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Interactive comment

Interactive comment on "Biomass burning events measured by lidars in EARLINET. Part II. Results and discussions" *by* Mariana Adam et al.

Anonymous Referee #1

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This paper presents observations of biomass burning observations, specifically aerosol intensive parameters, from a regional lidar network, EARLINET. The paper then discusses their origins as determined by back trajectory analysis. This paper is a companion to an earlier paper (acp-2020-320) which described the methodology for QC on this network of instruments of disparate data quality. This is certainly an important topic; however, I found the paper to be difficult to follow and review, first because the paper seems to have several different opinions about what its topic is, and second because the limited data available makes broader interpretation challenging. I would recommend the authors consider what exactly they want to convey from this paper, and make sure that the paper accomplishes this.

I have some further concerns about the analysis presented in the paper, especially much of Section 4 (4.2 and 4.3), again largely due to the limited data available for analysis. Of course this doesn't mean analysis can't be performed, or that the results are meaningless, but only that they must be more carefully considered, and I would like to see much more to that effect (maybe some of this is in there; if so, I couldn't easily follow it). NB: I have not reviewed the companion paper, but I have skimmed it to try to see whether I've missed some information which would address these concerns, but I wasn't able to find much that was relevant. The topic presented here is interesting, but the paper as written does not address it in a satisfactory manner. At the very least I think major revisions are in order before it can be considered for publication.

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Specific comments, structure and focus:

–The title and the abstract seem to be describing two different papers. The title talks about biomass burning events as a whole, but the abstract focuses heavily on BB due to long-range transport, and specifically LRT from North America. Why this framing? And then according to the first sentence of Section 3.2 (and the first page of 4.1), N America isn't even half of the LRT cases? Although this sentence says "We encountered 168 measurements over the 24 LRT periods. . . from these measurements, 77 have a North American origin and 91 have different BB origin (local)." Are the 168 measurements then all long-range, or also local? In other parts of the paper it seems that this "local" transport is also included under the "LRT" category. I would have thought these would be two distinct classifications. There's also a discussion of transport from Asian and African sources, so why is the abstract solely focused on N Am sources?

–The abstract also doesn't make any mention of the case study [edit: studies] which is discussed in Section 3.1. Why not? Isn't this a big part of this paper?

– I found it very difficult to follow all the multiple sets of acronyms used (site names, groups of sites, plume origins, parameter names. . .). A list of the acronyms is provided, but it's in the last page of a separate supplement file, rather than in an appendix or even in the main text. This really inhibited my ability to understand the paper. I would suggest at the very least moving the acronym list to an appendix within the paper, if not to the main text itself. I think the authors rely on these abbreviations enough that it would belong in the main text.

– Does Figure 3 use one color to indicate two completely different things (e.g. am I reading this right that WAW and AS are both marigold, but the marigold asterisks are not what's used to determine the marigold circle-whisker?)? This is unnecessarily confusing. Pick different colors or consider e.g. using shape distinctions for one or the other. (this comment refers to the final pattern of Fig 3. . . I guess the previous page is also Fig 3).

– On that subject, a lot of the figures are simply too large, and the captions are insufficient to describe them. There are also a whole lot of supplementary figures too. Consider splitting them into smaller sets of figures, or reassess whether the information really needs to be presented this way. Figures that span 3 pages with a single (small) caption are very confusing to follow. Especially e.g. cases such as Figure 4: you conclude there is no correlation between the variables presented in panels d-f. Maybe this isn't necessary to show at all, then?

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Specific comments, scientific analysis:

– perhaps this was addressed in Part I, although I found no mention of it: it seems this classification is entirely determined based on one run of the HYSPLIT trajectory model run over ten days. What have you done to assess whether these trajectories are robust? Did you run it in ensemble mode to verify whether the trajectories were consistent? Or did you run the model forward from a fire location to see if the airmasses ended up in roughly the same location? Perhaps the authors have done this, but I would like to see much more information regarding how they addressed the uncertainties inherent in HYSPLIT or any trajectory model. Especially since many of the conclusions presented here are based on very few (sometimes just one) points, I think this is a necessary exercise.

Related, p.6 Line 11: "Moreover, the ensemble backtrajectory for 3000 m a.g.l. at 19:00 using GDAS 0.5 showed more backtrajectories towards North America." More appropriate would probably be something like "the ensemble back trajectory corroborated that the air mass originated in N America," if that's what the ensemble trajectory showed. Consistent ensemble paths back to NA would give extra confidence to the result that this air mass originated there, but doesn't necessarily mean that more air came from there. Conversely, if the ensemble shows e.g., half from NA and half from Asia, that would indicate the HYSPLIT trajectory is not particularly robust in this case and we may need to reassess that confidence. I don't see much other text indicating whether these types of tests have been performed. I do notice that p.6L14-15 seems to say that "the results are not in agreement" [for two different configurations of HYSPLIT trajectories]? Is that to what this sentence is referring? Then how can you believe them? Maybe I'm misunderstanding this section. . .

– I don't follow the logic behind Section 4.3. Examining means can be a decent way to analyze two different populations, but in this case (with very small sample sizes and large ranges between them; e.g. the error/range points/whiskers presented in Figs 3, 4, 5) I'm not convinced studying just the means is meaningful. For that to be the case, you'd want to have populations where the properties were known to be 1) normally distributed and 2) fairly uniform, but as the authors themselves state on p. 17 (Lines 14-17), these episodes are a mixture of several fires already. So this doesn't seem a good approach to me. Perhaps medians/percentiles, or simply present the ranges (rather than stdev) especially when the points are so few?

