Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-206-RC2, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

Interactive comment on "Shipborne Wind Measurement and Motion-induced Error Correction by Coherent Doppler Lidar over Yellow Sea in 2014" *by* Xiaochun Zhai et al.

Anonymous Referee #2

Received and published: 16 October 2017

Summary:

A newly developed ship-borne wind lidar, consisting of a coherent wind lidar from a Chinese manufacturer is presented in this manuscript. There are a few other papers on ship-borne wind lidars (e.g. Achtert et al. 2015 and from NOAA, e.g. Tucker et al. 2009) and thus this kind of application with its specific challenges (ship movement and environment) is challenging and still provides some novelty. In contrast to other earlier reports (e.g. Achtert et al. 2015), no active stabilisation of the complete lidar is performed, but the movement and angles are measured and corrected in post-processing. Some comparisons to radiosondes from a cruise in the Yellow Sea are shown in ad-

dition to two cases with vertical and horizontal wind measurements. Thus the topic of the manuscript fits to AMT. Major comments from my side are related to the description of the motion correction approach with GPS/INS, which is not clear at some places and lacks details to assess its novelty. Indeed the manuscript is very similar to the one of Achtert et al. (2015) in terms of description of methodology (correction algorithms), statistical comparison and evaluation with radiosonde, assessment of errors (spectral approach). Also numerous minor comments are related to the presentation of the topic. Thus I would recommend that the manuscript can be only accepted after major revisions of text, figures and additional material is included.

General and Major Comments:

1) The differences to the NOAA HRDL and the system by Achtert et al. 2015 should be mentioned more explicitly in the introductory paragraph (p. 3, 1st paragraph "it can be seen .." is not clear) Achtert et al. (2015) use an active motion-stabilized platform; so the difference to the described system here is clear. The NOAA HRDL uses a SDS to point the scanner LOS direction. But all systems need a motion-correction in the post-processing afterwards due to the limited accuracy of the active systems. So it is understood that the described system in the paper is neiter on a motion-stabilized platform nor the scanner LOS pointing direction is controlled by use of the ship attitude angles. Is this correct? If yes, then also the limitations of this approach (e.g. high ship movements, rough sea) should be discussed in the main part and summary more explicitly. On the other hand it is mentioned on p. 9, ch. 3.2 that, "the hemispherical scanner maintains the pointing of the lidar beam to zenith stare mode..". Does that mean that the scanner direction is controlled by the information from the INS?

2) The main part of the manuscript deals with the motion correction. Thus relevant parameters of the used GPS and INS system (type, accuracy, precision, data acquisition rate) should be provided and discussed. Why are 2 antennas shown in Fig. 1? Also the limitations of this approach, e.g. for high wind speeds or high angular rates during rough see conditions need to be discussed in the main text. Why did the authors not

chose an approach the control the scanner LOS direction, especially for the vertical pointing mode, by using the attitude angles from the INS (or is this applied)? Also details of the hard-target calibration need to be discussed. Is this performed once (before the cruise)? What angular offsets are determined, are different hard-targets in different direction used (range, elevation)? It is stated that "It can be seen that there exists no laser direction error ..". How do you come to this conclusion? Can you provide more details on that (e.g. data, Figure)?

3) The temporal resolution of the determination of the ship-induced Doppler shift (eq. 6) and the correction of the LOS velocity (eq. 7) needs to be stated and discussed. A figure showing a time-series of raw-data from the sensors (angles, velocity) could illustrate this to provide an impression about the time scales of the ship movement during anchored and cruising measurements. Also the timing of the DBS is not clear: How long is 1 LOS obtained, how long for the vertical velocity, and how long is the averaging time for the horizontal wind? Especially for the vertical pointing measurements the variability of the off-zenith angle should be shown in a Fig. The vertical velocity determination does need a correction for the horizontal wind. What is the time separation between the horizontal and vertical wind measurement?

4) In Achtert et al. (2015) the influence of the distortion of the flow due to the ship is discussed and modelled. In this manuscript this issue is only mentioned in 1-2 sentences on p. 9. What was the geometry/height of the ship? What would be the maximum height for a flow distortion, taking some numbers and scaling from the approach of Achtert et al (2015)? The lidar and radiosonde data is shown only above 150 m for this manuscript, but you conclude from your statistical comparison that the height of 200 m might be still affected by the flow distortion. So some more discussions on the geometry/height of the ship and the expected flow distortion around is needed.

5) Ch. 3.3. Error analysis: The authors deal here with the derivation of systematic errors (bias) to the horizontal wind retrieval I am wondering, if the error sources from the knowledge of the ship velocity and the lidar pointing angle are really systematic

(over longer timescales) or random, and would add to the random error of the wind retrieval. A clear distinction needs to be made in the underlying assumption for the ship velocity and lidar pointing wrt systematic and random errors. Are the provided numbers for ship velocity and pointing only the systematic part? What would be the random error of these quantities?

6) The authors could consider moving some of the equations related to the correction algorithms (Ch. 2) and error analysis (Ch. 3.3) to an appendix. At least for these parts, which are well known (e.g. coordinate transformations, descriptions of angles, DBS technique). I would restrict the description in ch. 2 and 3.3 to the novel aspects of this work.

7) I am missing a description of the overall objective of the deployment in the Yellow Sea in 2014 in the introduction. Was this only for technical demonstration, or were further atmospheric-oceanic processes studied. I am also missing a discussion of the open questions for turbulent flux measurements or wind vector measurements over the sea, which would need a shipborne Doppler wind lidar. One should discuss some objectives for the development of a shipborne wind lidar in the introduction. Also it might be useful to provide a paragraph in the Summary about future plans and campaigns.

