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Atmospheric Measurement Techniques Discussions

Interactive comment on "Rayleigh wind retrieval for the ALADIN airborne demonstrator of the Aeolus mission using simulated response calibration" *by* Xiaochun Zhai et al.

Anonymous Referee #2

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This paper presents an alternative technique for retrieving LOS wind estimates from the molecular channel of the Aeolus Airborne Demonstrator (A2D) using modeled response functions ("Simulated Rayleigh Response Calibration" or SRRC) derived using best-fit instrument models and the given atmospheric conditions (temperature and pressure) when available from other observations.

The SRRC approach provides some advantages over the "traditional" double-edge approach of measuring calibration response curves during the test process (the MRRC approach), but the authors could do a better job of explaining the reasoning behind this (vs. just listing numbers) at the beginning of the paper and in the abstract. The ap-

proach is a good idea, especially when faced with consistent Mie contamination during flight tests. Have other double-edge wind lidar researchers done anything similar to this before?

An explanation of the physical differences between the internal reference channel and the atmospheric channel would be helpful. For example, does the IRC have a different set of field angles into the FP etalons than provided by the telescope/receive path returns? Does the IRC only see narrowband light?

The paper would also benefit from a short, clear discussion on the topic of Mie (aerosol) contamination on the Rayleigh calibration as the topic comes up several times in the paper. Present the reasons for the aerosol induced bias and reference the literature. This could be followed by cleaning up some paragraphs that vaguely refer the issue, without explaining it.

Some additional proofreading for english language/grammar should catch some minor errors. Remaining comments listed by page/line#.

A "Fair" rating is listed under Scientific Quality because it's not quite at the "Good" level with respect to referencing related work and being clear on the issues addressed, but with minor improvements as listed above and in the following comments, it will likely be above good.

Overall, this is an interesting and useful paper for the field of double-edge direct detection Doppler wind lidar systems.

Page 2 line 24-26: This sentence describing Aeolus is awkward. Perhaps reword as "The novel combination of these two techniques, integrated for the first time into a single wind lidar, expands the observational altitude range from the ground to the lowermost 30 km of the atmosphere." Line 29: Can delete the words, "as well" from the end of the sentence since it begins with "Furthermore".

Page 3 Line 9: Can the authors expand a little bit on the causes of ". . .the atmospheric

and instrumental variability" for readers not familiar with the observation approach. For example, how atmospheric pressure/temperature impact the MRRC and what varies in the instrument (temperature impacting alignment? Variations in the field of view/field angles entering the etalon? Etc.?) Line 12: update to read, "It is based on an accurate theoretical model of the FPI transmission function. . .." Line 28: edit to read, "Table one lists FPI-based direct detection wind lidar systems that are capable of measuring wind information. . .." Note that not all existing FPI systems that can be modeled this way are listed in the table, there are others in existence.

Page 4 Line 10 – Should be "atmospheric conditions"

Page 5 Line 19: fix to read, ". . .the transmission functions of the FPs for the atmospheric path are slightly different compared to . . ." Then please explain why this is (physics causing the differences). Line 24: "regardless of measurement or simulation method, any angular alignment drift will change the incidence angles on FPIs, and hence change their transmission characteristics." Technically, the FPI transmission characteristics should be a function of incidence angles, field of view, temperature, pressure, thickness or gap length, finesse, etc. so perhaps the better term here (and elsewhere) is to say that "any angular alignment drift will change in the incidence angles on the FPIS, resulting in a different transmission value." (or something similar).

Page 6 Line 5: This is an unusual mix of variables (wavelength and frequency shift), but ok. Line 17 and 19: The authors state that Equations 3 and 4 represent convolutions, but this is not mathematically so. These are integrations over frequency of the product of the FPI transfer function times the specific input spectrum value at that frequency. Likewise, integrating this product over only one free spectral range implies that the authors assume there are never any signals outside the etalon FSRs (e.g. where the etalon can start to transmit again). This may be practically true for most applications/wind speeds/platform pointing motions, etc. but should at least be stated as an assumption.

Page 7 Line 5: defects could be in the FPI mirror surface(s) (plural) right? Line 7: Why not also mention/reference the works of Spinhirne, McGill Line 9: R is the mean reflectivity of the etalon mirrors? (again, plural?) Line 14: Suggest instead to say, "An easily calculated analytical expression. . .." Lines 16-21: The paper might read more easily if this paragraph was moved up earlier in the discussion. Line 21: The "magenta" filled area appears more "pink" – perhaps use that term instead, or "light magenta"

Page 8 Line 1: Here the authors could clarify for the readers not familiar with double-edge approach why the biases are worse when Mie signal is significant but not good enough to measure winds using the Mie channel. Can this be shown somehow in Figure 2? Line 4 (paragraph 2): clarify that the procedure is done assuming no Mie interference (or otherwise?) Line 9: The text says that the red-square marks +/- 850 MHz, but the figure looks like its closer to 1 GHz. Please rectify one or the other to match. Line 22: clarify that the "Then the fit of the SRRC for the internal reference and atmospheric paths can be expressed as a sum of a linear fit plus a 5th order polynomial:"

Page 9 Line 5: replace "In the frame of. . ." with "As part of.." The rest of this paragraph would benefit from additional proofreading for English grammar. Line 19-20: Suggest a rewrite to read "Time-space matching datasets between dropsonde and A2D can be used as both references to validated A2D wind measurements and to provide essential. . .." Line 23-24: This sentence repeats a little bit of what was written before, now referring to "illumination properties" - can you be more specific? Is this a function of differences in the spatial (e.g. the pupil) distribution or in the field (e.g. angular) distribution?

