

Dear Saroja,

Thank-you for the work that you and your co-authors have undertaken to address the reviewers comments on your manuscript. Overall I think you have responded sufficiently to the comments from reviewer 1 and your new section 2.3 is helpful in clarifying the methodology. However this clarification has highlighted a difficulty for me in the comparison between PAAF and PAAM, which I think may also be relevant to your response to reviewer 2.

My concern is that PAAF is a relative measure since it is dependent on your prior flux. In this regard the comparison of PAAF across inversions (using the same prior) is reasonable but I think you need to be more careful in how you describe the comparison between PAAF and PAAM. For example, if you ran your inversion with an unrealistic prior (e.g. no seasonal cycle) then it seems plausible to me that PAAF would easily be larger than PAAM everywhere for all seasons whereas if you started with a prior that closely matched the observations then PAAF would be small and you would conclude that uncertainty from meteorology dominates. The relative nature of PAAF is not always clear in your manuscript. For example, on p27, line 2 you write 'the zonal structure in the tropics is trustable only in ...' where perhaps this should be written 'the zonal structure is ... differentiable from the prior only in ...' Likewise, is it clear that the GOSAT inversion adds zonally asymmetric structure as you suggest, or only that it changes the zonal asymmetry relative to the prior? With these examples in mind, please could you check through your manuscript again to ensure that your text reflects the relative nature of PAAF and that the conclusions you draw from your PAAF/PAAM comparison are justified.

Reviewer 2 provided you with two options for making the manuscript more significant. You have taken their 2nd option, focussing on the PAAF/PAAM analysis, but my reading of their review is that they would like to see more analysis and discussion of PAAM particularly. They suggest 5 additional questions that you could address, but you do not appear to have taken up any of these suggestions or provided a response to the reviewer as to the value or feasibility of their suggestions. Please can you respond to these 5 suggestions and consider whether one or two could be helpfully added to your paper. Personally, I think suggestion 2 could provide some interesting insights into transport error that might be helpful for other inverse modellers. Given your paper is already quite long, please also consider whether any sections can be shortened or figures removed to accommodate any additional analysis. For example, I wonder whether Figures 13-15 are all needed to illustrate the main points you make in Sec 3.2.4, and likewise for Figures 16-18/Sec 3.2.5. Alternatively perhaps the comparison to TCCON, HIPPO and the aircraft profiles could be shortened.

My re-reading of the manuscript also identified some possible technical corrections.

P17, line 18. 'uptake' needed after '2.5 Pg C per year'?

P18, line 6. Replace 'relatively lower' with 'less negative'?

P22, line 22. 'global mean' instead of 'globally averaged zonal mean'?

Please feel free to contact me by email if any of these comments are not clear.

Regards,

Rachel Law, rachel.law@csiro.au