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Interactive comment on "Characterizing Uncertainties in Atmospheric Inversions of Fossil Fuel CO_2 Emissions in California" *by* Kieran Brophy et al.

S. Basu (Referee)

sourish.basu@noaa.gov

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The authors have presented an estimation of errors in fossil fuel CO_2 ($ffCO_2$) flux estimates stemming from errors in prescribing the prior $ffCO_2$ flux and errors of imperfect atmospheric transport. In my evaluation, this is a very through and well-structured exploration of the topic, and of high relevance given the focus of regional entities such as the California Air Resources Board (CARB) to quantify regional emissions. I have a few questions and suggestions, and once those are addressed I recommend publication of this work in Atmospheric Chemistry and Physics.

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Discussion paper

1. Line 58, "INFLUX ref" is missing.

2. Lines 64-65, the phrasing "simulated and observed measurements" sounds awkward to me. I understand the authors phrased it this way because the Fischer et al (2017) study is an OSSE study. I suggest rephrasing this as "Recent studies with both real atmospheric measurements of $\Delta^{14}CO_2$ and $\Delta^{14}CO_2$ simulated in observing system simulation experiments (OSSEs) at a network of sites have shown that atmospheric $\Delta^{14}CO_2$ can be used to estimate monthly mean Californian ffCO_2 emissions with posterior uncertainties of 5-8%", or something along these lines.

3. Lines 72-73, I would omit the qualifier "broadly consistent", since the range (-43% to +133%) is rather large. I would suggest just stating the result of Turnbull et al (2011) as "were found to be within +X/-Y% of Vulcan".

4. The β of equation (1) is not discussed in the text other than to say that it includes the influence of other terms like the biospheric disequilibrium flux. Is it assumed that β is perfectly known? If so, that is fine, but that should be explicitly stated. Or, one could also given an estimate of β and say why it is unlikely to be a big factor for the estimates derived in the paper.

5. Likewise, Δ_{bg} is not discussed after equation (1). I notice that the authors estimate a total emission outside of California in their inversion. Is this equivalent to estimating Δ_{bg}?

6. Lines 143-144, the four inventories mentioned cover different time periods. Are they normalized to the same California total before calculating the spread? If yes, then what determines the prior uncertainty of the California total ffCO_2? If no, then isn't the spread artificially large because the inventories span different years?

7. Lines 146-147, was the standard deviation calculated across the four inventories, or was the spread (max to min) across four inventories assumed to be the 1-σ uncertainty?

8. Lines 184 and 205, the footprints are aggregated over six days, beyond day 1. Does this mean that the flux adjustments, beyond the first 24 hours, are all coherent across six days? Is that realistic? I'm curious why this was done, since I would assume the transport model would be able to distinguish between signals coming from flux 2 days ago vs 6 days ago (say).

9. Lines 272-273, saying "where there are regional errors in the magnitude of prior emissions" is not quite exact, I think. I suggest rephrasing this as "... in the simplest case where the only errors in prior regional flux estimates are biases in their magnitudes".

10. Lines 287-288, it's common practice in OSSE studies to use the more realistic scenario as the truth (nature run) and the simpler scenario as the prior. However, here the authors use annually averaged Vulcan (less realistic) as the truth and temporally varying Vulcan (more realistic) as the prior. Why?

11. Lines 289-290, I would have thought that annually averaged Vulcan would have the same total as temporally varying Vulcan, since averaging conserves the total. So why was scaling necessary? Was it because the inversions only covered a few months and not an entire year?

12. Lines 298-299, similar question as before. The authors used annually averaged Vulcan (simpler scenario) as truth and prior instead of the more realistic temporally varying Vulcan. Why?

13. Lines 458-459. While I certainly understand the value of more observations, and am all for increasing the observations coverage of the $\Delta^{14}CO_2$ network, I do not

think that having more observations will necessarily reduce the impact of transport model uncertainty. As the authors have themselves noted, the impact of transport model uncertainty is higher for smaller regions, while for larger regions (entire California) there is some cancellation. This is because the difference between transport models is typically more prominent at smaller scales (e.g., in the CO_2 inversion world, the global total flux is the easiest thing to estimate). So having more observations from a denser network could also sample these model differences even more and increase the impact of transport uncertainty on posterior flux estimates.

14. Lines around 490, and figures 3-6. The posterior bias is typically lower than the posterior uncertainty, barring a few exceptions. This could either be because the posterior biases are low (good outcome), or because the posterior uncertainties are large (less desirable outcome). Let's say that in an ideal world, we commit to making more $\Delta^{14}\mathrm{CO}_2$ measurements of higher precision, which will reduce posterior uncertainty. Will that also decrease the biases in figures 3-6? Or will it increase some of the biases (see earlier point about transport uncertainty), and may decrease others? Basically, what I'm trying to get at here is whether the good outcome for most of the flux esimates (bias < 2σ) is a happy accident of the specific 2018 network and measurement precision, or whether there is a more fundamental reason we can expect biases to be lower than posterior uncertainties under different (possibly increased) coverage scenarios.

15. Line 515, the Bagley et al (2017) reference is missing from the bibliography.

16. Line 534, suggest changing "much larger uncertainties" to "much larger percentage uncertainties".

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