DTU Library

Applications and improvements of continuous wave Doppler lidars for wind energy

Jin, Liqin

Publication date:
2023

Document Version
Publisher's PDF, also known as Version of record

Response to Examiners' Comments

We would like to thank all the examiners for the time and effort that you have dedicated to providing your insightful comments on my thesis. We have been able to incorporate changes to reflect all the suggestions provided by you. Here are our responses to all the comments.

General comments:

1 **In chapters 2-4, the introductions to the chapters need improvements, since the current introductions represent a mix of supplementary material and short descriptions that require the readers to read the paper first in order to understand the introduction. Instead, the introduction should provide the readers with a quick overview of the paper with all the relevant information. The introduction should ideally cover the main motivation of the research (if not already covered in Chapter 1), the major assumptions behind the method, and the results of the research need to be interpreted properly in view of these assumptions and limitations. This applies also to the relevance of the results. It would be good to highlight what is the contribution of the research result and how it advances the state-of-the-art as well as potential impacts (the impact and relevance can, however, also be stated more precisely in Chapter 5, see comments below).**
Thank you for pointing this out. It's a good point. We have reformulated the introduction part of each chapter by covering the main motivation of each research, the major assumptions, and the results of each study. It's too long to list all the introductions of three chapters, the examiners can check the revised thesis.

2 **Since the papers are written at different times, incoherence is rather inevitable. Therefore, it is important that the PhD candidate provides in the respective introductory chapter an up-to-date and critical interpretation of the results given that new knowledge and research may have been produced between the time of the submission of the papers to the submission of the thesis. For example, the study in Chapter 4, contains a recent reference to a study by Wildmann et al. concerning the shortening of the probe volume. This reference is not given in Chapter 3, which has the probe volume shortening in focus, possibly because it was written earlier.**
Thank you for pointing this out. We agree with this comment. We have added this point in the third paragraph in subsection 1.4.1 "A recent study introduces a correction for advection in the retrieval method of turbulence parameters obtained by lidar measurements derived from velocity-azimuth display (VAD) scans, allowing the volume-averaging effect to be taken into account [Wildmann et al., 2020]".

3 **The PhD candidate sometimes uses imprecise and qualitative statements and we wish to see more precise wording (see detailed comments, below).**

Thank you for pointing this out. We responded to this comment in the second part.

4 **Some of the notations are inconsistent in the text, for example, sometimes lidar is used, and other times Lidar is used.**
Thank you for pointing this out. We used the same notations in the whole text. The notation "Lidar" only appears in section headings or in paper titles.

5 **The PhD candidate is strongly encouraged to reconsider the logical order of the chapters and papers, e.g. starting with the paper that deals with methodology development and improvement, which are the main focus of the study, and finishing with the paper that deals with the application of the CW lidar.**
Thank you for pointing this out. We have carefully thought about this comment. We prefer to stick to the original structure. From the application of CW lidars, we found that it is necessary to improve the spatial resolution of CW lidars and make lidars less sensitive to the out-of-focus moving objectives, which leads to the study of Chapter 3.

6 **Also, we encourage you to change to a more precise title that accurately reflects the covered work.**
Thank you for pointing this out. We agree with this point and changed the title to "Applications and improvements of continuous-wave Doppler lidars for wind energy" to align with the whole thesis.

Specific comments:

1 **1.1 First paragraph below Figure 1.1. The figure shows that wind energy is not currently the backbone ("backbone" meaning the part of the renewable energy system that provides its main strength) of the renewable energy technologies, but rather one out of three technology groups with almost equal shares in today's and tomorrow's energy system. Please rephrase to a more correct description of the graph. Please also state what emissions are reduced and by which mechanisms this has a positive impact on the environment (just a few extra words would be needed).**
Thank you for pointing this out. We agree with this comment and rephrase this paragraph in subsection 1.1 "As an important part of renewable energy, wind energy has grown rapidly in recent years and is now considered one of the lowest-cost means of supplying electricity and reducing CO_2 emissions to mitigate climate change [Luderer et al., 2017, Pietzcker et al., 2017, Bistline et al., 2018, Lazard, 2019]".

2 **1.1 First paragraph, page 2. It is correct that it would be expensive to rely on masts only for the experimental characterization of the atmospheric boundary layer in the height range relevant to tomorrow's wind turbines. But the statement "rather, it requires a deep and thorough understanding of the dynamics**

of the 3D wind field for wind energy science and wind engineering", has nothing to do with the first part of the sentence, since mast-based instruments can provide 3D observations for a deep and thorough understanding of boundary layer dynamics. Please rephrase the sentence such that it makes sense.

Thank you for pointing this out. We agree with this comment and rephrase this paragraph in subsection 1.1 "These giant turbines will, undoubtedly, tower up hundreds of meters into the sky, with their blades sweeping vast areas bigger than several football fields. Therefore, the detailed characterization of the ever-changing wind and turbulence fields over the entire rotor plane upwind and downwind of a single wind turbine as well as the whole wind farm will be challenging. For wind data collection, cup or sonic anemometers mounted on meteorological masts (met masts) are still widely used, but the increasing size and deployment of modern wind turbines in complex onshore and offshore environments require the construction of taller masts that become extremely expensive. In addition, very tall masts are strictly regulated in several countries around the world, like Japan [Goit et al., 2019]".

3 **1.2.1 What is meant by "popular" on line 4 of the first paragraph? Popular typically implies that something was widely used – was this really the case within the 1970s or was it rather one or two groups that used this technology?**

Thank you for pointing this out. We agree with this comment and changed the word "popular" to "viable". Now the new sentence is "In the 1970s, with the maturation of the CO_2 laser, pulsed CO_2 lidars with a wavelength of 10.6μm and stable single frequency output became a viable wind measurement tool [Bilbro and Vaughan, 1978]".

4 **1.2.1 second paragraph "situations have changed dramatically" should be "the situation changed dramatically" (if there really was a dramatic change). Was this development really driven by the wind power generation industry and not the development in the telecommunications sector?**

Thank you for pointing this out. We agree with this comment. Now the new sentence is "After 1990, situations changed dramatically.".

