

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2022-5

Anonymous Referee #2

Referee comment on "Validation of StreamLine XR Doppler LiDAR wind observations using in-situ measurements and WRF simulations" by Tamir Tzadok et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-5-RC2>, 2022

General comments

I have some issues with the novelty of the manuscript. As pointed out by the authors in the introduction, a major selling point of the manuscript is that the validation here has a wider range of reference instruments compared to previous validation exercises. However, on the other hand the manuscript only considers one site and a narrow range of atmospheric conditions. Therefore, I am not fully convinced that the manuscript achieves its goal to provide a more general validation.

Another aspect of the manuscript – the inclusion of WRF data – is interesting, but as it is, it does not go anywhere in my opinion. The supporting role of WRF in the interpretation of the lidar measurements could have been substituted with existing literature and reanalysis of global weather models.

The introduction should include more relevant literature and it could be more precise. The methods are too sparse with information on the instruments to which the Doppler lidar is compared. The manuscript would also benefit from further polishing (consistent formatting, correct notation for units, etc.).

Specific comments

Line 23-25: Statement is too general. For example in foggy conditions or in the absence of aerosol other instruments are preferable.

Line 27: "Emitted laser beam" would be more precise than "reference".

Line 28: Pearson (2009) and subsequent citations therein might be a better references here, because they relate to the instrument used in this study. (Pearson, G., Davies, F., & Collier, C. (2009). An Analysis of the Performance of the UFAM Pulsed Doppler Lidar for Observing the Boundary Layer, *Journal of Atmospheric and Oceanic Technology*, 26(2), 240-250).

Line 28-30: Statement is too general. I recommend listing specific advantages over a particular measurement technique.

Line 31-34: The provided examples should be supported with references to literature.

Line 39: More importantly, the mathematical manipulations require certain assumptions on the state of the atmospheric flow.

Line 43: Please provide references for the various studies mentioned.

Line 45-47: The following study also validates this lidar type against a tower: Newsom, R. K., Brewer, W. A., Wilczak, J. M., Wolfe, D. E., Oncley, S. P., and Lundquist, J. K.: Validating precision estimates in horizontal wind measurements from a Doppler lidar, *Atmos. Meas. Tech.*, 10, 1229–1240, <https://doi.org/10.5194/amt-10-1229-2017>, 2017.

Line 83: The site description could be more precise. What is the topography surrounding the site? Are there surface roughness elements like plants, trees or buildings and what is their approximate height (important to know if the measurements might be affected by the roughness sublayer)?

Line 105: Specify which frequency that is (e.g. the laser-repetition frequency or the sampling frequency of the return signal).

Section 2.2, 2.3 and 2.4: Information provided for the instruments other than the lidar is thin. What is the accuracy / precision for those instruments? Did the tethered balloon record its position and drift? Why the random sampling time for the tethered balloon? On which site of the tower were the instruments located (tower effects)? How often were the radiosondes launched?

Line 157-159: How many pulses were averaged for one estimate of the radial velocity?

Line 213-214: Doppler lidars are also able to observe the turbulence state from the variance / standard deviation of the velocity, which can provide information on atmospheric layers, too.

Line 230: Is there a significant trend between the average wind direction difference and the wind speed?

Fig 3a: I counted something around 67 data points, which is far less than I would expect for a three-day period. Is the difference explained by the filtering criteria of the LiDAR SNR and the height deviation of the tethered balloon? If yes, then that should be explained in the text and stated how much data was rejected. Also, the variable names and formatting of minus sign in equation of the linear regression could be improved.

Caption of Figure 3: Unless I missed it earlier, this is the first time that the duration of the measurement campaign is mentioned. That information should be provided in the main text at the beginning of the results or in the methods.

Line 240-241: Is there also a difference between the two height levels? One would also expect to see larger differences closer to the surface. If the authors can extract stability information from the measurements, the dependency of the errors to the stability could be also interesting (as it affects the horizontal homogeneity).

Figure 4a: As for the previous figure, the number of the data points is not clear to me. Is the figure showing both tower booms together? Has there been some filtering criteria applied to the data?

As it is, I believe there are not enough data points for both booms and too many for a single boom.

Figure 5 and 6: It should be clarified if the SNR threshold from the methods section applied to those figures.

Line 350: While wind direction seems to agree, the wind speed is overestimated quite a lot by the model (more than a factor of two at times it seems). Therefore, I recommend providing objective error values instead of using satisfactorily to describe the agreement.

Figure 9 and 10: It might improve clarity of the figures, if the wind speed and wind direction information is separated into two panels.

Figure 11: Indicating the times of sunrise and sunset would help following the discussion.

Line 407-408: Sentence should specify, that the preference of 60° over 80° is for the extraction of wind direction and wind speed (because it might be different, if one would extract other quantities from the measurements).

Also, the limitations of the study should be highlighted. Only one site is considered here and the conditions only covered what is considered a radiation driven diurnal cycle of the atmospheric boundary layer.

Technical corrections

Line 10: Capitalize "D" in Doppler. And I recommend checking the journal guidelines for the capitalization "Lidar".

Line 45/46: Inconsistent spelling of "Stream Line".

Line 68 and 70: Consider using "horizontal" instead of "lateral". Lateral is some fields used to specify a velocity component perpendicular to the streamwise direction.

Line 74: I believe the abbreviation WRF was not introduced.

Line 90: Dayan and Ednizik (1999) are also a good reference here. Dayan U, Rodnizki J (1999): The temporal behavior of the atmospheric boundary layer in Israel. J. Appl. Meteorol. 38(6):830-836.

Figure 1: I might be wrong about this, but I believe the acknowledgment policies of Google also require the acknowledgment within the figure itself.

Line 96: I recommend spelling out "four" here (similar instances throughout the text later on e.g. line 123,).

Table 2: Check units and number formatting throughout the table. Also, is the angle range reported for the zenith correct?

Line 125: Spaces between number and unit.

Line 143: Double citation.

Line 147: Remove brackets from citations that are embedded into the sentence (also in line 166, 177 etc.).

Line 151 and 211: Formatting of the subsection heading.

Line 184: Remove "out".

Caption of Figure 3: The beginning of the figure caption is usually capitalized.

Line 239 and 246: The "s" in "m/s" should be written with a power to "-1".

Line 246-247: Inconsistent use of spaces.

Line 254: As it is a continued discussion of Table 2, I believe a new paragraph is not needed here.

Figure 6 and 7: The formatting of dates is inconsistent.

Line 404: Replace "Out" with "Our".