

SPECIFIC COMMENTS

Use of term “transmission” e.g. p 28364 line 7 and elsewhere:
Isn’t the standard radiometric term “transmittance”?

Fixed.

p 28370 lines 11 & 12. Reference to Russell et al. (1979)

You cite Russell et al. (1979) for your use of 1% as the uncertainty in the molecular transmittance arising from uncertainties in the GMAO temperature profile. However, these authors state that this value is only appropriate above the boundary layer and “within about 100 km and 6 h of the radiosonde measurement”, “provided there are no frontal air mass movements”. They then state “When density models or interpolations are used (i.e. above about 30 km and at great distances from a radiosonde) much larger errors can result unless great care is taken to use the most appropriate model or interpolation procedures. If these precautions are taken, the density errors can be limited to 3%”. (Note too that this latter estimate is based on “personal communication”.) I suggest that you need to include more justification for your use of 1% instead of the higher uncertainty.

Good point – based on Russell et al., (1979), the use of 1% does seem to low. Our reasoning was that the locations and times that HSRL flies the CALIPSO tracks were not considered a great distance or temporal separation from the radiosondes assimilated into the GMAO model. Note that radiosondes generally cover the regions in the HSRL flight track map (e.g. see the maps at the integrated global radiosonde archive). Looking at Table III in Russell et al., (1982), we will increase our error budget to $\pm 2\%$ for the molecular attenuation between 8 km and 30km as 2% seems to be more appropriate.

p. 28378 bottom and 28379 top, Reference to Ansmann (2006): You probably need to qualify the figure of 20% as it is likely to be unduly pessimistic in the present context. The 20% difference was found when Ansmann used a column effective lidar ratio to solve a complete atmospheric profile, instead of using values appropriate to each individual layer. When he used the latter approach he found much closer agreement between the downward- and upward looking lidars. I suggest that in the current context, where neither the HSRL nor CALIPSO uses the same lidar ratio for the whole column, the situation is closer to the latter example given by Ansmann. Your text needs qualification.

Ansmann’s reference is intended to highlight the difficulty in comparing nadir vs zenith lidar profiles, especially when both are elastic lidars. Unfortunately, most Raman lidars are limited by SNR in the daytime, so rely heavily on their elastic channels during the daytime. It applies to this paper only to clarify for readers that the method presented here, with a zenith based HSRL, does not have the assumptions, and therefore uncertainty, that many of the previous validation studies have had in the daytime. We have added the following to better clarify the statement, “For example, Ansmann (2006) found that Klett retrievals from zenith-viewing lidar could differ in extinction and backscatter profiles by as much as 20% from a nadir-viewing lidar observing the same scene due to the viewing geometry in complex scenes (e.g. multilayered), with better agreement in simple schemes (e.g. single lofted aerosol plume).”

p 28379 - discussion of results of Mona et al. (2009) in the PBL – also in Table 3. I am surprised that you include reference to the differences that these authors found between their lidar's measurements and CALIPSO's in the PBL given the very different nature of the locations and their separation. To quote the authors directly "CALIPSO is typically smaller than the corresponding one obtained by PEARL profiles, because the 2 sampled volumes are more distant than 40 km and very different in nature: mountain region with industrial and rural area for PEARL and low altitude and coastal region for CALIPSO." Given that you are so careful in your present work to ensure that you only compare signals that are spatially and temporally correlated, the inclusion of the other work seems rather anomalous, unless it is used to reinforce the need for the care you have taken to ensure that correlation.

The inclusion of Mona's results has several purposes. First, we found a free troposphere bias very similar to hers and it makes our argument stronger that that CALIPSO calibration is accurate ~3% because two separate lidars have found a similar bias. However, we do have to address the differences in our study, which is in the PBL. Mona states that the low bias is "probably" due to the spatial/temporal differences – but the question we've had to ask is, "what if there IS a low bias in CALIPSO?" Our spatially matched analysis did not have this low bias, but because our free troposphere bias was in agreement we think it strengthens her argument that it is due to spatial/temporal matching. This is certainly worth repeating because it has implications not only for level 2 validation of CALIPSO, but also carries the notice to users that care must be taken when using CALIPSO data off the track (especially in a case study type of event).

