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## Comment on acp-2021-117

Anonymous Referee #2

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Referee comment on "The unexpected smoke layer in the High Arctic winter stratosphere during MOSAiC 2019–2020 " by Kevin Ohneiser et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-117-RC2>, 2021

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Ohneiser et al. present a summary of a persistent wildfire smoke aerosol layer observed during the MOSAiC field campaign in the high Arctic. The authors primarily focus on observations obtained from the "Polly" Raman lidar operated onboard the Polarstern, but also compare with other lidar platforms (e.g., CALIPSO and KARL). With the unique observations made possible by Polly and the MOSAiC campaign, the authors are able to provide quantitative descriptions of the optical and microphysical properties of observed aerosols for a region where such measurements are sparse. These measurements are made even more important by the unique atmospheric and aerosol conditions related to the 2019 Siberian wildfires and the unique polar vortex and ozone conditions of late-2019 and early 2020.

I am not a lidar measurement expert; in fact the editor has asked me to provide my views for portions of the manuscript related to polar vortex conditions and ozone depletion. However, I of course read through the entirety of the paper (multiple times), and so I feel comfortable saying that the paper is generally well-written. While I can not provide an expert assessment of the observational aspects and descriptions, as a "knowledgeable layperson", I'd say The authors mostly gave thorough and convincing descriptions of the observational data and their characterization. The paper is clearly cutting-edge and would be a perfect fit for ACP. However, I do have a few concerns, some more major than others; most of these are related to the aspects surrounding the polar vortex and ozone depletion in Section 4. Since Section 4 is a relatively small portion of the overall paper, I would say the revisions required to address my comments would be relatively minor to implement, but I do feel strongly that they are necessary changes.

### General comments

(1) In reading through the paper, reaching Section 4 felt like a definite shift. Up to that point, everything felt (as a reader) mostly authoritative and backed up by quantitative

results. In contrast, Section 4 felt very vague and overall speculative. One paragraph particularly stuck in my mind -- p14, L10-15 -- as it included many instances of language such as "were probably ..." and "were likely". Three "precise" research questions are posed early on in the section, and yet the remaining results and discussion do nothing to answer them and instead involve results that only hint at possibilities. Section 4 essentially says "the smoke aerosol layer could have been important for ozone depletion" simply because the PSC, smoke, and ozone depleted layers overlapped in the vertical (Figures 15 and 16).

There are many details that would have to be worked out in much more detail to answer the questions posed in Section 4. For instance, if the MOSAiC measurements were to be of use in answering these questions, then there would need to be details about the time-varying geometry of the vortex. While it's a fairly safe assumption that the vast majority of measurements (lidar, ozonesondes, etc) sampled air within the polar vortex, on weekly timescales the vortex and its edge are quite mobile (and similarly, so is the region of air cold enough to form PSCs). This means that measurements could sometimes sample air in different parts of the vortex, meaning some measurements would be less relevant than others in establishing where things were "in the right place at the right time".

As written, I do not feel Section 4 should be kept. I would suggest that it should be shortened and perhaps folded into the current Section 5, with content from Section 4 forming the basis of some discussion (i.e., providing motivating context for future work). At the very least, Section 4 should be shortened and rearranged so that the authors present the "possibility" before posing the big picture questions. The authors should also be clear (if they keep any of Figures 15 or 16) that conclusions cannot presently be drawn from their analysis.

In my specific comments below I provide more detailed questions/comments related to the content of Section 4.

(2) Again, this is not my area of expertise so perhaps these are naive questions: The title of the paper and the introduction generally outline the assumption that the smoke aerosol layer measurements were tied to the Siberian wildfires, but are there not other potential confounding sources? Can the authors provide more evidence to say that the Siberian wildfires were likely the dominant/overwhelming contribution to the observations discussed throughout the paper? The paper lacks some descriptive context as well: how severe were the 2019 Siberian wildfires in comparison to prior years? It is perhaps fortuitous that the MOSAiC campaign was able to sample aerosol conditions largely influenced by Siberian wildfires, but is there a quantitative measure of how unusual 2019 was that would better emphasize the importance of the MOSAiC measurements? These kinds of details may be present in the articles that the authors cite, but I think it's worthwhile for the authors to make them explicit where possible.

