Atmos. Chem. Phys. Discuss., 13, C3264–C3272, 2013
www.atmos-chem-phys-discuss.net/13/C3264/2013/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol extinction to backscatter ratio derived from passive satellite measurements" *by* F.-M. Bréon

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Received and published: 7 June 2013

First, I want to appologize for being so long to answer the reviews and comments. This is due to an exceptional conjunction of work, including the drafting of the forthcoming IPCC report, family issues, and "normal" work. Second, I want to thank the anonymous reviewers as well as A. Ansmann and S. Burton for their comments. As they have very well noticed, I am not a specialist of lidar measurements and their comments helped a lot making this manuscript more accuracte and usefull.

I give below the comments and my answers. They are also available as a pdf in which there is a better distinction between comments (Italic) and answers (plain text)

Anonymous Reviewer #2 This is a well-focused original paper based on an interesting

new approach to quantify backscattering properties of tropospheric aerosols based on POLDER data. Although the passive space observations do not yield pure backscatter from aerosols, the careful analysis described in this paper makes enough sense to be significant. I recommend the publication of this paper as is

We thank the reviewer for his reading of the manuscript and his positive comment

Anonymous Reviewer #1

This article presents a method of retrieving aerosol lidar ratios from extinction retrievals and reflectance measurements obtained by the POLDER instrument. This is a potentially valuable contribution, as it enables a gridded climatology of lidar ratios over oceans that could potentially be used to assist the CALIPSO aerosol optical depth retrievals. Unfortunately, details are lacking in this paper, so it is difficult for the reader to have confidence in the results. Also, the authors used a mathematical approximation (in their Eq 1) that is not appropriate for this application, in my opinion.

We thank the reviewer for his/her careful reading of the paper and the detailed analysis of the methodology. We agree that our paper is rather short and that some readers may prefer a longer –more detailed- description. Yet, we feel that others prefer a well focused and short description of a new idea. We certainly agree that it is only a first step, and that more work is needed for a full assessment of the method potential. Detailed answers are provided below, in particular to the reviewer main concern.

Major Issues My main problem with this article is that the authors do not attempt to assess the accuracy of their lidar ratios or their phase function derivatives (i.e., their 'V' values of Eq 4). Their retrieval begins with Equation 1, which includes an approximation that requires the exponential term to be much less than 1. That is, the authors are using the approximation $\exp(x) \approx 1-x$ when $x \ll 1$, although they do not state this (they should have!). The error associated with the "small x" approximation is 10% at $x \leq 0.4$, but it rapidly increases to 47% at $x=0.75$

In equation (1), we provide the “true” expression on the first line, and the “approximated” expression on the second line, using the curly equal sign to clearly indicate that it is an approximation. We should have (and will in the revised version) explains the domain of validity of the approximation.

Now, the view zenith angle for POLDER at an aerosol scattering angle of 180 degrees is equal to the solar zenith angle (authors state this on page 3), so the exponential term in Eq. 1 is x= 2 τ /μs$\leq$0.4. Thus, we require 2 τ /μs $\leq$0.4 in order to maintain 10% accuracy in the approximation of Eq 1, or τ $\leq$0.2 μs. The corresponding maximum optical depths for 10% error for 3 SZAs are shown in the table below: SZA τ max 0 0.2 45 0.14 70 0.07 Note that the maximum allowed is 0.2 at high sun (SZA = 0), and decreases with increasing SZA. On page 6 the authors state that they limit their retrievals to 0.2, but their restriction needs to be much more stringent. Indeed, x= 0.8 when τ = 0.4 at SZA = 0, and the “small x" approximation results in an error of 55% wrt the exact equation. The errors rapidly get much worse at non-nadir SZAs. This is a major problem, because the authors use the approximation in Eq 1 to “correct" the scattering phase function. Thus, the authors need to present an extensive discussion of the errors associated with this approximation, and argue why they believe that this is a valid approach. In my opinion, the authors should not bother using this approximation (it is also possible that this approximation is causing some of the zonal gradients that they mention on page 8).

The mathematical analysis made by the reviewer is correct. The exponential approximation leads to significant errors when τ /μs gets too large, and this is the reason why we have set a maximum threshold on the retrieved optical thickness. The reviewer argues that our threshold is too large and that significant errors are expected even for optical depths that are smaller than our threshold. However, it must be clear that the approximation, and its error, only apply to the correction of the phase function from the retrieved aerosol model. Indeed, the inversion procedure uses a Look-Up-Table approach to select an aerosol model that fits best the measurement. We use this model

phase function as a first guess. The top of atmosphere reflectance for this model, which is provided in the LUT, has been computed without approximation. The relative error that is estimated by the reviewer does not apply to the phase function, but rather to the correction from that which is obtained by the LUT approach. The quality of aerosol retrievals over the ocean, demonstrated through comparison against sunphotometer measurements, indicates that the LUT phase function is already a very good approximation to the real one, so that the correction is not a dominant term. We have added a paragraph to address the reviewer comment after equation (3)

The other problem is that there are no statistics and no sensitivity studies in this paper. This paper aims at presenting a new idea, with a first discussion on its potential but also its limitation. We feel it is adapted to the scope of the special issue. We certainly agree that additional analysis is needed in the future for its full assessment.

