Geoscientific Instrumentation Methods and Data Systems Discussions

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Comment on gi-2022-1

Anonymous Referee #2

Referee comment on "Accuracies of field CO_2–H_2O data from open-path eddy-covariance flux systems: assessment based on atmospheric physics and biological environment" by Xinhua Zhou et al., Geosci. Instrum. Method. Data Syst. Discuss., https://doi.org/10.5194/gi-2022-1-RC3, 2022

- General comments:

The manuscript is innovative in trying to quantify the overall uncertainties in the measurements of CO2 and H2O amounts by Open Path Eddy Covariance (OPEC) gas analysers due to their different sources. The aim is pursued by means of a Campbell Scientific IRGASON, and then generalised. A simple model is developed to combine the different sources of errors, and the resulting uncertainties are plotted under different conditions of temperature and gas concentrations. In addition, some applications of the results are reported, together with some suggestions for the users during field calibration. This preprint follows the same approach of a paper published last year by the same main Author relative to the Closed Path Eddy Covariance (CPEC) sensors.

The study has some points of strength and some points of weakness. Among the strenghts it is the fact that the study addresses relevant scientific questions within the scope of GI, using in part novel ideas, and using a proper language. One of the more important strenghts is that it uses the specs of the sensor to define its uncertainties, and it defines the uncertainty in terms of range: the worst case scenario is depicted for each source of error as the limits of the range, and then combined with the others. This is a valid approach to visualising the uncertainty in a straightforward way.
The more important weaknesses are in my opinion: #1. the poor link with the eddy covariance method, despite this is mentioned since the beginning; #2. the generalisation from the IRGASON/EC150 to all the Open Path sensors is not robust enough; #3. Applications and calibration suggestions are only partly relevant; #4. more references are needed, as the most cited are not peer-reviewed papers but sensors manual; #5. the discussion section is more dedicated to other things (recap of what done, applications), but the real discussion is limited; #6. conclusions should be strenghten as well
I'll recall these points in the comments below when relevant

- Specific comments

Suggestion to fix the above:
->1. The EC method includes a very long chain of steps from field measurements to calculation of the fluxes. In this chain, the specs of the sensor are in general considered less important in terms of final uncertainties. Also, the uncertainty are more relevant to the EC method in terms of fluxes (as the result of covariance between sonic and IRGA signals), not concentrations: this is clearly out of the scope of the manuscript, but should be mentioned and maybe discussed a bit. Also, an OPEC system is made of two main sensors: the IRGA and the sonic. But the latter is almost not considered in the study: this could be reconsidered, or at least the reasons for excluding this sensor should be given. A possible alternative could be to reconsider the link with EC: is that really needed? The study may focus on the Open path IRGA (so the EC150, not the IRGASON), just mentioning that it is often used for eddy covariance measurements, but clearly state since the beginning that the study will not focus on EC. (please note this will clearly impact the title as well)

->2. it should be shown that the specs used are all necessary and sufficient, and provide guidance to the reader in case some of them are missing on a different sensor specs (better if also considering additional specs that may be found). In some occasions the authors refer to "OPEC systems" while dealing with the specs of the IRGASON - which may be not the case

->3. the suggestion of calibrating on an "average" temperature (Ta) to basically avoid to be in the worst case scenario (Ta and Tc at the extremes) is not robust as this is what normally happens, also because the range of Ta between two calibrations can be very large. In addition, it is based on the assumption of linear relationship between the difference Ta-Tc and the drift magnitude, which derives from a simplification not so deeply documented. Also the applications proposed are not very impactful: if the EC method is kept (see #1) many more interesting applications could be thought of (but again, probably out of the scope). Even without that, I would use this idea of "applications" to improve the point above: the first and more relevant application should be "how to calcuate the uncertainty for a generic IRGA"

->4. I think the paper from Fratini et al. 2014 (Fratini, G., McDermitt, D. K., and Papale, D.: Eddy-covariance flux errors due to biases in gas concentration measurements: origins, quantification and correction, Biogeosciences, 11, 1037–1051, https://doi.org/10.5194/bg-11-1037-2014, 2014) should definitely be included in the discussion, as it develops a correction of EC fluxes based on the drift of the IRGA as measured during field calibrations. It is different from what presented in this preprint, strongly bounded to the EC method; however it cannot be omitted in a paper dealing with the drift of the IRGA. Some publications on the theory beyond the IRGA working principles, from which the working equations presented are derived, should also be included, in addition (and in support) to the ones in the LICOR manual (not peer-reviewed). Also, publications dealing with uncertainties in EC method should be present, if the link with EC (#1) is maintened

->5. if some more applications are described, I would opt for a separate section of the paper, and for enriching the discussion section with discussion, citing different papers and going more details on what the results suggest.

