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Comment on acp-2021-643

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Referee comment on "The contribution of Saharan dust to the ice-nucleating particle concentrations at the High Altitude Station Jungfraujoch (3580□m□a.s.l.), Switzerland" by Cyril Brunner et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-643-RC1, 2021

General Comments

This is a very interesting study using the automated HINC instrument, which is packed with useful results and honest (refreshing) caveats, limited only perhaps by the assessment being made at a relatively low temperature (243K, for conditions that emphasize water droplet co-activation and expected immersion freezing). That is a modest critical comment, really, as it does not terribly limit the insights reflected in the deep analyses conducted. The study reveals important links between ice nucleation at lower temperatures and Saharan dust transports in this region, an apparently ubiquitous and strong influence, made further relevant by the observation that higher altitude dust appears to be drawn down via virga following likely heterogeneous ice formation at cirrus levels. The authors emphasize that this raises the potential issue of pre-activation and the known inability of most INP instrumentation for assessing this without special preconditioning of sampled air (needs more discussion). I wonder if this behavior does not also point to an important potential link between dust and precipitation in storms where deep clouds form over the region under dust transports. Precedence for this occurrence already exists in the literature over other regions that are more distant from major mineral dust sources. If so, transported dust INPs at high altitudes could more importantly impact precipitation (via ice sedimentation effects) than one might guess based on their characteristics of activating only at lower temperature, versus some other INP types that are imagined to have a greater impact on mixed-phase clouds due to their higher activation temperatures. I feel that a potential missed opportunity in this paper is relating the INP data more directly to aerosol size distribution measurements. An important relation to aerosol surface area is only inferred in the paper, and indirectly supported via the relation shown with attenuated backscatter. Closure with actual measured aerosol surface area to show relation to INPs could be useful (not required, just a suggestion). Presently it is only stated that some of the surface area (35% estimate) is missed by the measurement limitations on capturing all sizes. That seems a minor amount to miss, something that I think should be emphasized as a positive aspect of the real-time, automated methods. I have only an assortment of other specific comments added to this, which I do below in order of appearance.

Specific Comments

Abstract

Line 4: I suggest that this statement is not true in general, and so requires more generalization without attribution of a specific temperature. There are many reports of other influences on ice formation dominant at this temperature that are not only mineral dusts. For example, also soil dusts, which made me think about whether (or why not) this site does or does not see influence for more local/regional soil sources.

Line 8: I suggest to mention the method for derivation of Ångström exponent here (nephelometer), just as satellite retrieval is mentioned for mass concentrations.

Line 15: Can you distinguish mineral from soil dust particles (due to attribution scheme for SDEs)?

Introduction

Line 52: This is obviously not critical, but just to note that DeMott et al. (2003) was revised in 2009 to indicate a correction to maximum INP concentrations, reducing them to a few hundred per liter (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2009GL037639).

Lines 52-55: I feel it is necessary to say that Zhao et al. was a modeling study, depending both on proper modeling of aerosols and parameterization of INPs.

A general comment on the introduction: It could be better introduced how you plan to identify SDEs specifically.

Methods

Lines 92-93: "There is no appreciable natural source of mineral dust in proximity of the site." Is it true that there are no regional sources through the year of other dusts, such as agriculture?

Lines 109-110: Similarly, "If the plumes appeared not to originate from within the planetary

boundary layer (PBL), the period for which the plume is observed between 3200 - 4000 m a.s.l. is marked as SDE." Is it always so that origination in the boundary layer is so easily determined, for the entire history of a parcel? This statement confused me because it becomes clear later in the paper that SDEs entail air reaching the site both from the boundary layer and the free troposphere.

Section 2.2.2: Could you add to say a little more about the Ångström exponent calculation, and why a negative value clearly marks an SDE? I think it would help the general reader.

Lines 174-179: This is an important discussion, perhaps for reasons that are not highlighted. Although there is size limitation on transmission of particles to the HINC, the losses of INPs are not so great. That is, if indeed mineral dust INPs scale with surface area, then a 35% loss is not so significant in consideration that different INP measurement methods easily disagree at times by numeric factors up to 10. Hence, it would be great to see documentation of this statement with a size distribution typical of SDE periods. Not a requirement, but a suggestion. And could you also state if an upstream impactor is used for the HINC or not as part of INP detection?

General comment on Section 2.3: The nature of the correction for frost background is unclear. Simple subtraction? Counts exceeding the 1 sigma value of the filtered air value? And in general, it would be useful to repeat how INPs are distinguished from other large particles in the HINC, especially during SDEs. I realize that an entire instrument paper was recently published, but some of these things bear repeating in short form.

