Dear Editor,

I have revised the article following the editorial suggestions provided and I have therefore greatly modified the article as well as the answers provided to the reviewers. In particular, as suggested by the editorial board of WCD, I have dropped the analysis of large scale versus convective snowfall. This deep revision of the article is articulated on the following different axes:

1) **More tests to check consistency between EOBS and ERA5 datasets**
2) **The analysis of trends at the level of NUTS0 (countries) as suggested by one of the reviewer**
3) **A more careful analysis of thermodynamic and dynamic factors that can explain positive trends, on the line of editorial requests. For the thermodynamics I now use CAPE and t2m fields. For the dynamics I perform a sea-level-pressure analysis as well as a weather regimes analysis and an analogs search.**
4) **Extension of the bibliography to support the claims made in the manuscript.**

I provide below a detailed answer to both editorial and referees comments and I invite the referees to base their future comments on the answers provided hereby and not on those posted on the interactive discussion. I hope that this new version of the manuscript will be suitable for publication in WCD.

Best Regards,
Davide Faranda

<u>ANSWERS TO EDITORIAL COMMENTS</u>

While I agree that your study and the documentation of snowfall trends is an important topic and in the scope of WCD, I concur with both reviewers, that the dynamical interpretation is not elaborate enough in the current version to explain the observed trend. Having also consulted the editors of the journal this is a major issue which must be addressed prior to publication in WCD. Therefore a more thorough investigation of the drivers for the observed trends is needed.

I wish to acknowledge the editorial board for finding the time to provide an additional review of the paper and I have undertaken the steps suggested by the board to improve the manuscript.

The reviewers give valuable input in this direction. In particular, the example of how ambiguous trends in Japanese snowfall can be explained by the the JPCZ in Kawase et al. 2016 (Section 4) is an excellent example of a potential dynamical interpretation in their case. Evidence in that direction should be presented; although I see the difficulty in finding common drivers for the various focus regions you investigated compared to them.

Following the suggestion of the editorial board, I have performed both a weather regime analysis and analogs search of the atmospheric circulation patterns associated with heavy snowfalls. The detection of cyclonic and anticyclonic structures is now based on the sea-level pressure, instead of the geopotential height. Absolute and anomalies fields are commented, as suggested by reviewer 2. The dynamical analysis suggests that there is a prevalence of Atlantic Ridge patterns associated with the extreme events with a tendency to increase in recent times. The new section 4.2 (and Figures 9-11) is entirely dedicated to the connection between heavy snowfalls and weather regimes.

We are afraid that your suggestion to investigate convective vs. large-scale snowfall from ERA-5 will not solve the problem. This separation highly depends on the grid spacing in the model and can therefore not be a proper indicator of a physical separation into thermodynamic and dynamic processes. Convective snowfall simply shows the fraction which is processed in the physical schemes vs. dynamical core (or resolved vs. unresolved scales) and therefore not necessarily attributable to actual convection.

Indeed, I agree with the editorial board on this point. I have also consulted our modellists and they agree that the separation is somehow artificial and not attributable to actual convection. In the new version of the manuscript the role of convection has been assessed using convective available potential energy (CAPE).

It is striking that most of your positive trend locations are along Mediterranean coasts and it is likely that the large-scale circulation is involved. I would therefore suggest that you directly show the synoptic environment during these snowfall events. E.g. parameters indicating stability, flow direction towards the coast and local orography. This would yield a more dynamical interpretation, e.g. that starker sea surface - troposphere temperature contrasts might enhance moisture uptake combined with reduced stability aloft, change ascent, local stability, and local convergence during snowfall events. The body of literature looking at cold-air outbreaks, air-sea interaction, and baroclinicity might give additional guidance here (e.g. Czaja et al. 2019, Papritz and Spengler 2015, 2016).

As suggested both by the editor and by reviewer #2, the new version of the manuscript focuses on positive trends and at the scales of countries. I have identified four countries with positive trends to perform the analyses: Albania, Macedonia, Switzerland and Turkey. Instead of looking at convective snowfall, the role of convection is now analysed through the CAPE as suggested in the existing literature. CAPE analysis shows that there is indeed an increase of convective instability for the second period and the high CAPE values are presented over the Mediterranean Sea, thus I cannot exclude that thermodynamic factor plays along the dynamic factors to enhance snowfalls.

