

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-64

J. Ignacio López-Moreno (Referee)

Referee comment on "Elevation-dependent trends in extreme snowfall in the French Alps from 1959 to 2019" by Erwan Le Roux et al., The Cryosphere Discuss.,
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The paper analyzes changes in heavy snowfall events in the Alps for the period 1959-2019. The topic is of interest and the methodology very robust and well presented. The manuscript is well written and the figures are very easy to be understood. In my opinion the paper can be a very good contribution for TC. I do not particular criticisms to the work as is. I only have some minor comments and some curiosities that perhaps could be adressed by authors or at least discussed in a revised manuscript.

-The first comment is about the selection of the 100 yr return period, as targeted intensity of heavy snow event in this work. Perhaps authors should justify more why this recurrence (that involves very rare events) was chosen. I think that replicate some of the analysis (i.e. Figure 6) for a more frequent recurrent time (i.e. 5-10 yr) could give good information about the sensitivity of the results to the return period selection, and in case there are significant diferences the results of the paper could be more interesting for a management point of view. At least, I think this question should be discussed.

- Another question is that the assignation of existing trends to warmer climate or changes in precipitation intensity is discussed in a very qualitative way (based on some references) when this is a very interesting topic from a climate change perspective, as associated uncertainties of precipitation intensities are much larger than the ones for temperature warming. Perhaps simply presenting a map of temperature and 100 yr precipitation intensity during the snow season, and may be simple cross tabulation test could answer much of this question. The works cited about changes in extreme precipitation over the Alps do not present in most cases their respectives study periods. They should be presented as trends on this parameter may change a lot depending on the selected period, and only those covering a similar time span than this study can be used as reference.

- It would be also good to have an idea in which period of the snow season happen more frequently the very intense heavy snowfall events. The sensitivity of these events to climate change is supposed to be very different if they tend to happen in the coldest part of the snow season, or during the shoulder periods. This could be also a explanatory factor the the spatial heteregoneity shown between massifs.

- In line 25, and later in discussion, is mentioned that optimal temperature for heavy

snowfall events is slightly below 0°C. Probably in Alps may be true, associated to the humid arrival of oceanic/mediterranean air masses, but this is not generalizable worldwide (I guess that heavy snowfalls in Colorado or Hokkaido will happen well below 0°C). I would just clarify the sentence.

- In line 66 is said that the study area is typically divided in 23 massifs/units. I think this classification is typical for SAFRAN but probably not very geographical (maybe I am wrong). I would again clarify or slightly modify the sentence.

- I agree with the other review that a stronger validation of the dataset would be desirable, but I also wonder how to make it properly, as for my knowledge the best observations of snow in the region have been used to be assimilated in the SAFRAN-CROCUS. This complicates a comparison with observations. Maybe this question should be mentioned in methods or discussion section.

Hoping my comments will result useful,

Ignacio López-Moreno