Reviewer 2:

(1) I have made comments at specific places in the text in my uploaded .tex file, so it is very clear to you the places where I am suggesting manuscript edits.

We appreciate that the Reviewer added the comments directly to the tex file. We applied the suggested edits to the manuscript. Here are some specific answers:

R2 [regarding the comparison of sounding and wind lidar data] This seems a rather large time to me, given variations in ABL winds. Maybe some discussion about this is needed?

We did not want to compare the sounding data to only a single lidar wind profile. The requirement of the mean of at least two lidar scans for comparison increased the time interval to 30 min.

R2 [values of the ABL height] must have a huge uncertainty, and maybe more so in the arctic? I think discussion is needed here.

We agree with the reviewer that this is a topic worth studying in more detail – maybe in a follow-up study. However, we omit the comparison to the wind mast in the revised manuscript. Thus, ABL heights are also no longer presented and a discussion is no longer necessary.

How often is the motion control system synchronised to UTC? Are corrections logged?

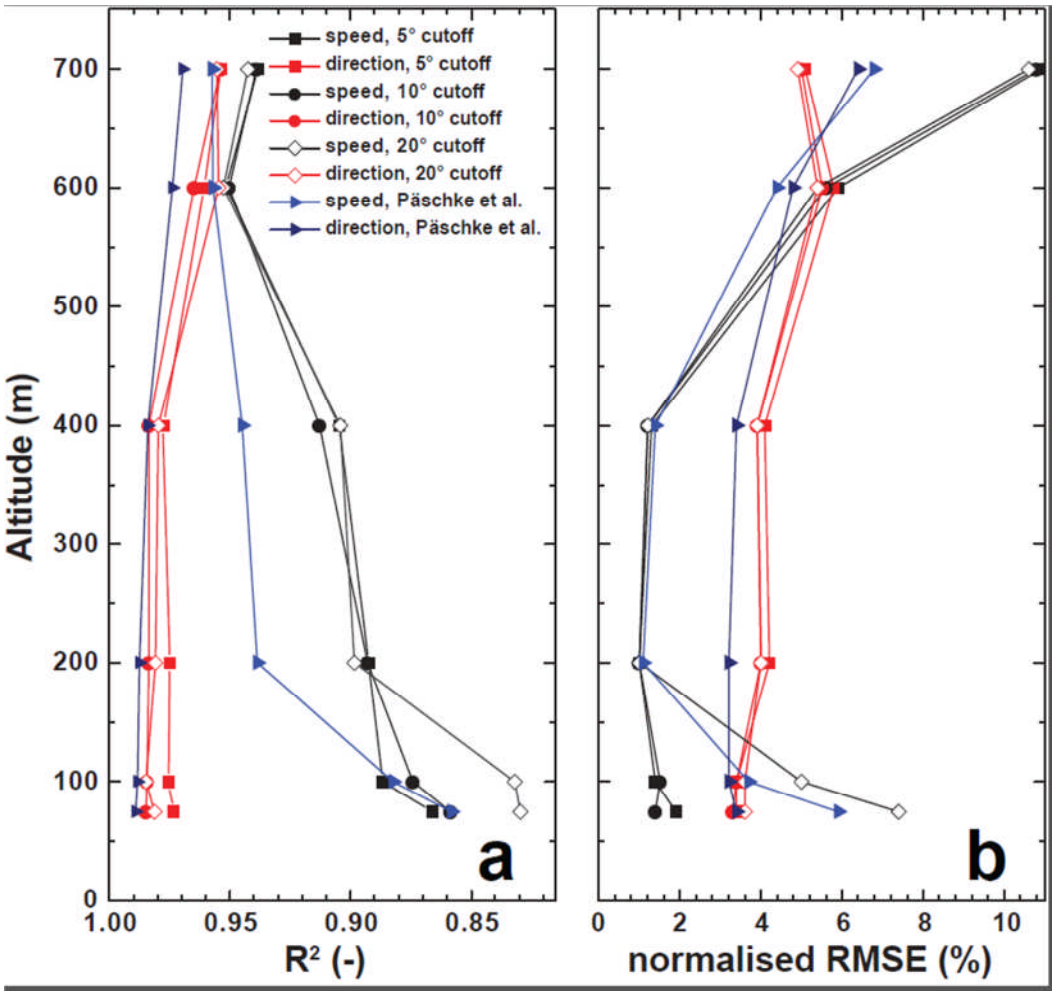
The lidar data logging system and the motion control system are synchronised hourly to UTC. The corrections are currently not logged, but they are below the sampling frequency.

R2 I am very uncomfortable with this extrapolation. Really the lidar needs to be compared with other observations not some theory-obs hybrid. What can be done? Remove from paper? Put huge caveats on?

We included this analysis into the original manuscript to increase our options for assessing the motion-corrected lidar measurements. We saw this necessity as soundings were launched on a six-hourly basis only, which considerably restricted the amount of possible comparisons to the lidar observations. From our findings, we believe that the method is robust enough for our purpose, despite the required assumptions. Nevertheless, we decided to omit the part on comparisons to wind mast measurements and related discussion in the revised manuscript and now focus on the comparison to the soundings.

(2) The biggest criticism that I have is with over-extensive use of correlation coefficient (coefficient of determination Rˆ2). It is not appropriate in almost all scientific analysis. Much more useful if rmse, bias, etc. I have given suggested reading material in the .tex file. But if that Rˆ2 issue is resolved, I expect to recommend this work to be published.

The corresponding values of normalised RMSE have been added to Tables 2 and 3 (see answer to Reviewer 1 for details). Figure 5 has been revised to also present the change of normalised RMSE with height.



(3) In general many aspects of this work are not described well enough that they could be repeated by others. So please add much more about experimental setups.

We added the respective description to the text. We also added an Appendix that describes the motion-correction algorithm in more detail.

"far field vertical velocity is zero" is vague to me - do you mean average? And is this realistic?

This refers to model. The 'far field' is the flow well away from the ship. The inflow vertical velocity is specified to be zero, and stays zero until perturbed by flow over the ship. Note that the CFD model is non-turbulent by definition.

(4) I think one case-study figure is needed, e.g. of a wind speed and direction profile time series for a day. It is hard for readers to fully interpret based on statistical summaries alone.

We included a measurement example to the results section. Please see the answer to the comments of Reviewer 1 who had the same request.