5 **1.2.1 Pag 3, first paragraph: Was the first Qinetic really made for nacelle-based observations and not wind profiling? Please double-check and add a precise statement.**

Thank you for pointing this out. We checked the reference Slinger and Harris [2012]. In the second paragraph of the Introduction part, it is written as "The first all-fibre lidars were demonstrated in the late 1990's, and a commercial prototype unit (ZephIR) was mounted on a turbine to demonstrate wind speed detection in front of the rotor plane in early 2003.". In the thesis, we wrote as "In early 2003, the first commercial all-fiber lidar "ZephIR™" was launched by UK company QinetiQ, to demonstrate wind speed measurements in front of the rotor plane from a turbine nacelle [Slinger and Harris, 2012]".

6 **1.2.1. page 3, second paragraph: the statement about direct detection is not precise enough to be understood. Further, "Background detection" → "background noise detection"??**

Thank you for pointing this out. We agree with this comment and reformulate this paragraph, which is "The incoherent technique, on the other hand, directly measures the Doppler shift experienced by a pulse of narrow-band laser light as it is scattered by the particles in the atmosphere [Skinner and Hays, 1994, McGill et al., 1997]. The incoherent detection is accomplished by applying an incoherent interferometric determination to measure the laser wavelength before and after scattering.".

7 **1.2.1 Page 3, third paragraph: Do the pulsed lidars really have a constant weighting function given that they emit a Gaussian pulse? What is meant by "blind measurements"? Also, the under-representation of smaller scale in large measurement volumes is not only an issue in the pulsed lidars but also in CW lidars at large ranges (given that the motivation of the thesis is made by stating that lidars should be used for heights beyond affordable-mast-heights, this is important).**

Thank you for pointing this out. We agree with this comment and reformulate this paragraph. "Constant" here means identical. The "bind measurements" means the dead zone of a pulsed lidar. The new paragraph is "Laser beams can be continuous-wave (CW) or pulsed in coherent lidar systems [Karlsson et al., 2000, Rodrigo and Pedersen, 2012]. Pulsed lidars, in the left panel of Figure 1.3, are assumed to have identical weighting functions across all range gates and they are capable of long-range measurements, e.g., for distances of more than 10km [Cariou et al., 2011]. Besides, they can obtain independent wind measurements simultaneously from many distances.

The dead zone of a pulsed lidar, however, should be considered when programming the length of the pulse sequence, which is used to avoid receiving multiple echoed signals at the same time from different distances during scanning [Chen et al., 2020]. Depending on the duration and width of the pulse, this dead zone can range from tens to hundreds of meters away from the beam source [Liu et al., 2019]. The measurement results from Boreysho et al. [2020] demonstrated that the dead zone of the employed pulsed lidar of the IR range is 100 meters. Additionally, when using a pulsed lidar, it is much more difficult to extract turbulence information from the width of a Doppler spectrum, compared to a CW lidar [Sathe and Mann, 2013].".

8 **1.2.1 Page 3. It is strange that there is such a strong statement about CW lidars having "good spatial resolution" within several 100 meters. In general, scientists should avoid qualitative statements, especially when it is easy to make a quantitative statement. From this paragraph the reader understands that the measurement volume of a CW lidar has much shorter probe volumes than pulsed lidars – is this really the case for ranges up to several hundred meters? An important part of the thesis is about making the CW probe**

volume shorter – why is this important? It would be interesting clearly have it shown, at what height range the current measurement volume of a pulsed lidar really exceeds that of the CW lidars. New lidars enter the market, especially on the pulsed side, and it would be nice with an overview of the current status of the market.
Thank you for pointing this out. We agree with this comment and rephrase this paragraph as "CW wind lidars, in the right panel of Figure 1.3, are designed to measure wind field quantities (speed, direction, and turbulence) at heights of tens to a few hundred meters. Different from pulsed lidars, CW lidars receive observations from one height at a time and their probe volume is heavily influenced by the focus (measurement) distance. In the study from Mikkelsen [2009], a detailed comparison of the probe volume characteristics with the commercially available pulsed lidar (WindCube from Leosphere) and CW wind lidar (ZephIR from National Power U.K.) is presented. The two lidars have similar probe volumes at 148.5m, but the CW lidar has a more compact probe volume at shorter distances. Aside from this, the CW lidar's probe volume at a focus distance of 150m is five times larger than that at 100m.".

9 **1.2.2 This section focuses on bistatic Doppler lidars, but in section 1.5, it is stated that the thesis is focused on mono-static lidars. Please clearly introduce the mono-static lidar.**
Thank you for pointing this out. We agree with this comment and add a few lines to explain the mono-static lidar both in the text and in the figure's caption. In the text, the new sentence is "A monostatic Doppler lidar, on the other hand, combines the laser and detector in one device, so both transmit and receive paths share common optics [Gordienko et al., 2008], making the system more compact.". In the caption of Figure 1.4, the added sentence is "In a monostatic setup, the laser and detector are combined in one device.".

10 **1.2.2 first section last sentence "backscattered laser" should be "backscattered laser light".**
Thank you for pointing this out. We agree with this comment and changed "backscattered laser" to "backscattered laser light". Now the new sentence is "The backscattered laser light is detected and then mixed with a small portion of the emitted laser (local oscillator, LO), which will be accurately captured by a detector.".

11 **1.2.2 Third paragraph: Please add a reference for the statement that there are three methods for determining the velocity from the peak. Please add references supporting the statement that the "median method is often employed in many analyses" (recommend rephrasing the sentence to be more precise – its current wording is so vague that it has no meaning). Remove the word "therefore", since Eq. 1,1 is used for all methods.**
Thank you for pointing this out. We agree with this comment and rephrase the sen-

tence. Now the new paragraph is "There are typically three different methods that can be used to determine the LOS velocity from the Doppler spectrum, which are the maximum method, the centroid method, and the median method [Held and Mann, 2018]. In terms of measuring average wind speeds, all three methods are equally effective. It is recommended, however, to use the median method to reduce bias in case of detecting weak signals [Angelou et al., 2012], to mitigate the effects of turbulence attenuation, and to obtain wind speed time series with low noise [Held and Mann, 2018]. According to the optical Doppler effect, the LOS velocity v_{LOS} leading to the Doppler shift is calculated by".

12 **Figure 1.5. If the figure is taken directly from Harris et al. 2006, this should be stated as "Figure from (Harris et al 2006)", otherwise the reference can be moved to the text.**
Thank you for pointing this out. We agree with this comment and changed the caption to "Stages in typical lidar signal processing from Harris et al. [2006]".