->6. with the improvements above, the conclusion section will become more robust

abstract
13-14: as the IRGAs can be used for several scopes, if the link with the EC is maintained it is preferrable to mention "fluxes" (#1)
14: As the focus of the manuscript seems to be only the IRGA, and not the sonic, this should be clearly stated (#1)
20-22: please specify that it refers to IRGASON/EC150 only: it seems to be a generic statement for OPEC systems (#2)

introduction
38-39: If the link with EC is maintained, It may be relevant in my opinion to mention that the exactness of EC measurements depends also on this, but not only. There is a long way to get to the fluxes after the field measurements, and each step sources uncertainty. This should be mentioned in my opinion, also referencing the papers dealing with other sources of uncertainties. (#1)
55-56: this is likely the case. However several publications exist trying to quantifying the uncertainties of EC measurements: this should be mentioned and the difference between this study discussed (here we are dealing with the exacteness of the measurements of the IRGA only, there they are considering the EC flux. In some works the instrumentation uncertainty is included in the overall uncertainty). (#4)

specification implications
91 (tab1): if you want to make it more general, you should specify whether or not this list is sufficient and necessary: what if a different sensor is missing some info? And what if there are more sources of uncertainties listed for a sensor? This should be reported (here and/or in Appendix A) (#2)

Accuracy of CO2 density measurements
148 (eq 5): while I think this equation is general, as it is proposed in a sensor's manual (i.e. not peer-reviewed) in my opinion it is not very robust to include it in a scientific paper without an indepth analysis. As sources are present in LICOR's manual, I would prefer to see it derived from there. Otherwise, in addition to not being scientifically robust, this may also be felt as ambigous in terms of at which sensors can be generalised: its applicability at sensors other than the one the manual is referring to should be shown (IRGASON and beyond). In alternative, if some other publications exist that already "validated" LICOR's equation, they could be referenced here. Then, the parameters in the equation can guide the reader in understanding its applicability, e.g. all the IRGAs using a 5th order polynomial for CO2, etc. (#2, #4)

CO2 zero drift uncertainty
173: ref needed. Indeed, other reasons for the drifts are: dirt contamination, ageing of the IRGA's components, errors in pressure correction (absorptances are normalised to P), and errors in field calibration. If only Ta has to be considered, all of the other sources

should be assumed to be zero - which should be at least mentioned. See also Fratini et al. 2014 (#4)
195: For EC150, not for OPEC in general (#2)
203-204: these values are again for the EC150 only. (#2) please also note that "rh" may be misunderstood for relative humidity

CO2 gain drift uncertainty
211: again, if it has to be generic, sentences from LICOR manuals shouldn't be used alone, as 1. they are not peer-reviewed and 2. things could be different for different models (#2, #4)

Evaluation of ΔρCO2
271: Again, it should be noted that these specs, and then the results below, are relative to the EC150, including the operational range: are you sure you can generalise to all the OPEC systems? (e.g. LICOR LI7500DS has a range of -25 to 50°C) (#2)

Δρ H Oz2 (H2O zero drift uncertainty) and Δρ H Og2 (H2O gain drift uncertainty)
323: see the comments in section 4, in particular at line 148 (#2, #4)
335: I think an important point should be taken into consideration here: Tc must be significantly lower than Ta at the moment of field calibration for H2O span to avoid condensation (3-5°C, as reported in the LICOR manual) (#3)

sensitivity-to-CO2 uncertainty
347-348: I understand the logic behind this, however some evidence should be provided that this is the case, against the case, for example, that the lowest cross sensitivity to CO2 unc. is with CO2-free air - or viceversa in the CO2 case, that the lowest cross sensitivity to H2O is with dry air and not with a "stardard" water vapour concentration (somehow related to #4, and to #5 as it could be matter of discussion)

H2O measurement accuracy
360: here could be a good candidate to mention the generalisation point (#2)

ρH2O range
367: please consider rephrasing: this is a plausibility range, and the calibration range of both EC150 and LI7500. It is likely the same for most analysers, but again I think it can't be generalised in absolute terms. (#2)

Discussion
393: in general this section is often more a (even useful) recap of what has been done and an application study (also useful) than a discussion of what done, also against other studies (#5)

Accuracy model
407: here it is a good candidate to discuss the fact that any other uncertainties are lacking in the model (#2)

Formulation of uncertainty terms in Model (2) for accuracy equations
424-426: this is quite a critical point: I agree that such a relationship is not modeled yet, and that considering the maximum range is what the users may want and understand; however, I think assuming a linear scaling of the uncertainty and including it in the computation is a bit risky. At least, how far from the actual uncertainty is that one? Some more discussion needed, also checking Fratini et al. 2014 (#5, #4)

