

Overall, this is a clear and thorough examination of the use of APO as a tool for quantifying fossil fuel CO₂ emissions. The authors have conducted numerous sensitivity studies and model comparisons and conclude that APO can't give robust estimates of fossil CO₂ emissions with our current knowledge of oceanic APO fluxes.

The results here are important, the work is sound and the presentation is good. I have some corrections and suggestions to offer, but most of these are minor and I feel that the paper should definitely be published (after my concerns are addressed).

Three substantive matters come to mind that are worthy of attention:

This work relies heavily on the Jena Carboscope (JC) Inversion for background values and also regional oceanic fluxes. I am little concerned that there is no mention of the risk of circularity here: JC is an inversion that is based in part on the data from Weybourne. I would like to be convinced that the JC results give a background estimate that is truly independent of the record from which it is being subtracted.

The abstract and introduction led me to believe that all three sites (Weybourne, Heathfield & Ridge Hill) would play significant roles in this work. While Heathfield and Ridge Hill do add substance and breadth to the analysis, the truth is that Weybourne is by far the most important site in this study and most references to the other two are relegated to the supplement. I would prefer that the secondary nature of Heathfield and Ridge Hill was more explicitly acknowledged throughout. Anything else feels like bait-and-switch tactics.

Finally, the "future work" section is limited to developing better/more certain ocean fluxes. This ignores two possible avenues for exploration: a different species that might give insight into oceanic oxygen fluxes (noble gases?), or a different oxygen-based "tracer". APO was originally formulated to eliminate terrestrial signals and be as sensitive as possible to oceanic oxygen fluxes. Any ability to learn about fossil CO₂ fluxes from APO is simply luck, capitalizing on the fact that $\alpha_F \neq \alpha_B$. However, these oxidative ratios aren't actually very different. I am left wondering whether one would be better able to use oxygen for quantifying fossil CO₂ fluxes by combining O₂ and CO₂ into a tracer that was minimally sensitive to oceanic fluxes and then use terrestrial models (which might be better constrained than oceanic models, in this context) to take out the land contribution, leaving a more robust fossil signal. I recognize that actually doing this work is well beyond the scope of this study, but a brief mention of these alternative approaches (or others) would make the "future work" section much less *pro forma*.

A few minor requests for elaboration or clarification:

Line 56: I would like to see a sentence comparing this work to the aforementioned studies of Kuijpers and CHE. In particular, state your expectations for the UK sites. Do you expect to see just what the other studies saw, or do you expect to be more sensitive to, for example, marine influences?

Line 213-214: If this is a period of minimal terrestrial influence, why are you comparing to a simulation with no ocean fluxes? Perhaps this is a standard modelling practice, but without more detail, I don't

understand what is meant by “the 90 percentile of APO in a simulation with no ocean fluxes”, nor how it defines a period of minimal terrestrial influence.

Lines 229ff: As I understand it, you are asking “If the actual emissions of ffCO₂ go up or down, will we capture those variations if we start with APO and infer the ffCO₂ emissions?” However, instead you state: “we study the sensitivity of the modelled fossil fuel contribution to the atmospheric concentration of CO₂ and O₂”. I believe you are using “modelled fossil fuel contribution” for the value of ffCO₂ inferred from APO, and “the atmospheric concentration of CO₂ and O₂” is actually the variation in atmospheric CO₂ and O₂ mole fractions arising solely from fossil fuel combustion. If I am correct, I find your wording very confusing. If I am not correct, I am truly confused. Either way, please clarify.

Lines 249ff: Again, only after close study do I *think* I understand what you’re doing here. I believe the crucial point here is that “modelled $\Delta(\delta\text{APO})$ (calculated using equation 5)” is a fully model-based prediction of the change in APO that results solely from sources and sinks *within the region*. If I am correct about this, please state it more explicitly.

Very minor editorial points:

Line 12: should read “contribution of simulated fossil fuel CO₂ to APO.”

Line 37: New paragraph beginning “When considering ocean fluxes...”

Lines 61 & eq 1: The spacing in “reference” is odd. Maybe this is just a quirk of latex. Did you use \mathit for the subscript?

Lines 75 and following: Please be a little more explicit about the units in these eqs. In particular, it requires some work for the reader to determine whether $\Delta(\delta\text{APO})$ is a difference (in permeg) or a flux (in permeg/year) or some other units.

Line 85: The citation of Pickers is incomplete.

Line 87: “Variations in...” is a run-on sentence. A new one should begin at “however”.

Line 144: Should read “for the influence of rapid variations in CO₂ flux on the mole fractions, footprints are...”

Figure 3 caption: I assume panel D shows the footprint for WAO. This is not stated in the caption.

Figure 4: In the figure caption, the distinction between the groups of panels (a,b,c vs. d,e,f) is opaque. In my mind, there is no meaningful difference between “regional contribution” and “overall regional [contribution]”. I believe you have just combined the four respective sets of traces for CO₂ and O₂ in a,b,c (with appropriate weighting) to get APO in d,e,f. The word “overall” does not convey this relationship. Please make this easier to discern from the caption.

Line 320: “The APO model...” Exactly which model? By eye it’s not obvious which lines have been included in the average, yielding the quoted R² value of 0.24 for December.

Figure 6 caption: Is there a superfluous “and” in the 3rd line?

Line 340: The correspondence between α_F and α_B in the panels of Fig. 7 is not as stated, and it’s not simple to state correctly, so just remove “the top and bottom panels of” and “respectively”.

Table 1: The extension of the vertical line (in the column headings) separating August 2015 from December 2015 is in the wrong place.

Line 413: Should “large-timescale” really be “long-timescale”?

Figures 6 & 10: In the first case you simply say “ R^2 ” and in the second you say “The Pearson correlation coefficient R^2 ”. Either switch the order (giving a more complete introduction and subsequent abbreviation) or just stick with the simpler R^2 in both cases.