Answers to Reviewers

Answers to Reviewer 1

1. An article by Stoffelen et al. (Bull. Amer. Meteor. Soc., 101, 2020) on the requirements and capabilities of future satellite-borne wind observations was recently published. As the study, amongst others, discusses the way forward toward an operational wind profiling mission, its conclusions should also be considered and referenced in the present manuscript.

This reference is now included in the revised version of the manuscript and cited in the introduction. Other references are also added to this respect (Baker et al., 1995, 2014, Stoffelen et al. 2005), or further in the text for radiation objectives (Stephens et al., 2002, Illingworth et al., 2015) in the text in coherence with the reference list (corrected).

2. In the entire paper and especially in the introduction, I am also missing a reference to the Optical Autocovariance Wind Lidar (OAWL) developed by Ball Aerospace (Tucker et al., J. Atmos. Ocean. Tech., 35, 2018), although the citation is included in the reference list. The OAWL instrument also relies on a quadrature Mach–Zehnder interferometer and thus represents a very similar concept as the one described in the manuscript. Therefore, I suggest expounding on the advantages and disadvantages of the proposed lidar design compared to the OAWL system.

References on OAWL development and results (Tucker et al., 2018, 2019, 2020) are added in the introduction. Coherence with reference list has been checked.

The all-prism design a priori appears more stable than the free-space cat-eye design which depends on the mechanical stability of independent optical elements. Nevertheless it is difficult go further in this discussion without a careful comparison of the two systems which would be too long and out of the scope of our paper. The main differences between the OAWL and HSRD-LNG designs are discussed in section 3.2.

3. Page 5, line 152: Please clarify what is meant by off-axis MI interferometer. Otherwise many readers might be lost in the following section.

Better than trying an explanation that may be long and awkward, we refer to Liu et al 2012 to introduce the off-axis (or tilted) MI.

4. Page 7, lines 202 ff.:

- The proposed design envisages the use of multimode fibers for guiding the signal and reference beams to the receiver. What is the expected signal loss introduced by the fibers? Do the authors expect speckle noise to be an issue for the measurement accuracy?

Additional texts are added in section 3.1 to give more information. Section 3.1 is modified as

An optical mode scrambler is inserted on the fiber path in order to obtain a uniform illumination distribution at the interferometer input independently of the illumination distribution in the telescope focal plane (field stop). The mode scrambler consists in two lenses, the first lens images the output of the first fiber on the second lens while the second lens images the first lens aperture on the second fiber. This way, the near and far fields at the output of the second fiber are uniform and fill the whole fiber beam étendue. This arrangement ensures that the interferometer response is not biased by transmitter/receiver misalignment. A small plate and a symmetrical lens arrangement inserted in the mode scrambler allows the injection of a small amount of the emitted pulse used as the reference signal and transported by a second optical fiber. The fiber and the scrambler ensure the complete depolarization of the signal before it arrives on the MZI (even when including a polarization splitter in the front optics). This was implemented and successfully tested on the airborne LNG system. The overall efficiency of the fibers and mode scrambler on the atmospheric signal path is around 80%. The output of the fiber is then collimated by a 15-mm-focal-length lens at the MZI input port.

and in section 4.1

The speckle noise is not an issue for the measurement accuracy, as the design is based on a large aperture and wide field of view. The receiver number of spatial speckles Ms, defined as the ratio of the solid angle of the transmitted beam to the diffraction solid angle of the telescope is 2.75 10^4. The number of temporal speckles Mt is around 500, due to the extended range gates. The total number of speckles Mspec=Ms.Mt is then approximately 1.4 10^7. As the number N of collected photo-electrons per shot and range gate is on the order of 10^3, the speckle noise (given by the ratio N/Mspec) is much smaller than 1, so that the noise variance is not modified. Note also that the fiber beam étendue is necessarily larger than the telescope beam étendue in order to accept the received beam with a substantial alignment margin. The maximum number of spatial speckle of the fiber (9.10^4) is then larger than Ms and does not limit the total number of speckle of the signal.

- How is the mode scrambler realized and what is its expected transmission based on the experience with the LNG?

The beam scrambler principle is based on the fact that the near field (illumination distribution) at the fiber output, after a propagation of about 1m, is always uniform, whatever the light illumination at input (in the limits of the fiber beam acceptance). The mode scrambler arrangement is now detailed in the modified section 3.1 (see above).

