

Atmos. Meas. Tech. Discuss., referee comment RC1  
<https://doi.org/10.5194/amt-2021-42-RC1>, 2021  
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## Comment on amt-2021-42

Anonymous Referee #1

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Referee comment on "Evaluation and Impact Factors of Doppler Wind Lidar during Super Typhoon Lekima (2019)" by Xu Wang et al., Atmos. Meas. Tech. Discuss.,  
<https://doi.org/10.5194/amt-2021-42-RC1>, 2021

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The paper Evaluation and Impact Factors of Doppler Wind Lidar during Super Typhoon Lekima (2019) is reviewed with the following comments.

### General comments

It is an interesting study on Doppler wind lidar data vs. radiosonde data during and near typhoon conditions at three locations in China. The general impression though is that the background for the study, the results and conclusion need much more work to appear in a final form. There appears misinterpretation and lack of focus on the working hypothesis. The fundamentals on the theoretical insight is too limited and not presented in a clear and structured way. The paper is rejected.

### Specific comments

Lines 16-19 on bias and distance from the typhoon center is not possible to follow in a logical way. This situation also goes for other sections in the paper related such as section 5.3. In case the bias mentioned is shown in Figure 11a and 11b, it does not correspond to the text, and again in lines 472-477 is discrepancy. Overall question is how would you argue that longer distances are giving other results than shorter distances, why and how?

In the introduction several works are cited related to correlation coefficient and RMS but bias is not mentioned. Why is that? It might give a better overview with a table on previous researchers work summarizing their statistics.

Lines 66-74 The text could benefit from more fundamental information on the challenges of Doppler wind lidar and radiosondes in typhoon conditions. It becomes difficult to follow what is the key information and why the measurement techniques is expected to have lower performance than in less windy and rainy conditions. A thought here, is this your hypothesis you are testing? In case so it would be valuable to state this clearly.

Line 81 wording "generally good" is not sufficiently clear

Section 2.1 and 2.2 might be complemented with other auxiliary information if available, e.g. model results of the case and/or local wind speed and direction measurements or from aircraft, to better understand the variability in time and space.

Section 3.1.1. this section would greatly benefit from fundamental knowledge on the two types of lidars. Technical information should appear in the introduction in general terms, so that during the subsequent part of the paper, it is clear what to expect. The specifications on expected accuracy on all the observed parameters from the WindPrint and the WindCubes should be clear. The entire paper is about this topic. Table 2 is included but without further description or explanation (or reference to the values).

Table 1. To clarify, from Bashan there is 43 hours of data, from Zhoushan 24 hours of data and from Taizhou 72 hours of data? It would be valuable to list the number of available valid samples for each. Also in case some samples are invalid, it is relevant to understand this, as it is part of how well given instruments perform.

Section 3.2 is a very basic text and as reader one is left wondering what is the situation when aerosol content, humidity, precipitation, etc. comes into the equations. What effect do you foresee in typhoon conditions. This background would have to be given carefully for both types of lidars, and as well the two radiosonde types. How much more can we expect to trust the various instruments? This remains unclear as is.

Line 191, I do not understand what you mean with "data efficiency", please clarify

Lines 225-229, this text on aerosol is unclear and not precise. It is difficult to follow what you explain. In case it is fundamental background, it should be presented much earlier in the paper.

Lines 245-250 This text on turbulence and the instruments should be given in background, and corrected, as it is unclear what you are stating. The text is loosely structured, it cause confusion for the reader.

Lines 257-260 This is not correct. Figure 5 shows hardly any DWL data below 100 m height range and in plots a), d) and e) the sonde gives higher values while in plots b) and c) the sonde gives lower values.

Line 285 rephrase "this was due" to e.g. "most likely". The interpretation of results is not definitive, so you need caution.

Table 3 Would suggestion to include the number of samples and the bias values.

Section 5.1 On precipitation raised a couple of questions. The measurements were taken on the ground (I presume). Do you assume the rain to be homogenous during the entire atmospheric column? Is this valid, or if not always, what may be the case. SNR (signal to noise ratio) explains how good data you have but it appears as you take is as a measure.

Figure 8a and 8b have different scales. Is this intentional, please clarify and note in the text. It would be relevant to related to these plots and at this time revisit why the dB are so different from the two types of lidars, and what we learn from this. If you are comparing the two plots directly between each other (or wish the readers to do so), it is recommended to use the same scale x-axis (rain intensity) and secondary y-axis (sample size) in both plots.

Lines 386-390 on humidity would be place better in the introduction or background section.

Figure 10, why do we hear about bias here for the first time? It is recommended to report on bias throughout the paper, not only for humidity.

Figure 11, would figure b) be a mistake. Is the bias on wind direction around 65 degrees?

You have written about a small RMS on wind direction but if there is a very significant bias, that would actually be even more important to report.

Section 5.4 and in particular Figure 12 raises many more question than give answers to the data analysis. Either rework or omit. It is not clear what you wish to present and what can be concluded (also keeping in mind the other uncertainties dealt with earlier).

Technical corrections

Line 29, Cot

Line 34 Kopp would be Köpp 1984?

Line 48 carried out by

Line 55 with the height range (several other places you could add height to clarify your text)

Line 61 Korb? Not in your reference list

Line 189 Goit? Not in your reference list

Line 254 Li et al 2018, you have two references of this, so please add a and b to distinguish

Reference list is not in good shape, as the format changes very much. There is several 'gray literature' please remove.

There are many more technical corrections (but not listed).

Further English editing is necessary.