13 **1.3.1 This chapter is about sonic anemometers and unfortunately includes several incorrect statements:**
(1) Eq 1.2 and accompanying text give the impression that sonics will have a bias in high wind speeds. This is, however, not the case. The equation is correct, but it is not the one used for determining the velocities along the paths.
We have changed the equation to:

$$U = \frac{l}{2}\left(\frac{1}{t_1} - \frac{1}{t_2}\right) \tag{1}$$

where $c_s \approx 340\text{ms}^{-1}$ is the speed of sound. By measuring the two travel times t_1 and t_2 to determine the velocity component that is parallel to the acoustic path, therefore, any flow component perpendicular to the path will not affect the calculation by Equation 1.
(2) in the second paragraph, the spatial averaging of sonic anemometry is mentioned as an issue, but it is typically 1-2 orders of magnitude smaller and rather insignificant compared to the spatial filtering by Doppler wind lidars. The references to Högström et al 2004 and Bowen 2007 are imprecise. Högström et al (2004) discusses the flow distortion effect mentioned in the following paragraph, whereas Bowen (2007) compares the deficiency of cup anemometers to sonic anemometers at low velocities. Sonics typically can measure the high-frequency turbulence for all wind speeds in the surface layer.
We have removed this statement in the text. The new paragraph is "Unlike cup anemometers, which measure only the length of the horizontal wind vector, sonic anemometers can retrieve the entire velocity vector by combining three transducer pairs [Wyngaard, 1981]. Further advantages are their rapid responses to turbulent fluctuations of higher frequency,

as well as accurate three-dimensional wind measurements after standard calibration, making them ideal for studying lower atmospheric turbulence [Kaimal et al., 1990, Högström and Smedman, 2004, Bowen, 2007]. Currently, most of our understanding of atmospheric turbulence still comes from measurements performed with sonic anemometers mounted on met masts. Additionally, it is possible to run sonic anemometers for a long period of time without a resident operator, and they do not need to be calibrated frequently [Cheng et al., 2015].".

(3) No citation is given for the statement that flow distortion is a serious problem for sonic anemometers, please add. For the sonic anemometer used in this study, a correction scheme for flow distortion is available. Was this correction used?

The text has been updated to introduce this issue and mention the fact that we used the measured sonic data as a reference after applying a correction function, which is "It is important to note that sonic anemometers are intrusive, so they can significantly disrupt the flow at the location where they are positioned. Furthermore, their measurements may be deteriorated by flow distortion caused by the structures where the anemometer is mounted as well as the anemometer itself [Dyer, 1981, McCaffrey et al., 2017, Peña et al., 2019, Mauder et al., 2020]. In consequence, sonic anemometers can underestimate wind velocity and turbulent fluctuation because of the deficit velocity in the wake of transducers along the streamwise direction, as well as wakes from support structures. Aside from flow distortion, another measurement error observed in sonic anemometers is caused by the angle of attack due to the position misalignment of the instruments [Cheng et al., 2015]. However, both errors are considered in this project, by applying a correction scheme to the measured sonic data before being used as references.".

(4) Why is it hard to measure the path length of a sonic anemometer and what makes the rotation of the sonic anemometer raw data "cumbersome" (the same principle is used for lidar)?

We have used an imprecise statement here. Therefore we delete the wrong sentence in the text.

(5) Sonic anemometers are generally capable of measurement during rain, and it is a misunderstanding of the measurement data that this is not the case for the used data set.

Thank you for pointing this out. We did misunderstand the model of a sonic anemometer. The corresponding paragraph has been removed.

(6) The sonic temperature parameter can also be estimated during non-fair weather conditions.

Thank you for pointing this out. We have modified the paragraph as "Apart from wind velocity calculation, the virtual temperature can also be estimated by sonic anemometers in fair weather observations since sound speed c_s is a function of temperature [Bohren and Albrecht, 2000, Wyngaard, 2010]. Furthermore, buoyant flux can be calculated based on the temperature measured by sonic anemometers, thereby quantifying atmospheric stratification, i.e. neutral, stable, or unstable [Berg et al., 2012].".

14 **1.3.2 Both pulsed and CW profiling lidars calculate mean wind speed based on an assumption of homogeneity over the horizontal area defined/encircled by the beams. The way this part is written, it seems like it only applies to CW lidars. It would be suitable in this section to state something about the WindScanners' accuracy. How good are they when it comes to mean wind speed and turbulence measurement?**

Thank you for pointing this out. We agree that both pulsed and CW profiling lidars calculate mean wind speed based on an assumption of homogeneity. In the text, we wrote, "In order to retrieve wind velocity from a single pulsed or CW lidar, there is an implicit assumption presumed that the wind statistics are unchanged from the scanned plane to the area of interest.". Both lidars are mentioned in this sentence.

We have added some statements about the WindScanners' accuracy, which is "Experimental verification of LOS velocity calibration has been conducted in a wind tunnel [Pedersen et al., 2012], which shows a very good agreement between a short-range CW lidar (a modified ZephIR 300 lidar) and a reference pitot tube. Based on this, the first wind and turbulence study with short-range WindScanners was performed to rapidly scan a 2D vertical plane within the wake of a small building [Sjöholm et al., 2012]." and "According to the recent study by van Dooren et al. [2022], it was found that the 10-minute wind speed time series (mean wind speed about 10ms^{-1}) measured by two short-range WindScanners and the hot-wire are in good agreement. The mean average error of wind speed is about 0.1ms^{-1} and 0.2ms^{-1} when the turbulence intensity is 3% and 22%, respectively. However, the turbulence measurements by the WindScanners match the hot-wire spectra very well only at the lowest frequencies, which corresponds to larger eddy structures. By using the short-range WindScanner system, it is possible to detect turbulent wind fields remotely in complex terrain, around wind turbines and buildings without disrupting the flow.".

15 **1.4.1 Whereas in section 1.2, the CW lidars had good spatial resolutions and the pulsed lidars suffer from spatial filtering, the opposite is stated here. Which is correct? Please state at what ranges the statements are valid.**

Thank you for pointing this out. We agree with this comment and changed the sentence to "It is advantageous to use CW Doppler lidars for a variety of wind energy applications. However, in contrast to pulsed lidars, CW lidars are limited in spatial resolution when the focus distance exceeds a few hundred meters (see Section 1.2), depending on the exact configuration of the instruments and the effective aperture of the telescope."

16 **1.4.1 Is the Sonnenchein et al (1971) expression for a theoretical ideal case or is this averaging really achieved in current lidars?**

Thank you for pointing this out. This expression is for a theoretical ideal case when an infinite wide lens is used. We changed the sentence to "When an infinite wide lens is used, the weighting function of CW monostatic lidars can be characterized by a Lorentzian function [Sonnenschein and Horrigan, 1971], but not achievable in practice, which is expressed as:".