5. Although the theoretical error estimation yields convincing results, it would be desirable if the authors could add some experimental results from the LNG to support their theoretical findings. Is it possible to transfer the results from Bruneau and Pelon (2015), particularly in terms of the SNR and the impact of solar background, to the proposed spaceborne lidar in order to estimate the systematic and random wind and backscatter ratio errors based on actual atmospheric measurements? In the discussion chapter, the authors state that "a better overall performance is expected to be achieved as based on realistic parameters derived from airborne operation with a minimized risk and increased design compactness and reliability". At the end of the conclusion chapter, it says "The performance analysis on the QMZ interferometer is supported by measurements performed in the frame of the UV HSRD-LNG airborne lidar developed and operated by LATMOS." Could they please elaborate on these statements?

*We agree with the reviewer that confronting observations and numerical simulations of measurements with our airborne lidar would be of added value to support our design proposal. We thus added a new Appendix (*Appendix C. Analysis of observed HSRD-LNG performance*) to the paper, to show and discuss dispersion of measurements (bias and accuracy). We used slant observations obtained as the aircraft was turning. This allows to compare lidar VHLOS measurements through VAD analysis to collocated radiosonde measurements and analyze bias in retrievals as well as fluctuations. The Appendix includes additional figures (profiles, histograms of differences) based on dispersion analysis between VHLOS observations and VAD fit. The differences between lidar measurements and theoretical error calculation are derived from our simulation model (from Eqs 6 and A17) to show* *statistical agreement for both wind and backscatter coefficient retrievals (X-Y plots). A table is added to synthetize results on wind measurements.*

6. The quality of the figures should be improved. In particular, the resolution of Figs. 3 to 11 is rather low which makes it difficult for the reader to get the details of the plots.
The quality of these figures has been improved using bold fonts.

Technical corrections listed by the reviewer are made in the text.

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Answers to Reviewer 2

The paper has some confusing wording and sentence structure and would benefit greatly from editing by a native English speaker.
We adopted English edits proposed by the reviewer and did our best to improve wording and sentence structure.

Most of the figures, at least in the version this reviewer was provided, have font that is too small or blurry to read (especially figures 3-11). Please replace with figures with higher resolution and/or larger font.
We now use bold fonts

The paper presents a somewhat limited view of work done in the fields of wind lidar and HSRL. There is similar work being done in this field that the authors could use to further support and even enhance the important case that they are making in this paper. For example, presentations on OAWL at the Aeolus CalVal workshop have also supported the idea of using a short optical path difference QMZ interferometer for Aeolus and have demonstrated comparison with Aeolus data. While the interferometer implementation is different, the mathematical concept and listed advantages are quite similar.
Although a reference to OAWL was already included in the reference list, the text corresponding to the reference was partly removed. References to the new results (which provide additional support to our paper, as mentioned by the reviewer) as reported at the Aeolus Cal/Val workshop were added.

A number of variables in the text (including italic font, subscripts, symbols) do not match those in the equations. This will presumably be caught in a more thorough editing process. A few of the publications listed in the references section appear to be missing in the text: Baker, Benedetti, Lux, Reitebuch 2020, and Tucker. (Note, the text refers to Reitebuch 2019a, and Reitebuch 2019, but there is only one Reitebuch 2019 in the list of references. Perhaps one should be the Reitebuch 2020 reference?)
The text and reference list have been edited and we have added reference to Baker et al., 1995 and 2014, Stephens et al., 2002, Illingworth et al., 2015 in the text.

• Line 14: Might reword to say, "…wind profiler using a *single* fixed line-of-sight lidar from space." As some other proposed concepts have looked at fixed dual lines-of-sight.
OK

• Line 19: "This ability is…" To what ability is the sentencing referring?

To make it clearer, the sentence is rephrased as

"This ability to profile wind and cloud-aerosols radiative properties is meeting"

• Line 30: could clarify to say that, "… backscatter coefficients can be retrieved with uncertainties better than a few percent where backscatter levels exceed XX%, such as in the boundary layer and …."

