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Discussions

Interactive comment on "Intercomparison of aerosol measurements performed with multi-wavelength Raman lidars, automatic lidars and ceilometers in the frame of INTERACT-II campaign" *by* Fabio Madonna et al.

Anonymous Referee #2

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1 General comments:

The paper describes an assessment of the performance of a miniMPL and two ceilometers using collocated Raman lidar measurements during the INTERACT II field campaign. This reviewer greatly appreciates the paper's clear organization and good writing, which made it relatively easy to read and review. Another strength is that the discussion is realistic and straightforward about the findings although the findings are not all positive. I think it is an important part of good research to straightforwardly

describe both positive and negative findings and I commend the authors for not over-reaching in their motivational discussion or "spinning" their conclusions to sound more positive than what's justified.

On the negative side, some of the graphs don't seem well designed to answer the questions being asked, and consequently some of the interpretations of the results appear somewhat off-base. There is at least one remaining error in data labeling, and some details need to be explained better. These should be addressed in revision.

The introduction gives a bullet list of the objectives of the campaign but not the objectives of the paper. It would be good to be explicit whether the intention is to address all of these objectives in the paper or only some of them.

Then, please be sure to address those specific objectives in the paper's conclusions also.

Although I was somewhat uncertain about the intended objectives for this paper, some of the objectives listed in the bullet list are addressed rather superficially or even incorrectly. For example, "assess the signal to noise ratio and dynamic range". I don't see analysis specifically addressing these, although some of the analysis of the figures includes some confusion about SNR (see specific comments below). Similarly, "assess the ceilometers' calibration stability and accuracy". There is some discussion about the relatively poor accuracy and about the stability of the lidar instrument itself, but there appears to be confusion in this paper about how to assess stability of the calibration.

2 Specific comments:

44. I'm not sure this link is an adequate reference. It links to a pdf of documentation of a piece of software for getting data but doesn't say where to get it. Maybe instead use the link that allows users to get the data–

47. The authors criticize the lack of global lidar coverage and lack of homogeneity within current lidar networks as if this is a motivator of the current work, but it isn't clear how the current work advances the goal of homogeneous lidar coverage. I do understand (from the next paragraph in the manuscript) and agree that vetting cheaper lidars might enable more global coverage, but it doesn't follow that such a network with a wide range of lidar capabilities will be more homogeneous than existing networks. Better to delete the sentence starting "Even when federated" or put in more discussion making the motivation and link to this work more clear.

58. "have been already investigated". That's fine, but in the next sentence or soon thereafter, you need to explain what the new contribution of this paper is.

62. "retrieval . . . can be performed using the molecular backscattering profile". Please be more specific. You mean the calibration can be performed using the molecular backscattering profile in a region where there is negligible aerosol. Without these additions, the phrase "the retrieval can be performed using the molecular backscatter profile" sounds like the Raman or HSRL retrieval.

215. Each lidar instrument has its own version of the quantity being considered, all with different names: attenuated backscatter, range-corrected signal, and normalized relative backscatter. It's a little confusing, but with some effort I see why you made these choices. It would be helpful to have a paragraph (earlier than this) where all three quantities are described in one place and the reasons for using different quantities for each instrument are provided.

275. Please explain the normalization further. Is the MiniMPL normalized to match PEARL in the normalization region on a profile-by-profile basis for every profile?

322. If you mention after-pulse correction as a possibility, I think it needs to be supported. Otherwise it just sounds random and speculative.

325. Are the 12 cases all the measurements available from the whole six month deployment period, or have these been selected from a larger dataset? (Were the lidars operating continuously?)

325. RCS or normalized relative backscatter? I thought that RCS meant non-normalized signals, so they could not be compared between two instruments? If there's no useful distinction in the terminology, it would be better to just use one name instead of three.

