Responses to Review of Anonymous Referee #2

We would like to acknowledge the anonymous referee for his/her useful remarks and comments which have helped to improve the manuscript. All comments have been addressed as detailed hereafter in blue.

Anonymous Referee #2

This paper presents a comparison of water vapor flux measurements made by open and closed-path sensors above a deciduous forest in China. As the author's point out there is a paucity of comparisons to enable good quantification of measurement uncertainty and bias by different water vapor senors. This paper will provide a useful service to the community and valuable data. However there are some additional points and analyses that need to be considered in order to make this intercomparison as fair and useful as it can be.

Response: thanks for this positive comment. We have carefully revised the manuscript according to below constructive and helpful comments.

page 4715; line 20, The description of calibration procedure is incomplete. What is the balance gas for the CO2 standard? Note that nitrogen rather than air introduces a bias because the spectral characteristic of air and N2 are different. What if anything is done to check the calibration for water vapor? This is critical to assure that none of the bias between the two sensors comes from difference in calibration.

Response: Thank you for your very careful review of our paper. We used nitrogen gas as the balance gas for the CO_2 standard, and the calibrations of the water vapor span were performed simultaneously using a dew point generator. We have added this information in the Section of Materials and methods. We acknowledge that the use of nitrogen as zero gas may possibly cause bias, while we haven't found any literature or publications concerning this topic, and this is also out of our ability to clarify such effect. Actually, it is routine to take nitrogen as zero gas in the FLUXNET community (please see below example).

Novick et al. 2013, Agricultural and Forest Meteorology 181:17– 32: Calibrations of the analyzerwere performed weekly using an ultra-high purity N2zero gas, a1000 ppm CO2span gas (balanced with nitrogen), and a dew pointgenerator (LI-610, Li-Cor Biogeosciences, Lincoln, NE) for watervapor span.

Aires et al. 2008, Agricultural and Forest Meteorology 148: 565-579) : Calibration of the IRGA was done once a month using nitrogen gas and 350 ppm CO2 gas to calibrate the CO2 and water vapour zeros and the span of CO2, respectively.

Pg 4718, Line 14; The comparison in figure 1 is not at all convincing that the two analyzers are in agreement. A longer set of data is needed to show that they are in overall agreement. Comparing the mean values from each analyzer by plotting one against the other to confirm that there is not a bias or offset between the two should be the first step. In comparing the high frequency, which analyzer provides the true signal? It would help to show temperature or CO2 in order to demonstrate whether the high frequency variability in the open path sensor is real atmospheric variation or instrument noise. Secondly, use spectral analysis to more conclusively demonstrate the point that the closed path sensor is attenuating some signal.

Response: as suggested, we have replotted Figure 1 and updated it with a longer dataset (30min raw data). We also added a figure (Fig.2) to show the typical spectral characteristics for the open and closed path data, and the data quality assessment was further conducted in the revised manuscript.

Page 4719, first paragraph You need to note that long lag and dependency on RH is a problem

with the inlet tubing and perhaps a function of the material used, or an accumulation of foreign material on the surface. Different tubing materials (such as teflon), or having fresh clean tubing may give different results. The problem with lags is not inherent to the instrument, though it is unavoidable to some extent when there is an inlet. Nevertheless, a useful methods comparison should assess how well the problem can be corrected by choice of tubing material.
Response: We acknowledge that the time lag is probably a function of the tubing materials or accumulated material on the inner surface of the tube but we don't think it is necessary to address this problem specifically in the manuscript, because as we know, for the flux site with the closed-path eddy covariance system, the Teflon is the most commonly used material with characteristic of high thermal stability, water resistance and flexibility. Besides, no matter what tube material is used, the time lag is mainly the function of the flow rate, tube length and inner diameter.

Page 4719, line 19. Once you have demonstrated that a fixed lag time is inappropriate no further comparison should be made between the open path and closed path data calculated with a fixed lag. Please make it clear at this point in the text that fixed lags are not used in any of the subsequent analysis.
Response: we agree, We have checked the following paragraphs to make sure there is no such problem occurred as above mentioned.

There also needs to be some discussion around this point about what corrections have been made for loss of high frequencies. An important question to answer is whether or not the data from close path sensor can be adequately corrected using the best available data processing tools.
Response: we agree. We have added below description to the text to further clarify this topic.
"All sensors are affected by high-frequency damping due to several reasons including instrument time response limitation, sensors separation, etc. Closed-path systems are specifically affected by the damping due to fluctuation attenuation in the sampling tube. Therefore, the spectral corrections are especially important for closed-path analyzers. In this study, Spectral corrections for high-pass filtering and low-pass filtering were implemented following Moncrieff et al. (2004), and Fratini et al (2012), respectively. For OP system, spectral losses due to instrument separations were account for according to Horst and Lenschow (2009). The results showed that the corrected LE values were generally on the order of 6.2–11.0% of raw fluxes, while the CP system measured LE received considerable spectral corrections ranged from 6.7 to 11.5 % of the raw flux signal."
"

Section 3.5. There is no need to separately compare the results for water exchange and latent energy. The underlying measurement is water vapor flux. Conversion to energy flux is made using the heat of vaporization that is the same for both data sets
Response: we agree. As suggested, we have combined Section 4.4 and 4.5.

Page 4721, line 17. I do not think it is correct to state that the Fluxnet community generally accepts that Open path sensors need less maintenance. Each of them have different needs and present different issues that are more or less important depending on the site characteristics. This paper should just focus on presenting the comparison between the two analyzers and the question of how large is the difference after applying the best data processing approaches to each.
Response: we partially agree with the comment. Both of these two systems need careful maintenance, while according to our experience, as well as the other researcher's opinion (eg., Haslwanter et al., 2009) , the operation and maintenance of OP system is more simpler. The Campbell Scientific company posts its product brochures on its website and also states that: "Open-Path Versus Closed-Path Eddy-Covariance Systems: Simpler operation and maintenance". Please check the below website for this information: https://www.campbellsci.com/ec150-ec155.