17 1.4.1 Second paragraph, first sentence: This sentence would make sense if the cross-section of the Lorentzian was stated as a function of range. Since the cross-sectional diameter is important in Chapter 4, it would be advantageous to state it already here. The current wording is hard to understand.

Thank you for pointing this out. We have rephrased the sentence to make it clearer to understand "With z_R (or the probe length) increasing quadratically and the beam waist w_0 increasing linearly with the focus distance r, the effective probe volume $(2z_R \cdot \pi w_0^2)$ of a CW lidar varies as the 4^{th} power of r [Peña et al., 2013].".

18 (In general, new paragraphs should not start with the words "Additionally" or "However", since these words typically refer to the sentences above. Consider whether new paragraphs are needed).

Thank you for pointing this out. We agree with this comment and have replaced these words with others or merged two paragraphs into one.

19 1.4.1 Third paragraph: please give a number instead of "substantial".

Thank you for pointing this out. We have rephrased the sentence to give a number instead of "substantial", which is "In the event that a CW lidar is installed on a large wind turbine to characterize wind velocity in $2D$ or $3D$ up and downstream of its rotor (D refers to the rotor diameter, which can be over 200 meters nowadays), the corresponding probe volume will be 16 or 81 times the volume at the focus range of $1D$.".

20 1.4.1 Fourth paragraph, first sentence: "Additionally, this..." – what does "this" refer to?

Thank you for pointing this out. We have rephrased this paragraph. "this" refers to the underlined part in the following sentence "A substantially larger probe volume at a longer focus range, along with a longer tail of the Lorentzian weighting function (its sensitivity decays away from the focus point, but only as r^{-2} [Mikkelsen, 2009]), will also make CW Doppler lidars more susceptible to moving objects far away from the intended measurement point.".

21 1.4.1. Fourth paragraph: It is hard to understand this section. We guess it is about what is later introduced as the imprecisely worded "fat tails" of the

Lorentzian weighting function and that the combination of high backscatter from in-homogeneous aerosol concentration (or solid objects) and long tails of the weighting function can give a significant contribution to the detected Doppler shift? Please also add a reference for the statement that clouds are influencing the signals (or show a situation when it does), and a citation for the claim that cloud detection algorithms are available in commercial lidars (if this is the case).
Thank you for pointing this out. We agree with this comment. Therefore, we added one paragraph to explain the influence of cloud returns, which is "It has been assumed that atmospheric particles positioned close to the laser beam focus move at the same speed as the wind, which induces the Doppler shift. However, if the laser beam intersects a solid object (such as a bird) or impacts a strongly scattering low-hanging cloud base in the probe volume (Figure 1), and both move at different speeds than the wind, a biased measurement may result from the spurious Doppler shift caused by these objects.
The removal of cloud signals at high altitudes can be accomplished by identifying their characteristics (usually higher velocity, typically narrower spectral width, independent Doppler spectrum of focus range, etc.), which has been implemented in ground-based ZephIR Doppler lidar to eliminate the cloud return effectively [Peña et al., 2013]. However, low-hanging clouds are more difficult to distinguish and remove. In relatively clean air with slow low-hanging clouds, a conically profiling lidar may measure the speed of the clouds rather than the speed of the air at the focus point."

22 **1.4.2 First paragraph: The first sentence lacks some words to make its meaning clear. "Very accurate" is a subjective opinion and should be replaced by something quantitative that has been documented in the scientific literature.**
Thank you for pointing this out. We agree with this comment and have rephrased this sentence. The new sentence is "If the laser wavelength is stable and signal processing is correctly performed, CW Doppler lidars are generally good at measuring Doppler shift.".

23 **1.4.2 Second paragraph: Maybe the paper in Chapter 4 should be cited to substantiate the sentences about raindrops causing errors in the wind speed estimation.**
Thank you for pointing this out. We agree with this comment and have cited the paper in Chapter 4, which is "Raindrops moving downwards may lead to some errors in wind speed measurements because the projected speed of the raindrops in the propagation direction of the beam will be different from the LOS wind component [Jin et al., 2023].".

24 **1.4.2 Third paragraph: For the VAD scanning lidars the Doppler peak by the rain should be further away from zero than the peak belonging to the wind**

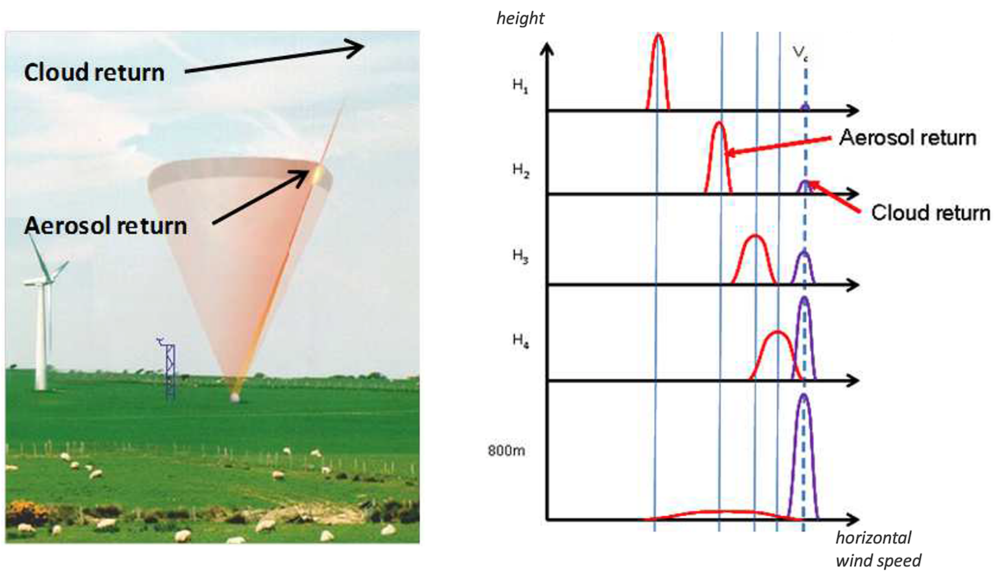

Figure 1: A spurious return is produced when the laser beam intersects a strongly scattering cloud layer (*left*) and the aerosol (red) and cloud (purple) returns at different focus heights with the ground-based CW Doppler lidar (*right*). The easily identified cloud signal at the 800m focus will be used to eliminate the cloud return at the intended measurement heights. Pictures are taken from Peña et al. [2013]

speed, whereas horizontally scanning lidars should have the Doppler peak close to zero, based on the assumption that the rain falls vertically and the VAD scanning angle is less than 45 deg. Please rephrase the section to make it clearer.