338. "The good stability of the MiniMPL calibration ... is shown by the small variability (10%) of differences in the normalization region". I have a major problem with this statement. This one must be addressed. Aren't the profiles for all 12 cases normalized to the Raman lidar in the normalization region? So, for each profile, the normalization constant is divided out and each profile is independently set to have zero average difference in the normalization region. That means that to assess the profile-to-profile variability in the normalization constant, you'd have to explicitly look at the 12 normalization constants. That information is not present in the data shown in Figure 5. Variability in the normalization region in Fig 5 is only representative of high-frequency noise within the normalization region, so it informs you about the precision, but not about the stability over time.

Figure 7. The differences in the scatter plots are very hard to make out given the data being compared are in two different plots. A figure showing both in the same figure would be better. For example, consider making a scatter plot of CL135 vs. MUSA/PEARL attenuated backscatter directly, and color code by extinction or stratify different ranges of extinction into multiple sub-plots. Accompanying them with another set color coded or stratified by altitude would also be helpful, I think, given that the interpretations of this figure in the text are related to specific altitude regions (the overlap region and the free troposphere). (Same comment for Figure 13.)

348. You say that the choice of aerosol extinction for the y-axis of Figure 7 (and 13)

is to reveal differences in sensitivity to different aerosol types. In fact, you have not mentioned different aerosol types in your interpretation at all. Indeed this task would be quite difficult with the information given in the figures since the relationship between attenuated backscatter and aerosol extinction is related not just to lidar ratio (indicator of aerosol type) but also to the amount of attenuation, and the attenuation may be a more dominant effect in this data set. If you really wanted to distinguish different aerosol types, you might consider including lidar ratio (from the Raman lidar) in the analysis. If you don't care about aerosol type, then probably just delete the statement about them.

354. "The most evident differences between the two lidars can be identified for values of extinction larger than about $5.0 x 10^{-5} m^{-1}$ where miniMPL shows a broader scatter." It's very difficult to see this. I see that miniMPL has a bit more data at the low end of the x-axis and MUSA has a bit more data at the high end of the x-axis, but it is by no means obvious.

355. "Described above" What does this refer to, the unexplained statement about after-pulse correction?

371. SNR. There seems to be some confusion between signal level and signal-to-noise ratio. The text says the CS135 SNR decreases above 3500 and the CS 51 SNR is higher. To me, it looks like the signal of CS135 decreases and the signal of CS51 is higher, but the noise in the CS51 signal is also quite high and it clearly does not agree with the more reliable Raman lidar, so it's likely this higher signal is an artifact. Your graph doesn't show SNR explicitly enough to aid in analyzing the SNR. I think you would have to look at both signal and noise and analyze noise levels explicitly to be able to make quantitative statements about how the SNR for the two instruments compare. From figure 8 I think you can say "The CS135 signal strongly decreases . . .. The CL51 signal is higher but the noise suggests that it is not reliable to detect the residual aerosol. . ."

Figure 10. Please check the units. Is the exponent -6? Or -5? Compare to Figure 7 which I think should be the same PEARL profile.

422. Similar to my comment at line 338, I don't agree with this. Is each case normalized separately? If so, then the variability of the normalization constant is not represented in this plot. The error bars are related to the amount of variability over the few hundred meters of normalization range, but not to the stability of the normalization constant over time.

425. "The standard deviation of the normalization constant." Is this calculated separately by keeping track of the individual profile normalization constants? That's the correct way to do it.

439. "better performance of the CL51 when the values of extinction are larger for corresponding small values of backscatter and therefore indicates its improved SNR in the FT". Is it really true that these large values of extinction with corresponding small values of backscatter are in the free troposphere? Wouldn't small backscatter values in the free troposphere more likely be accompanied by small values of extinction? It seems more logical that if there are small backscatter values when the extinction is large, that means there is significant attenuation, so the points are more likely low in the atmosphere below significant aerosol layers. This is important to check and clarify since you seem to be drawing a major conclusion (better performance of CL51 in the FT) almost wholly from this subtle and hard-to-interpret pattern.

443. "The overall stability of ceilometers' calibration constant . . .has been addressed in a statistical sense." I don't see any analysis of the overall stability of the calibration constant, see comments at line 338 and 422.