Specific Comments

p.1 Intro: A number of studies are referenced for turbulent fluxes over the sea surface (Axford, 1968, . . .). Could these studies be grouped by objective, technology or geographical region to be more specific. Otherwise this long list of references is not very informative.

p.3, line 15: "Few studies .. in this region". Are there any references for these studies?

p. 5, L 11: The different elevation angles are probably due to ship rotation and movement during the time period of measuring different LOS directions, which should be stated here. Thus it is important to mention the duration of the measurement of each

LOS direction, and the complete 4 beams, and the relevant movements of the ship during these periods. How is the expected elevation angle $\theta 0$ obtained?

p. 7, 1st paragraph: It should be stated, how the background noise signal is obtained, e.g. via the recorded signal after a sufficiently long laser travel time, or via a separate measurement w/o laser pulse emission. Do the authors see an advantage of their SNR definition over the one from Banakh et al. 2013?

p. 7, L24: It should be described how the wind fluctuations are determined. Is it the standard deviation of wind measurements of higher temporal resolution (resolution?) during the 10 min.? Why are bars shown only for part of the profile in Fi.g 4 and 5? Is it smaller than a specific value below 1.4 km in Fig. 4? Do the fluctuations represent instrument noise or atmospheric fluctuations? What could be the reason that there are higher fluctuations in the layer of 1.4-1.6 km in Fig. 4?

p. 7, L27: Same question related to the method to determine the STD for the angles. Determined from the variability during the 10 min using raw data with of temporal resolution of xx s?

p.7, L28: Which SNR threshold was used here?

p.8, L26, last sentence: What is a "multipath effect"? This should be clarified. Also the difference in radiosonde and lidar location should be stated quantitatively. What is the difference in mean wind speed and direction between radiosonde and lidar above 1 km? Can a lidar instrumental effect excluded to explain the difference? I am not convinced that it is only colocation.

p.8 and Fig. 6: I would propose to plot the radiosonde on the x-axis and the lidar on the y-axis and also perform the linear least square fit with these coordinates. I consider the radiosonde as more accurate and the usual linear LSF procedures assume that the x-parameter is without errors (minimization of vertical differences). I also consider the criteria of excluding data with 1*SD as too strict. Only gross outliers – deviating from

a Gaussian distribution – could be excluded. This would typically result in a criteria of >3*SD. It needs also to be stated, how many data-pairs were excluded from the statistical comparison in order to judge the numbers of gross outliers. Also the SD typically refers to the SD of the difference (lidar-radiosonde). I am wondering how the SD of the lidar data ydata was obtained here. It is clear that the statistical parameters for bias, SD, R, and RMSE need to be calculated without a rigorous excluding of the data (with 1*SD). This point needs to be revisited and clarified.

p.9, ch. 3.2 and Fig. 7: The dots for MABL height are shown for the first 1/3 of Fig. 7 in a region of SNR around 10, where no obvious gradients can be seen, whereas for the second 2/3 it is more in the region between 10 dB (light blue) and 0 dB (dark blue). Please check and comment. Is there a reference about the ABL height determination using the first negative gradient?

p.11, L5: I consider an error of only 0.1° for the ship heading as very small. Is this justified by the hard-target measurements?

p.11, L8: What quantity is derived in eq. (14) in comparison to eq (13); Both are called "bias" LOS_N but eq. (14) with a "N′". Text should clearly state, the difference. What eq. (13 or 14) is then used in the estimates for the bias (eq. 17 and 18)?

p.11, eq. 15/16: These eq. could be moved to ch. 2 after eq (10), because it deals with u, and v retrieval and not with error estimates as in ch. 3.3.

p.11, L13: Here it is stated, that the lidar pointing angles are very small (and assumed to be perfect), but on p.12, L2 it is stated the errors are dominated by ship velocity and lidar pointing errors. This is in contradiction.

p.12, L6ff: Here the method of obtaining the random error is described ("In this case, a . . ."). But no resulting spectrum is shown in Fig. 9. This needs to be added or reformulated.

P12: L12: Are you sure that it is an elevated aerosol layer and not a cloud, which provides the high SNR around 1.5 km?

p.12, L23: speckle-induced phase noise is not discussed in Achtert et al. 2015. Another reference needs to be provided

p.13 Summary: The limitations of the approach in comparison to existing systems need to be mentioned in the summary. Also I am missing an outlook about future algorithm or hardware improvements or future deployment during ship cruises.

p.13, L14: The number for the bias and the STD from the statistical comparison of all radiosondes should be stated here.

Ref. Liu et al. 2010: More details should be provided for this reference, which is not really accessible, or the reference should be removed or replaced. Also Achtert et al. (2015) provide these transformations.

Fig. 1: An additional Figure should be shown of the ship to illustrate the location of the CDL on the ship and possible disturbances of the flow.

Fig. 1: The location of the INS on the CDL should be indicated in the Figure.

Fig. 2: The symbols used for the angles pitch, roll, yaw should be placed also in the Figures.

Fig. 7: the legend within Fig. 7b is too small

Editorial: A large number of editorial comments were directly added to the PDF-Version of the manuscript. In addition the term “et al” needs to be replaced by “et al.”. The manuscript needs thorough proof-reading after revision.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2017-206/amt-2017-206-RC2-supplement.pdf

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-206, 2017.