OK. We propose to refer to the scattering ratio more commonly used to define backscatter level. The new sentence is

"… better than a few percent when the scattering ratio exceeds 2, such as …"

• Lines 65-80: This is a limited list of HSRL approaches and could be expanded to include the OAWL approach as well as work being done by Fahua Shen:

o Fahua Shen, Jie Ji, Chenbo Xie, Zhao Wang, and Bangxin Wang, "High-spectral-resolution Mie Doppler lidar based on a two-stage Fabry–Perot etalon for tropospheric wind and aerosol accurate measurement," Appl. Opt. 58, 2216-2225 (2019)

The work of Shen et al. is referring to Fabry-Perot interferometric analysis but the goal of these lines is rather to focus on MZI. The proposed reference was thus not included but the other suggestions from reviewer 2 have been accounted for. These lines have been rewritten as:

"In the United States, Ball Aerospace developed a Doppler wind lidar including a spectral discriminator based on the same principle of Quad-channel MZI (Grund et al., 2009) which was successfully operated onboard the NASA WB57 aircraft (Tucker et al., 2018) and further involved in the Aeolus CA/VAL (Tucker et al., 2019, 2020)."

o Tucker, S. and Weimer, C., 2018, October. Benefits of a quadrature Mach Zehnder interferometer as demonstrated in the Optical Autocovariance Wind and Lidar (OAWL) wind and aerosol measurements. In Remote Sensing of the Atmosphere, Clouds, and Precipitation VII (Vol. 10776, p. 107760E). International Society for Optics and Photonics.

More recent references on OAWL will be added (see above)

• Line 123: DE-FP and Fizeau interferometers will have a "small" acceptance angle based on the finesse. Low Finesse etalons will have higher acceptance angles than higher finesse ones, but will provide poor frequency discrimination. Perhaps this can be clarified in the sentence by adding something like…, "*If they are providing high frequency discrimination*, both DE-FP and Fizeau interferometers will have small angular acceptance."

As the sentence is referring to the ALADIN context (and not a more general one), no modification is to be made

• Line 125: Don't the narrow interferometer fields of view (FOVs) *also* impose a high accuracy requirement on the alignment between the telescope and the receiver?

Yes agreed, the sentence is modified as

"… the large telescope aperture is resulting in a very small field-of-view and imposes a high accuracy requirement on the transmitter-receiver co-alignment."

• Line 134: Could also include Grund 2008

o Grund, C.J., Howell, J., Pierce, R. and Stephens, M., 2009, April. Optical autocovariance direct detection lidar for simultaneous wind, aerosol, and chemistry profiling from ground, air, and space platforms. In Advanced Environmental, Chemical, and Biological Sensing Technologies VI (Vol. 7312, p. 73120U). International Society for Optics and Photonics.

The reference to Grund et al., 2009 has been added (see above)

• Lines 149-153: The frequency stability is only required IF an accumulation detector is used for the observation. If a higher speed detector is used, such as a PMT or APD (e.g., CALIPSO detectors) then the reference can be updated every pulse, if needed.

Agreed. The sentence is modified as

"Appropriate frequency stability is just required during the signal accumulation needed for a single measurement"

• Line 161: 1.4 or sqrt(2)? Is this just for simplicity?

It is sqrt(2), but 1.4 reads better and it is thus kept as a decimal number. An explanation has been added at the end of the sentence as

"… higher optical efficiency (factor 2)."

• Line 164-168: This may be due to the grammatical structure of the sentence, but it seems that the authors are implying that (because of the reflection from one edge of the Aeolus ALADIN DEFP is being used in the other edge) the Aeolus approach has a theoretical 1.4x advantage over the QMZI approach. However, it's unclear how it is possible to provide perfect efficiency through a double edge approach while maintaining the necessary frequency discrimination. Unlike with the QMZ approach, there will always be some molecular backscatter signal that is not allowed to pass through either edge filter and thus will be lost. I am aware the authors are quite familiar with how Aeolus (and DEFP) operates, so perhaps this was not the intent of the paragraph, but it's important to not lead other readers into an unintended comparison.

This is just a factor 2 in energy (split or combined beams), as above. Modified as

"… larger than 1.4 (limit case of a factor 2 gain in the transmitted energy)"

• Line 176-179: These statements could use a little bit of qualification. For example, *for short OPDs*, the particulate signal will be near unity, but not if the OPD is long compared to the laser linewidth. Perhaps just state that the discussion refers to the 3.2 cm OPD concept being presented here.

Yes, rephrased as

"Provided the emission linewidth is sufficiently narrow, the particulate signal produces an interference contrast near unity, significantly higher than that produced by the molecular signal (see appendix A, Eq. A14). "

• Line 189: The term "emitter" is used here (and elsewhere in the paper, including Table 2) however the term "transmitter" is used in much of the Aeolus literature (e.g., Reitebuch, et al., 2009).

