

Atmospheric Measurement Techniques Discussion  
Response to Referees' Science Review Comments – August 2019

“Cloud-Aerosol Transport System (CATS) 1064 nm Calibration and Validation” - Pauly, R., J.E. Yorks, D.L. Hlavka, M.J. McGill, V. Amiridis, S.P. Palm, S.D. Rodier, M.A. Vaughan, P. Selmer, A.W. Kupchok, H. Baars, A. Gialitaki

We received referee comments from two referees and one document with short comments from a member of the scientific community. Our responses to the comments of two of the referees of our submission: amt-2019-172: Pauly et al., “Cloud-Aerosol Transport System (CATS) 1064 nm Calibration and Validation” are below. The referees were very helpful in clarifying our explanation of the method, as well as the importance to future missions and CATS retrievals. We hope the editor will find our responses address the major and minor comments of the referees. Our response to the short comments from the member of the scientific community will be provided in a separate document. We believe the manuscript is clearer and more robust, and we look forward to the new step towards publication. Note that the referee comments appear in black while **our responses appear in red**.

### **Franco Morenco's Comments (Referee)**

I have read the paper by Rebecca Pauly and co-authors with great interest. The article describes the calibration of CATS 1064 nm attenuated backscatter and depolarization (level 1 data). Calibration is achieved on a per-granule basis, by normalization of nighttime signals with modelled atmospheric backscatter at an altitude of 22-26 km, where account is made for Rayleigh scattering (derived from the MERRA-2 re-analysis) and aerosol scattering (inferred from CALIPSO measurements). Moreover, attenuated backscatter by opaque cirrus clouds is exploited for two further calibrations: daytime calibration, on a monthly basis, is achieved by matching the overall frequency distribution of daytime and nighttime opaque cirrus attenuated backscatter, and the calibration of depolarisation signals, on a yearly basis, is obtained by matching the parallel and perpendicular signals for this type of clouds. The uncertainties that derive from this approach are discussed and quantified, and comparison with a number of validation sources is described: CALIPSO, airborne lidar, and ground-based lidar.

This research has a high significance, due to the fact that two and a half years of CATS data have been collected in 2015-2017, on-board the International Space Station. This dataset is still to be exploited in full, and it provides information on global aerosols and clouds, under an unusual orbit type (the one of the ISS) which permits an investigation on diurnal cycles (as opposed to the more traditional sun-synchronous orbits). It also demonstrates that, depending on instrument design, direct calibration of 1064 nm lidar channels is possible, without needing to transfer the calibration from channels at shorter wavelengths.

The paper is well written but a few more points need to be addressed before it can be published, in order to clarify better the methods to the reader. I feel that there are still a few major points as follows, some of which were already raised in the previous "quick review". Underpinning science to the CATS processing is addressed here and I believe that the explanation of the methodology should clarify all doubts.

MAJOR COMMENTS:

- 1) I suggest to add more in the conclusions. CATS has been used and will continue to be used for cloud, aerosol, and radiative budget studies that will benefit from the new data version. What are the most significant results obtained so far from CATS datasets? how would they be affected if they were to be reprocessed using V3 data? how does the V3 level 1 calibration affect the level 2 data (before any changes to the V3 level 2 processing)? are there any useful lessons from your research that can be useful for EarthCARE and Aeolus? and for future follow-on missions?
  - a. More detailed discussion of the most significant results obtained from CATS data so far has been added to the first paragraph of the conclusion, in addition to the text added about how the calibration affects the L2 data products addressing Anonymous Referee #4's comment #4. Most studies using the retrievals of optical properties (e.g. extinction, optical depth) have used the V3-00 data. Since EarthCARE's lidar is a 355 nm HSRL and Aeolus is a 355 nm Doppler wind lidar, the 1064 nm atmospheric normalization technique shown here isn't very helpful for those missions. However, a sentence was added to the second paragraph of the conclusion to elaborate on how decreasing the laser repetition rate of a future CATS-like backscatter lidar could provide a larger data frame, and thus a higher calibration altitude (minor comment #34).
- 2) In the daytime calibration (section 2.2), you specify that you are looking for a specific type of target: opaque and geometrically thin clouds, and you specify "A layer is classified as opaque if no layer or ground signal is detected below it". In the previous review I raised the question of how you could know that such a cloud is physically thin, since opaque and deep clouds could look similar on a lidar signal. I don't believe that this point has been addressed.
  - a. What we are really trying to say here is that for strongly scattering, rapidly attenuating opaque cirrus, there should be little difference between nighttime and daytime iATB retrievals. Thus, that is why we selected these types of clouds for the daytime calibration transfer procedure. We have added text in Section 2.2 (first and second paragraphs) to address this and have removed all mention of "physically thin" clouds from the paper and replaced it with the phrase "strongly scattering, rapidly attenuating opaque" clouds.
- 3) Equation 9: discuss numerical differences between  $C_{day}$  and  $C_{night}$  and their evolution; what causes them? instrument temperature?