Use of relative accuracy for infrared analyzer specifications
439: applications should probably go in a dedicated section. However, the first two suggested applications of relative accuracy is just a way to define sensors' specs, then in my opinion they should be just mentioned, not reported in such details. (#3, #5)

Application of H2O accuracy in data use
473: As you are considering eddy covariance applications, mentioning only Ta is a bit reductive in my opinion (no user will buy the IRGASON to calculate Ta...). Also, Ta is more related to sonic temperature Ts, and here you are only considering the IRGA uncertainties, not the sonic ones: deltaTs is reported in the sensor's specs, right, as it is deltaCO2 and deltaH2O. Ts is probably less sensitive (e.g. not cross-sensitivity present), but still can drift with temperature for example (see for example Mauder et al. 2007 https://doi.org/10.1007/s10546-006-9139-4). (#1, #3)

Application of accuracy equations in analyzer field maintenance
490-491: this is correct and probably the most relevant part of this section. However, this is strongly related to the drift uncertainty that is rescalded to the difference Tc-Ta, and this is said above to be not exact (@425), and is also based on the assumptions that only the Ta dependency impacts the drits. As no other demonstrations are given, this is also not very robust in my understanding (#5)
500-502: this is also a good place to discuss Fratini paper, which is based on field calibration data (#4, #5)

CO2 zero and span procedures
512-513: it is also true that the widest possible range would apply only if calibrating in extreme conditions far from the daily avarage (#3, #5)

H2O zero and span procedures
524: see comment at line 335: the span procedure with a dew point generator MUST be performed at a much lower temperature than ambient to avoid condensation in the tubes and a bad calibration. This should be mentioned (also, does it worth it to "risk" to perform

a bad calibration for correcting this? This is probably out of topic for the manuscript, but a short note could be beneficial to the reader). This risk is also reported in the LICOR manual (a note on "Checking the span" section) (#3, #5)

H2O zero procedure in cold and/or dry environments
568: again, the worst case scenario is also less likely... (#3, #5)
571-572: ref. needed (#4, #5)

conclusions
577: some of the comments above clearly applies to this section as well (all the points)
617-618: this suggestion is mostly for sensors producers (#3, #6)

- technical corrections

abstract
25: "narrow the accuracy" is improving it? Please consider rephrasing

introduction
48-49: such an example at the beginning of the intro is misplaced in my opinion. Also, Ts accuracy is not under discussion. I would keep it for later
64: also CPEC
72: to be more clear: density measurements. It is probably worth it to state that in the manuscript CO2/H2O measurements alway refer to density, not flux (as EC techinque estimates CO2/H2O fluxes)
83: a (typo)

specification implications
88: amount

Accuracy model
128: this is matematically shown in Appendix A: please clearly refer to it (not only later)
133-134: not clear: please consider rephrasing

Accuracy of CO2 density measurements
143 (eq 4): under the assumption that the errors are normally distributed? Please also specify that sigmaCO2 is the std. dev of...
166: please consider expliciting here which parameters of eq. 5 are defined at the factory, and which ones can be corrected by field (or lab) calibration, even if reported in details later.

CO2 zero drift uncertainty
168: is that part of the experiment? not very clear how it relates to the rest
170-171: bad wording
180-182: I feel it as a "manual-like" text. I suggest avoiding expressions like "must be simple", "indeed", and be more descriptive
197: what do you mean? Almost?
199: respect to...?

CO2 gain drift uncertainty
207: see comment at line 168. I think you are referring here to what is done at the factory during production and/or recalibration. If so, please explain better
214-215: this is correct, this is what is done in 80-90% of the cases. However, there exist the possibility to perform more than one span calibration, e.g. one slighlty below the ambient CO2 concentration and one at a much higher value, to have a better reconstruction of the sensor behaviour: this should be mentioned in my opinion
216: yes, but you can adjust it twice in the case of two span calibration (in LICOR IRGA, actually this parameter is a linear function relating absorptance to density, and what is set by the software is the offset, as the slope is fix and determined at the factory)

sensitivity-to-H2O uncertainty
255 (eq. 12): please recall to the reader that 44 gH2O m-3 is a threshold for H2O concentration in air based on dew point values

Accuracy of H2O density measurements
318 (eq. 16): where sigmaH2O is the standard deviation of the random errors...

Δρ H Oz2 (H2O zero drift uncertainty) and Δρ H Og2 (H2O gain drift uncertainty)
324 (eq. 17): even if defined earlier in the CO2 section, it is probably worth it to report again what Ac, Aw etc are. Probably a symbols list would help the reader
333: I would also mention the same assumptions as above (i.e., rhoH2O is the closer proxy for true rhoH2O)

sensitivity-to-CO2 uncertainty
348: typo, subscript should be H2O
351: typo

ΔρH2O range
385-386: At which Ta?