Thank you for pointing this out. We agree with this comment and have rephrased this part. The new paragraph is "These studies found that the spectral peak close to $0\mathrm{ms}^{-1}$ corresponds to the Doppler signal of vertical wind speed, while the raindrop-induced Doppler signal would be further away from $0\mathrm{ms}^{-1}$. This can be easily recognized from detected Doppler spectra. However, when CW Doppler lidars are horizontally oriented at certain cone angles, it can be difficult to differentiate between the aerosol-derived Doppler signals and those from raindrops, due to the possibility that both signals may occur at close frequency bins as shown in Figure 2.".

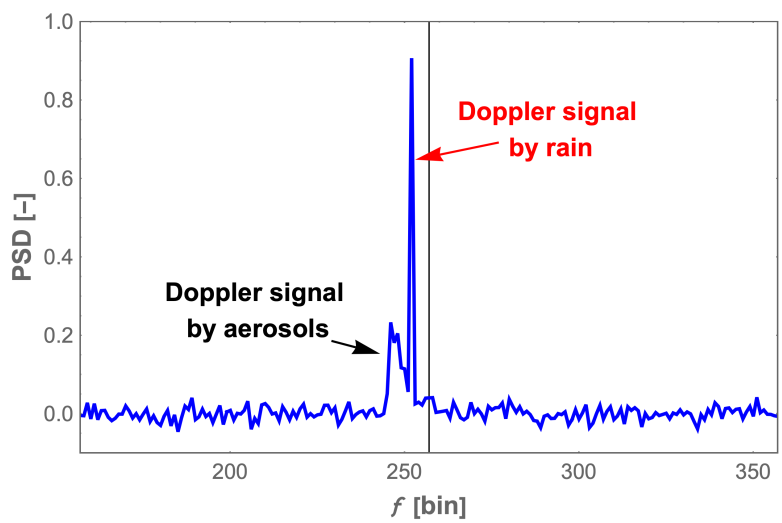

Figure 2: Example of a 50Hz Doppler spectrum containing both aerosol- and raindrop-induced Doppler signals, taken on September 27$^{\text{th}}$, 2022. The solid black line stands for the zero-Doppler shift at frequency bin 257.

25 **1.4.2 Last paragraph: It is unclear what is shown and discussed. Is it the results using the methodology in Chapter 4 for the same minute (in this case, please refer to Chapter 4) or is it three different minutes, where it rained in one and not the others? In scientific writing, the wording "so obvious" is generally not appropriate. "Sieving" → filtering?**

Thank you for pointing this out. These three minutes were from the same measurement campaign as discussed in Appendix A. We have made it clearer for readers to understand. The new sentence is "An example of three 1-minute averaged spectra (the raw, the wind, and the rain spectra) measured at a high sampling frequency during precipitation on

October 5th, 2021 is shown in Figure 1.11. A detailed explanation of how the measured Doppler spectra were processed is given in Jin et al. [2022], as shown in Appendix A.". We have removed "so obvious" and changed "sieving" to "filtering".

26 Should spectra have units? Fig. 1.9

Thank you for pointing this out. Due to Bartlett's method of obtaining power spectral density (PSD) [Press et al., 1988, Chap. 13], which is the square of the absolute FFT value for each spectrum, the unit would be "V^2" if the detected signal is voltage. But it is still not really relevant to keep it in the figure.

27 1.4.3 First paragraph: The first and second sentences don't seem to connect.

Thank you for pointing this out. We agree with this comment and have corrected this sentence. The new sentence is "The optical signals from coherent Doppler lidars have to be down-converted into an intermediate frequency (IF) or baseband (also known as zero-IF) by different mixing methods [Jacobs, 1988], which can only provide the radial velocity without the direction of the movement. The inability of coherent Doppler lidars to discriminate the sign of the LOS wind velocity in a symmetric spectrum is a serious issue in many applications.".

28 1.4.3 Second paragraph: Are there several methods or are there two methods? Do they apply to CW lidars only or do they also apply to pulsed lidars?

Thank you for pointing this out. There are two different methods discussed in the thesis, so we changed the word "several" to "different". As this PhD project focuses on CW Doppler lidars, we didn't include the comment on pulsed lidars in this section.

29 1.5 What is meant by "short-range" here? Given that the motivation of the thesis is the need for accurate measurements for the large turbines of tomorrow (not short-range?), please add an explanation/remark about why no measurements were taken at long range.

Thank you for pointing this out. "short-range" here is just part of the name of CW lidars, for example, short-range WindScanners or long-range WindScanners. To remove this ambiguity, we changed the sentence to "In this work, mono-static and coherent CW Doppler lidars are employed.". The reason why no measurements were taken at long range, we explained as "CW lidars have advantages such as a higher measurement frequency, but when the range gets long, the measurement volume becomes too large. Therefore, in this thesis, we investigate whether it is possible to reduce the measurement volume of CW lidars over a relatively short focus range (up to one hundred meters)."

30 **1.5 Research questions: Good research questions are not binary, but rather exploratory and open. Their answers should ideally lead to a quantification, for example: "How well can rotary-wing etc", "To what extent is it possible to improve CW lidars' spatial resolution... " and "Under how severe rain can a novel filter for rain spectra ... etc". Please, reformulate the research questions to be more scientific.**
Thank you for pointing this out. We wrote a few sentences in the text that "At first glance, the three questions in Figure 1.13 may appear binary, but they are kept compact with the intention of serving as a guide for further research. However, by defining how precise one wants to be, the answers open up to scientific quantification.".

31 **1.5 It is strange to introduce what is in Chapter 1 at the end of Chapter 1.**
Thank you for pointing this out. We agree with this comment and have deleted this part.

