3. Figure 2, do the authors know why VPRM looks so different than others?
4. Table 2, the author can one row of the choice of observation for each group.
5. Line 485, I would be not surprised by the smaller error reduction of the annual results than that of the monthly results due to the annual net NEE is close to zero.
6. Line 704, change km2 to km^2