



INSTITUTE OF APPLIED ECOLOGY, CHINESE ACADEMY OF SCIENCES

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RE: Response to referee's comments on GI-2022-1R

Dr. Salvatore Grimaldi
Dept. for Innovation in Bio., Agro-food, & For. Systems
University of Tuscia, Viterbo, Italy

Dear Dr. Grimaldi,

Thank you for inviting us to revise our manuscript, "Accuracies of field CO₂-H₂O measurements from open-path eddy-covariance systems: Assessment based on atmospheric physics and biological environment," to address the comments included in the referee's note for publication in *Geoscientific Instrumentation, Methods and Data Systems (GI)*. Through the review process, we have been enjoyed our professional discussions with the referee about his/her professional and constructive comments, which significantly improves the quality of our manuscript. We have felt lucky to meet this referee over the review process. We appreciate referee's knowledge and dedication. Many thanks to the referee for his/her two rounds of reviews.

The authors carefully checked every comment from the referee for this revision. Our discussions and proposed revisions in response to the corresponding comments are given below. While responding the comments, the atmosphere CO₂ background value of 415 ppm (760 mgCO₂ m⁻²) in 2021 was brought into current as 419 ppm (767 mgCO₂ m⁻²). Accordingly, Table 2 and Figure 2 were updated. After our revision, proofreading was requested from the communication team of Campbell Scientific. Ms. Kati Kovacs proofread the manuscript. The corrections from her proofreading are indicated in this version (GI-2022-1RR) with trackers.

In our response to the comments, we directly follow the indexes of referee's comments for the sections and subsections as well as the line numbers. Because the line numbers were used by the Referee while reviewed GI-2022-1R, these line numbers are more correlated with manuscript GI-2022-1R than GI-2022-1RR.

We appreciate your favorable consideration for publication of this manuscript in *GI*.

Sincerely,

Ning Zheng, Ph.D., Application Scientist
Eddy-Covariance Flux Instrumentation

Response to Referee's comments on "Accuracies of field CO₂-H₂O measurements from open-path eddy-covariance systems: Assessment based on atmospheric physics and biological environment"

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<https://doi.org/10.5194/gi-2022-1R>

Response to Referee's Comments (*gi-2022-1-referee-report.pdf*)

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.

Author response

Again, we have been so enjoyed our professional discussions with you about your professional and constructive comments. We have felt lucky to meet you over this review process. We appreciate your knowledge and dedication. Many thanks.

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 Ta" 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.

Author response

Our review and response processes are truly academic and professional discussions. We truly enjoyed your comments. No apology necessary. I believe we are sharing the same goal to gradually improve the flux measurements in our community.

Author proposed revision

A revision is not suggested by the Referee.

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.

Author response

I agree with your "disagree". Our wording "equal credibility" might not be fully fair although we did emphasize on "industry-trusted manufacturers". Overall, for sure, journal publications have better credibility than manufacturer manuals. In our community, it is well-known that LI-COR Biosciences did publish credible manuals for almost 30 years, particularly related to gas analyzers. In this manuscript, the models in a gas analyzer manual from LI-COR Biosciences are used to explain the specifications from a gas analyzer manual by Campbell Scientific. Both manufacturers are competitors in CO₂ and H₂O analyzers, which is the reason we try to balance the use of literature from both. To the best of our knowledge, we are not aware of any publication to explain the models of CO₂ and H₂O measurements as clearly as LI-COR Biosciences (2001 ~2021) does although Fratini et. al (2014) principally used the models. Therefore, we feel that the manuals of LI-COR Biosciences are the best sources for our citations.

Author proposed revision

A revision is not suggested by the Referee.

#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 elected 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.

Author response

Thank you so much for your positive comments. Again, our review and response processes are truly academic and professional discussions. We truly enjoyed your comments. No apology necessary.

Yes, common users rarely calibrate CO₂ and H₂O infrared gas analyzer in extreme conditions. Similar to Fratini et al. (2014) did, more work is needed to ensure the quality of data from extreme conditions. Other approaches are needed to stabilize the performance of CO₂ and H₂O infrared gas analyzers in extreme conditions. This study provides a pilot analysis.

Author proposed revision

We revised some wording while thoroughly reading and checking the manuscript. See the version of GI-2022-1RR with trackes

Technical comments

line 81: overall (typo).

Author response

Corrected

line 83: available (typo).

Author response

Corrected

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

Author response

Corrected

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.

Author response

We understood your concern. We are always struggling with the use of "hourly" for this context. We are trying a new approach to this expression.

Author proposed revision

Throughout the manuscript

"Hourly CO₂/H₂O flux" was revised to "CO₂/H₂O flux data"

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

Author response

Rephrased.

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

Author response

Corrected.

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.

Author response

The full official name plus the EddyPro reference would be helpful to readers who are not familiar with EddyPro.

Author proposed revision

Line 544: Revise the EddyPro program to “EddyPro® Eddy Covariance Software (LI-COR Biosciences, 2021a).

LI-COR Biosciences (2021a) is added to References and the references are reordered.

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

Author response

The sentence was rephrased.

