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Comment on egusphere-2022-1305

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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., EGUsphere,
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General comments:

Initial work on the leaf COS uptake was based on the notion that the carbonic anhydrase (CA) conductance (gca) would be relatively large (or the corresponding resistance low) since CA is highly efficient in catalyzing COS. As a consequence, it was assumed that the leaf COS uptake would be mainly limited by stomatal conductance (gs), opening interesting avenues for using the leaf COS uptake as a proxy for transpiration and photosynthesis. By now more and more experimental data are surfacing which suggest that gca may be of similar magnitude as gs or even be the rate-limiting step for leaf COS uptake. There is thus an urgent need to better understand gca, both in terms of inter-specific differences and what these relate to, as well as with regard to the short-term drivers, and this information needs to be included in models which simulate the leaf COS uptake.

The manuscript by Cho et al. makes an important and timely contribution to this field by suggesting a peaked as opposed to the previous purely exponential temperature response of gca in the model SiB4. The updated model is able to reproduce the temperature response of the canopy-scale COS at two different forest study sites and in a global application the COS uptake is increased in higher latitudes and decreased in the tropics. In addition, the authors constrain the parameters of the stomatal conductance model inside SiB4 by means of the COS flux measurements.

Overall, most of my comments are minor, but there are many of these, aimed at improving the clarity of the writing, as summarized below.

The one, possibly, major, comment relates to the fact that the authors optimized parameters affecting the supply side of photosynthesis, i.e. the b1 stomatal parameter,

against experimentally derived GPP, but not the demand side, e.g. Vcmax. I presume that all parameters the authors did not optimize, were left at the default values for the corresponding PFTs. This could mean that by optimizing the b1 parameter, the authors might have mapped differences between the (unknown) true and pre-scribed Vcmax into the b1 parameter. Furthermore, since gca is scaled to Vcmax, this might have further consequences for the estimated alpha parameter and possibly even the temperature reponse parameters of gca. I would like the authors to state why they did not choose to optimize some parameter representing the demand side of photosynthesis and discuss what the implications of not doing so might be. Ideally, they would underpin their arguments with some evidence which convincingly shows that any bias in Vcmax does not affect the parameters they retrieve and their interpretation.

Finally, I would like to suggest, following Sun et al. (2022, 10.1111/nph.18178), to replace the term gca with gi as conceptually all conductances/resistances other than ga, gb and gs are mapped into gca, notably the mesophyll conductance.

Detailed comments:

- 14: "… respond differently to temperature."
- 15: the original paper on this stomatal conductance model was written by Ball, Woodrow and Berry – I think we should not forget about co-author Woodrow and name the model accordingly (BWB) – here and anywhere else in the manuscript
- 18: but the model is driven by Tcan not Tair …
- 19-22: all these numbers may be too much detail for the abstract
- 26: these gaps are poorly identified and it is also not shown how these new estimates help close these gaps
- 34: during nighttime ecosystem respiration can be measured … the problem is during the day when there is both GPP and RECO, but only NEE can be measured
- 39: gs is seldomly derived from NEE for many reasons; typically the H2O flux would be used, which has problems as well (which you discuss later); if the internal conductance to COS is known (aka gca), then COS fluxes in principle would allow estimating gs both during day and night
- 69: here or in the next section it would be useful to review what is known about the temperature response of CA from physiological studies
- 75-77: this could be actually be formulated as a hypothesis, giving the study a hypothesis-driven twist
- 90: why are multi-year measurements need to constrain the model parameters?
- 95: with "observation-based gs" you apparently try to express that gs was not directly measured but inferred from measurements through some model; as this idea has not been introduced here yet, I suggest to formulate in a more unambiguous way; note that also GPP is not measured, but inferred through a flux partitioning model
- 103: remove "land" in "land surface energy"
- 105: it is unclear here how satellite information was used by SiB3 and how SiB4 differs – suggest to reformulate
- 118: "… or conditions are unsuitable for photosynthesis."

