To the authors and TC,

I provide here a review of the article "Can Saharan dust deposition impact snowpack stability in the French Alps?", submitted to The Cryosphere (TC-2022-219). My recommendation is for publication in TC pending only minor revisions.

Summary: In this study, an ensemble snow cover model was used to investigate the impact of dust deposition on snow properties and mechanical stability by comparing simulations with and without dust deposition for one synthetic and one observed dust deposition event. The modelling chain, used to perform the simulations, employed SAFRAN-SURFEX/Crocus-MEPRA for meteorological conditions, ALADIN-Climate for dust deposition fluxes, and TARTES for radiative transfer calculations in snow. Although each of these models bear with them their own set of uncertainties, not addressed explicitly within the study, the multi-physics ensemble modelling framework ESROC (Ensemble System CROCus), an extension of the snowpack model CROCUS, was used to account for uncertainties in the snow physical processes expected to drive the results of the simulations, given all other model inputs/outputs were held constant between the two comparative cases of interest (clean vs. dust-contaminated snow). To evaluate mechanical stability, a stability indicator was derived from MEPRA, a simulation tool that is used operationally in France to assess the mechanical stability of simulated snow profiles. After establishing the framework, the authors used this model-chain to thoroughly evaluate both dry snow and wet snow conditions over a variety of aspects and elevations for their numerical study site, the Thabor Massif, which was also applied to one case-study of an actual dust-on-snow event that occurred in 2021.

Strengths: Although as a reviewer I am admittedly not an expert on all the inner-workings of the models mentioned above, I found the ensemble approach to handling snowpack property uncertainty and the use of a simple stability indicator from MEPRA convincing. Perhaps more importantly, I found the results of the study interesting and of broad significance to those attempting to better understand the role that light absorbing particles (LAP's) play in snowpack metamorphism for both climate-related and avalanche forecasting applications. For instance, the conclusion that LAP's deposited on the snow surface could act to both weaken and/or strengthen its mechanical stability of a dry snowpack, or that the spring wet snow avalanche cycle might occur in advanced timeframes on southern exposures, are both areas that are of relevance to operational avalanche or hydrological forecast programs. Given that there is currently a severe lack of controlled studies on this topic in the cryospheric sciences (numerical, laboratory, or field-based), to my knowledge, I applaud the authors in tackling this topic and providing a strong basis for future work in this area of research. Last, I particularly appreciated what seemed like an innovative approach to displaying the wide variety of statistical results produced, utilizing relatively easy-to-comprehend plots, elevation/aspect rosettes, and tables to present their data.

Weaknesses: The only weaknesses I found in the study, also acknowledged by the authors in the Discussion, was 1) that their study was limited to only one simulated and one actual dust-on-snow event, 2) that ESROC cannot resolve the vertical resolution of millimeter-scale snow processes that may occur at ice-snow interfaces, such as enhanced faceting, therefore rendering the MEPRA-derived stability indicator as somewhat limited in its regard for predicting some likely cases of snow instability, and 3) that weak layers were not tracked in such detail as to be

able to be continually reassessed as additional snowfall further buried them or compared against other persistent weak layers already in the snowpack prior to the dust-on-snow event (e.g. depth hoar). This being said, given the scope of the study presented, and the abundance of data already needing to be parsed in their analysis, it seems acceptable to me that these potential weaknesses in the study only be acknowledged, which they were by the authors in the Discussion, and then perhaps left to be revisited in subsequent studies.

In conclusion, I found no major weaknesses in the study presented, and again applaud the authors on providing the snow research community with a detailed numerical study on a topic that is of both relevance and need to a broad set potential end-users. Minor edits are given below.

Sincerely,

Kevin Hammonds
Director, Subzero Research Laboratory
Assistant Professor, Civil Engineering
Montana State University, Bozeman, MT, USA

Minor Edits and Clarifications:

Page 2, Line 5: I don't see a need to use the language, "The lack of a clear message…", when this is a very current area of research for multiple research groups in the cryospheric sciences.

Page 2, Line 15: recommend replacing "dropped off" with deposited.

Page 2, Line 26: "albedo decrease in the visible and near-infrared". Isn't snow highly absorptive in the NIR at 1500nm? Such that dust impurities on the surface could actually increase the albedo? Perhaps clarify via the wavelengths that are being referred to here and elsewhere in the article…or clarify that with your model you are only using the integrated spectral albedo and over which wavelengths.

Page 3, Line 2: Grammar…recommend deleting this sentence and extending the previous sentence with something like ", such that we only focus in the present study…"

Page 3, Line 3: Replace "Up to date," with To date.

Page 4, Lines 23,24, 27: Could you please expand upon what "snow physical processes" exactly are being evaluated and/or considered relevant to the remainder of the study? As opposed to just referring to them somewhat generically?

Page 4, Line 28: Could you provide some examples of what meteorological forcing uncertainties are not accounted for? For instance, I'm curious about solar angle, diffuse vs. direct lighting conditions, localized heat advection in the Thabor Massif, etc.

Page 5, Line 8: Similar to above comment...what are the "targeted processes" exactly? Could you be more specific, where here or elsewhere these processes could be outlined more formally?

Page 5, Line 13,14: It is not clear to me why black carbon is used as an impurity in the no-dust ensemble, could you please elaborate? Is a constant background of some black carbon in the snowpack always assumed? What concentration? Why not run the clean-snow case without any impurities?

Page 5, Line 20: Do you have any ideas about modelling uncertainties on the radiative transfer scheme? Orders of magnitude or otherwise? Potential impacts of these and other uncertainties?

Page 6, Line 18: Could you please list all the snow mechanical properties that are output from MEPRA? As ooposed to "e.g., shear strength or ram resistance"…is it one or the other or both? More?

Page 7, Line 20: Delete ""we" from "member we the number"

Page 7, Line 30: Please provide a citation for 0.03 kg per m^3.

Page 10, Line 1: Delete "of" from "of of"

Page 22, Line 7: Delete "the one" from "than the one our"

Page 23, Line 1: Add "a case" to "case"

Page 23, Line 25: Add an s to "eruption", to make the plural "eruptions"

Page 23, Line 26: Delete "es" from "ashes".