32 **Chapter 2:**
(1) As stated above, we recommend putting this chapter last and also, to emphasize that the study can indeed be linked to the same context as the rest of the chapters.
Thank you for pointing this out. We would like to stick to the original structure. This is because, from the application of CW lidars, we found that it is necessary to improve the spatial resolution of CW lidars and make lidars less sensitive to the out-of-focus moving objectives, which leads to the study of Chapter 3. Besides, lidar measurements in the drone experiment also provided a good clue for the rain measurement in Chapter 4. Using a CW lidar to sample Doppler spectra quickly in both studies allows the lidar to detect spurious Doppler signals, which can then be suppressed or filtered away for better data processing. Hope this can be accepted by the committee.
(2) Overall, Chapter 2 is a well-designed study with very interesting results. However, we are surprised at how the PhD candidate and co-authors have expressed their interpretation of the results. The low absolute difference between the simulation and the observation (0.2m/s) actually represents a large relative error (about 20%). This discrepancy is explained by shortcomings in the observations (long probe volumes), but no reflection is made regarding the obvious limitation of the simulations (uncertainties related to turbulence closure, no turbulence in the inflow, a simplified representation of the drone geometry, no grid resolution study that shows that the simulation results are properly converged etc)? Do the authors really mean that all differences between the lidar measurements and the simulation results are only due to shortcomings in the measurements)? If a 20% error is really considered to show that the simulations are trustworthy, it would be necessary to state why this is the case and at what error, the simulations would be considered inac-

curate.

Thank you for pointing this out. For the comparison of nine cases, only one case has a larger difference of 0.2 ms^{-1} and the others differ by 0.1 ms^{-1}. In fact, we have mentioned this in the manuscript from Line 212 to Line 218, which is "A good agreement can be observed between the CFD simulations and lidar measurements for horizontal and vertical wind velocities, with differences of only about 0.1 ms^{-1} at all three heights studied, except for the vertical velocity W at $\Delta h = -1.6$ m, which differs by 0.2 ms^{-1}. Several factors contribute to these differences. The simulations are conducted with laminar inflow without turbulent fluctuations, which is not the case in reality. Besides, the propellers are simplified based on the actuator disc theory, limiting the ability to accurately estimate the flow, especially when it comes to the turbulence generated by the real propellers. Lastly, the averaging effect of lidars' measurement volume also leads to differences with simulations.". We have mentioned in Chapter 2 that However, it would be beneficial to investigate the statistics of the velocity difference in more detail, as well as to conduct more experiments at higher free wind speed levels.

33 **Chapter 3:**

(1) In section 3.1, a new method is introduced that effectively suppresses "fat" tails of the probe volume related to cloud returns, but it is hard for the reader to understand this problem since no reference to (or example of) the issue is presented. We recommend introducing the issue more thoroughly and also to define "fat tails" into something quantitative already here.

Thank you for pointing this out. We agree with this comment and have explained more about this issue in Chapter 1 with an illustrated figure, which is "A substantially larger probe volume at a longer focus range, along with a longer tail of the Lorentzian weighting function (its sensitivity decays away from the focus point, but only as r^{-2} [Mikkelsen, 2009]), will also make CW Doppler lidars more susceptible to moving objects far away from the intended measurement point. Figure 3 illustrates that when the focus range is $r = 150$m, the full width at 0.05 of the maximum power spectral density of the weighting function (the dashed orange line) is 9 times longer than when r is 50m."

(2) Section 3.2

Thank you for pointing out some comments for Section 3.2. Since this is not a mature paper, we removed the whole section.

34 **Chapter 4:**

(1) Chapter 4 introduces a new method to improve lidar wind speeds in the case of rainy weather. Another related measurement campaign is described in a conference paper in Appendix A. In Chapter 4, the relation between the two papers should be clarified.

Thank you for pointing this out. We agree with this comment and have clarified the rela-

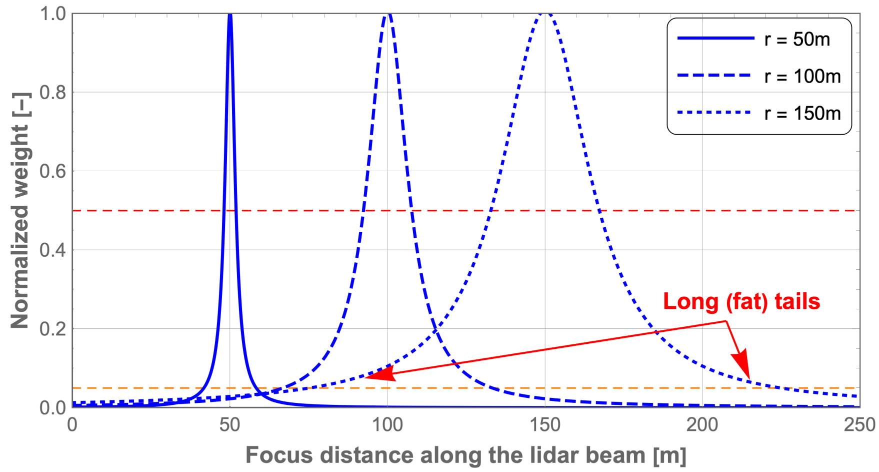

Figure 3: Normalized weighting functions for a CW Doppler lidar with focus distances r of 50m, 100m, and 150m, based on an $a_0 = 18$mm and the laser wavelength λ of 1.565μm. A dashed red line indicates FWHM, while an orange dashed line indicates 0.05 of maximum power.

tion between the two papers in the introduction part of Chapter 4, which is "By setting the spectral sampling frequency faster than most raindrops' beam transit time (the time it takes for the raindrop to enter and exit the laser beam), which is 3kHz, the rare instances where a raindrop resides in the beam could be identified by lidars. Two different methods were investigated to mitigate the adverse influence of raindrops on accurate wind speed estimation by CW wind lidars.

The first method is to filter away the Doppler spectra where the maximum value of a Doppler spectrum is larger than a given threshold. In comparison with a sonic anemometer measurement acquired at the same location, wind measurements by lidars are more accurate after removing rain signals, as described in Appendix A. However, for this method to be useful, a reference sonic anemometer measurement is required in order to determine the optimal threshold. Moreover, this method is tedious and unsuitable for big data sets, since the difference in wind velocities between lidar and sonic data is calculated first to obtain a reasonable threshold, and then the Doppler spectra are filtered accordingly.

By normalizing the noise-flattened 3-kHz-sampled spectra with their peak values before they are averaged down to 50Hz to retrieve wind velocities, the second method aims to suppress Doppler signals caused by raindrops."

(2) Also, we suggest that the concept of minimum beam-transit time is properly defined in the Introduction to paper 4.