(3) There were a couple of times in the paper where a Figure is introduced and a portion of it is discussed, but then the discussion moves onto one or more different figures before later coming back to the initial one. An example of this is Figure 9: On page 9, line 17

Figure 9a is introduced, but then within the same paragraph the authors move on to Figure 10. The authors go back to Figure 9a, and eventually introduce Figure 11 before eventually mentioning Figure 9b. I recognize that this may just be personal preference, but I think that cases like these generally suggest that text or figures should be re-organized to better maintain the serial nature of the text.

I understand that in the final manuscript the authors will generally not have much of a say where figures will end up in relation to the text, but it is still a bit awkward for a reader to have to flip between multiple figures for a given piece of text (whether on paper or digitally). I wouldn't classify this as a major issue in this paper, but I still urge the authors to consider whether their figures or text could be restructured to make things flow more naturally.

### **Specific Comments**

*P6, L17 and Figure 5:* The information about the arrival height of 10km should be included in the figure. Also, what about the vertical information? How did these trajectories evolve in terms of altitude/pressure over this time period? Is this not important information for discussing the airmass origins?

*P10, L22:* I do not believe the Lawrence et al ref here shows that the polar vortex collapsed on 20 April. In fact, their Figure 10 shows the vortex was unusually long-lived, lasting into May. Maps of the vortex on April 20 ([https://fluid.nccs.nasa.gov/reanalysis/clsic\\_merra2/?one\\_click=1&tau=00&level=50&field=epv&region=nps&fcst=20200420](https://fluid.nccs.nasa.gov/reanalysis/clsic_merra2/?one_click=1&tau=00&level=50&field=epv&region=nps&fcst=20200420)) show that the vortex was undergoing a split, so it's probably fair to say the vortex began decaying around this time. However, if you click through further dates at the above link, I think it's clear that a distinct polar vortex is present well into May. This comment is also relevant to other spots in the text where the authors mention the vortex collapsed in late April (e.g., P2, L11).

*P13, L14-15:* This requires more nuance; it wasn't just weak planetary wave forcing, but also the very strong **dynamical** influence of downward wave reflection events during the 2019/2020 winter season (this is discussed in detail in the Lawrence et al ref).

*P14, L3-9:* While questions 2-3 are interesting and are worth further exploration, I'm not sure question 1 has much scientific merit in relation to the polar vortex. There are too many issues with timing and location and other confounding factors. Early in the season, the polar vortex was generally not extremely strong or weak (see [https://ozonewatch.gsfc.nasa.gov/meteorology/figures/merra2/wind/u60n\\_50\\_2019\\_merra2.pdf](https://ozonewatch.gsfc.nasa.gov/meteorology/figures/merra2/wind/u60n_50_2019_merra2.pdf)). Furthermore, the very strong vortex conditions in Jan-Mar coincide closely with the dynamical downward wave reflection events mentioned above.

*P14, L18-22:* While true, I'm not convinced this is an appropriate apples-to-apples comparison. For instance, the 2019 Australian wildfires were severe enough to be comparable to a moderate volcanic eruption in terms of the impacts to solar radiation (as noted in the Khaykin 2020 paper already cited). I do not think that the Siberian wildfires have been established as being anywhere near as severe. There are other differences, but the point is, this kind of comparison runs the risk of equating inherently different events, which could be much more coincidental than this statement suggests.

*P14, L23:* How much of this introductory material is actually necessary for introducing the two figures of the section, which don't actually answer the "big questions" posed beforehand? The beginning of this section sets up big expectations that aren't met.

*Figures 15 & 16:* These figures appear to me to be mostly redundant. Figure 16 is mostly a coarse-grained version of Figure 15 with additional information about the ozone anomalies. It seems like this information could easily be combined into a single figure.

*P16, L16-22:* This paragraph and Section 4 (which has major issues) seems like a missed opportunity to provide expert guidance on how MOSAiC measurements could assist future studies that may attempt to answer such questions.