All the text has been modified to change "emitter" into "transmitter"

• Line 190: This point about "acceptance angle" (or "maximum field angle" or "range of field angles") is an important point to make!

Yes indeed

• Line 195: Suggest the wording "…can be mounted on the same plate, *as was done on CALIPSO*, and *boresight* mirrors…." (such language indicates heritage for the design)

Agreed. Done

• Line 220: perhaps add: "..to obtain a uniform illumination *and uniform field* distribution…"

Rewritten as

"uniform illumination at the interferometer input independently of the illumination distribution in the telescope focal plane (field stop)."

• Line 225: Also successfully implemented and tested on the airborne OAWL system (which further supports your case!)

OK

• Line 265: Do you mean that each spot fills an 8x8 pixel area?

Yes, rewritten to make it more explicit as

"Each spot fills a 8 by 8 pixels area in the …"

• Line 261-270: The discussion is focused on using the same A-CCD as was used on Aeolus, however discussions of using a new ACCD or different type of detector are under consideration for Aeolus. Could the authors suggest what approach would be idea for a QMZ design? Perhaps ranging from more ACCD rows, to a different type of detector?

The detectors are a critical part of the system in terms of intrinsic noise, this is the first parameter to consider, but various technical solutions are possible. This discussion is just intended to focus on this question without going into technical details.

• Table 2: "Emitter Linewidth": according to the text, the 200 MHz represents the allowed spectral jitter or drift over the 50 pulse (1/2 second) accumulation time and likely does not represent the laser linewidth. Perhaps clarify this by calling it "Accumulated Emitter Linewidth (1/2 s)" or even "Allowed laser linewidth". Also, (see above comment on "emitter" vs. "transmitter").

OK. Text clarified as Accumulated Emitter Linewidth

• Line 293: Clarify that "*For a 3.2 cm OPD*, the modulation by the molecular return…." For some longer OPDs, molecular return will provide NO modulation, just offset.

Modified accordingly

• Line 305: YES, this is true, however won't there be a backscatter ratio below which the contrast difference is too small to clearly estimate?

The random error on V_{LOS} is inversely proportional to M_{atm} (Eq. A16) which is itself given by Eq. A4. The error dependence with the backscatter ratio can be easily derived from a combination of these two equations, all things equal otherwise.

• Line 330: Be sure to clarify that because of the *specific type* of field compensation used in this specific design, the OPD variation is only dependent on 4^{th} power of the source angle. There are other types of field compensation (e.g. cat's eye) for which the OPD variation

The text is clarified as

“Note also that, for our on-axis field compensated design, the variation of OPD is only dependent on the fourth power of the source angle (the second power dependence is canceled and the third order dependence is null due to all on-axis optical elements). “

• Line 331: Please provide a reference for the equation provided in this line.
Instead of giving a reference, the calculation of the residual wavefront error given by the beam étendue is detailed in Appendix B (now renamed as MZI field compensation and aperture.*)*

• Line 340: Indeed, smaller incident angles on beamsplitters make it easier to balance reflection and transmission values.
Yes, but other considerations such as beam path separation in the interferometer require to increase the incidence angle to about 40-45°.

• Line 355: The authors suggest using the laser in multimode operation to perform the amplitude (sensitivity parameter) calibration– but for many seeded lasers, running them multi-mode comes with a risk of damage due to modal interference.
Yes, but the laser should be designed to withstand this multi-mode operation in case of seed laser failure.

• Line 337: modulation calibration can also be performed by allowing the laser to drift in frequency while remaining seeded. This can be achieved fairly easily by temperature tuning the seed laser source, or laser cavity.
Agreed. Added part to the sentence:
“or by tuning the laser seeder frequency”

• Line 369-374: Please indicate the wavelength for these profiles in the text for clarity. Have these profiles been validated in any sense using data from Aeolus and/or CALIPSO?
355nm information added

• Figure 4: Please increase the text font size and overall figure clarity. The text is too blurry to read. Sub figures (a) and (b) do not have titles indicating the wavelength, etc. Likewise, please indicate the wavelength for these profiles in the figure caption.
OK, improved.