Thank you for pointing this out. We have defined the concept of minimum beam-transit time both in the introduction part of Chapter 4 and in the paper, which is "By setting the spectral sampling frequency faster than most raindrops' beam transit time (the time it takes for the raindrop to enter and exit the laser beam), which is 3kHz, the rare instances where a raindrop resides in the beam could be identified by lidars."

(3) Further, it is unclear if it really is the time that should be stated in the legend of Figure 4.1. Isn't the idea to show that the choice of data processing (how many standard deviations that is needed for the threshold of the signal) is independent of the rain intensity?

Thank you for pointing this out. We agree and have linked the time and the rain intensity in the text as well as in the figure's caption, which is "In these minutes, different precipitation conditions are covered, including no rain (15:13), light rain (after 16:15), and moderate rain (15:48).".

(4) We further lack an assessment of the potential impact for commercial lidars (for example, nacelle lidars). Under what conditions can the proposed methodology be used?

Thank you for pointing this out. We agree with this comment and added one sentence in the introduction part of Chapter 4, which is "In summary, this proposed method would provide accurate wind speed estimations by commercial CW wind lidars for wind resource assessments, especially in rainy weather, as well as protecting blades from erosion.".

35 **Chapter 5: Please use more nuanced wording of the conclusions and please also return to the overall problem statement of the thesis concerning the need for replacement of the met masts. The current wording also tends to highlight only the positive results of the thesis, but the limitations and a critical view of the presented results and research are missing. We are also surprised that in Chapter 2 the number 0.2m/s was given to characterize the difference between lidar and simulations, but here the number 0.1m/s is used. There is likely a good reason for this, but it adds to the impression that all results presented are overly positively interpreted.**

If the candidate wishes to stick with the wording concerning Chapter 3, that the new probe volume shortening is "promising", in light of the challenges ahead, we wish that the statement is more thoroughly evaluated. A reader needs to be able to understand why this technique (which lost 90% if the signal strength) is superior to the other approaches presented in the thesis. Obviously, it was not promising enough for the candidate to pursue the methods further.

By rephrasing the research questions stated at the end of Chapter 1, as suggested above, we believe that it will be relatively straightforward to present a more nuanced and critical set of conclusions. The more nuanced conclusions will improve the thesis and give full justice to all the good work contained in the individual papers.

Thank you for pointing this out. We agree with this comment and have rephrased Chapter 5 with both positive and negative conclusions. The reason why we used 0.1m/s is that there are nine comparison cases and only one case has a larger difference of 0.2 ms^{-1}, while the others differ by 0.1 ms^{-1}. Therefore, it is acceptable to use 0.1m/s in the conclusion part.

Since the study results indicate that quarter-wave plates can compact the measurement volume under specific conditions without changing any part of the lidar system, we use the word "potentially" instead of "promising". Despite significant losses in laser power, further investigations should help to maintain the power. Due to the time limit for completing a PhD, we did not continue our studies.

Bibliography

N. Angelou, F. F. Abari, J. Mann, T. Mikkelsen, and M. Sjöholm. Challenges in noise removal from doppler spectra acquired by a continuous-wave lidar. In *Proceedings of the 26th International Laser Radar Conference, Porto Heli, Greece*, pages 25–29, 2012.

J. Berg, J. Mann, and M. Nielsen. Notes for dtu course 46100: Introduction to micro meteorology for wind energy. Technical report, Technical report, DTU, 2012.

J. W. Bilbro and W. W. Vaughan. Wind field measurement in the nonprecipitous regions surrounding severe storms by an airborne pulsed doppler lidar system. *Bulletin of the American Meteorological Society*, 59(9):1095–1101, 1978.

J. E. Bistline, E. Hodson, C. G. Rossmann, J. Creason, B. Murray, and A. R. Barron. Electric sector policy, technological change, and us emissions reductions goals: Results from the emf 32 model intercomparison project. *Energy Economics*, 73:307–325, 2018.

C. F. Bohren and B. A. Albrecht. *Atmospheric thermodynamics*. American Association of Physics Teachers, 2000.

A. Boreysho, M. Ilyin, A. Kim, M. Konyaev, G. Shchukin, and V. Y. Zhukov. Possibility and application of all-weather lidar-radio sensing complexes. In *Journal of Physics: Conference Series*, volume 1499, page 012025. IOP Publishing, 2020.

B. Bowen. Improved wind and turbulence measurements using a low-cost 3-d sonic anemometer at a low-wind site. Technical report, Lawrence Livermore National Lab.(LLNL), Livermore, CA (United States), 2007.

J.-P. Cariou, L. Sauvage, L. Thobois, G. Gorju, M. Machta, G. Lea, and M. Duboué. Long range scanning pulsed coherent lidar for real time wind monitoring in the planetary boundary layer. In *Proceedings of 16th Conference on Coherent Laser Radar*, volume 66, 2011.

L.-Y. Chen, A. K. Vinod, J. McMillan, C. W. Wong, and C.-K. K. Yang. A 9-μm precision 5-msa/s pulsed-coherent lidar system with subsampling receiver. *IEEE Solid-State Circuits Letters*, 3:262–265, 2020.

X. Cheng, Z. Peng, F. Hu, Q. Zeng, W. Luo, Y. Zhao, and Z. Hong. Measurement errors and correction of the uat-2 ultrasonic anemometer. *Science China Technological Sciences*, 58: 677–686, 2015.

A. Dyer. Flow distortion by supporting structures. *Boundary-Layer Meteorology*, 20(2):243–251, 1981.

J. P. Goit, S. Shimada, and T. Kogaki. Can lidars replace meteorological masts in wind energy? *Energies*, 12(19):3680, 2019.

V. Gordienko, A. Koryabin, N. Kravtsov, and V. Firsov. Wind doppler lidar with 1.5 μm fiber laser. *Laser Physics Letters*, 5(5):390, 2008.

M. Harris, M. Hand, and A. Wright. Lidar for turbine control. *National Renewable Energy Laboratory, Golden, CO, Report No. NREL/TP-500-39154*, 2006.

D. P. Held and J. Mann. Comparison of methods to derive radial wind speed from a continuous-wave coherent lidar doppler spectrum. *Atmospheric Measurement Techniques*, 11(11):6339–6350, 2018.

U. Högström and A.-S. Smedman. Accuracy of sonic anemometers: laminar wind-tunnel calibrations compared to atmospheric in situ calibrations against a reference instrument. *Boundary-layer meteorology*, 111:33–54, 2004.