Fig. 4: Would one infer from this figure that the SDE encompassed primarily boundary layer air transport? Again, I was confused because there was such an emphasis placed on indicating FT origin, and I was expecting that FT would encompass most SDE. Also, in this Figure caption and in the main manuscript text, I suggest writing out the meaning of hcSDE and lcSDE. Please note, I could not find anywhere else in the paper where these were specifically defined. That is, what makes for attributing hcSDE versus lcSDE?

Line 250: Particulate matter means mass concentration? Or total counts in a certain size range?

Section 2 general comment: It seems like the CAMS, FLEXPART modeling and CALIOP data are the key to assuredly attributing dust to a Saharan source. That is, an aerosol layer in ceilometer data and Angstrom exponent will only indicate larger particles present from somewhere. I am curious if there were ever indications of arable dust sources reaching JFJ. It seems common in other locales, including mountaintop sites that are perhaps not so elevated. Unfortunately, I cannot offer a reference for that because the separate works I am aware of are still in preparation. It is simply an honest question about whether any such influences were inferred during the non-SDE periods or if any lcSDE events could have captured such.

Results

Line 338-339: I appreciate the strong qualifications added to the conclusion on the major role of mineral dust outside of SDEs for the conditions examined, but I wonder if somewhere here or in the conclusions you could speculate on what other types of measurements (or extensions) could be done to support the major role of mineral dust that you hypothesize here for this site (and by proxy perhaps, these altitudes over the region)? I say that because, in boundary layer measurements, my impression is that mineral dust cannot be assumed as the only influence at 243K, though I will not list the references.

Lines 355-356: Do natural INP concentrations scale with aerosol surface area? Were particle distribution data co-collected to test this assumption for this situation that appears to reflect one primary INP type? Does CAMS provide such predicted information or is their size distribution fixed?

Line 434: Regarding these results apparently linking cirrus virga to descent of mineral dust plumes to levels where they can impact ice formation at lower levels, the mystery to me is why the virga connect to regions of apparent higher dust concentrations than ever seem to appear aloft. Is it the case that the higher regions are more typically shrouded in clouds because ice will so readily nucleate ice there when ice supersaturated conditions exist? It may be useful here to note that during a study intended to assess Asian dust impacts on INPs at a mountain site in the western United States, Richardson et al. (2007) inferred that dust frequently was concentrated well aloft on the basis of aerosol modeling (c.f., Fig. 6 of that study), and often this was predicted to descend over time although connection to virga was not possible to investigate. It seems possible that dust descends with the virga in your case as well, perhaps not only as activated ice crystals? Hence, it

may not all represent pre-activated particle populations. However, other studies have indicated the critical role of elevated dust plumes on precipitation formation under circumstances where that occurs (Creamean et al., 2013). The seeding impact from higher altitudes with longer inherent ice growth times and fall velocities could play a critical role in precipitation for orographic locations. I think your data suggests that this could be common due to the ubiquitous presence of dust aloft and its propensity for freezing there.

Lines 440-442: You say as much in the abstract, but I expected a few more words here. It is possible I suppose to explore this topic in a laboratory setting, or by exposing instruments to the natural environment. I suspect that unreported efforts have already been attempted. It will be immensely more difficult to do this in an aircraft setting, where the actual nature of the inlet outside the aircraft would have to be modified in order to not heat particles due to pressure. In any case, I think you should expand the discussion a little bit, for the sake of those who may not have deeply considered how this can be done (or how hard it may be).

Editorial notes

Line 25: It is unusual to start a sentence with a number. Perhaps write it out.

Line 31: cover clouds ☐ do clouds cover

Line 69: Suggest to spell out 20 “minutes” for the INP measurement time resolution.

Line 90: hosts ☐ has hosted

Line 119: previously is misspelled.

Line 127: with is misspelled.

Line 272: where ☐ were

Line 425: Probably do not need the word “for”.

Line 461: Suggest rewrite, e.g., “and sediment to lower altitudes where they sublimate in drier air…”

Line 463: loose ☐ lose

References

Creamean, J. M., K. J. Suski, D. Rosenfeld, A. Cazorla, P. J. DeMott, R. C. Sullivan, A. B. White, F. M. Ralph, P. Minnis, J. M. Comstock, J. M. Tomlinson, and K. A. Prather, 2013: Dust and Biological Aerosols from the Sahara and Asia Influence Precipitation in the Western United States, *Science*, **339**, 1572-1578.

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