

Interactive comment on “The impact of improved satellite retrievals on estimates of biospheric carbon balance” by S. M. Miller and A. M. Michalak

Anonymous Referee #2

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This is a well-written study and concise analysis of the improved constraint on biospheric CO₂ fluxes associated with improvements to the retrieval algorithm for OCO-2. Some major and minor comments on the manuscript are noted below.

Major comments:

- * It is interesting that the retrieval bias reductions from Version 7 to 8 helped so much with the biospheric flux constraint at the biome-scale. It would be nice for the authors to comment a little more on subtle differences between versions 8 and 9. Looks like the constraint went down in some regions, e.g. the drylands and dry monsoon areas. Why is that?
- * Did you try estimating any sub-biome scale regions? Given that the biomes tend to

C1

be multi-continental, it would be interesting to see the results using smaller regions that are (mostly) spatially contiguous within a given continent, especially with Versions 8 & 9. The statement on p. 3, lines 8-10 sounds somewhat misleading: “We begin with large, hemispheric regions and then decrease the size of those regions until we are no longer able to detect any variations in biospheric CO₂ sources and sinks.” It looks like you could potentially go to even finer spatial scales in the tropical grasslands/ forests and drylands/ dry monsoon biomes with Version 8 & 9 retrievals.

* This may not be the focus of your study, but I was very curious to see the results of your model selection and estimated betas from the regression with the selected bio models (and anthro/ biomass burning/ ocean fluxes). Which biospheric models were selected in different region/ month combinations? When was just one model selected vs. multiple models? Can these results help to inform which models are performing best in which regions? Does the “best” model for a given month change as a function of spatial scale? This could be potentially useful information for biospheric model developers. Also, I don’t see a supplemental material, but do you list anywhere which bio models went into the model selection algorithm?

* Not clear why you would include or exclude glint observations. It looks like in Miller et al, 2018, you exclude glint observations from results shown in the main manuscript. Why? How has the quality of these observations improved in Versions 8 and 9? And why are glint observations helping especially in tropical regions? Are they able to improve the density of observations in cloud-covered areas, or is a single glint measurement more informative than a single nadir or target measurement in these regions? Please don’t assume too much satellite-based knowledge on the part of the reader!

Minor comments:

- * P. 3, lines 31-33: it might be nice to put an equation or diagram or even table here showing the potential inputs that go into the model selection and your regressions. Do you run model selection on all months simultaneously? That’s what it sounds like, but

C2

please make that more clear.

* P. 4, lines 4 and 8: please replace the terms “former” and “latter” with something more descriptive, e.g. biospheric model output and constant fluxes.

* P. 4, line 19: “to avoid potentially biasing the results”. This is true, but please make clear that XCO₂ reflects the contributions of all these different types of fluxes (ocean/ FF/ BB/ terrestrial bio), so you need to account for the non-bio fluxes in order to isolate the signal of the bio in the regression. Can also comment that the uncertainty on the FF/ ocean/ BB fluxes is thought to be much smaller than that on the terrestrial bio fluxes (with reference).

* P. 5, line 13: “in about half of all months in the tropics”, but didn’t you say on line 10 that “variations in CO₂ fluxes are detectable across tropical biomes much of the year?” In Version 9, it looks like you can constrain bio fluxes in the tropical grasslands and forests for 8 and 9 months of the year, respectively.

* P. 6, line 17: please add references for the ACOS retrievals and bias correction, and also for OCO-3 and GeoCarb.

* P. 7, lines 6 to 9: it is not clear to me, at least, why a bias correlated across regions larger than those examined in this study (e.g. a time-dependent drift for the whole globe) could potentially qualitatively impact the results of your study. Given that you are looking at a single year, would this time-dependent drift matter at all? Wouldn’t the constant component in your regression account for this global bias between obs and convolutions?

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