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Interactive comment on "An attempt to explain recent trends in European snowfall extremes" *by* Davide Faranda

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General comments: In this study, the author investigates recent trends in yearly total snow depth and maximum snow depth in the European region, and for the latter discusses the relationship between the trend and atmospheric circulation and global warming. The reviewer agrees that the manuscript contains a lot of scientific interests to be published since the author focuses on a counterintuitive result: the increasing maximum snow-depth trend under global warming.

I thank the reviewer for the positive comment. Besides the "counterintuitive result obtained for the Balkan region", I would like to stress that there are also

several regions for which snowfall extreme trends are negative. I believe that also for those regions it is important to explain trends in light of the atmospheric circulation, or attribute them to a thermodynamic feedback. I hope that the new version of the manuscript will be clearer for the reviewer and therefore for the readership of WCD.

The author tried to understand the relationship between the result and change of atmospheric circulation. However, the relationship or causality would be not fully discussed to be published in this manuscript, and in the current status it seems not suitable to the scope of the WCD journal, because the current manuscript contains less investigation on the atmospheric dynamics that causes trends of yearly total and maximum snow-depth. Therefore, I would recommend to resubmit this paper after substantial revision for discussion on the atmospheric dynamics, or the author may address to more elaborate on an observational study such as the comparison with in-situ observations and the ERA5 reanalysis datasets.

I understand the criticism raised by the referee namely that the manuscript should identify more robust links between the trends and the atmospheric circulation. In the new version of the paper, I include an additional analysis based on the ERA5 dataset decomposition of snowfall into two components: snowfall from large scale precipitation (lsf) and convective snowfall (csf). The use of the two components enable to attribute the changes in the snowfall for the two periods either to large scale flow dynamical changes (and therefore to the anomalies of Z500 fields) or to the thermodynamic changes. Figures 1,2 provide an overview of the climatology of the three components of the snowfall. Following a suggestion of reviewer 2, I have substituted trends with differences between the two subperiods. Indeed, when separating positive and negative trends (see Figure 3), we get that the majority of changes largely due to the large

scale snowfall and therefore can be attributed to the extratropical cyclones. The convective snowfall (thermodynamic component) is generally smaller, but convective events can perturb trends in the total snowfall, as their distribution is highly non-Gaussian.

Specific comments: The conclusions described in Abstract and Conclusions seem not be supported by the results in Sections 2-4. It looks to me only the result that support the conclusion is "This suggests a non-trivial relation between the occurrence of extreme snowfalls, global mean warming and the internal, long-term variability of the atmospheric circulation" (L136-137). Discussions about atmospheric circulations are too few and thus it is difficult to conclude that "the subtle effects of atmospheric circulation in driving extreme events and the non-trivial relation with global warming: a warmer Mediterranean Sea may enhance convective precipitation in winter-time and trigger heavy snowfalls" (L7-9). At least, there is no figures and discussions on specific humidity, climatological temperature that can determine whether snowfall or rainfall, and sea surface temperature and its related surface fluxes (latent and sensible) on Mediterranean Sea.

The reviewer stresses that there is not enough discussion on the relation between extreme snowfalls and large scale atmospheric circulation VS local convection. I agree that on the first version of the manuscript I have relied on the results of D'Errico et al (2020) to claim that the positive trends on the Mediterranean basin were caused by convective events. I do however agree with the reviewer that more evidence should be provided. For this reason, in the new version of the paper, I will include the analysis on both large scale snowfall and convective snowfall. The new analysis included shows that the trends are largely due to large scale snowfall and not to convective snowfall. The paper will be updated in this direction.

Also, the reviewer is dissatisfied that the ambiguity of which scale of the atmospheric circulations the author focused on: the synoptic scale or the low-frequency variability? This point was difficult to be understood in Introduction and Section 4.