The authors present seasonal maps, but the reader does not have any idea of how many points are included in each region (other than the white regions, which contain no valid retrievals). The authors to state in page 9 that “the number of valid observations is relatively small compared to..," but what is that small value? The reader only knows that colors in the maps have 1 or more data points, but that is not enough information. In the revised version, we show next to the maps a curve showing the number of valid observations per 10° latitude band.

Minor Issues There are many cases where the authors use ‘BER’ when they mean ‘EBR’. This is corrected

Eliminate the adjective ‘so-called’ throughout the article, as it has no real meaning or value. We thought that this is a standard English term. It is now corrected

Replace “bi-dimensional" with “two-dimensional." Done

Page 3: The nomenclature “independent sensitive areas" is a bit odd... This refers to the individual pixel of the CCD matrix.

Page 4: The authors state that the aerosol inversion procedure uses a set of bimodal aerosol models, but they do not tell the reader how many models are used. Since aerosol optical depth is such an important component of the EBR, the authors should provide a brief overview of the POLDER retrieval procedure, and the corresponding errors. The detail of the aerosol inversion procedure have been described in the literature cited in the paper. We do not feel the need to describe the inversion procedure here.

Page 3: The authors talk about snapshots every 20 seconds, and mention “a large overlap between snapshots." Yet on page 8 they state that the observations are separated by “140 km (the distance travelled by the satellite during the 20 seconds). Why the inconsistency? There is no inconsistency. One snapshot provides an observation roughly 1800x2400 km. A shift of the FOV by 140 km leads to a large overlap (as 140 is much smaller than 1800 km)

Page 5: Are the authors using the POLDER extinctions, or computing their own values? This needs to be stated. What is the effect of using a "corrected" phase function with an uncorrected extinction? In the paper, one use the POLDER retrieval extinction. It is derived not only from the backscatter measurements used in this paper, but also from many other directions (scattering angles). This is discussed in the revised version

Page 5: The authors discuss using measurement sequences that encompass the backscattering geometry to determine "V," and state that the time difference is ≈20 sec, but what is the typical difference in angles? The scattering angle depends on the satellite-sun geometry. Typical conditions are shown in Figure 1 and 2. These figures show variations in scattering angles between 5 and 10 degrees.

Page 5: Burton et al. reported on High Spectral Resolution Lidar (HSRL) measurements, not Raman lidar measurements. We apologize for that mistale. It is corrected

Page 8, last paragraph: I found this confusing: “Let us recall that the POLDER measurement principle is that of a very wide field of view that acquires one shot for each

spectral band every 20 second. For each such acquisition, there is one point on Earth that is observed in the backscatter direction." This sounds like backscatter sampling should occur every 20 seconds, but the authors go on to say that they only obtain 1 sample per orbit. Some clarification would be helpful, here. We do say that there is a backscatter sampling every 20 seconds, which is roughly 140 km along the orbit. The paper says “As a consequence, the backscatter sampling per day is 14.5 (the number of orbits) roughly north-south sets of observations, distant by 25° of longitude, each set providing observations distant by ≈140 km (the distance travelled by the satellite during the 20 seconds).” It mentions “sets of observations [. . .] distant by 140 km”. We see no ambiguity here, but have nevertheless change the wording, detailing what is a set of observations

A. Ansmann comments This is a nice paper on extinction-to-backscatter ratios derived from satellite observations and appropriate for ACP. The paper is useful for the lidar community. It shows in addition that lidar ratios are useful to describe different aerosol types. However, it becomes obvious (to a lidar-ratio expert) that the author is not familiar with the lidar ratio literature and is also not just familiar with lidar techniques.

We certainly agree that our expertise is on passive observations rather than Lidar measurements. We are grateful to the reviewer for correcting our inaccuracies and pointing to appropriate references.

Page 2358, line 5: Burton et al. (2012) does not show Raman lidar observations. The paper deals with high spectral resolution lidar observations. Anonymous Reviewer #1 also points to this error. It is corrected

Page 2358, lowest paragraph: The discussion should include relevant lidar ratio papers. There are so many published by the EARLINET group (Amiridis, JGR, Mattis, JGR, Mona, JGR etc...). But one paper has clearly to be referenced: Mueller et al., JGR, 2007: aerosol-type-dependent lidar ratio. We have read this paper, and we now use this paper to provide further support to out own observations together with BU12

Because lidar ratios around the coast of India are mentioned, please check Franke et al., GRL, 2001, JGR 2003, INDOEX Raman lidar observations of lidar ratio in the tropical Indian Ocean, they fit very well to your values. We are grateful for these references that indeed provide nice support to our own results. It is added to the revised version.