• Section 4.2: It’s nice to see this study on the impact of the horizontal light of sight angle on performance, though it would be interesting to compare it to previous studies done for Aeolus back when the mission was selected. This type of angle analysis shouldn’t be unique to this particular design, but if it is (for some reason this reviewer is missing) then please clarify. It indicates that the difference in 1/R^2 is less than the error in retrieving the horizontal LOS from a smaller pointing angle.
We don’t have information on reasons of the choice of a 35° angle for Aeolus. But clearly, our calculations show that a slant angle around 45° gives a smaller V_{HLOS} *error.*

• Table 3: The authors may wish to clarify here that the total power is the same for the two systems. Aeolus requirements were based on a 2x higher laser pulse energy, and the QMZ approach is based on the as-operating Aeolus pulse energy but with a 2x pulse repetition frequency.

Mentioned in the text.

• Line 481: "where such an accuracy may be more acceptable than bias for assimilation purpose…" The terms "accuracy" and "bias" are often tied together. Do the authors mean that precision (or "uncertainty" or "random error") is more acceptable than bias?
Clarified (precision as random error)

• Line 508: The authors refer to a 10-3 relative error in the sensitivity calibration of each QMZ channel, however it might be good to clarify that this does (or does not) include the detector response sensitivity. For example, if one channel sees a high portion of the interferometric fringe during the outgoing pulse, will the detector response have any impact on returns for that channel?
Yes, the calibration of parameters a_i clearly includes the detector. Our calculations give the sensitivity of the measurement bias with regard to these parameters. If the linearity of the detector is an issue, this calibration should be done at different illumination levels and corrections should be applied accordingly. However, we will not discuss this possibility because it depends on the characteristics of the detectors, on which we have few details.

• Line 564: the authors should clarify that "…the *short OPD* QMZ does not attempt to separate molecular…." There are long OPD QMZI designs that easily separate these two.
Yes, but the paper focuses on the use of a single MZI, and this turns to the use of a short OPD. Long OPD MZIs are discussed in BP03 (referred to in the text before). The sentence is modified as " …the proposed QMZ …*"*

• Line 619-620: Is it possible to show a scaling of the performance of the UV HSRD-LNG airborne lidar to space-based operation (e.g., see Baidar et al. 2018).
• Appendix A: This is a good review of previous papers by the authors describing QMZ performance. Has data from the LNG system been used to validate this model and if so, how did they compare?
*We added a new Appendix (*Appendix C Analysis of observed HSRD-LNG performance*) to present results obtained with LNG and discuss bias and errors as compared to simulations.*
The simulation model has been applied to LNG observations. Sentences are added in the text to refer to this addition. More particularly at the end of section 4.1, it is added that
"Appendix C shows measurements performed with HSRD-LNG where the error calculated with equations similar to that used in the performance model is compared to the experimental error. Fig. C1 shows that the calculated and experimental errors are in good agreement. A comparison between lidar wind measurements, including calculated error, and collocated radiosonde profiling is also presented in Appendix C (Fig. C2 and Table C1). A similar comparison of measured and calculated errors on the backscatter coefficient shows also a good agreement (Fig. C3). We can therefore consider that these results validate the equations used in the performance model."

• Line 670-684: Why is 3 cm being used here for the OPD vs. the 3.2 cm chosen for the optimal system?
Though 3.2 cm is the final choice for the OPD, in this section we present only preliminary calculations of M_{par} and M_{mol} and a value of 3 cm is indicative for the range of OPDs considered in the paper.

• Line 689: The assumption that the background can be measured over a long duration may meet challenges with highly varying cloud albedo over orbit.

Long duration stands for a long range gate as compared to the range gate required for vertical resolution. Background and signal measurements can be separated by less than 0.001s that corresponds to a separation of a few meters along track.

• Appendix B: It's quite nice to see this analysis in a publication, showing the impact of OPD and maximum aperture driven by étendue and it would be nice to see this in the main paper, if room allows. It's important to clarify, however, that the analysis applies to QMZ structures using field compensation plates.

We made the choice to keep the main part as focused as possible. The field compensation is now more detailed in the new Appendix B (including 7 more equations).

• Appendix C: Many of the points made in this section are really important in the argument for using a QMZ type system for an Aeolus follow-on. If possible, can more of this be moved into the main part of the document?

*Yes, only a small part of it is in the text (sections 3.3, 4.4 for example). As mentioned, we want to keep the main text as focused as possible. We think that interested readers will read the new Appendix D (*Atmosphere-modelling dependent errors in the retrieval of particulate backscatter and extinction coefficients*).*

Modifications have been made accordingly to detailed comments