S. Jacobs. Optical heterodyne (coherent) detection. *American Journal of Physics*, 56(3):235–245, 1988.

L. Jin, J. Mann, and M. Sjöholm. Investigating suppression of cloud return with a novel optical configuration of a doppler lidar. *Remote Sensing*, 14(15):3576, 2022.

L. Jin, J. Mann, N. Angelou, and M. Sjöholm. Suppression of precipitation bias on wind velocity from continuous-wave doppler lidars. *EGUsphere*, 2023:1–26, 2023.

J. Kaimal, J. Gaynor, H. Zimmerman, and G. Zimmerman. Minimizing flow distortion errors in a sonic anemometer. *Boundary-layer meteorology*, 53(1-2):103–115, 1990.

C. J. Karlsson, F. Å. Olsson, D. Letalick, and M. Harris. All-fiber multifunction continuous-wave coherent laser radar at 1.55 μm for range, speed, vibration, and wind measurements. *Applied optics*, 39(21):3716–3726, 2000.

N. Lazard. *Lazard's Levelized Cost of Energy Analysis—Version 13.0*. Lazard Zurich, Switzerland, 2019.

Z. Liu, J. F. Barlow, P.-W. Chan, J. C. H. Fung, Y. Li, C. Ren, H. W. L. Mak, and E. Ng. A review of progress and applications of pulsed doppler wind lidars. *remote sensing*, 11(21): 2522, 2019.

G. Luderer, R. C. Pietzcker, S. Carrara, H. S. de Boer, S. Fujimori, N. Johnson, S. Mima, and D. Arent. Assessment of wind and solar power in global low-carbon energy scenarios: an introduction. *Energy Economics*, 64:542–551, 2017.

M. Mauder, M. Eggert, C. Gutsmuths, S. Oertel, P. Wilhelm, I. Voelksch, L. Wanner, J. Tambke, and I. Bogoev. Comparison of turbulence measurements by a csat3b sonic anemometer and a high-resolution bistatic doppler lidar. *Atmospheric Measurement Techniques*, 13(2): 969–983, 2020.

K. McCaffrey, P. T. Quelet, A. Choukulkar, J. M. Wilczak, D. E. Wolfe, S. P. Oncley, W. A. Brewer, M. Debnath, R. Ashton, G. V. Iungo, et al. Identification of tower-wake distortions using sonic anemometer and lidar measurements. *Atmospheric Measurement Techniques*, 10 (2):393–407, 2017.

M. J. McGill, W. R. Skinner, and T. D. Irgang. Validation of wind profiles measured with incoherent doppler lidar. *Applied optics*, 36(9):1928–1939, 1997.

T. Mikkelsen. On mean wind and turbulence profile measurements from ground-based wind lidars: limitations in time and space resolution with continuous wave and pulsed lidar systems. In *European Wind Energy Conference and Exhibition*, volume 6, pages 4123–4132, 2009.

A. T. Pedersen, B. F. Montes, J. E. Pedersen, M. Harris, and T. Mikkelsen. Demonstration of short-range wind lidar in a high-performance wind tunnel. *Proceedings of EWEA*, 2012:20, 2012.

A. Peña, C. Hasager, J. Lange, J. Anger, M. Badger, F. Bingöl, O. Bischoff, J.-P. Cariou, F. Dunne, S. Emeis, M. Harris, M. Hofsäss, I. Karagali, J. Laks, S. Larsen, J. Mann, T. Mikkelsen, L. Pao, M. Pitter, A. Rettenmeier, A. Sathe, F. Scanzani, D. Schlipf, E. Simley, C. Slinger, R. Wagner, and I. Würth. *Remote Sensing for Wind Energy*. Number 0029(EN) in DTU Wind Energy E. DTU Wind Energy, Denmark, 2013.

A. Peña, E. Dellwik, and J. Mann. A method to assess the accuracy of sonic anemometer measurements. *Atmospheric Measurement Techniques*, 12(1):237–252, 2019.

R. C. Pietzcker, F. Ueckerdt, S. Carrara, H. S. De Boer, J. Després, S. Fujimori, N. Johnson, A. Kitous, Y. Scholz, P. Sullivan, et al. System integration of wind and solar power in integrated assessment models: a cross-model evaluation of new approaches. *Energy Economics*, 64:583–599, 2017.

W. H. Press, W. T. Vetterling, S. A. Teukolsky, and B. P. Flannery. *Numerical recipes*. Citeseer, 1988.

P. J. Rodrigo and C. Pedersen. Field performance of an all-semiconductor laser coherent doppler lidar. *Optics Letters*, 37(12):2277–2279, 2012.

A. Sathe and J. Mann. A review of turbulence measurements using ground-based wind lidars. *Atmospheric Measurement Techniques*, 6(11):3147–3167, 2013.

M. Sjöholm, N. Angelou, K. H. Hansen, J. Mann, and T. Mikkelsen. First measurements by the dtu wind energy short-range windscanner. In *Proceedings of EWEA 2012-European Wind Energy Conference & Exhibition; Proceedings of EWEA 2012-European Wind Energy Conference & Exhibition*, 2012.

W. R. Skinner and P. B. Hays. A comparative study of coherent & incoherent doppler lidar techniques. Technical report, Marshall Space Flight Center, 1994.

C. Slinger and M. Harris. Introduction to continuous-wave doppler lidar. *Summer School in Remote Sensing for Wind Energy, Boulder, USA*, 11, 2012.

C. M. Sonnenschein and F. A. Horrigan. Signal-to-noise relationships for coaxial systems that heterodyne backscatter from the atmosphere. *Applied optics*, 10(7):1600–1604, 1971.

M. F. van Dooren, A. P. Kidambi Sekar, L. Neuhaus, T. Mikkelsen, M. Hölling, and M. Kühn. Modelling the spectral shape of continuous-wave lidar measurements in a turbulent wind tunnel. *Atmospheric Measurement Techniques*, 15(5):1355–1372, 2022.

N. Wildmann, E. Päschke, A. Roiger, and C. Mallaun. Towards improved turbulence estimation with doppler wind lidar velocity-azimuth display (vad) scans. *Atmospheric Measurement Techniques*, 13(8):4141–4158, 2020.

J. Wyngaard. Cup, propeller, vane, and sonic anemometers in turbulence research. *Annual Review of Fluid Mechanics*, 13(1):399–423, 1981.

J. C. Wyngaard. *Turbulence in the Atmosphere*. Cambridge University Press, 2010.