



EGUsphere, referee comment RC2
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Comment on egusphere-2022-1305

Anonymous Referee #2

Referee comment on "Optimizing the carbonic anhydrase temperature response and stomatal conductance of carbonyl sulfide leaf uptake in the Simple Biosphere model (SiB4)" by Ara Cho et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-1305-RC2>, 2023

General Comments

This paper addresses some important factors that influence the modeling of carbonyl sulfide, with the goal of improving our ability to use it to estimate GPP. Plant-specific optimization of conductance parameters is a really useful way to approach model improvement. The authors used some nice measurement datasets at a couple of different forested sites and were able to demonstrate reduced model-obs mismatch with their optimized setup. Globally this also addresses some of the gaps pointed out by previous studies (e.g. missing sink at high latitudes). While there is still room for improvement, this is a good first step in improving our ability to model OCS. My comments are mostly minor or eliciting clarification. As with RC1, I was also hoping the authors would circle back to the impact on V_{cmax} and ways to potentially optimize that independently, but there is always the next manuscript!

Specific Comments

- Para from line 41: Soil emissions may also play a role in some specific regions (e.g. hot areas or agricultural fields). See refs cited in Ogee et al, Biogeosci 2016.

- Lines 44-45: More recently, Hu et al (PNAS 2021) also showed existence of this missing sink at higher latitudes.

- Line 105: add the word 'prognostic' to SiB4 description

- Line 202: technically, GPP is not an 'observation' but a 'derived/modeled quantity' so should not be included in this list of obs.

- Line 282: change wording to 'observation and observation-derived quantities' since 'GPPobs' and 'gs' are not direct observables but rather derived quantities.

- Fig 4 comment: is the reason for missing hours in the HVFM All plot that there is no data for certain phenological stages (apart from growth and maturity for which you have data at all hours)?

- Line 332-333: does this imply you used the new $f(T_{can})$ estimations for forests and applied them to grasslands as well? That seems like it could cause additional problems.

- Line 344 comment: did you investigate whether 100 was sufficiently large?

- Table 3: where does the prior error range come from? Perhaps a reminder is in order referencing Appendix A where the prior error is estimated (as mentioned in sec 2.3.2)

- Fig 7a comment: the red and orange lines don't seem that different here, perhaps cite some calculated statistical significance to emphasize that they are different?

- Fig 7b: why not also show the equivalent to the orange lines for Harvard Forest? (i.e. with optimized $f(T_{can})$ but original α).

- Line 475: your result seems to imply that above-canopy RH is a better observational quantity to use to derive g_s , but this is counterintuitive in that the ' g_s ' specifically involves resistance (or conductance) at the leaf surface, and so theoretically we should use RH at the leaf surface. One alternate explanation here is that it could be incorrect leaf temperature which can lead to a bias in leaf surface RH which propagates to g_s .

- Line 484: what are the alternatives to 'stomatal transpiration'?

- Line 493: clarify 'indicating humidity stress only shortly at midday'. Do you mean that the impact of humidity stress is short-lived or only important around midday?

- Line 498: which 'pseudo-observations'? maybe just use 'observationally-derived X' where X is the quantity you're referring to here and mention it explicitly.

- Line 515: and also consistent with Hu et al (2021, PNAS)

- Line 542: how does the improvement in b0 compare to night-time conductance values calculated for CLM by Lombardozzi et al 2017? (maybe this citation could be discussed earlier where you mention b0 results)

- Appendix A comment: I think your prior errors are based on the 'initial value +/- 1.5 state errors'? So for example prior alpha should then be 1400 +/- 700 (as is shown in Fig B1A for HYYT). But this is inconsistent with Table 3 where you list 1400 +/- 1000. Can you please clarify?

Technical Corrections

- Line 109: replace 'heterogenic' with 'heterogeneous'. I think the former is more related to genetic/species aspects.

- Line 234: delete 'the', or add the word site after 'Harvard Forest'

- Line 264: delete 'to use'

- Line 266: delete 'of' before 'uncertainties in GPP'

- Line 338: change 'humidity impact' to 'humidity stress impact'

- Line 340: Capitalize 'we'

- Fig 5 caption: replace 'in' with 'at'

- Line 403: replace 'in' with 'at', and 'In' with 'At' (and in any other instances when

mentioning the sites, such as lines 407, 413, 433, Fig 7 caption, 463...)

- Line 411: 'higher and smaller respectively'

- Line 430: replace 'pseudo-observations' with 'derived' or 'observationally-derived'

- Line 491: 'At the Harvard Forest site..'

- Fig A1 caption: 'where the cost is minimized' or 'where the cost reaches a minimum value'.

- Line 585: Reiterating comments from above, GPP is not an observation.