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Interactive comment on "Global atmospheric CO_2 inverse models converging on neutral tropical land exchange but diverging on fossil fuel and atmospheric growth rate" *by* Benjamin Gaubert et al.

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General comments

This paper uses the HIPPO dataset to evaluate recent atmospheric CO2 inversions and compares the spread of fluxes from the inversions with earlier inversion results and the Global Carbon Project budget. It is a useful presentation of the current state of inversions but I wonder whether more could have been done to increase our understanding of why the current inversions are more convergent than the earlier ones. I also

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wonder whether more information could have been extracted from the comparison with the HIPPO data. While these two points could be explored extensively, clearly some choices will have to be made as to what should be added to this paper and what should just be highlighted as areas for potential future work. I will try and provide some more specific suggestions below.

Specific comments

Use of HIPPO data: the analysis presented in the paper effectively reduces the HIPPO data to 3 numbers, the 2009-2011 annual, JFM and JAS mean northern extratropical vertical gradient, for comparison with equivalently sampled model output. I suspect this allows model errors to cancel out in the averaging and makes it harder for the HIPPO data to discriminate between inversions. This is seen even in the seasonal results; the one inversion that matches the JAS gradient is also the one inversion that does not match the annual mean gradient. This raises the question around what value is put on matching the annual mean if seasonal means are not correct. This should at least be more fully discussed in the paper.

Impact of harmonic fitting: Fig S1 shows observations that are not well fitted by the harmonic, and this flows through into Fig 1 where the average HIPPO fit can be a long way from the mean observation point. Is this also the case for the model samples? If they were plotted on Fig 1 or Fig S1, would they scatter around the HIPPO observation point (e.g. the low point at day $\sim$240) or would they scatter around the fitted lines? Is there information in the outliers to the fit which the models are able to capture? It could be useful to create scatter plots of each binned HIPPO gradient against each equivalent model generated gradient. Do they cluster on a one-to-one line and with what correlation? Do different models give different scatter patterns/correlations? If these figures are informative, perhaps they could be added to the supplementary information. My main concern is whether fitting the harmonic is minimising differences between the models and making it harder to discriminate between them? The low sensitivity to synoptic modelling bias presented in the Supplement would tend to confirm this. Would

Fig 2a look different if instead of taking the annual mean from the harmonic fit, you just averaged the 9 data points - it wouldn't necessarily be representative of an annual mean but as long as the observations and models were treated in the same way that probably isn't important.

Convergence of current inversions compared to previous ones: it would be good to provide some additional discussion in the paper as to why the current inversions are likely to be more convergent in their land estimates than the T3L2 ones. Since the T3L2 inversions all used the same method and priors, it would suggest that either there has been convergence in the transport models or that current methods are less sensitive to the atmospheric data and are more constrained by their priors. This seems possible if current methods are all using data at observed times rather than monthly means and not solving for large regions. This information is not currently in the paper. It would be good to add it (at least to supplementary material) i.e. the inversion method used, and some indication of the number of flask/in-situ atmospheric sites used and how they are used e.g. at measurement time, with what selection in the in-situ case. It would also be helpful to know what the magnitude of the prior land and ocean fluxes are for 2009-2011. This could either be provided as part of the model information in the supplement or perhaps plotted similarly to Fig 3a and Fig 4a. Knowledge of the prior may be particularly useful for understanding the land-ocean partition as I'm guessing it may contribute to some of the inversion differences.

Fossil emissions and atmospheric growth rate: I'm not sure I agree with some of the discussion around Fig. 4d. For example, the sentence on p15, line 17 starting 'One might expect ...' and 'counterintuively' (p18, line 5). The intent of an atmospheric inversion is to constrain fluxes by the atmospheric data. Thus I would expect the AGR to be well fitted and any difference in fossil between inversions to be compensated by variations in the other fluxes. This is what you see in Fig 4c. Hence a near horizontal line (as in Fig 4d.) for the modelled WAGR is what I'd expect from an inversion perspective. I don't think the suggestion of an opposite relationship (p15, line 19 'generally

the opposite is true', also p18 line 5 'lower AGR') is strong enough to make this point, rather that the WAGR is insensitive to the FF estimate used. If we were looking at different timeslices with real variation in fossil emissions (rather than just a variety of FF estimates for the same period) then it might be reasonable to expect a relationship between FF and WAGR but not when we are looking at a fixed period (2009-2011).

In general I found the 'WAGR', 'full AGR' and 'AGR' terminology confusing. Perhaps it is best to just use 'total flux' instead of WAGR since this is what you actually use from the inversion. Alternatively, it may be reasonably easy to calculate the MBL AGR from the inversions since I'd expect each inversion uses these sites and would likely keep information about their posterior fit to these sites. This measure could then be used as a direct comparison to the GCP calculated AGR and would be a useful additional indication of inversion spread.

