Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-41-RC1, 2021

Comment on amt-2021-41

Anonymous Referee #1

Referee comment on "MicroPulse DIAL (MPD) – a diode-laser-based lidar architecture for quantitative atmospheric profiling" by Scott M. Spuler et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-41-RC1, 2021

Review of: "MicroPulse DIAL (MPD) – a Diode-Laser-Based Lidar Architecture for Quantitative Atmospheric Profiling" by Scott M. Spuler et al.

General remarks

In this article, Dr. Spuler and his colleagues detail the technical advances made for the new generation of the unique micro-pulse DIAL lidars they have been developing at NCAR and the Montana State University for more than 15 years now. These network-able systems may well represent the future of operational high-resolution atmospheric sounding for water vapor, temperature and aerosols. The 5th generation of instruments is here described in greater detail than in previous articles, and many technical difficulties are addressed and solutions are found that are highly relevant to the operational capabilities of these systems. The new water vapor measurements are validated, with a focus on the lowest altitudes (below 500 m), where accuracy is clearly improved.

Therefore, this work is of great interest for the lidar community, and for the validation of an MPD network as a reliable tool to the greater atmospheric science and meteorology communities. The science and technical aspects presented are perfectly sound and clearly part of a staggering expertise gathered by Dr. Spuler's team on these systems, requiring mastership of cutting-edge aspects both in electro-optical systems and lasers on the one hand, and in atmospheric physics on the other hand.

As a lidar scientist, I can testify to the fact that because of this, it is never straightforward to find the right platform or means of expression to satisfy readers of both backgrounds when writing such a paper. In the case of this article, my thirst for technical details on the lidar is well-satisfied except on a couple aspects that I would ask be developed. But I wonder if the average reader from the atmospheric science background would find the article too dense as it is. Maybe another reviewer from another background will confront this aspect: a lot of very specific technical aspects are addressed with no real common thread leading from one to the next. The importance given to some developments is not proportional to their relevance to the whole. And mostly there seems to be a lack of structure that would allow a reader to skip certain specific parts and get to what interests them. Some sections are unclear and must be made much easier to understand for the non-expert reader. The overall schematics of the system is given too late in the paper, and there is no outline, yet the reader definitely needs a map to understand where they are in this complex system.

The main path I see out of this issue is to rewrite the introduction with a clear statement of the focus of the paper, an outline, and to separate the introduction sub-sections in a Section 2 that will also include the lidar main schematics. Only then the authors may delve into more details with the transmitter and receiver. Also in those sections there could be more structure, maybe with sub-sections. I am indeed sorry to ask this because these recommendations just amount to reshaping the paper into the orthodox, classical, almost catalog-like structure for a lidar validation paper, whereas the current structure was more out-of-the-box, but I must confess the current shape does not work.

This, and the several new technical aspects I think were not developed enough or unclear, and would maybe entail a new appendix, requires what would be called "major" changes in the article before I would state it is ready for publication. However this should not require too much work, and I am certain the authors would even be able to submit a suitable version in a matter of days.

Specific remarks

I have provided all of my specific remarks in the shape of an annotated PDF manuscript. I also copy the remarks that are not about typos or syntax below, so that they can be more easily addressed by the authors in their reply. Please see the annotated PDF for other corrections.

L12: Please specify at the end of the abstract that the paper will validate the capabilities of the new generation on water vapor measurements only. Maybe give some summarized results.

L47: This intro is a bit unorthodox, with no outline and Figures in the introduction; this is not a problem in itself, but at this point I do not see where the paper is going. There is indeed a lot of information and it would help if an outline was specified somewhere in the introduction. I would suggest here, because the introductive subparts are rather long. And maybe separating them as Part 2. Please also specifiy that the focus of the qualification part of the article is WV measurements and that the HSRL/T capabilities are not developed.

L185: It is not mentioned whether the previous part of the setup is made with PM fibers. But if up until then none of the fibers are polarization maintaining, how is it that the polarization alignment required to the passing axis of the Farady isolator / the slow axis of the TSOA does not fluctuate with temperature / fiber flexing?

L208: Meaning unclear.

L216: by attenuation or by modifying the current on the TSOA?

L216: I would not dare criticize the English of a native speaker. But 's possessives are seldom used in academic writing for inanimate objects or concepts. Common exceptions being of course eponyms such as "Student's t-test" or "Parkinson's disease", or the phrase "according to the manufacturer's instructions". Please consider whether this should be modified throughout the article.

L221: I had much trouble understanding this paragraph even though I am familiar with the technology, which make me think this should be made much clearer for other readers.

1) It is unclear what etalon we are talking about here. I do not think the etalon filter at the reception has been mentioned let alone sufficiently described at this stage of the article, nor that several transmission peaks remain within the IF bandpass. But actually the etalon is not what's important here, it is given too much focus. => Consider omitting it and talking about the receiver bandpass that necessarily allows both ON/OFF wavelengths in.

