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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential of European ^{14}CO$_2$ observation network to estimate the fossil fuel CO$_2$ emissions via atmospheric inversions" *by* Yilong Wang et al.

Anonymous Referee #2

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The authors present an OSSE study of the capability of ICOS $^{14}CO_2$ observations to constrain European fossil fuel CO_2 fluxes and their trends. The study is well structured and should be published. I have a few comments which I'd like the authors to address before publication.

Major comments

1. **Line 112:** The assumption that $^{14}CO_2$ measurements can be accurately translated into FF CO_2, i.e., there are no spatial patterns introduced due to the other

terms (especially disequilibrium and nuclear plants in the European context), is a big one. Those terms will not only affect annual emission estimates, but also the ability to detect trends, as countries change their nuclear power generation capacity and switch to wood-fired domestic heating (e.g., Germany). I understand that modeling the full $^{14}CO_2$ budget is beyond the scope of the authors' framework, but it should be possible to estimate the impact, by e.g. modelling just the nuclear or disequilibrium contribution as a tracer in a transport model and looking at the change in $\Delta^{14}C$. Have the authors done that? Unless that concern is addressed, the actual numbers from the manuscript are hard to trust.

2. **Line 145:** The authors say that the inversion interpret the gradient between JFJ and other sites. I do not understand how that is implemented. Is it that JFJ is the only background site in the network, and hence the inversion implicitly interprets gradients w.r.t. JFJ (much as a global CO_2 inversion might interpret everything w.r.t. MLO and SPO)? Or is it that the pseudo-obs are fed in after explicitly subtracting the JFJ time series, in which case the model's observation operator looks like "site – JFJ" at each individual site? Basically, the authors say in words that they interpret the gradient w.r.t. JFJ, but I do not understand how that is implemented in practice.

3. **Line 201:** I'm having trouble deciphering the meaning of "mismatch reduction", and its bounds and limits. Instead of describing it in words after equation (4), could the authors please write down the mathematical expressions for ε_a and ε_b? Since I did not know what those ε's were, I also could not interpret maps of MR (e.g., Figure 4). In particular, I did not understand what negative vs positive MR meant.

Minor comments

1. **Line 151:** For NET233, each grid box is supposed to have one urban and one rural site. I'm not sure that's a good strategy. Wouldn't it be better to designate urban/rural depending on the nearest NET43 site? I mean, there could easily be grid boxes where it was more realistic to put two rural or two urban sites.
2. **Line 233:** In the ICOS protocol, are the two-week samples going to be filled continuously, or are they only going to integrate mid-afternoon (or nighttime) air? That would very much change the sensitivity of the observations to FF CO_2, and the impact of transport errors.
3. **Line 237:** Are the authors assuming that two week average $\Delta^{14}CO_2$ will translate into two week average FF CO_2? What the two week average $\Delta^{14}CO_2$ represents depends on the method of collection; an open tray will fix CO_2 proportional to the partial pressure of CO_2, while a bubbled trap will fix all the CO_2 in the ingested air. The former represents average FF CO_2 weighted by the total CO_2 mole fraction, while the latter represents average FF CO_2 over two weeks. Which one applies for the ICOS protocol?
4. **Line 253:** Did the authors model a diurnal cycle in FF CO_2 emissions? According to Nassar et al. (2013), the diurnal cycle can be fairly large over populated areas. Along with the selective mid-afternoon sampling used by the authors, the impact could be sizeable.
5. **Line 264:** Does "practical" refer to the operator used to generate pseudo-observations from the "true" fluxes?
6. **Line 296:** Is the covariance model global, or is this done only over Europe?