I focus on the synoptic scale as opposed to the convective scales. The reviewer would however agree with me that the synoptic scale is not unrelated to the low frequency variability mechanisms: in particular, as shown by D'Errico et al. (2020) there are subseasonal conditions that can favor or not the occurrence of cold and snowy waves over Europe: the presence of stratospheric warming, the presence of an important snow cover on Siberia, the presence of large scale blocking structures. Furthermore, a recent study (Mori et al. 2019 Nature Climate change https://www.nature.com/articles/s41558-018-0379-3) has shown that also the snow cover and the presence of ice in the Arctic region plays an important role. I am aware that this point was not clear in the previous version of the manuscript, in the new one I will be more clear by stating that three different scales (convective, synoptic and sub-seasonal) are relevant for the occurrence of heavy snowfalls and clarify the aspects of the study. In particular, the additional analysis of convective VS large scale snowfall enables to discuss both the aspects.

Another concern is that the author compared the daily composite fields of the period 1979-1998 with those of 1999-2018 (Figs. 6-9). If my understanding is correct, this comparison is the average of 20 daily fields versus that of 20 fields. It seems to me that the number of composite fields is not enough to discuss the daily atmospheric fields, since the daily fields can emphasize synoptic disturbances such as locations of extratropical cyclones. Thus we may need more larger number of daily fields to be composited, or focus on longer timescale fields for low-frequency variability (e.g., Nakamura et al. 1997). (Nakamura et al. 1997: "The Role of High- and Low-Frequency

C2 Dynamics in Blocking Formation ", Monthly Weather Review)

The problem of having small samples due to poor quality of snowfall data was already acknowledged in the first version of the manuscript. However, following the suggestion of the reviewer, we can extend the statistics focusing on a 3 day window centered on the event analysed. 3 day seems like a fair choice because this is the typical time scale of development of extratropical cyclones. This allows us to get more robust statistics on the atmospheric circulation features.

In addition, there would be less discussion on the relationship between atmospheric circulation and global warming. For example, could you compare the increasing/decreasing snow-depth trends with estimation of the Clausius-Clapeyron relationship?

I believe that the analysis of the convective VS large scale snowfall provided in the paper answers the question raised by the referee.Since no significant trends are present in convective snowfall (see additional figures 1-3) there is no significant thermodynamic effect that could be related to global warming. The trends are largely due to large scale structures.

Instead, the author could focus on the observational part. I am not familiar with observation research, yet it would be valuable and novel to compare the ERA5 snow estimations with observations. It will provide useful information for reanalyses that are crucially important for weather and climate researches.

I am afraid this time I have to disagree with the reviewer. I have already used the EOBS dataset (regridded observations) and compared the trends with those

provided by ERA5 in the first version of the manuscript.

It would be helpful to refer to Kawase et al. (2016) who investigated future changes of averaged (yearly total) and extreme (maximum) snowfall events over Japan (East Asian regions), and their results seem partly consistent with your results here. Also you can find Steenburgh and Nakai (2020) for some reviews of snowfall over Japan. (Kawase et al. 2016: Enhancement of heavy daily snowfall in central Japan due to global warming as projected by large ensemble of regional climate simulations, Climatic Change. Steenburgh and Nakai 2020: Perspectives on sea- and lake-effect precipitation from Japan's “Gosetsu Chitai”, Bulletin of the American Meteorological Society)

I thank the reviewer for this inspiring literature, which will be integrated in the new version of the manuscript

Technical corrections:

-L143: What is the "ERA5 data per NUTS2"? Please describe.

-L153: "hep" => "help".

- What is "the block-maxima procedure"? Please explain.

All the minor points will be fully addressed in the new version of the manuscript

Detailed caption of additional Figures of this review

- **Figure 1: Climatology of the components of snowfall for the NUTS2 regions. a-b) total snowfall, c-d) snowfall from large scale precipitations (lsf), e-f) snowfall from convective precipitation. a,c,e) average of yearly accumulated snowfall, b,d,f) average of yearly maxima .The data are expressed in units of cm.**

- **Figure 2: Differences of the averages for the (1999-2018) and those for the (1979-1998) periods for the NUTS2 regions: a-b) total snowfall, c-d) snowfall from large scale precipitations (lsf), e-f) snowfall from. a,c,e) average of yearly accumulated snowfall, b,d,f) average of yearly maxima .The data are expressed in units of cm.**

- **Figure 3: Convective and large scale snowfall contributions to the differences observed in total snowfall divided by sign of the total snowfall differences: a) positive differences in the mean sf, b) negative differences in the mean sf, c) positive differences in the max sf, d) negative differences in the max sf.**