Page 2359 , top paragraph : Tesche et al., Tellus , 2009, 2011, unique pure Saharan dust lidar ratios are presented see also Gross et al. in Tellus 2011. Same as above

Page 2359, top paragraph: The discussion is misleading and wrong. The CALIPSO data processing bias is mostly caused by multiple scattering effects (Wandinger et al., GRL 2010, Tesche et al., JGR, 2013, paper in press). The irregular shape of dust particles is another issue, but not responsible for the systematic large bias, which can easily be explained by the ignored forward scattering of laser light (multiple scattering) in the CALIPSO data analysis.

We will correct the sentence. We agree that multiple scattering is an issue in the processing of Lidar measurements. The statements has been corrected.

Page 2361: final paragraph: Again many incorrect statements "...Raman lidar in various environments..." This statement only fits to the paper of Mueller et al. (JGR 2007, aerosol-type-dependent lidar ratio).

All in all, the paper is fine, and when considering the mentioned literature it will be even better!! Thanks you both for this positive comment and for pointing to the relevant litterature

S. P. Burton comments I would like to ask the author to consider quoting more accurate values for the ranges of lidar ratios reported in Burton et al. (2012). In that paper, we presented lidar ratios for various types only in the form of a figure and not a table, which may have made it difficult to read off accurate values. The 25-75 percentile ranges for the 532 nm lidar ratio for the aerosol types addressed on page 2358 are 17-27 sr for maritime, 45-51 sr for dust, 55-73 sr for smoke, 53-70 sr for urban. Please

consider using these ranges instead of the very rough estimates that are quoted in this manuscript. We now quote the values as suggested by S.P. Burton.

The values measured by our group (as well as the 5-95 percentile ranges if you prefer) are available in tabular form for greater convenience in a follow-on paper currently under review in AMTD: http://www.atmos-meas-tech-discuss.net/6/1815/2013/amtd-6-1815-2013.html. I would also like to thank Albert Ansmann and Anonymous Referee #1 for pointing out that Burton et al. (2012) discuss measurements made with airborne High Spectral Resolution Lidar and not Raman lidar. This is now corrected. We apologize for that mistake

In addition, the lidar ratios for CALIPSO for the polluted dust on page 2358, line 13 are outdated. The values for the current version of CALIPSO processing are 55 sr for 532 nm and 48 sr for 1064 nm. The values can be found in the CALIPSO documentation here: http://www-calipso.larc.nasa.gov/resources/calipso_users_guide/data_summaries/layer/#dq
We have corrected the value and made a reference to the CALIPSO users guide

This manuscript addresses a retrieval of a lidar ratio climatology from the POLDER satellite. A global climatology of lidar ratio is indeed of great potential interest to the lidar community. However, I think this paper would be more useful to lidar scientists if a few additional items are addressed more clearly. First, the extinction-to-backscatter ratio is described prominently in the abstract and elsewhere as the inverse of the scattering phase function at 180 degrees. This is incorrect. More accurately, the extinction to-backscatter ratio or lidar ratio is the inverse of the product of the single scattering albedo and the phase function at 180 degrees, as indicated correctly on the last line of page 2356. Would the author discuss how the aerosol single scattering albedo is measured or retrieved, and whether the results for the lidar ratio are appropriate for cases for which the single scattering albedo is not 1? We have added a paragraph in the discussion section: An important question is the behavior of the passive algorithm in case of absorbing aerosols. The passive measurements rely on scattered photons, so that the POLDER retrieval is sensitive to the scattering rather than the extinction optical depth. The retrieval algorithm makes no attempt to retrieve the single scattering albedo. As a consequence, one may expect a low bias on the POLDER estimate of the EBR in case of absorbing aerosols. At 670 nm, the typical single scattering albedo range between 0.85 for smoke and some urban pollution to 0.95 for dust [Giles et al., 2012]. The expected bias is then between 5 and 15%.

Also, please consider adding a discussion of the fact that the POLDER-retrieved lidar ratios are column equivalent values rather than range-resolved (altitude dependent) values such as would be measured by Raman or HSRL. How does the presence of more than one aerosol type in the column (e.g. dust and marine or pollution and marine) affect the retrieved value? Are all altitudes weighted equally or do the aerosols near the top of the atmosphere have a greater effect on the retrieved lidar ratio? We have added these sentences in the discussion section: Another limitation is the fact that the POLDER retrieval provides a column-integrated value. In case of multiple layers of different aerosols (such as a smoke layer over a marine aerosol), the retrieved EBR is a weighted average of the two layers values. Although the weighting is essentially proportional to the optical depth of each layer (for the range of optical depth that is selected here) the interpretation of the retrieved values would be difficult. Multiple aerosol layers are certainly present in our database, but the presence of clear patterns in the global distribution indicates they are not significant.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C3264/2013/acpd-13-C3264-2013-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 2351, 2013.