**Review of Achtert et al. (2015), AMTD, 8, 9339-9372:**

**Measurement of wind profiles by motion-stabilised ship-borne Doppler lidar**

**Summary**

A newly developed ship-borne wind lidar, consisting of a commercial wind lidar and a customized active stabilisation platform, is presented in this manuscript. Comparisons to radiosonde winds and a sonic anemometer are shown from a 3-month long cruise in the Arctic. The topic certainly fits very well to AMT, and the deployment of a ship-borne wind lidar is quite novel – only few references from NOAA are reported for these kind of measurements. The deployment during an Arctic cruise is also quite novel.

My main comment is that the manuscript is limited to the statistical comparison and no atmospheric measurements in form of a case study are presented. Also some more evaluations of the statistical comparison must be provided. Thus, I recommend publication after these major comments are addressed.

**General and Major Comments**

1) The manuscript focusses only on the statistical comparison of the ship-borne wind-lidar to radiosondes and sonic anemometer. A short chapter on a measurement case for an interesting or typical situation needs to be added to demonstrate the new measurement capabilities of this lidar for atmospheric research. Figures of time-height cross-sections for horizontal wind speed and direction could be shown and discussed, together with complementary measurements (e.g. ceilometer obs., surface wind, radiosonde profiles). This would add much more confidence in the performance evaluation of this new ship-borne Doppler lidar than showing only statistical results.

2) The innovative part of the manuscript wrt measurement methodology is related to the ship-borne DWL. Only few details w/o any equations describing the correction method are provided in ch. 2.2. Thus more details (e.g. equations or at least citation of the equation numbers in the references, simplified sketch of correction scheme) should be provided here, e.g. about the derivation of the remaining Doppler shift from the pointing or the filtering approach. A figure could be shown with AHRS data and the low-frequency ship data and how they are combined.

Also more technical details about the accuracy of the AHRS sensor should be provided; also the weight of the lidar, which is stabilised is missing. In addition the data source for deriving the platform motion is unclear: Does the AHRS sensor include a GPS for that, or is the horizontal motion derived from acceleration sensors? Where does the low-frequency horizontal velocity correction from the ship´s navigation data come from (GPS?) and what is the accuracy of this? Typical values for the residual correction, e.g. horizontal velocity, ship-induced LOS velocity should be provided, in order to assess the magnitude of the correction in comparison to the atmospheric wind speed.

I would also propose to write 1-2 paragraphs about the performance of the wind-lidar wrt. ship vibrations and other environmental conditions during this Arctic cruise? Any problems/degradations of the lidar performance (e.g. laser) due to the ship vibrations? Any issue with the harsh environment (sea spray, temperature)?

3) The statistical comparison provided in the manuscript lacks some important quantities as the mean of the difference (bias, systematic error) and the standard deviation of the difference (random error) between the lidar and radiosonde/sonic anemometer. The linear fit coefficients do not cover all aspects of a performance evaluation; the intercept of the linear fit is only equal to the bias, if the slope is 1. Also statistical quantities for all altitudes (e.g. in Table 2 and 4) should be given. On the other hand the line "Pearson´s r" could be deleted, because it is already covered by the quantity "$R^2$". The bias and standard deviation of the comparison should be discussed in chapter 3.1, and also included in the conclusion. I consider the discussion in ch. 3.1 as too much limited to the squared correlation coefficient $R^2$; it is only 1 parameter to show correspondence and is dependent on the range of values for the measured quantity. Thus high values are achieved for wind direction ranging from 0° to 360°; I consider the systematic and random error as more important. Thus more room should be given in Ch. 3.1 to discuss these quantities.

4) Abstract: A measurement random error of 0.2 m/s is stated in the abstract. This low value was not derived from the comparison with the radiosonde/sonic anemometer for which most of the statistical comparison values and Figures are included in the manuscript. The value of 0.2 m/s is derived from the lidar measurement itself derived using the autocorrelation technique. I would propose to add (instead of the 0.2 m/s or in addition) at least the main parameters from the statistical comparison from lidar to the radiosonde in the abstract: I found a systematic difference of 0.3 m/s (p. 9), the correlation coefficient and the standard deviation of the comparison for all altitudes needs to be derived (see my comment 3)

I was confused about Section 3.2. It is not clearly stated in the first sentence, which measurements were used for the auto-correlation approach (although it becomes clear, when reading the full section). Was it the horizontal wind speed or the vertical velocity as usually taken in these approaches (e.g. Pearson et al 2009, O`Connor et al. 2010) This needs to be clearly stated. If it is the vertical velocity, then the derived value of 0.2 m/s is representative for the lidar random error in radial direction and not for the horizontal wind speed. This needs to be corrected in the formulation of the abstract, the discussion of Section 3.2 and conclusion. This derived value cannot be taken as the measurement error for the horizontal wind speed, because here several LOS measurements are combined (reducing the error) but horizontal homogeneity has to be assumed (increasing the error). Also it needs to be stated, if the vertical velocity is taken from the vertical LOS measurement ($5^{th}$ beam direction) or if it is an output parameter after the sinusoidal fit (or geometrical solution).

**Specific Comments**

p. 9340: There is a more recent WMO 2014 reference for the SoG for global NWP; also a reference for ADM-Aeolus (see below) could be added for documenting the need for additional wind observations.

p.9341: I would propose the replace the reference for ship-borne lidars (Baker et al. 2014) by a more appropriate (e.g. Tucker et al. 2009), because Baker et al. 2014 is focussed on spaceborne wind lidar.

p. 9341: It is stated that: "Only a few studies have used Doppler lidar on ships, and fewer still have actively stabilised the system against ship motions." I am wondering, if there is a reference of a Doppler lidar on a ship w/o active stabilisation, which is suggested by the sentence. If not, I would propose to re-rewrite the sentence.

p.9341: The following reference should be added to the introduction and also referenced in ch. 2.2.

