

Interactive comment on “Measuring turbulent CO₂ fluxes with a closed-path gas analyzer in marine environment” by Martti Honkanen et al.

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General Comments

The work laid out in this methods paper is thorough and up-to-date, with the current best practices in the field. The researchers designed and clearly described what appears to be a functional eddy covariance tower for measuring CO₂ fluxes in a marine environment. Which is not an easy task. With regard to scientific quality, I believe the authors have done an excellent job.

There are several areas that the paper could be improved. Broadly, the paper needs to stay more focused on CO₂ flux measurements. Several analyses seemed unnecessary (e.g., roughness length and similarity theory), veering away from the overriding

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goal. These could be replaced by a more in-depth assessment of the performance of the system with regard to CO₂ flux (e.g., gas transfer velocity or bulk flux comparisons). Also, because the method (i.e., dried closed-path IRGA) has been presented in previous papers, this paper would improve its contribution by delving a little deeper into the challenges facing the system (e.g., distorted spectra). Doing so could enable those deploying similar systems in the future to address those challenges and improve data retention.

This is a good paper. The work is thorough, the methods are sound. I recommend it for publication and look forward to seeing the science papers that follow.

Specific comments

The paper should attempt some way to corroborate the magnitude of the measured fluxes with previous results. While we have no reason to doubt that the fluxes are good, we also have no evidence that they are good. Accomplishing this should be straightforward given the fact that waterside pCO₂ was measured. This could be done by presenting gas transfer velocity. Or it could be done by comparing against bulk CO₂ flux calculated using an existing parameterization for gas transfer velocity.

The percentages of data lost due to non-stationarity (63%) and wind direction (51%) are reported separately. It's worth reporting somewhere in the paper the combined loss / total percentage of data that made it to the final analysis.

Page 3 – line 13 –The closed-path design does not automatically mean increased sensitivity to motion. Miller et al. (2010) found that the LI6262 and LI7000 were more sensitive to motion. This doesn't apply to all closed-path IRGAs on the market (e.g., the LI7200 [closed-path], which has the same internal design as a LI7500 [open-path], doesn't show the same degree of motion sensitivity as those named by Miller et al. [2010])

Page 5 – I like the photograph (Figure 2). It makes the case that the virtual impactor

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is necessary. But it seems like a photograph that is a little closer to the tower, and that shows the instrumentation a little better, would be more helpful here (where you're describing the physical design of the system) and that this one could potentially be moved to the Appendix. Just a thought.

Page 6 – How do you generate your purge air for the nafion? And zero gas for the LI7000?

Page 7 – line 14 – Suggest removing the “flow” in “stationary flow conditions”. It's not purely flow that is involved ($w'c'$).

Page 7 – Section 2.3.1. Spectral analysis:

Here you've applied a limit of $|z/L| < 0.05$. But later, in Figure 8, it appears that about half of the data fall outside of this range. Why did you apply this more stringent criteria? Was there something wrong with the spectra outside this range? Or was it just so the peaks of your spectra lined up more cleanly?

Within the more limited selection of 612 observations, over half are discarded because they are 'distorted.' I assume this is so you can use the 'good' spectra to get a reasonable/workable transfer function. That makes sense. But then are you applying that transfer function to correct all observations that pass the stationarity and wind direction criteria, even the ones with distorted spectra? The transfer function should not be expected to fix the fluxes for these other intervals.

I think this is an area of the paper that can be developed. If the quality control for CO₂ fluxes (shown in Figures 6 and 9) consists only of the wind direction and stationarity criteria, then the distortion of spectra mentioned in this section needs to be addressed. What is causing the 365 of 612 intervals that satisfy $|z/L| < 0.05$ (and presumably some percentage of intervals with z/L outside this range) to have distorted spectra?

Is the distortion in the spectra from low or high frequencies? Or a consistent spike? Does it happen under specific environmental conditions? Is there some way to mea-

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sure and account for the error such distortions likely introduce into the measured flux? The shape of the spectra, especially if they're consistent, will be useful for indicating what problems may be affecting the tower. Maybe it's low frequency contribution coming from the residual water vapor, maybe the tower is swaying, maybe there is high frequency noise from the IRGAs. If you can give the readers more information here it will help those who plan to deploy similar systems to address those challenges.

