Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-48

Brad Weir (Referee)

Referee comment on "Complementing XCO_2 imagery with ground-based CO_2 and $^{14}CO_2$ measurements to monitor CO_2 emissions from fossil fuels on a regional to local scale" by Elise Potier et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-48-RC1, 2022

In "Complementing XCO2 imagery with ground-based CO2 and 14CO2 measurements to monitor CO2 emissions from fossil fuels on a regional to local scale", Potier et al. investigate different approaches to estimating gridded fossil fuel emissions from different types of in situ and satellite observational data. These tests are often referred to as Observing System Simulation Experiments (OSSEs), although even the terminology is a subject of debate. The contents of the paper are clearly relevant to the 2015 Paris Climate Accord, the observational and modeling capabilities needed to support it, and the subject matter and scope of Atmospheric Measurement Techniques. OSSEs are often criticized for their lack of representativeness of real data. For example, this manuscript assumes all biases and spatial correlations are zero, while observational and transport biases (Schuh, Peiro, etc.) are arguably the greatest barrier to improvements in both surface flux estimates and the science they can enable. Nevertheless, the goals of the Paris agreement are rapidly approaching, and systems must develop even more rapidly to meet those needs. This paper is a necessary first step in that direction that could be strengthened in several ways.

Major issues:
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1) Woodbury matrix identity: Equation 1 and Lines 522-524. This is an unfortunate oversight that has persisted in the geosciences literature despite being a basic result from linear algebra. If you "push the inverse through", you get the more appropriate equation A = B + HRH^T, which not only is simpler, but you can compute! This is a fundamental result from data assimilation and is a little unnerving to see in a paper about data assimilation. It also makes the statement on Lines 522-524 false, which is a good thing, because you can in fact estimate uncertainties in much higher dimensions than considered in this paper.

2) Assumptions about uncertainties: Line 114, Section 2.4.2 and 4.1. The assumptions of zero bias and zero spatial correlation in the experiments are unrealistic, but perhaps necessary to do any meaningful analysis. Almost any assumptions made about uncertainties could be criticized, so it's hard to pinpoint exactly what matters and what doesn't. Maybe that's the whole point. I do think you could make an argument that retrieval and transport biases (Crowell, Peiro, Schuh refs below), combined with our lack of understanding of appropriate spatiotemporal correlations (my own speculation) are the major roadblocks for further development in the field, which leads me to have some serious reservations about the claims in Section 4.1 and to wonder if they're even supported by the paper's analysis. Fortunately, I think almost all of this argument is tangential to the goals and actual results of the paper. Section 4.2 alone is enough to motivate the work. All OSSEs are flawed, but they will be needed to develop our monitoring capabilities as done here.

3) Plumes? I was expecting to see a figure of the things you're actually observing here: XCO2, in situ surface CO2 and 14CO2, perhaps even showing a plume? It would be nice to see those from the model and data somewhere. Maybe right after Figure 1?

4) Data availability. This paper lacks a data availability section that I thought AMT required. Hopefully this was an oversight, but I cannot see how it would be appropriate to publish this paper without clear links to the surface flux priors, simulated XCO2 retrievals, and other input data used in this study.

5) Linearity assumption: Line 144. I do not understand this. Are you using a linear transport operator or are you just using the linearization to compute uncertainties? The latter is common, while I have a hard time understanding how the former would be justified, especially during the day in the Summer. Can you please explain/clarify?

Minor issues:

Our COVID-19 paper (https://doi.org/10.1126/sciadv.abf9415) seems relevant to this work. It would be nice to address that in this paper, but not necessary.

Bulky notation: Coming from a math background I find multi-character sub and super scripts unnerving, but realize it is more common in the geosciences literature. Still, I think many of the equations could be clarified by removing the excessively verbose sub/super-scripting. For example: H_s, H_t, and H_d instead of H_{sample}, H_{transp}, H_{distr}; C, CO_2, or [CO_2] instead of C_{a,CO2}, F^{14}_N instead of F^{14}_{Nucl}, etc. This would be particularly helpful in Equation 9 that has something like 33 characters in subscripts and 7 on the baseline, making it particularly difficult to parse.

Line 138. Who is "they"?

Line 141. 300 hPa seems very low. The paper cites the results of Santaren et al. (2021) as saying that uncertainty in boundary conditions have a negligible impact. It might be helpful to still say where those boundary conditions came from (maybe I missed this) and why Santaren et al. concluded that their influence did not have a strong impact on the inferred fluxes.

Line 188. I'm assuming the daily partition coefficients are used just to apply a diurnal cycle, but I'm not sure. Could you be more explicit?

Line 199. "contains" I think this is maybe a typo.

Signs. Are the signs of the delta values and Equations 3 and 4 consistent? I think if you're using NPP instead of NPE a positive sign would imply a flux from the atmosphere to the land, but that would make the signs in Equation 3 incorrect. Can you please address this.

Section 4.1. I find much of this highly speculative and unrelated to the results in the paper.

Line 511: "...". Is this a typo?

Lines 540-541: "it hardly provides information on plants, cities and regions outside its FOV". Again, I find this speculative and not necessarily shown in the paper, or necessary. CO2 data can potentially have an impact on fluxes upwind of its observation. Please either support or remove.

Line 548: "precision". And accuracy too?

Lines 560-567: This seems to be the strongest part of the paper, but I'm not sure the abstract gives the same impression.

Line 569: "Tn". Definitely a typo.

References

Crowell: https://doi.org/10.5194/acp-19-9797-2019
Schuh: https://doi.org/10.1029/2018GB006086
Peiro: https://doi.org/10.5194/acp-22-1097-2022