Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-88-RC1, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Flywheel calibration of a continuous-wave coherent Doppler wind lidar" *by* Anders Tegtmeier Pedersen and Pedersen Courtney

Anonymous Referee #1

Received and published: 11 August 2020

Pedersen and Courtney present a study dealing with a calibration setup for Doppler wind lidars. The setup is a flywheel that is connected to the lidar that is to be calibrated with a solid frame and a tilting mechanism for the lidar. The authors do a great job in deriving the math to obtain a measurement of the flywheel's rotational speed from a sweep of lidar measurements across the wheel's curved surface. They also provide a detailed derivation of the uncertainty components which is best practice. The manuscript is however very vague when it comes to the actual potential of the method for lidar calibration, this is mostly because no targeted accuracy of the calibration is defined to which the results are compared. This makes it difficult to assess the relevance of this work. I recommend major revisions before the manuscript can be accepted for

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Discussion paper

AMT. Below, I give some general coments and more specific comments to parts of the manuscript.

0.1 General comments

- The style of the text is very pessimistic and suggests that the authors do not believe in the value of their work. Why would they want to publish it in this case? I personally believe that there is a lot of value in this work, and I suggest that the authors try to reformulate in a more objective, quantitive way.
- The mathematical formulations should be revisited. Some of the equations are hard to read. Double subscripts should be omitted, variable names should be consistent and using u for uncertainties in the context of wind measurements is a bit unfortunate. A nomenclature in the appendix would be helpful.
- The manuscript is a bit hard to read, because the structure is unclear from the beginning, and the goal of the work is not well described in the beginning. Some of the derivations of rather simple geometric relations could be shortened or moved to the appendix to make the manuscript text flow more nicely.
- My main criticism for the content of the manuscript is, that it does not describe well what are the requirements for calibration accuracy in practice. The authors only briefly mention possible sources of error in Doppler wind lidars, but do not give an idea about the quantity of such errors. How accurate do lidar measurements have to be for the industry or for research purposes? Maybe the calibration accuracy here is good enough after all? In fact, it cannot be known from this experiment if differences between wheel speed estimation through the calibration and reference wheel speed are due to inaccuracy in the calibration setup or the lidar itself.

- Until the last sentence of the discussion Section 6 I was wondering if it was not a possibility to measure from zero wind speed perpendicular to the wheel surface to get a more continuous calibration curve. It is a bit unsatisfactory and hard to understand why the tilt angle of the lidar cannot be mechanically adjusted for that.

0.2 Specific comments

- Title: The title is a little bit misleading, because I was expecting to see results of the calibration of the lidar, but actually there are only evaluations about the accuracy of the calibration itself, not the lidar.
- Introduction:
  - p.1,l.20: I think it would be worthwhile to expand on the kind of laser errors that can cause errors of line-of-sight velocity to better motivate the work.
  - p.2, l.1: Shinder et al. 2013 should be inside the parantheses. The citation style should be checked throughout the manuscript.
- Section 2:
  - p.2,l.22ff: Since this is a commercial product, I think it would be sufficient to name the manufacturer and give the accuracy of rotation speed measurement. Measurement principle is not so important for this study.
  - Table 1: I recommend to divide the table in two, separating calibration rig properties and lidar properties.
- Section 3:
  - All equations: I suggest to use φ (varphi) to be consistent with the plots.
  - p.4, l.2: Just a suggestion for wording: I do not think it is a severe problem, just a technical one.

- p.4, l.7: Please expand why the resolution of the Doppler spectrum is a problem. It does not define the resolution of the velocity measurement, because the typical MLEs interpolate the spectrum to estimate the Doppler shift. I think the main benefit of several tilt angles / wheel speeds is that the tangential speed can then be extrapolated!?
- p.4, l.24: I would not call it a trick, it is geometry after all.
- p.6, l.8f: I think the connection between L'Hospital and the following Taylor's expansion is not made very clear. Maybe Eqs. 9-10 could even be put in the appendix and just be referenced at this point to make it easier to follow the main storyline of the paper.
- p.7, l.11: Why does part of the beam even have to go above the wheel? Wouldn't it make it easier to just choose tilt angles where the whole beam is on the wheel?
- p.9, Eq.17: I think the whole derivation does not have to appear here, especially since the 3D model does not have a significant benefit over the 2D model. It could still be put in the appendix.
- p.10, l.10f: Could the beam width maybe be estimated from the tilt angle sweep if it is done so far until none of the beam hits the wheel any more? Or if instead of trying to hit the tangential, the beam was set to hit the wheel frontal, which would lead to zero velocity measurement?
- p.12, l.11f: It is not clear to me why the extrapolation from angles larger θ_1 is not the real tangential speed ratio ($b_i = 1$!?).

• Section 4:

- p.14, Eq.27: It would be good to introduce a variable for $\frac{V_{\mathrm{LOS}}}{V_{\mathrm{wheel}}}$ and use this as subtext in this equation.

• Section 5:

- p.16, l.26: "quite well", "some of the truth" and "at least relatively" are such weak statements that I strongly recommend to reformulate.
- p.20, l.18: $\%/\circ$ is not a good notation and it is also mixed with $\%/^{\circ}$.
- p.23, l.2ff: I was wondering about this possibility throughout the whole derivation of the calibration models for $\theta < \theta_1$. Maybe it should be explained earlier, why it is not possible.
- p.23, l.25: "its" instead of "it's".
- p.23, l.19f: Is it enough to calibrate continuous wave lidars at a minimised beam width? What would an industrial calibration have to cover?

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