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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Smoke of extreme Australian bushfires observed in the stratosphere over Punta Arenas, Chile, in January 2020: optical thickness, lidar ratios, and depolarization ratios at 355 and 532 nm" *by* Kevin Ohneiser et al.

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Review of Ohneiser et al. (2020). (Hereafter "O20") Reviewer: Mike Fromm O20 report on a small set of very recent groundbased lidar observations of tropospheric and stratospheric wildfire smoke over southern South America, Puntas Arenas. They attribute the smoke to fires in Australia. The bulk of the paper consists of analysis of the lidar data and a nearby AERONET photometer. O20 also compare the POLLYNET lidar observations with stratospheric smoke observations over Europe in 2017 obtained with a similar lidar setup. To the extent that stratospheric smoke observations

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are now available from at least two different events with a similar instrument configuration, this is a useful study. The subject matter is well targeted with ACP. However, I find several weaknesses in the manuscript leading me to the assessment that the manuscript, in its present form, does not merit publication in ACP. I discuss these next, and follow that with a list of minor issues O20 that should consider. The Australia wildfire event to which O20 refer and attribute for their smoke observations occurred very recently (since November 2019). The manuscript's Introduction contains several statements giving facts regarding the seasonal set-up weather/climate conditions without citing any sources. In Section 3, O20 call out "Bureau of Meteorology" but provide no matching listing in the Reference section. Hence I was left with the impression that this manuscript, which became available just weeks after the fire events unfolded, is rushed and premature. The authors acknowledge that this is a "rapid" report. If there is a separate standard for evaluating such a "rapid" report I am unaware of it. Hence I applied the standard of scrutiny I use for all ACP-D manuscripts.

Due to the recentness of the Australian wild fire events (and there are several, dating back to spring 2019), very little citable information about the fire and fire-storm conditions is available. For instance, to my knowledge there are no details about the spatial-temporal nature of the fire emissions yet established in the citable literature. At this point the information is relegated to general media reports and blog posts. O20 attempt to describe the source conditions but generate/present no content on which to constrain the source term. The only reference they give is a NASA blog posting. Conceivably, this single callout might be sufficient to establish a plausible connection with the Puntas Arenas lidar data. However it fails to help O20's analysis, which consists only of a set of 20-day back trajectories, none of which make a connection to Australia at the times of claimed smoke initiation. In my assessment the trajectories offer little more than evidence of general westerly winds, adding little value over what one expects from meteorology. O20 recognize this yet proceed as if the trajectories prove a connection with the areas/time of Australia smoke emissions and pyroconvection. Given that O20 make an attempt to connect their observations to a very distant source,

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it is implicit that this connection is important to their thesis. Not only is the hypothesized source very distant from South America, the timing of Australia's pyroconvection is also unknown to the reader. An informal collaboration of scientists following pyroCb action observed pyroCbs in western and southeastern Australia on several dates since mid-November 2019. UTLS plumes were thus put in place over a two-month period sufficiently in advance of the Puntas Arenas observations that tighter constraints on the source of these reported observations are necessary. And given that there are available data sets and methods with which to establish the source-receptor relationship, data and methods they do not employ, this effort is deemed inadequate in its present form. An example of the above-mentioned method was employed by Foth et al. (ACP, 2019) who analyzed another stratospheric smoke plume observed by POLLYNET lidar at Puntas Arenas n 2010. In that case FLEXPART trajectories and upstream CALIPSO curtains together were critical in identifying a plausible candidate for the source of the smoke. Foth et al. also provided details on a pyroCb event in western Australia to support the end-to-end connection. Surprisingly O20 did not cite Foth et al., a critical weakness. Moreover, since O20 devote considerable attention to examination of the derived lidar properties in Puntas Arenas and a European POLLYNET lidar, it seems that an invaluable opportunity to compare the 2020 lidar observations with those examined by Foth et al. was not taken. O20 acknowledge that this is a "rapid" submission and that they are presently processing/analyzing the unfolding smoke event's data with an eye toward future papers. Given that acknowledgement and the paucity of essential information provided herein, my assessment is that this manuscript is unsatisfactorily weak in its present form. For the authors to meet their expressed intentions, and for the benefit of ACP, I recommend that this manuscript be strengthened by adding more substance on the source term, a convincing analysis connecting the South America smoke with the source, and a direct comparison of the 2020 smoke observations with those of Foth et al.

Additional Substantial Concerns/Questions Introduction, P2, L2-4. O20 report observations of CALIPSO-detected smoke at 20+ km on unspecified dates and provide a

citation that leads to a generic CALIPSO web site. The reader cannot find the intended evidence at that link. Introduction, P2, L8-9: “The stratospheric aerosol spreads across the southern hemispheric latitudes from about 20-30_S to the south pole within few weeks. . .” No evidence of this is produced in the manuscript and no citation is given.

Introduction, P2, L11-12: “Stratospheric aerosol perturbations influence radiative transfer, ice cloud formation in the upper troposphere, and chemical processes in the lower stratosphere over months.” This statement lists smoke-aerosol effects in the lower stratosphere, a subject that to my knowledge is unexplored. No evidence is produced herein and no citations are given. If published results along these lines are available they need to be cited. Otherwise, O20 should characterize the statement as suggestive or speculative.

Introductiom, P2, L13-14: “The height-resolved documentation of this unique event including the decay of the stratospheric perturbation will be mainly based on observations with. . .” O20 go on to list a subset of satellite-based aerosol profile data but do not include several important ones, e.g. OSIRIS, OMPS/LP, SAGE III ISS, ACE-Imager.

P4, L6-7, discussion of Fig. 1a: O20 describe smoke between 4-10 km, but it is not obvious to me. I see mostly cloud. The data represented in Fig. 1 is raw, unccalibrated range-corrected signal, and no depolarization ratio image is provided. Hence it is very difficult to determine cloud-aerosol differences. O20 go on to discuss the clouds that dominate the curtain, label them all as ice clouds, and make inferences regarding smoke nucleation. The raw information provided in this figure is not sufficient to support their description or inferences.

P4, L14, discussion of Fig. 2: Twenty days is a very long interval. None of the trajectories pass over southeast Australia. Two of the three don’t even get to Australian longitudes by 27 Dec. On its own this figure provides little more information than that the winds are general westerly, which is not illuminating.

P4, L15-17, more discussion of Fig. 2: “As can be seen, air masses obviously started

over Australia at heights of 15-16 km. . .” This is not only not obvious, it’s not even true. None of the trajectories pass over Australia.

P5, L9: “Typical values. . .” How does the reader know what is "typical" linear depolarization for stratospheric smoke? This implies a sufficiently large data set exists and is available to the reader. Such a data set is not presented here, nor citation given. Double-digit depolarization ratios are certainly unusual for tropospheric smoke. The literature on stratospheric smoke depolarization exists but is quite limited. Substantiating the claim made here requires a fuller overview.

P5, L20-26, discussion of depolarization/particle shape: This argument is confusing. To first order, depolarization increases with asphericity. Double-digit depolarization ratio then would suggest a more irregular shape than "almost spherical." Please clarify.

P5, L27, discussion of what Aeolus lidar would report: How does the reader know what the ALADIN lidar ratio would be? Is it appropriate to invoke unreported Aeolus measurements? If so, how is the reader to assess these Aeolus data?

Technical issues: (See the accompanying pdf of the manuscript with comment

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2020-96/acp-2020-96-RC1-supplement.pdf

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