Given my comments above, I think you need to be more careful with some of the messages that you draw out of the paper in the title, abstract and summary sections. Specifically: (a) 'diverging on fossil fuel and atmospheric growth rate' as used in the title - diverging compared to what? The converging vs diverging language implies that the fossil and AGR terms now have larger uncertainty than the land term. Using the numbers in Table 2 and Table 3, it would be fair to say that choices about fossil prior now have as large an uncertainty as those retrieved for tropical land from the inversions, but both are still larger than that for WAGR. Perhaps this is the main point to be made - that reductions in the inversion spread for land and ocean fluxes, now means that more care needs to be taken with how the fossil term is included in the inversion. Likewise in the abstract, p2, line 3, I don't agree that fossil and AGR terms 'dominate' the model spread since their uncertainties in Table 3 are smaller than those for land and ocean. Perhaps 'contribute to model spread at the largest scales and thus our ability to assess ...' Similarly at p18, line 18. (b) The abstract makes a general statement about agreement with HIPPO (p1, line 9) but the paper only really presents the northern vertical gradient so perhaps this statement should be modified to only include the 'in

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particular ...' phrase (unless any of the analysis suggested above provides further insight into the agreement with HIPPO data across inversions) (c) Abstract, p1, line 15. I am not convinced that the large disagreements on ocean-land partitioning are strongly influenced by the prescribed fossil fuel and associated differences in retrieved AGR.

Technical comments

p1, line 6: 'latitudinal distribution' - is this the best term to use since the paper really only focusses on the split between north and tropics+south.

p2, line 19: I found the 'NE' and 'SE' acronyms distracting as I had to stop myself from reading them as North-East and South-East. Perhaps just use 'N/S' or 'NEx/SEx'

p3, line 34: The GCP2016 acronym is used here but not defined until p4.

p4, line 9: missing ')' after Boden et al 2016.

p4, line 10: You note the possibility of using the ACTM inversions to assess the sensitivity to the FF, but it wasn't obvious to me that you actually do that within the analysis presented in the paper.

p6, line 20: It would be good to note somewhere here that the model is sampled along flight tracks i.e. move the comment from p7, line 2-3 earlier.

p7, line 4: suggest replace 'curtain averages' with '150 W transect'

p7, line 5: Add '(Section 4)' after 'supplement'

p8, line 8: perhaps add 'and model' before 'vertical gradients'

p8, line 9: Should this be -2.24?

p8, line 11: It's not clear to me what number is being quoted here. A range of 3.54 seems to match the Fig 2b, but what does the 1.04 refer to? It seems unusual to quote an uncertainty on a range. Perhaps just give the mean and 1-sigma as this would then

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be comparable with the number given in line 9.

p8, line 12: 1.3 ppm - this is from one outlier, perhaps better to give the typical mismatch ∼0.5 ppm.

p9. line 12: Perhaps 'Results shown in Fig. 2d and Fig. S7 are consistent.'

p9, line 24: Insert 'of' before 'retrieved land fluxes'

p9, line 25: what did you mean by 'and on 2.'?

p9, line 27: Add 'from the GCP2016 estimate' after 'disagreement on the total land sink'

Figure 3 caption: Is this complete? There was no specific information about panel D.

Table 2: The T+SE land flux is the same (0.34 +/- 0.27) for RECCAP Group 1 and This Study. Is this correct?

p13, line 13: 'full AGR'. If this is intended to be the same as the WAGR, then just use WAGR.

p14, Figure 4 caption. line 3 'We' not 'we'. line 6 'Here' not 'He'. I was confused by the use of 'WAGR-FF line' and 'WAGR line' in line 8. If these are for GCP, are they from AGR not WAGR?

p15, line 3: 'Because of the intentionally different FF source'. I don't think this is the explanation for the difference between ACTM-CDIAC and TM5-4DVar, since their FF values seem relatively similar (∼9 PgC/y). Perhaps the different prior ocean flux used makes a contribution.

p15, line 16: 'again defined' - perhaps qualify this as just for the models, assuming that the GCP line is from the MBL AGR.

p15, line 25-26: 'counterintuitive spread away from the mass balance line in Fig 4d' - does this require the assumption of a constant airborne fraction? If so, it might be good

to note this.

Figure 5 is difficult to read. Perhaps plot across a smaller range, or plot as differences from GCP. I assume that panel B is basically the same information as the y axis of Fig 4d just as a sum over 3 years rather than the mean. CAMS looks slightly smaller than GCP in this figure but slightly larger in Fig 4d. Panel B is not described in the Figure caption. Replace 'full AGR' with 'WAGR' in figure caption.

p18, line 5: 'lower' instead of 'slower' - though I'm not sure the signal is strong enough to really make this point - ellipse on Fig 4d is close to horizontal.

p18, line 7: not sure where the +/- 0.05 number comes from and exactly what it means.

Supplementary material

p1: define STL

p1: how different are the MLO reference trends subtracted from the models? I would assume they are quite similar since the inversions would do a good job at fitting the MLO data, but it might be worth mentioning.

p1: You might like to add a comment interpreting the results presented in Fig S2 and S3.

p2: The text says your averaging was for 5 degree bins but the example given seems to be for a 10 degree bin. Since most of your analysis only uses 20-87N and below 400 hPa perhaps it would be simpler to only plot these regions in panel 1 and 3.

p3 and p4: In Fig S2 and S3 it would be helpful to put all the CT cases in one row, and put the ACTM cases next to each other. I thought the ACTM cases looked surprisingly different in Fig S2.

Inversion descriptions: Jena, biosphere and fires: 'Constant' - do these priors include a mean seasonal cycle?

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