7. **Line 363:** Can the authors explain how they obtained the IER hourly inventory? I tried to download it from their website, but given that each month had to be separately downloaded, that was very inconvenient. An email to the contact person listed on the website bounced, so that was a dead end too.
8. **Line 366:** This will only get at the random error in transport, not any systematic error in transport modeling. Have the authors tried quantifying the impact of systematic errors in LMDZ, say by using ^{222}Rn or SF_6?
9. **Line 367:** I know it is usual practice in the OSSE world to perturb the measurements according to the error statistics of R, but I have never understood why, unless it is done in an ensemble of multiple realizations of the measurements. In an ensemble of inversions with different measurements from the same network, it makes perfect sense to produce those measurements using perturbations according to R, since the resulting spread in the flux estimates then gives the uncertainty due to R. However, for a single inversion, perturbing the measurements according to R only ensures that the posterior will be different from the "true" flux, without any way to infer the significance of that difference. As in, how do the authors know that the MR's they estimate are not because in the one realization of the measurements they used, some of them just happened to be skewed in one direction? This is especially a concern for the NET17 network, since there are so few measurements, with scant opportunity to average over the perturbations.
10. **Line 374 and Figure 4:** The authors solve for monthly emissions over a year, but report a single UR/MR map of monthly emissions. Is this the RMS of UR/MR values over 12 months, or the UR/MR calculated from the RMS of the posterior errors, or...? As in, can the authors give a mathematical expression of what is being shown in Figure 4 as the "monthly" UR/MR, in terms of their control vector and/or covariance matrix?
11. **Line 381:** I think the reference to Figure 4(d) should actually be to 4(e). Likewise,

in line 384, the refences should be to 4(e) and 4(g).

12. **Line 409:** "... the posterior misfits are even larger than the prior misfits." Why does the inversion allow this? For stations within the blue regions, is this obvious from looking at the atmospheric FF CO_2 time series, that post-optimization the time series is further away from the pseudo-data than pre-optimization? I suspect the perturbed measurements are to blame (see earlier comment).
13. **Line 417:** The correlation is not between uncertainties, but between corrections from the prior emission.
14. **Line 455:** I'm surprised at the low UR for the NET233 network. Why are there so many white areas (low UR) still?
15. **Line 484:** In real trend detection situations, the transport will vary year by year, as will the disequilibrium and nuclear fluxes of ^{14}C. Can the authors estimate how big an impact this will have on the trend detection?
16. **Line 499:** As far as I can tell, Basu et al. (2016) did not estimate UR.
17. **Line 531:** The authors seem to suggest that the boundary condition – a bane of most regional inversions – does not affect their flux and uncertainty estimates. Is this because everything is referenced to JFJ?
18. **Eqs. C-3 and C-4:** I believe there are errors in these two formulae. If $\vec{p}$ is obtained by minimizing the cost function J

$$J = \frac{1}{2}\left(\vec{y} - \tilde{\vec{y}}\right)^T R^{-1}\left(\vec{y} - \tilde{\vec{y}}\right) \tag{1}$$

where $R = cov(\vec{y})$ (R in their case contains the posterior error estimates on fluxes), then the optimal estimate of $\vec{p}$ and the corresponding covariance are

$$\vec{p} = \left(X^T R^{-1} X\right)^{-1} X^T R^{-1} \vec{y}$$
$$cov(\vec{p}) = \left(X^T R^{-1} X\right)^{-1} X^T R^{-1} X \left(X^T R^{-1} X\right)^{-1}$$

19. **Table 2:** In columns 2 and 3, I believe rows 3 and 4 have been flipped.
20. **Figure 2(a):** Am I supposed to see 56 colors in the world map? I don't. I think the problem is that the country and state boundaries overlap with the region boundaries. I would suggest, at least in the world map, only showing the region boundaries from the control vector and eliminating the country and state boundaries.

References

Basu, S., Miller, J. B., and Lehman, S.: Separation of biospheric and fossil fuel fluxes of CO_2 by atmospheric inversion of CO_2 and $^{14}CO_2$ measurements: Observation System Simulations, Atmos. Chem. Phys., 16, 5665–5683, doi:10.5194/acp-16-5665-2016, http://www.atmos-chem-phys.net/16/5665/2016/, 2016.

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