- a. The time evolution of the CATS calibration coefficients is correlated to the thermal stability of the cooling loop on the ISS, which in turn is attributed to the changing of the sun's angle with respect to the ISS orbital plane, known as its beta angle. The CATS nighttime calibration coefficients oscillate from  $4 \times 10^8$  to  $1.4 \times 10^9$   $\text{km}^3 \text{sr}^{-1} \text{J}^{-1}$  counts with a period of roughly 30-40 days. This oscillation is a result of changes in the CATS laser properties (i.e. wavelength, alignment, energy) due to thermal instability of the cooling loop. The thermal instability of the cooling loop and instrument was monitored by the cold plate temperature. Text has been added to state all of these changes on page 6, lines 30-38. Also, Fig. 3 has been updated to include the entire mode 7.2 dataset (top, April 2015 – October 2017) as well as a subset from January- April 2016 (bottom). The daytime calibration coefficients for each month have been added as red dots. A discussion of the daytime calibration values, variability, and comparison to nighttime calibration coefficients has been added starting at pages 7, line 36. Unfortunately, the funding for producing CATS data products has expired. But, if we were to ever receive funding to create another version of the CATS data products, we would make more rapid estimates of the daytime calibration coefficients than the current monthly estimates.
- 4) Section 2, lines 6-26: A few pieces of information on the instrument, that one deduces whilst reading the paper, should go in this section, so that the reader can begin thinking about them. I would discuss the following in this section: (1) how laser 1 and laser 2 are associated with modes 7.1 and 7.2; (2) the difference in PRF between the two lasers (4 and 5 kHz); (3) the signal folding due to the choice of PRF; (4) how this is reflected in the signal acquisition (with a data frame from -2 to 28 km); (5) the raw vertical and temporal resolution; and (6) any integration that is applied to the data prior to the signal processing described in the present paper.
  - a. All of the requested information has been added to the first paragraph of Section 2.
- 5) Equation 3: the colour ratio 0.4 is assumed because it is the value also assumed by Hair et al (2008). However, Hair et al do not give any explanation on why this value has been chosen, nor do they provide a reference! This should be discussed, and the error estimate on the colour ratio should be given explicitly. It could be useful to mention that this assumed colour ratio corresponds to a backscatter Angstrom exponent of 1.3, and that it is a colour ratio for "nearly clear air" (so is stated by Hair et al).
  - a. To the authors' knowledge, the value or variability of the stratospheric aerosol backscatter color ratio is not documented in the literature. For the mean value, we follow Hair *et al.* (2008), so  $\chi_p = 0.40$  is taken as a constant for the aerosol loading in the upper troposphere/ lower stratosphere. This value is originally derived from backscatter data shown in Spinhirne et al. (1997). Given that sulfate aerosols are potentially the largest contributor to the stratospheric aerosol loading, this value is also consistent with lower tropospheric measurements of sulfate aerosols. Text has been added to page 5, lines 6-9 that now states this. For the error estimate (variability), we performed an analysis of SAGE III extinction Angstrom exponent, averaged from June 2017 to August 2018 in the CATS calibration region, to find a