Hill, R. J., W. A. Brewer, and S. Tucker, 2007: Platform-motion correction of velocity measured by Doppler lidar. J. Atmos. Oceanic Technol., 25, 1369–1382.

p.9341: The provided values for the random errors (e.g. 0.3 m/s) are given in Pichugina et al. (2012), which should be added here.

p.9342: It would be useful to introduce the objectives of ACSE in chapter 2 with 1-2 sentences.

p. 9345: Ch. 2.3.: The calculation of the SNR is ambiguous, because of the used bandwidth of the signal. The bandwidth for the calculation of SNR or some more details should be given. The different SNR threshold values are only comparable, if the same bandwidths and the same calculation approach is used.

p.9346, 2nd paragraph: Is there a reference to the 5-point geometrical wind solution? It is also necessary to mention here (or in section 3.1), if the vertical wind is set to zero (w=0) for the sinusoidal fit, or if the vertical wind is retrieved as an independent parameter.

p.9346, 2nd paragraph: More information should be given, which QA is applied, e.g. by given the equations or by citing the corresponding equations in Päschke et al. (2015). Currently it is too vague, which QA is applied.

p.9347: The radiosonde uncertainty is provided: It should be clearly stated, if this is a random or systematic error or a combination of both.

p.9347/9348 paragraph about the lidar data coverage and the boundary layer height determination: It would be very useful to provide some data on the observed boundary layer height and the corresponding lidar coverage. The typical boundary layer height during the cruise could be provided within a Figure or a Table (e.g. daytime maximum, night-time) from the Ceilometer. This would be important to assess coherent wind-lidar performance in Arctic regions. It is only stated on p.6, that the lidar coverage is comparable in the boundary layer to previous studies, but no values for typical boundary layer height is provided.

p.9348, ch. 3.1, last paragraph and Table 2: What is the reason to show the data for different BLD in Table 2? The results for different BLD are quite similar. Also the results are not discussed (only mentioned in the text). Thus I see no need to show Table 2; one could mention that this was investigated but no significant differences were found.

p.9350: The effect of the ship on the radiosonde lowest level winds is discussed here. Is this really relevant for the comparison of the 75 m wind? I would expect that the arguments here are relevant for the lowest 10-20 m after launch of the sonde. Is there some evidence in the radiosonde data that it is influenced by the ship until 75 m?

p. 9351 and Fig. 6: A panel showing the absolute difference (Lidar-RS) in [m/s] and [°] should be provided to provide an indication of the time dependency (or not) of the bias. The relative difference is only relevant for the slope error.

p. 9352: equation number from O`Connor, which was used should be given.

p. 9352: I do not think that the larger std. dev. (compared to Pearson et al. 2009?) are due to lower aerosol load. The lower aerosol load leads to lower SNR; thus only more values with lower SNR should be present. The same SNR should result in the same standard deviation. I see the main difference in the additional contribution of the ship-motion. It seems that the ship-motion adds a constant random error of about 0.1 m/s to the lidar random error.

p.9353: I would use the word “projection” instead of “aliasing” for the horizontal wind error influence on the vertical. “Aliasing” is more commonly used for components of a frequency spectrum folded into another frequency part.

p.9353: I would clearly distinct here error sources, which add a systematic error to the vertical velocity to explain the derived mean value of 0.1365 m/s and components which add a random error (as visible in Fig. 7 of about 0.1 m/s). Ship motions and projections of the horizontal wind during 2 s would add a random component to the vertical velocity, because it could be positive or negative (and not as stated a bias). Constant biases could be due to flow distortions of the ship (as mentioned and shown).

p. 9354, ch. 3.3: It is mentioned that the horizontal wind speed is biased by 2% from the ship. It should be added, that this is a positive bias, meaning that the horizontal wind speed above the ship is higher than in the background flow. I am wondering, how this influences the horizontal wind speed retrieval from the lidar, which depends certainly on the direction of the LOS winds wrt the axis of the ship. I am also wondering, how this influences the comparison with the radiosonde and sonic. If both the lidar and the radiosonde winds are influenced by the ship, than this is not visible in the comparison, which certainly depends on the horizontal distance of the sonde wrt the ship.

p.9355: It must be added here that the measurement error of the lidar wind speed is relevant for the radial velocities (and not for hor. Wind speed) and was derived from vertical pointing measurements.

p. 9356: A reference for the ADM-Aeolus mission should be provided, e.g. ESA 2008, available from http://www.esa.int/Our_Activities/Observing_the_Earth/The_Living_Planet_Programme/Earth_Explorers/ADM-Aeolus

Fig 7: It would be nice to add the median of the measured occurrence in the plot as a line; also it needs to be added in the caption, that this was derived from vertical velocity measurements.

Fig. 8, caption. The horizontal wind speed for the CFD simulation should be mentioned in the Fig. caption.

**Editorial**

p.9348, ch. 3.1, L22: The 75-m results are reported in Table 3 and not as stated in Table 2.

p.9349, ch. 3.1, L11: The BLD results are reported in Table 2 and not as stated in Table 3; thus it would be good to change the orders of the Tables

p.9349, L7: no parentheses for the reference within the sentence.

p.9349, L26: must be 400 m ASL (instead of 40 m ASL).

p.9356, L361: less than “5%” instead of “0.5%”