Page 10 Line 13-14: The authors state that the, "measured response values obtained from A2D wind velocity measurement mode are brought into the fitted SRRC. . .." What does "brought into" mean here? Is this a mapping? What is the process for doing this? Line 19-20: Add "and possible vertical velocity components" to the end of this

first sentence.

Page 11 Paragraph 5.1: The figures described here would benefit from a diagram showing the campaign configuration. Line 8-10: This mentions of the difference between ATMG and INTG due to different illumination. The reasoning for this should be described earlier in the paper and referenced back. The authors seem to change terminology back and forth throughout this section (and the corresponding figures) which makes reading the section slightly more challenging. Specifically, on page 8 the terms defined in Equations 8 and 9 are referred to as beta=sensivity and alpha = intercept, but in Figures 7 and 9 only the terms sensitivity and intercept are used. Perhaps adding the variable names beta_ATM and delta-alpha_ATM to the captions for figure 7 and 9 would help. Likewise add the descriptive terms to the caption for figure 8. Line 10: What is the source of the atmospheric signal in the internal path on airborne testing (INTA)? Is there a delay in the internal reference path that causes the INTA signal to overlap with near field returns due to early overlap? Does multiple scattering play a role in these early returns? Lines 13-20: There are numerous papers discussing modeling of FPI performance. Perhaps some of these could also be referenced: âĂć Jack A. McKay and David J. Rees "High-performance Fabry-Perot etalon mount for spaceflight," Optical Engineering 39(1), (1 January 2000). https://doi.org/10.1117/1.602361 âĂć P. D. Atherton, N K. Reay, J. Ring, and T. R. Hicks "Tunable Fabry-Perot Filters," Optical Engineering 20(6), 206806 (1 December 1981). https://doi.org/10.1117/12.7972819 âĂć J.A. McKay and David Rees , “Space-based Doppler wind lidar: Modeling of edge detection and fringe imaging Doppler analyzers” âĂć Others by McKay, and Spinhirne, McGill, Gentry, etc. Line 21: What is meant by the phrase, “Different from ALADIN” ? Were the ALADIN transmission curves (internal and atmospheric paths) never measured?

Page 12 Equations 15 and 17 define variables “A” and “B” for the Atmospheric and Internal paths, but this terminology is confused with the use of those variables as names for “Filter A” and “Filter B” (the two edge filters) per the labeling in Figures 1, 5, etc.

Page 13 Lines 11-13: This information could also be included in a previous section on the impact of angles on FPI transmission functions. Line 13: “Assuming the center frequencies of filter A and B have the same offset. . .” Are there any challenges to this assumption? If angles get larger, does the center frequency shift more for A vs. B? A diagram (or a reference to a paper with a diagram) of the two paths through the system might help confirm that the offset is the same. Lines 15-20: The text refers to the plots in Figure 8 and talks about range gates, but the figure shows altitude bins. Which terminology should be used? Line 20: “all available range gates . . . are used to calculate the cost function. . .” – does this assume there is no aerosol present in this data set?

Page 14 Lines 28-29: The sentence, “However, the temperature difference between MRRC and the actual wind measurement must. . .” is confusing. Perhaps the authors meant, “However, differences in the atmospheric temperature profile between when the MRRC was obtained and when the actual wind measurements were acquired are a known important source of wind bias, which are especially severe in cases of large temperature differences.” Lines 21-33 (and line 11 on page 15): This issue is the basis for all the work done in this paper, right? So this should be right up front in the beginning of the paper, to help the reader understand why the work is being done and described.

Page 15 Line 11: This is the key point of the paper, but it is muddled a little due to grammar. Perhaps say “This is one of the limitations of the A2D MRRC approach which can be overcome using the SRRC approach” Line 15-17: Can you be more specific than saying the response calibration is affected directly? Perhaps say that the aerosol spectrum shifts the centroid of the atmospheric/filter transmission product, thereby biasing the wind speed estimates (or something like that)? Line 25: “Indeed, the Mie contamination. . .” – this is another key point for the paper and justification for doing the SRRC. While a detailed discussion might not be within the scope of the paper, the paper would benefit greatly from some discussion as the topic of Mie contamination

comes up several time.

Page 16 Line 17: remove the “are” from the beginning of the line. Line 28: “overcame” should be “overcome” here.

Page 17 Line 1: The sentence should probably read, “Overall, the SRRC allows correction for variability in atmospheric and temperature profiles, when known,. . .”

Figures Figure 2: Please also use the variable name (e.g. “Rx”) with “Response” (in the caption and the axis labels) Figure 3: Again, refer to the variable fc when discussing the cross point frequency. Figure 5: Should the blue dashed curve be labeled “TB from INTA” (vs. TA) ? Figure 6: The authors could clarify for the reader that the MRRC lines are repeated through out the plots, e.g. say “(red and blue dashed-lines, respectively, same on every plot)” Figure 9: clarify that (c) represents the retrieved LOS velocity Figure 13: Can the authors say anything about the potential presence of vertical velocities and their impact on the comparison? Can the authors provide error bars on the LOS velocity retrievals? Even CDL systems have errors.

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