2) It took me three readings to understand that what you were referring to was the fact that due to leakage in the SWITCHING SETUP (not mentioned), there could be a bi-modal seed.

=> Consider mentioning this instead.

3) This part comes between the description of the setup to measure spectral purity and the results of the experiment. It should definitely be put somewhere else, either before or after.

L233: Consider adding a very short sentence about the impact (or lack thereof) of the pulsed output of the TSOA on the observed spectrum. Basically, it means convoluting the CW spectrum by a frequency comb with 8 kHz FSR and 1.6 MHz envelope, right? So, no impact here.

L246: Please expand on why. In case there is a loss of purity/TSOA output power over its lifetime?

Fig10 legend: O2 has only been mentioned once before in the article, and it may not come to the unaware reader why it is addressed and that it is associated with temperature measurements (please mention it here). I think that despite the citation of Stilwell 2020 in the introduction, there should be a summarized explanation of the measurement principles before, in a third part of a new section 2 using also the current 1.1 and 1.2. Maybe the schematics of Figure 11 and the associated text could be put there.

L266: I think this problem could be solved by first describing the instrument as suggested hereabove.

Figure 11: The polarization controllers are not described in the text and may respond to a previous question of mine. Please consider adding a word about them in the adequate section.

L282: Could be expressed more clearly.

L294: between the emitted beam and the receiver field of view (overlap between what? undefined yet)

L295: Loose statement. Not as much typical as perfectly theoretical. The authors could be more precise and even give the textbook formula for hyperfocal distance H = D/FOV = 450mm/150µrad = 3km, which helps discuss what follows.

If I may delve deeper into that matter, if one focuses at 3 km, one gets both 1.5 and infinity in focus, and thus full overlap after 1.5 km. Was it achieved in practice?

L307: It unclear here why adding a separate smaller receiver with a larger FOV, compared to another channel with larger FOV on the same receiver, changed this aspect. But this can be explained using the formula above. Using the same receiver with Kfov times larger FOV you get H/Kfov full overlap range for the WFOV channel, but $Kfov^2$ more background noise, meaning the near range to reach a threshold SNR is not so good. Using a separate Kd times smaller receiver (independent of FOV), you get H/Kd full overlap range, but no more background noise relative to the signal. This could be better explained in Annex A.

Of course, this formula may be an oversimplification, but then an extra figure plotting Signal & Noise or SNR vs range for NFOV, WFOV, separate WFOV could be used, supporting the discussion about the noise figures of Appendix A.

This brings another question: Isn't the smaller receiver equivalent to adding a smaller pupil stop in the WFOV channel? I suppose the concentric emitter/receiver beam configuration prevents that.

In any case, I would be interested in seeing a small discussion in Appendix A.

L308: Unclear. Same sky background photocount, as explained in Appendix A, but not the same for the signal photocount, right?

L325: Thanks for pointing this out. Narrowband filtering (even in Raman lidars) is so terribly dependent on angles of incidence, and this effect of non-common path between Raman or DIAL channels so often overlooked.

L335: unnecessarily heavy

L357: Unclear. Indeed, this works perfectly. But in the Gen5 MPD, there isn't a field stop, right? Why not? Do I understand correctly that you prefer to validate thereafter that this is not a problem?

L362: Despite the experimental results, this statement is a strong one considering the opposing claims of Spath et al. 2020, and could be substantiated a bit more. I really think a theoretical/simulation approach would help here.

- What is the etalon CWL shift (or peak broadening maybe) associated with 3 mrad? Then what is the resulting change in the observed WV absorption cross-section? What is the WV concentration bias that ensues?

More generally, knowing the FOVs, diameters and focal lengths of the receivers, it is possible to roughly calculate or more precisely simulate how the etalon peaks would shift/broaden at short ranges. At what range do we only get xx% change in effective CWL, yy% change in observed cross-section, and zz% bias in WV concentration?

S3.4: This whole section is interesting and very important for operational deployment of these lidars. But it is very confusing as it is. Please consider rewriting it entirely, taking into account my comments below.

L366: heavy

L369: The spectral/modal behaviour of the TOSA is known to change tremendously between CW & pulsed operation, if the thermal load varies. How is this mitigated/taken into account?

L370: For this scan, one needs to make sure the backscattered light follows the exact same path as light gathered from the beam's propagation in the atmosphere. On the main lidar schematics I see it would be in the focal plane of the telescope. Please state this plainly and refer to the figure as this is quite important.

Also, laser light reflected back into the TSOA could be damaging. Please explain why it is not.

L373: This is very unclear and yet crucial. The central sentence states that the scan is simultaneous, which is, I believe, incorrect. How then could the respective transmissions of the online and offline channels be separated? The detector would add both of their contributions.