- mean/standard deviation of  $1.79 \pm 0.10$ . We use this standard deviation as a relative uncertainty for the backscatter color ratio, so we assume an absolute uncertainty in the stratospheric aerosol backscatter color ratio of 0.024. This is now explained in the text on page 8, lines 26-33.
- 6) P5 L12-17: please explain these criteria better: (1) why has each of them been chosen and what do they signify in terms of cloud physics? (2) why do they differ from the criteria used for daytime calibration? (3) how is the temperature determined? (please state if it comes from the reanalysis); (4) clarify how you determine the depolarization delta before you know PGR: are you using a previous data version for this? (5) equation 7 is missing the PGR as a multiplicative constant: have you already incorporated this into NRBperp? If this is the case, it is confusing, and I would recommend to write PGR explicitly, or to call  $\text{NRBperp}' = \text{PGR} * \text{NRBperp}$ .
- a. These criteria are used to identify scenes with dense cirrus clouds that can be used to compute the PGR. These criteria are VERY similar to those used by CALIOP to identify cirrus clouds for their 1064 nm calibration transfer. We have now updated the text to clarify these things, as well as address 3, 4, and 5. These changes are on page 5, lines 28-34.
- 7) is the daytime or nighttime calibration coefficient and the PGR stored in the level 1 data files available for download? please state in the paper.
- a. Yes, both these values are stored in the Level 1B files, and the paper now states this on page 5, line 23 and page 6, line 28.
- 8) The specifications of the units used is missing in several places: (1) Equation 1, what are the units for  $N_s$ ? counts? count rate? voltage? and what are the units of NRB (e.g. counts \* km<sup>2</sup> / J)? (2) Equation 8, what are the units for C (e.g. counts \* km<sup>2</sup> \* sr / j)? (3) P6 L7, specify the units with the calibration coefficients given here; (4) P7 L9, specify units of iATB values given (e.g. sr<sup>-1</sup>); Table 1 misses the specification of units (sr<sup>-1</sup>); etc.
- a. Anonymous Referee #4 also made this point. Thank you for catching this detail. The appropriate units have been added to the latest version of the manuscript throughout. You can see examples in the text corresponding to Equation 1 (page 3) and Equation 5 (page 5).

MINOR COMMENTS:

- 9) The paper uses the normalisation technique to calibrate the signal; however, since aerosols have to be accounted for at the altitudes considered, I suggest that it should not be called a "molecular" normalisation technique. This can be achieved by removing the word "molecular" from lines 17 and 35 (abstract) and in a few places within the manuscript. In the conclusions, line 22, "Rayleigh profile" → "Rayleigh profile corrected for aerosol contributions".

- a. Typically, this technique has always been called the Rayleigh or molecular normalization technique. However, most of the applications in the past were at 532 nm and in altitude regions with small aerosol contributions. Thus, we understand the referee's concerns. We have changed the phrases "Rayleigh/molecular normalization technique" to "atmospheric normalization technique" and "Rayleigh profile" to "Rayleigh profile corrected for aerosol contributions". A sentence on page 4, lines 16-19 defines this name.
- 10) P2 L26: we have no measurements of crystal size, hence I would either remove the words "comprised of large ice crystals", or I would word it as a caveat (e.g. "thought to be mainly associated with ice crystals larger than the lidar wavelength").
- a. Text was changed as suggested (page 2, line 30)
- 11) P3 L16: How is the laser energy E determined? Is it measured on-board? Is E an instantaneous value, a nominal one, or an average over a given time period?
- a. The laser energy per pulse is measured, then averaged onboard and reported at 20 Hz. Text was added to specify this (page 3, line 27)
- 12) P3 L22: "averaging the signal acquired after the signal attenuated by the Earth's surface" add the words "after correction for the signal folding time (see below)".
- a. Text was changed as suggested (page 3, line 35)
- 13) P4 L6: You earlier specified that the data frame is limited to -2 to 28 km; the fact that you use signals between -2.5 and -4.5 km for the evaluation of the background seems in my opinion to contradict this fact. Please explain, and please specify whether the data frame between -2 and 28 is limited by hardware design (acquisition electronics).
- a. This was a poorly chosen example. We have updated the text to use the example of -2.0 to 0.0 km, which includes signal from 37.5 to 39.5 km (page 4, line 24). Details of why the data frame was chosen to be -2 to 28 km are now provided in Section 2, first paragraph.
- 14) P4 L23: "28 km" → "26 km"
- a. Text was changed as suggested (page 5, line 4)
- 15) P5 L7: remove "reflected" (this is scattered light, rather than reflected).
- a. The language was modified for accuracy.
- 16) P5 L22: add "(multiplied by PGR)" after "perpendicular"
- a. Text was changed as suggested (page 6, line 9)
- 17) P5 L24: add the following before "To prepare", so as to clarify to the reader better what is the overall approach: "Nighttime calibration is applied on a per-granule basis, where a single calibration coefficient is determined as follows, for each data granule".
- a. New text has been added based on these suggestions and comment #3 from Anonymous Referee #4.

