

The paper has been thoroughly restructured and made much clearer. As previously stated, it will be a very useful publication for the atmospheric inverse modeling field. I suggest a few corrections:

General remarks:

- Please have the native english-speaking co-authors review the language style.
- Take care of formatting in the tables, e.g. capitalizations and indenting.

I also have the following specific remarks:

- line 19: "though often limited for bottom-up anthropogenic CO₂ emission" not clear for reader. Better say that it is not often known or available.
- line 30: "sensitivity studies", experiments better than studies.
- Abstract: the main result of sensitivity experiments 1 and 2 should also be included.
- lines 41, 42, repeats too much the phrase "for example" (could be omitted in some cases).
- line 43: "All measurements are assimilated by global tracer transport models to infer atmospheric CO₂ changes, or by flux inversion systems to estimate the large-scale surface CO₂ fluxes. " Is not correct because of the following reasons:
 1. The atmospheric transport models do not assimilate the measurements, the inversion systems assimilates the measurements (model mole fractions from transport models can be compared to observations for manual analysis)
 2. Not all measurements can be assimilated by models, it depends on the model. Some models are not able to represent certain measurements accurately because they are too coarse.
 3. It is not just global model but regional models as well.
- Line 48: The global transport models require an initial best estimate of the CO₂ emission fields with uncertainties, the so-called prior information. This Is not accurate. It is not the transport model, but the inversion that requires a prior to stabilize the calculation. Using the initial best estimate is an approach. There are other approaches such as using a yearly average, mask of emission regions, a linear model, etc.
- In page 2, there is too much use of "bottom-up", sometimes "emission inventories" would be enough
- Section 2.1 is better in the introduction except for parts of the last paragraph.
- Section 2.2.1: Not very clear what UC and AD stand for, you may consider want to consider σ as a standard variable for uncertainty
- Section 2.2.2:
 - One more reason why sectors are merged is that some sectors have very low emissions, which are not distinguishable from a global or large regional modeling perspective.
 - It is not clear why activity and emission factor uncertainties are not log-normal themselves

- Table 3 could be included in supplemental information and further deisaggregated into different tables to make it more readable.
- Section 3.4 is more a result
- A transposed presentation of tables 4a- could make the comparison between the countries easier, as well as having the curves in the same plot in figure 2.
- Table 5 highlight *this study* so we can know the relevance of this study just by looking at the table
- Table 6 could be replaced by map in which the countries are color coded according to type.
- Section 4.2: figure 5 because we also talk of atmospheric inversions, it might not be wise to use of "inverse" type because it can lead to confusion. Maybe with instead "inverse" type use "switched"
- Section 4.3 why such an arbitrary boosting factor and not simply uncertainty propagation?

$$\sigma_{monthly} = \sqrt{\left(\frac{\sigma_{yearly}}{E_{yearly}}\right)^2 + \left(\frac{\sigma_{monthly_factor}}{monthly_factor}\right)^2} * E_{monthly} \quad (1)$$

When aggregating the monthly emissions to yearly, the aggregated emissions should have the same uncertainty.

- Tables 9 and S8 could be clearer as 3 maps:
 1. EDGAR-JRC
 2. CHE_EDGAR-ECMWF
 3. Difference between both