Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-302-RC2, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Water vapor density and turbulent fluxes from three generations of infrared gas analyzers" *by* Seth Kutikoff et al.

Anonymous Referee #2

Received and published: 12 September 2020

Review comments on
Water vapor density and turbulent fluxes from three generations of infrared gas analyzers
by S Kutikoff, X Lin, SR Evett, P Gowda, D Brauer, J Moorhead, G Marek, P Colaizzi, R Aiken, L Xu, and C Owensby
for **Atmospheric Measurement Technology:** MS no. amt-2020-302.
Date of first review: August 14, 2020; Date of the present (second) review: September 11, 2020.

Summary Comment

This manuscript is topical and informative and should be useful to the micromet community. It is certainly appropriate for AMT. My specific comments and recommendation follow.

1 Overall the paper is clear enough, but I still think the use of English could be improved.

2 Lines 133-134 - The authors state "Implausible values of 20 Hz data, defined as greater than 30 g m^{-3} or less than 2 g m^{-3} , were removed . . . ." This range of values poses a bit of a puzzle to me. Why/how were these max/min values chosen and why are they implausible? I think the authors should include a histogram of the noise spikes. They really need to say more about their criteria for noise/spike removal. I will also note that mentioning "Welch's periodogram method" and citing Blanken et al. (2003) (Lines 139-140) does not really address or answer my concern here. Is it possible to show a Welch periodogram and discuss the details relevant to how these "implausible" values were determined?

3 Lines 156-157 - The authors state "Based on spectral losses and other corrections, E was calculated iteratively." This statement needs some clarification. What other corrections are involved and why does E need to be calculated iteratively? It would be helpful to show the equations and explain the need for the iterative approach.

4 Lines 165-166 - Here the authors state "The measured λE is assumed to be the difference between the actual flux and these errors (Lasslop et al. 2008)." This statement also needs some clarification. I do not understand the point of the referring to Lasslop et al. (2008). What exactly does Lasslop et al. (2008) show that is relevant to the authors' study in general and this specific statement in particular? What is the significance of or the need for the Lasslop et al. (2008)

reference. Do Lasslop et al. (2008) state something, either explicitly or implicitly, that is relevant to manuscript that could be restated for clarity?

5 Lines 166-173 - The definition and discussion of the systematic error must have at least one unstated assumption, i.e., that there are no comparable errors in the heat flux. While this may be true for many eddy covariance systems I don't think one can assume, a priori, that it is universally the case. Could I not define a systematic error (say δ_H) associated with the heat that mimicked Equation (2), i.e., $\delta_H = H(1/ERB - 1)$? If so, what exactly does this mean to the value and utility of using Equation (2) to define the systematic error associated with λE?

6 Lines 215-216 - Here the authors state "After this time, the LI-7500RS appeared to be more stable, with steady $rmsd$ over the final days compared to the other two instruments." This statement also needs some clarification. Because they define $rmsd$ with Equation (1), but this does not seem consistent with their statement. The problem is that they claim that one sensor is more stable than the others, but the $rmsd$ is defined as the difference between two sensors. So how can they claim that the $rmsd$ is a property solely of one instrument?

7 Lines 390-392 - Here the authors state "While it was paired with a different sonic anemometer than the other two IRGAs, flux differences were attributed to differences in variance of turbulent fluctuations of water vapor rather than sonic anemometer error." At the very least this statement is out of place. It should included in **2.2 Data processing and statistical analysis** or **3.3 Water vapor fluxes** or maybe a separate section devoted to discussing the influence that uncertainties in the other Non-IRGA instruments might have on the present IRGA results. My concern is that there have been at least half a dozen papers in the last 8 years (starting with *Kochendorfer et al.: 2012, Boundary-Layer Meteorology, **145**, 383-398* to the most recent *Frank et al.: 2020, Boundary-Layer Meteorology, **175**, 203-235*) about sonic transducer shadowing errors causing systematic

underestimation of w'. (Note: the other recent sonic papers will be referenced in *Frank et al. 2020.*) So that means the some errors in the water vapor flux that are ascribed solely to the IRGA are in fact caused by the sonic itself. Just how much of an impact does this assumption make on the results of this study? In addition, if w' is biased low, the heat flux, H, will also suffer from this bias. So what impact does this have on the ERB, Equation (3), and the systematic error δ, defined in Equation (2) and ascribed solely to the λE? How certain are the authors that δ is not dominated by the bias in the sonic vertical velocity rather than errors inherent in the IRGAs? I think the paper would be strengthened if the authors performed a sensitivity or error analysis to estimate how much of δ is related to non-IRGA errors and how much of δ can reasonably be ascribed to an IRGA.

Recommendation

The paper is acceptably written, but the writing could be improved. I don't think that the statistical analysis is well described. Furthermore, I think the paper approaches this instrument performance problem in a manner that is a bit naive and simplistic. They use the energy balance ratio and its closure as a measure of hygrometer performance. But the measurements of R_n, G and J are not free of systematic error or bias. Nor is the sonic necessarily free of bias. How then can they be certain that just because the LI-7500 produces a better closure that it performs better that the other two generations of the instrument? Additionally, they do not discuss possible biases and errors in the lysimeter measurement of ET. I think all sources of errors and uncertainties need to be at least acknowledged in their study. And I think the paper would be further improved it the authors tried to quantify or partition δ into IRGA and Non-IRGA contributions. Finally, although I would not require a Bayesian statistical approach to their instrument comparison study, I think their efforts and analyses would benefit greatly from such an approach. A Bayesian analysis would allow the authors to build in estimates of the uncertainties associated with the lysimeter and the energy balance instruments.

In turn, this would allow a much more realistic estimate of the inherent uncertainties in the different versions of the LICOR hygrometer and therefore a better estimate of their performance relative to one another. Nonetheless, I recommend the paper for publication after significant revisions.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-302, 2020.