Page 9 – Figure 3 – When printed the dashed cyan line on subplots d and e is not visible when it overlaps with the blue line. Might want to consider some way to increase the contrast.

Page 10 – Section 3.1.2 – With respect to z_0 , no one was going to complain that an extended open water surface does not satisfy horizontal homogeneity conditions for EC measurements. Yes, the figure does show that you've successfully selected the right wind direction window. But that seemed obvious just from your map. And you could always just include one sentence that says it was confirmed because z_0 was consistently below 1 mm in that window. There isn't anything wrong with this section, but it is not particularly necessary.

Page 7 – line 3 – Here you've handled the tube delays by taking the maximum covariance. I know this is a common practice. But my experience has been that it often results in the selection of fluxes which have large contributions from frequencies outside the expected range. Have you calculated the expected tube delays based on your known tube lengths, tube diameters, and flow rate? How well do the maximum covariance lags match the expected lag based on this calculation? If you base the tube delay on this calculation do you see improvement in the percentage of spectra that are distorted (page 7 – line 26)? I don't think this necessarily needs to be reported on in the final paper, but it's worth checking.

Page 11 – line 2 – What is the average high-frequency correction?

Page 11 – line 15 – How do you handle the range of f_0 (e.g., 0.04 to 0.22) in correcting

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the attenuation of latent heat flux? Did you calculate the different transfer functions for different relative humidities based on their f_0 ?

Your assertion that the flux attenuation (for RH=80%) is 44% of the real flux would be strengthened by a comparison of your result to an empirically-based model (e.g., COARE [Fairall et al. 2003]).

Not critical, but since you are using these latent heat fluxes to interpret your CO₂ fluxes it wouldn't hurt to show the CowH₂O cospectra of the undried system.

Page 12 – line 8 – So the reader doesn't have to flip back several pages to Figure 3 it would be helpful here to place a reminder that all the CO₂ fluxes in July were expected to be negative. Maybe insert something like “ (all of which were expected to be negative) ” after “July 2017”.

Page 14 – Section 3.1.5 Turbulence: I think this section is unnecessary. It distracts from the main purpose of the paper (CO₂ fluxes). In reading it felt like a distinct transition to a new subject. One which required more effort than the payoff was worth. I think this section would be better left to its own paper, when the authors have developed it further and have the space to discuss the implications. For this paper, there was no practical application. If there is a reason that this relates to the CO₂ fluxes that you've shown then that reason should be made more clear.

Technical Corrections

Page 3 – line 26 – Word choice on “effortless”. Might want to go with something like more feasible, practical, or straightforward (or conversely ‘not as logistically challenging’).

Page 3 – line 31 – Grammar: “and the drying of sample is straightforward to implement” to “and allows for straightforward implementation of sample air drying” (or similar)

Page 5 – Citations at the ends of each paragraph should be moved inside the period.

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Page 5 – line 17 – Change “A . . . tower is placed” to “A . . . tower was placed”

Page 7 – line 6 – “was considered” to “were considered”

Page 7 – line 23 – “based on the criterion that $R_{Nw}c' < 0.3$ ” would read better if it were enclosed in parentheses rather than commas. This would match the way you've presented the wind direction criterion.

Page 8 – line 3 – Consider reorganizing the sentence that begins with “To correct for. . .”. It is difficult to read.

Page 8 – line 26 – “seasons of carbon cycle” to “seasons of the carbon cycle”

Page 10 – line 13 – “Especially, a large swell . . .” is an incomplete sentence.

Page 14 – Figure 8 – “f” in the xlabel of subplot b should be “fc” for consistency with how it's written in the text.

Page 16 – line 25 – “the difference between the dried and undried sea-air CO₂ flux measurements were very similar” needs to either become “the difference between the dried and undried sea-air CO₂ flux measurements was small” or “the difference between the dried and undried sea-air CO₂ flux measurements were very similar.”

Also, the second part of this sentence doesn't work as it's written because the subject of the sentence changes.

Page 18 – Figure 11 caption – move “(standard setup – test setup)” to immediately after “CO₂ flux difference”

Throughout the text hyphens are used for minus signs.

References

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Miller, S. D., Marandino, C. A. and Saltzman, E. S.: Ship-based measurement of air-sea CO₂ exchange by eddy covariance, *J. Geophys. Res.*, 115, D02304, doi:10.1029/2009JD012193, 2010.

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