Author proposed revision

Original

“..., the measurement uncertainties for analyzer specifications are not expected to increase rather some current terms could be removed from the current specification list, ...”

Revised

“..., the number of these uncertainty sources for analyzer specifications is not expected to increase, rather some current uncertainty sources could be eliminated from the current specification list, ...”.

Additionally, the wording related to this revision in this paragraph also is accordingly revised.

line 664: please consider removing the word "more".

Author response

The word of “more” was removed.

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"

Author response

The extra word of “to” was removed from “to of”.

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"

Author response

Our expression is not precise.

Author proposed revision

Before revision

“from covariance of 3-D wind with a CO₂/H₂O density.”

After revision

“from covariance of an 3-D wind component with a CO₂/H₂O density.”

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

Author response

Subscript “i” is added to $\Delta\rho_\alpha$.

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.

Author response

Yes, we used the Reynolds rule.

Author proposed revision

Original

“the over bar is an averaging operator,”

Revised

“the over bar is the Reynolds’ averaging operator,”

line 838: an equation is missing after "and" (I think covariance between v and rho measured = covariance between v and rho true), while "are also" should be deleted in my opinion line 839: v² mean repeated (second one should be w² mean).

Author response

Yes, right. Apparently, our newly added appendix in last revision needs more attentions. Thank you so much for your such dedication. We will read through again and ask a professional to proofread this version although revisions are minor from last version.

Author proposed revision

a. “Both $\overline{u \rho_{\alpha i}} = \overline{u} \overline{\rho_{\alpha i}}$ and are” is corrected to be “Both $\overline{u \rho_{\alpha i}} = \overline{u} \overline{\rho_{\alpha i}}$ and $\overline{v \rho_{\alpha i}} = \overline{v} \overline{\rho_{\alpha i}}$ are”.

b. “..., $\overline{u^2}, \overline{v^2}, \overline{v^2}, \dots$ ” is corrected to be “..., $\overline{u^2}, \overline{v^2}, \overline{w^2}, \dots$ ”.

line 841: scaler should be scalar instead (typo).

Author response

Corrected.

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).

Author response

In coordination rotation process, unlike a mean of scalar unchangeable, the mean of vectors related to momentum flux is changed. Carefully examine the rotation matrices, the mean of scalar is not involved. Instead, all means of 3-D vectors are included in the matrices. We did not numerically analyze deeper.

Author proposed revision

No revision is suggested by the Referee.

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).

Author response

The expression is hard for readers although the expression is correct.

Author proposed revision

Before revision

“Therefore, the maximum covariance in magnitude among $\overline{(w'\rho'_{at})_r}$ (l from $-k$ to k) [$\overline{(w'\rho'_a)_{mm}}$] is equal to the maximum in magnitude among $\overline{(w'\rho'_{at})_r}$ [$\overline{(w'\rho'_{at})_{mm}}$] (Moncrieff et al., 1997; Ibrom et al., 2007), given by”

After revision

“Therefore, the maximum covariance in magnitude among $\overline{(w'\rho'_{at})_r}$ (l from $-k$ to k) is equal to the maximum in magnitude among $\overline{(w'\rho'_{at})_r}$ (Moncrieff et al., 1997; Ibrom et al., 2007). Denoting the former maximum covariance by $\overline{(w'\rho'_a)_{mm}}$, where subscript m indicates the maximum, and the latter one by $\overline{(w'\rho'_{at})_{mm}}$, this equality leads to”.

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

Author response

Yes, in terms of a correction factor in the context, both methods are valid, but Ibrom et al. (2007) and Fratini et al. (2012) methods are particularly for close-path eddy-covariance systems. This study is for open-path eddy-covariance systems. Therefore, both are not included in discussion for this context.

line 858: please consider rephrasing.

Author response

Rephrased.

line 862: please consider rephrasing.

Author response

Rephrased.

line 918: Biogeosciences (typo).

Author response

Corrected

References

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- Fratini, G., Ibrome, A. Burba, G.G., Arriga, N. and Papale, D.: Relative humidity effects on water vapour fluxes measured with closed-path eddy-covariance systems with short sampling lines, *Agricultural and Forest Meteorology*, 165: 53-63, 2012. <https://doi.org/10.1016/j.agrformet.2012.05.018>
- Ibrom, A., Dellwik, E., Flyvbjerg, H., Jensen, N. O., and Pilegaard, K.: Strong low-pass filtering effects on water vapour flux measurements with closed-path eddy correlation systems, *Agr. Forest Meteorol.*, 147: 140–156, <https://doi.org/10.1016/j.agrformet.2007.07.007>, 2007.
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- Moncrieff, J. B., Massheder, J. M., de Bruin, H., Elbers, J., Friborg, T., Heusinkveld, B., Kabat, P., Scott, S., Soegaard, H., and Verhoef, A.: A system to measure surface fluxes of momentum, sensible heat, water vapour and carbon dioxide, *J. Hydrol.*, 188-189, 589–611, [https://doi.org/doi:10.1016/S0022-1694\(96\)03194-0](https://doi.org/doi:10.1016/S0022-1694(96)03194-0), 1997.