Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-320-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biomass burning events measured by lidars in EARLINET. Part I. Data analysis methodology" *by* Mariana Adam et al.

Anonymous Referee #3

Received and published: 22 July 2020

Review of Biomass burning events measured by lidars in EARLINET. Part I.by M. Adam et al.

This paper describes, how a long-term data set from different lidar systems has been created and combined with hysplit backtrajectories and a mask for forest fires, to obtain a tool to analyse biomass burning aerosol via remote sensing.

This is a very important and relevant topic. I fully understand that, as the authors claim, such a large, diverse data set does not run fully automatically through any software, but that user interaction is still required to create the final product. Hence, I assume that lots of work went into this manuscript. However, in the current form I still see room for improvements, both from technical and conceptual aspects.

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Discussion paper

Let me start from the principal point of view. For a publication in ACP the described method should be so clear, novel and comprehensive that the same methodology could be applied elsewhere and stands out as an independent result. Reading the manuscript in the current form I was wondering, whether a publication in a data journal with a sound description of according metadata would be an alternative. In any case the authors may consider to publish the data in itself. My feeling is that this will be worth it (see below).

From a technical point of view the error estimation should be improved. In the manuscript (see my detailed remarks below) the authors state to have a SNR of 2 as a limit and derive astonishingly small error intervals for intensive quantities. To me this does not fit together. I am convinced that a trustful evaluation of backscatter with SNR >=2 is on the edge, but the derivation of extinction or intensive quantities will be impossible. Maybe the SNR > 2 refers to raw data of individual shots, but the final analysis is done with a coarser resolution? Please make this clear. One possible usage of this paper might be by researchers outside the lidar community for further analysis and then a trustful, transparent error analysis will be mandatory.

Further, there is hardly any description of underlying lidar data from the EARLINET data base. I was wondering, looking at BAE in Fig 15, whether all lidar stations are using the same boundary condition to assess the backscatter?

To me, one of the overarching questions (not necessarily to be answered by this paper alone) is “to what extent the information content of lidar data is sufficient to track changes in the microphysical properties of (BB) aerosol”. I assume that the existing data set, presented in this work, is the best and most complete from quality-assured ground based stations we have. Further immediate applications would be to not only apply a fire mask but generally an aerosol emission map to further distinction between pure BB from mixed events. Also, soil and vegetation maps may be included (In the future). A statement that a fire occurred in country XY at coordinates zz can only be the starting point for a deeper analysis. At the end, the question will be what type of vegetation on what kind of ground is burning at a shortage or not of oxygen in dry or

moist conditions. These factors will probably later on determine the values of the intensive aerosol parameters. Such a work can clearly not be done by the authors here but shows the value of their data set.

In section 5 the underlying hypothesis could be sharpened, e.g. by stating that with time / travel distance / max. encountered humidity the AEA, BAE, PDR may change like that ... and for this reason the North American events may stay out like ... or in the scatter plots the quantity xxx is plotted vs. yyy

So my opinion on this paper is mixed: provided that the error calculation is revised and honestly be stressed what can or cannot done with this data set, the scientific significance of the data set is very high. In its current form the papers is lacking substantial novel conclusions, though. Due to the principal importance of this data set and the work the authors performed so far to create it I rate the scientific significance as "excellent" (provided the error treatment will be revised). Due to the overall quality of the paper in its current form I rate the scientific quality as "low".

Some specific remarks are:

Intro line 19: what do you mean be fuel with respect to climate change? Fig 2, No.8 what are the limits for neglecting intensive parameters, what percentage of data has been rejected?

Section 4 P 7, l 13: with kappa you mean the extinction, right? Before you have used the symbol "e" L 20: is there a reason for the bin numbers? (the product of resolution * numbers is not constant) how does this different resolution later affects you layer selection? P8, L 4: I do not understand the possibility that a maximum is not surrounded by 2 minima. findpeaks is employed for beta1064, i.e. real data, so I assume intervals with constant values are at least not frequent? P8, L 7: in which way this last criterion is independent from the others (and hence needed)? L24/25: "Our approach ... in line with ..." this statement is completely unproven. Maybe you do not need it but to prove it you should apply your code to one of the examples of the

mentioned papers. P9 L 15 and 16: where is table S2? In my version of the manuscript the citations are not numbered. P10, L 24 I do not understand the definition of data availability: Isn't the ratio between layer thickness and resolution simply the number of data points? Did you use an additional criterion to flag a given data point as valid? P11, L 11 and following: is SNR >=2 really sufficient to determine particle extinction and lidar ratio? The SNR of the underlying Raman channel must have been much better, I assume. Fig 9 is interesting. However, presented like this: almost spherical particles, very high LR and very small particles I am wondering whether this is really BB or anthropogenic pollution. It would be interesting to show the hysplit trajectories for the extreme cases (maybe 2015, 2016, 2017). P13 L2: Sorry, I missed that: why PDR (at 532) increases with time? I would have assumed that coagulation makes the particle shape more spherical

P13 L 6 (and P14 later) I would omit mentioning Fig 2 for things which the reader cannot verify on his own. P14, L 7-8 the errors in the IP are unbelievable small and non-proven. If this is the error of many different measurements, you rely on the fact that the aerosol layer did not change during the sampling. Contrary, the different BAE between "bu" and "th" are much more understandable, if the fire conditions, the heat, the burning plants the amount of water vapour . . . etc changed.

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