- 120: what about the aerodynamic resistance/conductance – shouldn't this be included in Eq. 1? Worth mentioning that gca conceptually incorporates any conductances downstream of the stomatal one, e.g. also mesophyll
- 124-124: "The factors 1.94 and 1.56 account for the smaller diffusivity of COS with respect to H2O through the boundary layer and stomatal pores, respectively."
- 135: "… the drought response …"
- 141, "… most PFTs, but …"
- 144: "… using the carbon pool …" – unclear what is meant here – isn't photosynthesis simulated as the minimum of Rubisco, light or storage-export limitation carboxylation rate?
- 151-152: repetition from above
- 162: using a leaf energy balance approach?!
- 163: "air temperature"
- 178: correct – actually very often also an optimum temperature response function is used for Vcmax and Jmax
- 200: "Observations"
- 203, 206: GPP is not "observed", but derived from flux partitioning, i.e. a model
- 209-210: what you mean is probably that the COS flux was calculated as the sum of the vertical eddy covariance and the storage flux – this is not a correction but required whenever the storage flux contributes significantly to the 3D mass balance
- 211: why didn't you use GPP derived from CO2 flux partitioning as at Hyytiälä? This peculiarity might be should be further discussed given that it yields very different estimates compared to CO2 flux partitioning at HF
- 215: averaging does not improve "data quality", all it does it reduces variability due to random uncertainty, but not the systematic one
- 216: that means you excluded 50 % of the data in each 3-hour period?!
- 221-223: this sentence applies only to Hyytiälä?!
- 227: are these 25-75% before or after filtering for the 25-75% range?
- 234-238: by now much more elaborate algorithms are available for T/ET partitioning – see Nelson et al. (2020, 10.1111/gcb.15314) – there are also packages for easy application
- 248: does gb from SiB4 include the aerodynamic conductance?! Gb and Ga could be calculated from standard flux tower observations as done in the papers by Wehr et al.
- 251: does that mean that you just retained data in the interquartile range?
- 262: a sequential two-step process is not simultaneous …
- 266-267: the BWB model is applicable also in the darkness – in this case gs will represent b0; the point rather is that GPP should be zero without light
- 311: what uncertainty does this statement refer to? Random – systematic? How would systematic uncertainty be taken into account with your approach of calculating the CV over 3-hourly periods?
- 325-326: why not also take the other environmental drivers as measured at the flux towers?
- 332-333: what exactly does this mean? You used alpha, bo and b1 determined for ENF and DBF for these PFTs but used the standard values for all other PFTs?
- 340: something wrong with this sentence
- 358-359: please elaborate how/why this finding supports your two-step calibration approach
- Table 1 and 2: are these statistics combined for both sites? Given that GPP was estimated in quite a different fashion at both sites, I suggest to split the statistics
- Figure 6: same question as above – are both sites combined? If so, I suggest to split
- 396-397: "Thus the different optimum temperatures reflect the adaptation of the enzyme's temperature response to the prevailing temperatures"
- 397-398: since temperature is a key driver of the model anyway this should not be an issue – maybe rather say that accurate climate information is important?
- 399: for which sites/climates did Ogee et al. derive these values?
- 401: "… reduced from the default value of 1400 …"

- 407-408: this is not necessarily true as gi depends on both alpha and Vcmax and differences in the COS flux also depend on gs – that is to say that the differences in COS flux between both sites may also be due to other factors
- 410: similar to what?
- 411-412: to put these results into perspective – if you were to go into the field and quantify nighttime stomatal conductance using a porometer I would presume these differences would be buried in the variability of the measurements; that is to say these differences are really small
- 419-422: this is really important information in my view!
- 430: now you call these pseudo-observations? I suggest to use a consistent terminology throughout the manuscript
- 435: to emphasize this point the authors may want to add the number of measurements, e.g. in temperature bins, to Fig. 7
- 455: note sure I understand the "stationary" in the subheading
- 458: were the "original" SiB4 simulations also tuned to the site data? If not, isn't there a mix of structural model differences and tuning affecting this comparison?
- 469-478: this merits further discussion I think; when the model overestimates gs because of FLH, this means that FLH, which is the relative humidity of the air in the boundary layer close to the leaf surface, is too large; because FLH = eb/esat(Tcan), there are two options for this to occur – (1) eb, which is the vapor pressure of the air in the boundary layer close to the leaf surface, is too large, which could be the case because transpiration (T) is too large or the boundary layer conductance too small since eb = esat(Tcan) - T*P/gb for water vapor transport across the boundary layer; or (2) Tcan, the temperature of the saturated water vapor in the leaf intercellular space, is too low, which would make esat small and thus increase FLH; it might also be mentioned that using RH from the reference height instead of RH at the leaf surface is conceptually wrong as stomata would sense moisture at the leaf surface and not above the canopy; here in turn it might also be mentioned that the use of RH in the BWB model has been critiqued since a long time as experiments show that stomata do not sense RH
- 480: "significantly" in a statistical sense?
- 495: back up statement with reference
- 514-516: can you provide some numbers here on how much the new simulations would help resolving the differences?
- 529-530: move this sentence after the second one in this section?
- 537: Gimeno however studied bryophytes, which is quite different from the vascular plants which the PFTs in SiB4 mainly represent