464. "in general is embedded" – please be more specific. Do you mean "is directly proportional to"? If so, please say that. If the relationship is more complicated than that, please include the equation.

470. "the calculated embedded constant". Does this mean lidar constant? Please say that. I think it would be good to put in some clarification that the calibration constant is an operational assessment of the lidar constant (which may have some noise or error). So I think what you're saying is that if the true lidar constant has seasonal variability but a calibration constant is only assessed infrequently, then there will be a systematic error in the calibration constant.

471. "what was reported during INTERACT". What was reported? Be more specific.

471. "This partly explains". To me this finding of a temperature dependence suggests a hypothesis, but I don't see any testing or exploration of the hypothesis. Is there any indication that the variability during INTERACT was correlated with temperature? (I see in the earlier paper it was believed that there was, but there was no quantification of the correlation, and that information is missing entirely from this paper).

471. As your continuing discussion points out, it doesn't seem that the size of the effect matches well at all. If the lidar constant is linearly related to the number of laser pulses, then the variabilities are also linearly related, and so 10% variability in pulse count can hardly explain 58% variability in the calibration constant. I think maybe it would be best to change the wording to remove or further deemphasize the "This partly explains" clause. While I agree that you have demonstrated that operators must be aware of temperature as a source of variability, as an investigation of the cause of the observed variability in the INTERACT observations, this is inconclusive at best.

486. "most of the difference could be reduced after a reevaluation of the overlap correction". This statement in the conclusions is quite a bit stronger than the statement in the body of the text. In the text you demonstrated that reduction of the error was possible for a single case when the Raman lidar is available to show the true shape of the overlap region, but that it couldn't be corrected in most cases.

492. "The CL51 is able to detect the molecular signal in the free troposphere". I'm not convinced this was demonstrated.

500. Since the introduction suggested a main motivation was "to understand to what extent automatic lidars and ceilometers are able to provide an estimation of the aerosol geometric and optical properties and fill in the geographical gaps of the existing advanced lidar network", it would be good to see some conclusion about this question here. You have said earlier "the only possible CL51 normalization to provide a reliable estimate of attenuated backscatter profile must be performed over a profile of attenuated backscatter from a reference lidar (like MUSA or PEARL)." These seems to argue against the usefulness of ceilometers for filling in existing gaps. Whether or not I am correctly guessing your conclusion, some discussion belongs in the conclusion section.

3 Technical & grammatical:

143. Is it 16 optical channels? The description in the following sentences seems to say 16, not 17. Is something left out or is there a typo, maybe?

231. Probably "temperature" rather than "thermostat". A thermostat regulates temperature.

235. Instead of using "beta", spell out attenuated backscatter or use the symbol β' that was already introduced.

344, 353, 354, elsewhere? Fix formatting of numbers in scientific notation

367. Possible missing word "between" 2.5 and 3.5 km asl

416. Delete the word "average"? I think you probably are reporting the standard deviations of the fractional differences, not the standard error of the mean. If you are reporting the standard error of the mean, please use that terminology rather than "standard deviation of the average".

448. Replace indifferently with interchangeably

451. "over the time", delete "the"

504. "INTERACT-II". Should this be "INTERACT-I"?

Figure 1. A log scale might be more informative for this quantity.

Figures 3, 4, 6, 8 . The label "LIDAR" should be "MUSA", "PEARL" or "MUSA/PEARL"

Figure 7. the axis labels are really small and it's not possible to zoom them in enough to make them clear. It would be good to remake these with bigger axis labels. (But see above: I also have a suggestion for a different plot style altogether.)

Figure 7 caption. Please state the time & date of the comparisons.

Figure 8. "Using t[w]o normalization ranges (below 3 km and above 8 km)". It appears that this is incorrectly pasted from another figure. Figure 8 doesn't seem to have two normalization regions.

Figure 11 caption, line 841. "standard deviations of the fractional differences" not "average", I think (see above)

Figure 12 caption. "and along the whole observed atmospheric column". It appears this is a copy-paste from Figure 6 and should be deleted.

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