Plus, if you're trying to draw conclusions based on 2 points of one case and 1 of another, this doesn't seem statistically robust or generalizable. That's not to say that minimal data can't be useful, but its limitations need to be acknowledged. I think the data presented in Figure 5 are actually quite useful to that end, but again, I found

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Discussion paper

the presentation quite confusing (log-y axis on one side, lin-y axis on the other, with multiple colors and a not very informative caption).

Perhaps focusing on additional case studies would be a clearer presentation for the cases outside EU-origin.

– in Figure 4 you present values of R=1. This is because you are fitting through only 3 (2?) points. This R is meaningless in this context, especially since one has very large uncertainty bars and the other has only one point.

– Section 4.2: the authors draw conclusions that e.g., EAE values of 1.4 indicates a mixture of fresh and aged smoke, and 1.2 is only aged smoke. With such a small difference between the two, I'm not convinced this is robust; e.g., even the STD values in Table 3 are uniformly greater than this. What are the ranges?

– Further, (and assuming the above issue can be resolved) in Table 4 all except for three categories are classified as "aged," with two more "fresh/aged" and one "fresh." This is one of the conclusions in the abstract as well, but I'm not seeing any definition of what "aged" means in this case. And if this is truly the case, the classification of "aged" corresponding to different regions and origins is not very useful at all. Aerosol aging especially of BB can mean different things on different timescales (e.g. Haywood et al. https://doi.org/10.1029/2002JD002226 defined "fresh" as only a few minutes after emission), and a few days old likely won't be the same as a week old, so what exactly is meant by this? It may be more instructive to only focus on the cases which are *not* aged and examine what distinguishes them from the other cases. Or, conversely, if you're using back trajectories, can these determine exactly how "aged" (= time from emission) each population is? That might actually be more instructive than classifying observations as "aged" based on (as I understand it based on p.5L26) the EAE and CR, and could potentially allow for discussion of the property of BB different ages, rather than the regional Europe division.

– In the abstract, findings i) and ii) are saying basically the same thing, but inverted.

And (related to the previous comment), “travel time” might more appropriately be called “aerosol age” (=”time from emission”). But, I don’t really see any discussion of this beyond the case studies in Section 3 (and maybe a statement p.17L1-2, that it was aged based on high RH. . . I don’t know that this is always the case, e.g. African biomass burning in particular can see high humidity very near emission). And regarding the conclusion in the abstract and in Section 4.3.1, “A slight decrease of the CRPDR with travel time was observed, while the CRBAE maintained similar values for all the source regions,” I’m not clear whether this can be concluded from the data as presented. Was the same plume observed from multiple stations along its trajectory? Otherwise it’s hard to say whether the initial aerosol properties were the same, or whether they were different to begin with. I’d like to see more clarification as to how this conclusion was reached.

–p.6,L19-20: “IPs STD represents $\sim$ 15, 38, 27, 17 and 37 % of the mean, respectively, and thus we claim a relatively small variability over the whole three days of measurement.” I don’t follow this. First it will depend what the mean value and what dynamic range is typically expected from a particular parameter, and second, with only 8-13 measurements for several of these parameters, I’m not sure stdev is the best metric for variability. What’s the range, or maybe (for the 31-case BAE) percentiles?

–p.8,L10+: As above, what is the confidence on the back trajectories from NAm vs elsewhere? Further, I think this paragraph (“we noted several IP values. . . outside the range reported”) refers to both NA and NA+local mixed aerosol? What fraction of each is included under “mixed” conditions? Is it relatively constant, or does the percentage vary for different cases? Was there a threshold (e.g., only 5% local) below which EUNA->just NA? I didn’t see this in the paper. . . and while certainly it’s possible to report values outside what’s been previously reported, it seems prudent to discuss how much mixing and how much confidence goes into the present estimates, where they disagree. (Relatedly, what are the asterisk vs circle for the literature numbers in the Fig 2 panels? I don’t see this described anywhere.)

–conclusions, p. 16,L23-25: with the limited amount of data available, I think this is too strong of a statement.

–another couple points of clarification: when the authors say “fire” is identified by being within 100km,+/-1h from the trajectory (p.3L23), were altitude thresholds applied or not; i.e., if HYSPLIT trajectories were at 8km, was it still considered to be a smoke source even for surface-confined, non-pyrocb fires? Also, are the “total fires” on p.5L∼18 distinct fires, or just MODIS fire detections at a given time? (may be semantics, but surely a single fire will often be detected multiple times during multiple overpasses; are these considered distinct fires in these summaries?

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Other comments:

– Q: What is red in Table 2? A: local fire; this is buried in the text, add it to the caption.

– p.4L18: “same graphics as in Section 3” but this is Section 3?

– p.6,L21: “better sphericity” => “greater sphericity”?

– p.6,L23: “increase in LR” relative to what? Not clear.

– p.6,L27: If this event occurred on 14July, why is it within a section labeled 0708-0710? Expand the 3.1 title or make a new subsection. Same for the following paragraph, now you’re talking about the 2017-2018 event in this same section?

–p.7L11: suggest move the Peterson reference up to L4.

–p.11,L8-13: this paragraph in particular was pretty incomprehensible to me. There are some other phrasings throughout which are difficult to parse as well; I haven’t listed them all.

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