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Atmospheric Measurement Techniques Discussions

Interactive comment on "Analysis of the performance of a ship-borne scanning wind lidar in the Arctic and Antarctic" *by* Rolf Zentek et al.

Anonymous Referee #2

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The manuscript (Zentek et al.) explains how a commercial Doppler lidar (HALO Streamline) is operated on RV Polarstern. The results are compared to standard measurements of radiosondes and sonic anemometers onboard. The lidar was operated during two campaigns in the Arctic and Antarctic. Such measurements in the changing Arctic regions specifically and on the sea, in general, are of importance as those places lack such data. The manuscript focuses on the technical aspects of operating the HALO and analyzing the datasets from the ship. The wind profiles measured from the Doppler lidar agree well with the other sensors, as shown in many other studies before. Although the steps taken to derive wind profiles are fine the authors should write more explicitly what is new (approaches or findings) compared to other similar measurements. One general statement of the manuscript seems to be that the ac-

tive stabilization of the Doppler lidar is not required as shown as in Achtert et al. It should be explicitly stated in the conclusions that this is probably true only for measurements of horizontal winds with the VAD technique. Measurements in PPI scanning mode configurations, or even more importantly turbulence and sedimentation-speed measurements of clouds and ABL vertical-wind measurements are very strongly influenced by the motion of the ship. I found it a bit confusing to see three different things called "NOISE" in the manuscript. There is the signal detection noise in the SNR (which also determines the SNR threshold), then there is the error of the line-of-sight wind estimator (peak-finding accuracy or similar, which is connected to the Cramer-Rao Lower Bound theorem), there are outliers if the wind estimator fails, and finally there is the error of the VAD result on the final horizontal wind by non-perfect compensation of the ship's attitude. These quantities should be differentiated more carefully in the manuscript.

Minor comments: -The abstract is too long and can be shortened. It shouldn't include a motivation and lengthy formulations. Just the facts in a very condensed form. -RMSD is not explained -P1L19: the abbreviation AOI is not really needed as it is never used again in the manuscript. (It could be mentioned that AOI interactions are strongly related to turbulent processes in the ABL which can be observed with a lidar, too. Even though turbulence parameters and not measured here.) -P2: Most of the introduction/literature review deals with the specifics of the HALO lidar and not for Doppler lidars in general. This should be mentioned or revised. -P3L21: Could you please describe the HALO configuration in more detail? How can there be a 3m range resolution? I assume that the laser pulse is much longer? The effect of overlapping gates and non-independent measurements at those range gates should be mentioned. A bit is seen in Fig. 4, but the explanation could be more specific for the zigzag lines. -P4L34: ..."if data quality is not of importance". Better mention the errors with and without correction. This could help a reader to evaluate the effects of a/no stabilization better. -P5L6: How it the SNR defined for the HALO? This is important to follow the upcoming discussion about the thresholds. Some people also derive the CNR to be

more correct. I would like to see a bit more discrimination between those terms here or at P6L17. -P5L20: Could you please find formula signs (one character) for wind speed and direction other than dd and ff? -P5L25: 1st: "has a fixed elevation ANGLE". And 2nd: Is that really true for a ship? -P6L17: Should you even expect that the SNR threshold is the same for every of the HALO instrument? I'm not sure that these thresholds can be really compared. Maybe the laser power/pulse length/DAQ bit resolution is different? Again, it depends, how the dB's are defined here. -P6L22: Is it really true, that the spectra or ACF cannot be stored? One should always try to store the spectra (at least for a while) in low-signal regimes like the Arctic so that later post-averaging is possible to increase the SNR. -P6L26-27: If the PS96 data contain less noise then I would expect that the SNR is higher. How can this mean, that you need a different SNR threshold? This is a bit counterintuitive. Except the so-called "SNR" is the signal and not an actual SNR? -P6L27-30: Please explain why the vertical wind is around 0. This depends on the averaging period, on the precision of the angle of vertical stare (especially on a ship), and sometimes even on the synoptic situation (stationary waves, etc). In fact, you can determine the noise of your LOS wind by looking at the difference between the Autocovariance function at 0 and at the first leg. Or by evaluating the high-frequency tail of the wind power spectrum. And this could be done for different SNR thresholds. -P7L1-12: this paragraph is a bit hard to understand. Can you explain why you do not use the goodness of the VAD fit to determine when a VAD delivered good and bad results? -P7L5: It is mentioned that the VAD results are averaged for 20 min. But since it is possible that there are outliers in those VAD results shouldn't the median be a better indicator here? -P7-8L20: The bias of 10° for the wind direction is quite unexpected. One would assume that radiosondes and the lidar both use GPS? Or is there a magnetic compass involved somewhere which might show a bias in Polar regions? It is a bit unsatisfactory that the reason for this bias remains unclear here. What is the VAISALA specification for their wind-direction bias? -P8L31: here it should be -17dB. -P9L5-15: The occurrence of these 3 fast LLJs is interesting. Can you give any explanation of the processes? Usually, in these latitudes, the typical Ekman oscil-

lation time should be 12 hrs. So there must be another cause for these LLJs. Maybe you could reference some work on Arctic LLJ (e.g. Jacobson, ACP, 2013). -P9L17: A bit general comment: I believe the topic of the paper should not be: The radiosondes are correct and let's see how well the lidar can be verified by this. A Doppler lidar by its design is one of the best methods to measure wind. In the end, it just depends on how precise one can measure frequencies. It is more a question of how much errors are generated by a moving platform like a ship. And what are the best scan strategies on a ship? It would be nice to get some answer to these questions, as other researchers might profit from this. -P9L27: "The RMSD is 10° but we also find 5°". This sentence needs to be revised. -P10L1: "turning the wind perpendicular to the wind". Something is wrong here.

-Fig.3: Can this also be done for the YAW angle? It seems the wind direction error bias is highest. So probably one should have a look at this angle, too. -Fig.5: If Intensity is SNR+1, why does the axis start at 0.99? -Fig.7: It would be nice to include a correlation plot. -Fig.8: What is seen on the y-axis? Height? If it is height, how can there be measurements several hundred meters above the cloud base? Please explain in the text. -Fig.9: Theta can be omitted and the x-axis can be annotated "POT. TEMPERATURE (°C)". Also, the date can be omitted since it is mentioned in the figure caption. -Table 1: The SNR threshold is -20dB, but in the text, it is written -18.2dB is from the manufacturer. So what means "default"?

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