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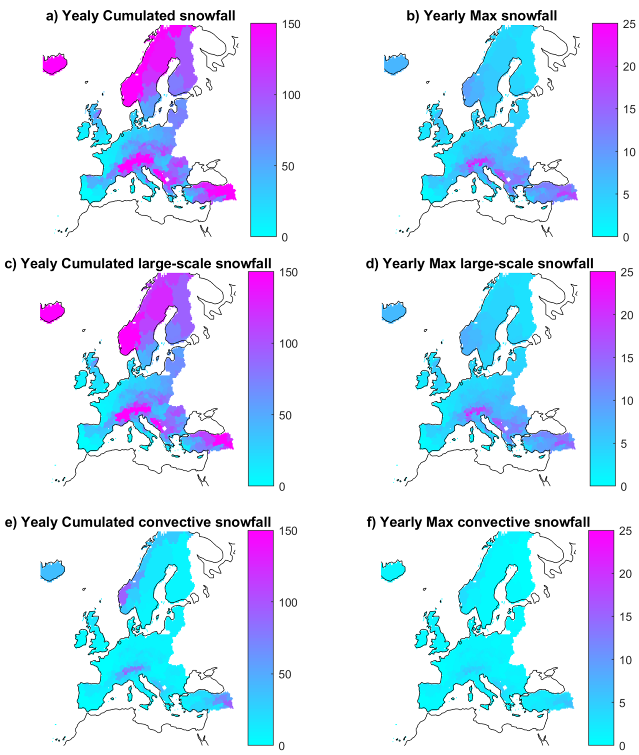


Fig. 1. Climatology of the components of snowfall for the NUTS2 regions.

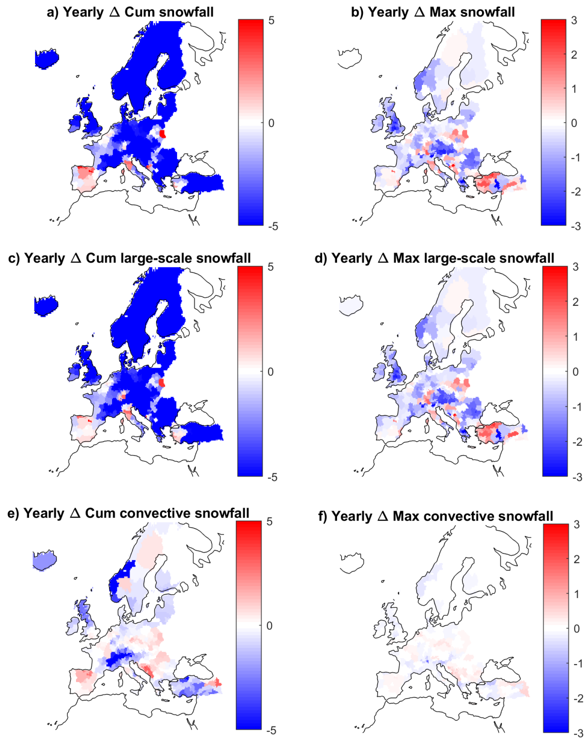
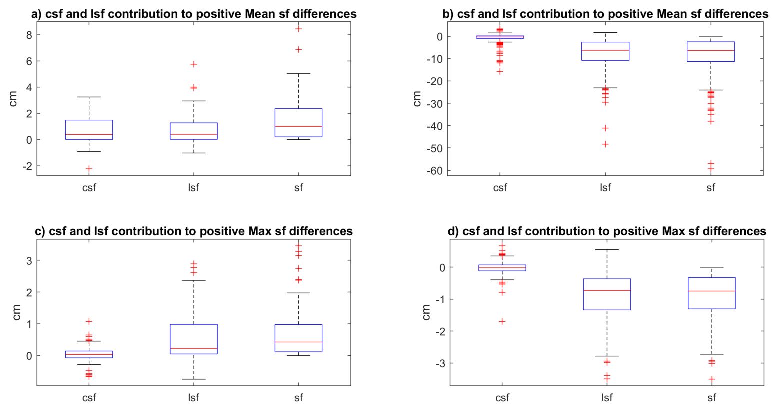


Fig. 2. Differences of the averages for the (1999-2018) and those for the (1979-1998) periods for the NUTS2 regions



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