Despite this, to avoid controversy, we replaced the sentence in the manuscript with:
"Generally, open-path eddy covariance system need less maintenance as compared to closed path systems (Haslwanter et al., 2009) and are more suited for field stations without power supply."

Line 23. Data rejection because of failing integral turbulence and stationarity tests ought to apply to both analyzers equally if it is function of the local site's turbulence structure. Unless you are suggesting that the closed path sensor fails this test, but the open path does not, then it is not a drawback of the sensor. If the number of points rejected by these tests for the two sensors are different, you need to give some explanation of why. Compare the amount of data that are retained by each after the turbulence and stationarity are excluded.
Response: It is commonly reported that a larger percentage of CP flux data failing on the stationarity test (eg., Haslwanter et al. 2009. Agric For Meteorol. 149(2): 291–302; Nordbo et al., 2012. Tellus B, 64, 18184, (Flux stationarity for OP system: 28.15%, for CP system: 33.08%). All these suggest that though the CP system yielded more physically plausible data as compared to the OP system, the quality of these additional data was compromised by non-stationarity of the time series. The possible reason for this is that the stationarity and intermittency criteria filter out LE data, especially during night with high relative humidity. For CP system, as we demonstrated in Fig.2, the time lags are very variable in high humidity condition, and hence calculated subperiod (5-min) fluxes, this leads to a high non-stationarity based on closed path data. Additionally, the turbulence distortion in the sample tube is also a possible reason.

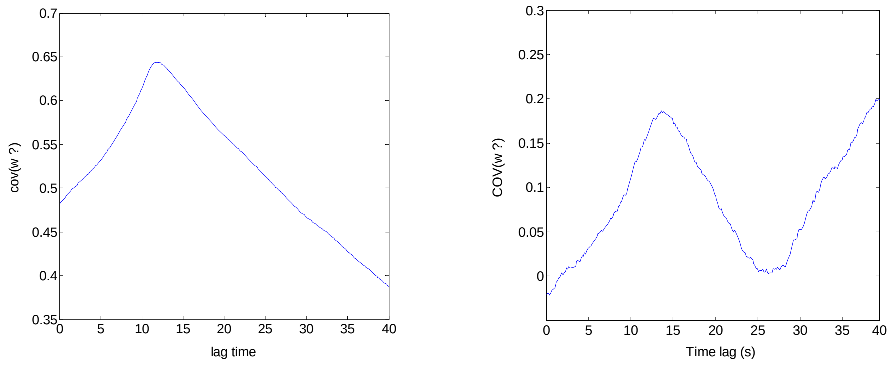
It is a valid point to mention that rainy conditions may cause significant data loss from the closed path, but please include a fair analysis of whether this can be adequately corrected by choosing better inlet material and accounting for the changing lags and difference in tube attenuation.
Response: we totally agree with this comment. However it is out of the range of our data to check this effect; and to answer this question, it will take time to reset our sampling system. Considering that it is not very relevant to the topic of our manuscript, we didn't address this in our manuscript.

Furthermore, in computing water or energy balances, how much water vapor flux actually occurs during rain events and when humidity is near 100%. Surely the rainfall itself transports more than the water vapor, and at 100% RH there is little gradient to support evaporation.
Response: all the data measured during rain events were excluded in the energy balance analysis, just as we mentioned in the manuscript: " the measured fluxes were excluded during precipitation (30-min precipitation > 0.5 mm) and high relative humidity periods (mean relative humidity > 95%)."

Page 4723. As noted above, I don't think additional examination of the results based on a fixed lag time are warranted. It is clearly not correct, so shouldn't be used.
Response: We have revised the text and have removed such wordings as the reviewer's suggestion.

The difference between optimized lag and maximum covariance needs to be further explored. How do you know which is right? If the maximum covariance within a broad window is truly computing unreasonable lag times you should demonstrate this by saving the lags that the maximum covariance method identifies and compare them to the optimized lag method.
Response: This is a very good question. With the Maximum covariance method, if a maximum is not searched within a broad time lag window (i.e., the covariance maximum is appeared at either endpoint of the time lag window), the consequence of using this method may lead to flux

overestimations when the fluxes are small. As shown below, the left panel shows the true lag time (11.2s), while the right panel is an unrealistic time lag (appeared at the endpoint of the time lag window). According to the right panel, the covariance of vertical wind speed and water vapor concentration with 40s time lag is obvious greater than the value with the true lag time (15.0s).



In addition, it would be helpful to contrast the patterns for water vapor fluxes by comparing them to the results for the more inert tracer CO2.
Response: as we stated in the Introduction section of the manuscript, the inter-comparison about the performance of OP- and CP system on CO2 flux measurements have been done extensively, hence in this manuscript, we specifically focus on water vapor flux measurement.

Finally, the points made in this paper about tube attenuation being a significant problem for the close path sensor would be better made by showing spectra and cospectra for the open and closed path water vapor data. Is there indeed a noticeable loss of cospectral power at high frequency? The intercomparison presented in the manuscript is not clear enough about how the known corrections for loss of high frequency variations are applied to each data set. One of the most important questions to answer is whether or not the data from closed path water vapor analyzers can be adequately corrected if the data are processed properly.
Response: as suggested we added a figure (Fig.2) showing the spectral and cospectra for the open and closed path data, and the data quality assessment were further conducted in the revised manuscript.