

- General comments

The authors carefully considered the comments provided by the reviewers, implementing the suggested modifications when deemed necessary and useful, and in my opinion significantly improving the quality of the manuscript. I thank the authors for this meticulous act of revision of their work, and for the detailed explanations and comments to my suggestions.

All in all, my final indication to the editor is to accept this manuscript for publication after a few minor and technical corrections.

For what stated above, I am not going to reply to each single Authors' response, but only focus on the very few points that still deserve attention, or some explanations, and on a few technical points to be addressed before publication. It is intended that all of the other comments fully answer my previous points. In doing so I will follow the system used by the Authors to index the sections and subsections in their "Response to Referee #2" (<https://doi.org/10/5194/gi-2022-1-RC3>) for what concerns the Specific comments, while for the Technical comments it is more straightforward to refer to the line numbers as in gi-2022-1-ATC1 file.

- Specific comments

1.3: I thank the Authors for such a detailed and convincing explanation. I agree with the Authors that the spectroscopic effect on the IRGA's precision is a "hot" topic, and that for that reason the temperature measurements are of great importance. What the Authors added on section 6, and on Appendix C, fully addresses my comments. I just wish to highlight that by writing "No user will buy the IRGASON to calculate T_a " I meant to say "only" for that: omitting this word may lead to misunderstanding, and I apologise with the Authors if that was the case.

2.3: this is the more tricky aspect in my opinion. I fully agree with the Authors on the reliability of the equations used: I am fully aware of the Scientific teams that work at LICOR and Campbell Scientific (even though I am not sure that 100% of EC systems in the World use sensors from only these two brands), and for sure their work is consolidated by tenth of years of expertise in the field. However, my point was not on the actual reliability of the Equations, but more on the fact that, in a scientific publication, using peer-reviewed references, when available, is the basis of the Scientific approach (I have to say that on that I tend to respectfully disagree with the Authors: "We believe the manuals from industry-trusted manufacturers have equal credibility to journal publications"). The new wording in part addresses this point: to definitely fix it, my suggestion to the Authors is to strengthen the link between using the manuals as "starting point" for the development of the method, and the fact that the approach proposed is based on the sensors' specifications: in that way the starting equations and the specs are both found in the same source.

#3: I think there were a few misunderstanding on this generic point, my apologies to the Authors for the not-clear-enough wording. First, the Authors claim their method can narrow the widest possible range of uncertainty to a significant degree, which is correct, in particular by calibrating far from the extremes of the temperature range. I just wanted to point out that the common user will rarely calibrate in extreme conditions, and so the widest range of uncertainty will rarely be the actual case ("this is what normally happens" referred to

calibrating in mild climatic conditions, not the opposite). However, I do agree that there exist several users working in harsh conditions, for who this recommendation is precious. Secondly, "the applications proposed are not very impactful" was likely a fully-unintended but still improper selection of words. I wish to apologise again with the Authors, I should have selected a different wording. In any case I wasn't referring to the overall manuscript. My point was simply that there may have been several additional applications in the EC framework deriving from this interesting analysis. The authors addressed this point within the new sections (6.1 and the Appendix C), and also I underrated the importance of the applications proposed for the users working in very cold climates. I am certain the manuscript is impactful - would have I thought the opposite, I wouldn't have accepted to review it.

- Technical comments:

line 81: overall (typo)

line 83: available (typo)

line 86: Lee et al. (1999) (typo, a 9 missing)

line 480 (and elsewhere): "hourly" fluxes may also be half-hourly, or calculated over other time scales. Probably better to use a different term, like "calculated and temporally aggregated fluxes" or similar; or to report earlier that you use "hourly" to refer to these fluxes as it is a common time scale, but all would remain valid for half-hourly fluxes or fluxes calculated over different time scales

line 491: "with an error as ranged by its accuracy and T_a with an error": please consider rephrasing for more clarity

line 513: is added only by (typo, "by" is missing)

line 544: this is the first time you mention EddyPro, probably you wish to consider explaining what it is. Or maybe reconsider including it at all

line 645: "measurement uncertainties" may be misleading: please consider selecting a different wording

line 664: please consider removing the word "more"

line 805: I think there is a typo in the title of Appendix C: "The relationship of measured to true covariance to of vertical wind speed with CO₂, H₂O, and air temperature" should be instead "The relationship of measured to true covariance of vertical wind speed with CO₂, H₂O, and air temperature"

line 808: please consider rephrasing to something like "from the covariance between each of the three components of the 3-D wind field and the density of CO₂/H₂O"

line 814 (Eq. C2): subscript "i" missing for rho-alpha

line 818 (Eq. C3): I think you are implicitly using Reynolds rule to derive the final equation (that is the average of the sum of two terms is the sum of their averages): please consider making it explicit.

line 838: an equation is missing after "and" (I think covariance between v and ρ measured = covariance between v and ρ true), while "are also" should be deleted in my opinion

line 839: v^2 mean repeated (second one should be w^2 mean)

line 841: scaler should be scalar instead (typo)

line 842: this means that this would not be valid for momentum flux (covariance between vectors)? (out of the scope of the manuscript, just came to my mind as a matter of curiosity)

line 845-846: please state that what is between square brackets is the notation for the maximisation of covariance (otherwise the reader may think you are multiplying things)

line 851-854: is this also valid for other spectral correction methods, like Ibrom et al. 2007 and Fratini et al. 2012 methods?

line 858: please consider rephrasing

line 862: please consider rephrasing

line 918: Biogeosciences (typo)