- 18) P5 L33: specify the value used for minimum and maximum thresholds.
- a. These threshold values vary based on the fluctuations shown in Figure 3. We added text specifying this (page 6, line 22).
- 19) P6 L1-4: is there any flagging of cases where the per-granule approach fails and you revert to using the previous week data? or is it exactly coincident with the flagging of files with a poor depolarisation quality?
- a. Yes. The L1B data products include a Quality Control Flag. The 23<sup>rd</sup> bit of this flag denotes when historical calibration coefficients have been used. The text has been updated to state this (page 6, line 30). This was a very helpful comment.
- 20) P6 L6-13: please explicitly state that an instrument temperature dependence is thought to be responsible for these fluctuations. Do you have any suggestion on which piece of the CATS hardware could be responsible?
- a. Please see our response to comments #3. We believe that the laser, given it is the most impacted by cooling loop temperatures, is leading to the changes in the calibration coefficients, but we don't have enough engineering data to determine what property of the laser is the source (wavelength shift, alignment with telescope, etc.).
- 21) P6 L22: precede line with "Instead,". "singular" → "single". "month": specify if this is calendar month (from 1 to last day of the month) or a rolling 30-day period.
- a. This sentence has been modified based on the referee's comments (page 7, line 12).
- 22) P6 L23: "colder than -20C": how is the temperature determined? see comment 6 on specifying how temperatures are determined.
- a. We added text specifying that the temperature comes from MERRA-2 data (page 7, line 14).
- 23) Figure 4: why does the shape of the distribution change so much? I would only expect a horizontal shift on the plot.
- a. As stated in lines 37-38 of page 7, the changes in distribution are attributed to changes in the layer typing algorithms implemented in CATS V3-00 data. For example, the V3-00 cloud phase algorithm removed the secondary peak in the nighttime distribution at 0.055 sr<sup>-1</sup> that was due to misclassification of liquid water clouds. See the [CATS ATBD](#) for more information about the feature identification algorithms. Yorks et al. (in prep) will include an update on these algorithms for V3-00.
- 24) P7 L6: add "on a monthly basis" after "V3-00".
- a. Text was changed as suggested (page 7, line 37)
- 25) P7 L8: I thought that equation 9 would ensure that the bias on the mean would be zero. Please explain better why a residual bias persists.

- a. The bias between the daytime and nighttime iATB is introduced by the temporal resolution of the calibration coefficient at nighttime (1 per file) and daytime (1 per month). This is now stated on page 8, lines 1-4.
- 26) P7 L28: "transmission" before "uncertainty".
- a. Text was changed as suggested (page 8, line 23)
- 27) P7 L37: please give a numerical estimate of  $(\Delta C / C)_{\text{sys}}$  before discussing the random error.
- a. The system error is estimated as 7%, and is now reported in the paper on page 8, line 35.
- 28) P8 L11-12: if the multiple scattering factor is the same for daytime and nighttime measurements, does it not cancel out? please explain if it is different for day and night instead.
- a. Yes, you are correct that the multiple scattering factor would cancel out. That entire paragraph in Section 3 has been rewritten.
- 29) P9 L20: scattering ratio of 1.27: how much is the comparison with CATS sensitive to R? discuss the consequences of this assumption and its effect on the estimate of measurement errors; please specify if R is specified at 1054 or 532 nm.
- a. We now specify that the scattering ratio is the 1064 nm particulate scattering ratio (page 10, line 27), and we recognize that this scattering ratio could be a reason for the better than expected agreement between CPL and CATS for this case (line 39, page 10).
- 30) P10 L2: "some of the differences in the ATB signal": I am not sure which differences you are referring to: the two do not look too different from each other!
- a. We have deleted this sentence since it doesn't really add value to the discuss of the comparison of CPL and CATS for the 7-15 km altitude region.
- 31) P10 L30: add "PollyXT" before "1064 nm"
- a. Text was added as suggested (page 11, line 39)
- 32) P10 L39: specify how many profiles are accumulated in 30 min of PollyXT measurements.
- a. PollyXT has a repetition rate of 20 Hz and they accumulate 30 seconds (i.e. 600 laser shots or single profiles). For a 30-min measurement segment, this makes  $60 \times 600 = 36000$  single profiles. This is now specified on page 12, line 8.
- 33) P10 L40: add "CATS-like" between "mean" and "signal"
- a. Text was added as suggested (page 12, line 10)
- 34) P13 L30: please specify what changes to instrument design could permit the use of a higher calibration region. I suppose that one of them could be a reduction of the laser PRF (responsible for signal folding).

- a. As discussed in our response to comment #1, a sentence was added to the second paragraph of the conclusion to elaborate on how decreasing the laser repetition rate of a future CATS-like backscatter lidar could provide a larger data frame, and thus a higher calibration altitude.
- 35) Figures 10 and 11, y-axis: please specify whether this is a relative frequency expressed in % or an absolute frequency distribution. In Figure 11 make the x-axis label consistent with Fig. 10 for a better readability.
- a. We have updated the text to specify that this is a relative frequency and remade Figure 11 with an updated x-axis label.