The only way I would understand the results is if two scans are not done simultaneously but in a sequence, both with the online laser turned off and offline laser emitting. The first (red) scan is done with the switches adressing the online laser. Because it is turned off, we only see the leaks through the switches. The second (black) scan is done with the switches adressing the offline laser. Then we see the full power. So the red curve is basically the black curve attenuated by the switches and with a noise floor.

If that is correct, this section is extremely confusing and should be rewritten from scratch, detailing the steps rigorously. The two channels online/offline turned on and off are inherently tricky to talk about in a clear way; please indulge the reader.

L376: These two results come too soon, they should come at line 399, and as already done, line 401.

L382: What are the n channels? This could be at least three things: the range channels i.e. the range bins as defined later in the paper, the online/offline channels, the NFOW and WFOV channels. I first thought it was range channels. But then at line 395, I saw g1/g2, so there are two of them and I was wrong. After much pondering, I still do not know.

L398: State here the results in terms of peak effective width, the distribution of w(x) that was found, and the associated standard deviation for x.

L403: Confusing, please rephrase. I am confused by "transmitted" as it could refer to the laser transmitter as you named it, and by the "location" of the wavelength, rather than its value; please avoid those terms and 's when rephrasing. Also, detail which wavelength:

"(the value of) the wavelengths transmitted by the etalon"

Is that correct? Or did I get it wrong and it actually refers to:

"(the values of) the emitted wavelengths"

On a side note, I have trouble with "Transmitter" (instead of "Emitter") in the whole document. I assume the former is more correct in English, but it certainly is confusing when we are talking about the transmitted wavelengths sent into the atmosphere by the laser source, and the transmitted wavelengths through the etalon. In other languages, a transmitter conveys a signal that is already there, does not create it.

L406: I see transmit*ted* pulse also below, line 427. Which is best? I would argue the one with the participate.

L411: I believe MCS is undefined until 3.6.

L415: corresponding to what range? This seems useful as a lower bound for Rmin, independent of laser pulse duration. Can it be chosen?

L415: How is this chosen? Can it be reduced?

L418: This paragraph lies right between the discussion on pulse length and its conclusion, i.e. the choice made for the Gen 5 instrument. As a result, we do not understand where the line of thought of line 417 is leading. Please consider reuniting line 417 and line 434. The discussion about after-pulses is interesting, but it should be put elsewhere. Maybe just after line 442.

Fig13: I wish I could see Rmin in Figure 13. For a while I thought there had been a confusion between R3 and Rmin. Also, what is R0, shouldn't it be just 0? And R3, just a random range?

Also I don't understand why the photon accumulation time doesn't just create a wider rectangle.

L438: SNR previously undefined.

L454: Please choose transmit, transmitted or transmitter throughout the document.

L457: Still, this requires photodetectors that are unspecified here, and not on Figure 11. What type of detectors? Are they calibrated?

L460: This long paragraph detailing the advantages of a custom FPGA system and Ethernet communication does not seem so important here and could be shortened to two sentences.

L471: there or here?

L490: It intuitively seems sub-optimal to the uninformed reader to perform smoothing in two steps. If it had to be done at 10 min, 170m resolution, why not do it in the first place (line 473). Maybe add a short sentence here to explain that.

L493: What is said here is very true, linear propagation of errors does not work at all and another method is required. This bootstrapping method, which in all honesty I did not know of, could work. However I do not find enough information here to properly understand it and evaluate if it does, given the problem of finding enough actually independent samples, but of the same atmospheric column, in the lidar data. Variability is certain at the 10-min / 170 m scale. So I do not understand which samples or profiles are compared. I could not procure the book Hastie et al 2001, but it seems to deal with random samples in general and not signals. In what I could find online, examples involve stationary random processes, but the atmosphere is all but stationary.

Also more generally in this paragraph, explanations are very short. For instance, I only understood what is called bootstrap iterations by looking at other resources. How do we know that 50 resamplings are enough? I see B>= 1000 is a recommended minimum for estimating variance on a mean value.

In the end, I am hesitating on what the best solution would for this section. Either explain the method in more detail, which would make it too lengthy and should be put in another Appendix, or find a better reference than Hastie et al., that would apply the method directly to non-stationary signal processing.

My recommendation is: If this is the first time it is applied to lidar, then it should be developed in a full appendix. If not, please find a suitable reference closer to the field where the reader can inquire about it. In either case, please explain where these independent samples with the same mean value are found despite atmospheric variability, and why the chosen B works best.

Fig17: I would suggest plotting also the mean error, which is informative about potential biases. Although that information is contained within RMSE, it is convolved with random error.

Please also note the supplement to this comment:
https://amt.copernicus.org/preprints/amt-2021-41/amt-2021-41-RC1-supplement.pdf