Use of "unattenuated" and "attenuated". Admittedly, this is a minor quibble, but I find the persistent use of the term "unattenuated" to be both unnecessary and unattractive. The quantities "backscatter" and "scattering ratio" are well defined in the literature and refer to atmospheric quantities. It could be argued that the quantity "attenuated backscatter" is really just another way of describing a scaled, raw (elastic backscatter) lidar signal. The beauty of the HSRL technique is that it produces a calibrated profile of scattering ratio as a direct measurement (after dividing your scaled parallel and perpendicular channels by your molecular signal.) You could make this point at the start and remind readers that the HSRL measures the atmospheric quantities of backscatter and scattering ratio, whereas CALIPSO and other such lidars measure the attenuated quantity, which is indicated in this paper and elsewhere in the literature by the primed symbols. (This is just a suggestion.)

We've taken this into account and reduced the repeated use of "unattenuated", while adding a sentence that the HSRL measurement is "unattenuated" to preserve the distinction.

TECHNICAL COMMENTS

Thank you for the technical comments. We've taken all of them into account except the following:

p 28377 lines 11 – 18 – discussion of Fig. 11. Again, as in the comment above, an expression would help clarify the link between the text and the bias and rms data on the figure.

Since we did not use the equation for this regression (as we did the previous equation, which we've added) we did not include it.

Reviewer comments:

Page 28356 line 23: “compared to” should be “compared with” when comparing similar things.

p. 28359 line 21: Hunt et al. 2009 reference is not listed in References.

p. 28361 lines 22 and 23: The subscripts for the perpendicular components are used instead of those for the parallel components.

p. 28363 line 1: Is the word “channel” missing after “parallel”?

p. 28363 line 5: Shouldn't this be “. . . perpendicular channels, K_{PGR} , to be determined.”

p. 28365 line 15: “. . . channels to produce the of 532 nm total . . .” needs correction.

p. 28367 line 26: “the altitude range . . . was restricted to . . .”

p. 28369 line 15: “. . . is due to aerosol . . .” ?

p. 28370 line 25: “. . . but to estimate an upper bound of a COT of 0.0125 is estimated. . .” ?

Check wording.

p. 28370 line 27: 0.075 should be 0.0075.

p. 28371 line 1: McGill et al. (2006) is not listed in References. Do you mean to cite the 2006 paper, which is listed?

p. 28371 line 15: Accent missing above “a” in “Jager” – See your References.

p. 28371 line 15: Rogers et al. 2009 is not listed in References.

p. 28374 line 1: “Table 2” should be “Fig. 6”.

p. 28374 line 7: “The means of these data points are . . .”. You have “mean”.

p. 28375 line 19, Hunt et al. 2009 reference is not listed in References.

p. 28376 lines 24, 25: “A total . . . were found” should be “A total . . . was found”.

p. 28376 lines 19, 25, 26: “criteria” should be “criterion” in each case if you are talking about just one condition (time offset within 30 mins). Otherwise you need “these criteria”.

p. 28377 line 3 – 10. It would be clearer if you used instead, or included as well, an expression of the form “diff = $(A \pm dA) + (B \pm dB) * Z$ ”.

p 28377 lines 11 – 18 – discussion of Fig. 11. Again, as in the comment above, an expression would help clarify the link between the text and the bias and rms data on the figure.

p. 28378 line 19: Again, do you mean McGill et al. (2007)?

p. 28378 line 21: Do you mean Kim et al. (2008)?

p. 28378 line 29: Ansmann (2006) is missing from references.

References:

Ansmann (2006) and Hunt et al. (2009) are missing

p 28381 lines 16, 17: Charlson et al. (1992) reference is not cited in text.

p 28381 lines 26, 27: Haywood and Boucher (2000) reference is not cited in text.

Figure Captions:

p. 28391 Caption to Fig. 4, line 1: “Relative differences were binned . . .”.

p. 28393 Caption to Fig. 7, line 1: “. . . same as Fig. 2 . . .”. (not Fig. 1)

p. 28395 Caption to Fig. 8, line 1: Should this be “Flight track map . . .”?