The role of blocking remains obscure although the abstract suggests you would look into this dynamical aspect in more detail (I see that this is ambiguous terminology with "block-maxima" referring to snow depth, but WCD readers might relate this to blocking anticyclones). Apart from this Eulerian investigation a Lagrangian investigation of air mass origin (location and changes of physical properties along trajectories) contrasting events in the early and late decades would also give dynamical insight. Another thought is that these trends might be driven by decadal variability, an aspect that could be discussed more comprehensively (the 2000s were more NAOcompared to the 1990s, e.g. Weisheimer et al. 2017).

Following the suggestion, I have better investigated the role of meridional patterns in the variability of extreme snowfalls. For the four countries analysed, there is a tendency to an increase of Atlantic Ridge patterns during extreme events, although the tendency in the analogs of those days is weak and non-significant. I hope that the new analysis in Figures 9-11 is more convincing and supporting than the one presented in the previous version of the manuscript.

Finally I outline a thought that has arisen from the discussion amongst the editors. The thermodynamic argument that a warmer Mediterranean Sea may lead to larger snowfall amounts at some places in Europe requires caution and might be flawed. The reason for this is that snowfall extremes (at least in the midlatitudes) tend to occur at or near the freezing point in both colder and warmer climates (see e.g. O'Gorman et al. 2014). Also the results in the present paper indicate that locally (at the location of the snowfall) the temperature difference between the cold and warm period is small (Fig. 7, although this is a bit hard to see). Now, it is this local temperature that determines the maximum atmospheric moisture content and thus the "thermodynamic" component of the snowfall amount. If the Mediterranean Sea warms a lot, enhancing evaporation there, but the temperature at the location of the snowfall stays the same, then all the excess moisture precipitates out during the transport to this location, and there is no substantial "thermodynamic" enhancement of the snowfall. So the question arises if there are more snowfall events rather than more intense events and/or if dynamics enhance the events rather than thermodynamics, again pointing towards variability of the large-scale circulation between the considered periods.

The arguments raised by the editorial board about the role of the thermodynamic vs dynamical components of changes are interesting and I have tried to address them in the new version of the manuscript. I do agree that atmospheric circulation plays an important role in the trends, as shown by the prevalence of Atlantic Ridge patterns evidenced by the analyses in Figure 10, but I am still convinced that there is an interplay of circulation and thermodynamic factors to explain the observed trends: the analysis of CAPE shows that large values of this quantity are associated with heavy snowfalls in the selected countries. CAPE values of 70 JKg-1 are enough to trigger convection during winter time and enhance snowfall precipitations (Olsson et al. 2017). For all countries analysed, the isobars associated with the cyclonic conditions embedded in Atlantic ridge patterns point to winds blowing from sea to land, thus favoring the advection of moisture and the

formation of convective precipitation. In addition, the four countries analysed are characterised by mountain ranges that, in presence of sea-to-land flow, favors Stau effects. Both thermodynamics and dynamics effects seem therefore to contribute to observed trends, although it is difficult to understand which factor prevails.

ANSWERS TO REVIEWER 1

Comments on " An attempt to explain recent trends in European snowfall extremes" by Dr. Davide Faranda submitted to WCD.

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. In the new version of the manuscript have provided more insights on the thermodynamic and dynamical factors that could cause such trends

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 have performed both a weather regime analysis and analogs search of the atmospheric circulation patterns associated with heavy snowfalls. This analysis is useful to show that there is a prevalence of Atlantic Ridge patterns associated with the extreme events with a tendency to increase in recent times. The new section 4.2 (and Figures 9-11) is entirely dedicated to the connection between heavy snowfalls and weather regimes.

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 in 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 include the analysis of the stability via the convective available potential energy. This analysis shows indeed that, for the countries showing positive trends, the instability increases in the second period, thus supporting the claim that thermodynamics play a role in snowfall changes.

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.

In the new version of the manuscript, it is made clear that this study focuses on synoptic scales. In the introduction I have added (L30-L32): "The goal of this paper is to shed a light on recent changes in the dynamics of extreme snowfalls, by projecting the recent changes in frequency/intensity of extreme snowfalls on the large scale (synoptic) dynamical drivers and identifying possible small scale convective thermodynamic feedback." The role of low frequency variability is discussed in the conclusions (L300-306): "There are sub-seasonal to seasonal conditions that can trigger snowy waves over Europe by modifying winter atmospheric circulation patterns: the role of stratospheric warming, the magnitude of snow cover on Siberia and in the Arctic region could be taken into account in future research on this topic, e.g. by following the approaches of ~\citet{handorf2015impacts,handorf2017arctic} and \citet{mori2019reconciled}. At smaller scales, where convection is important, further studies could be based on searching the origin, transport pathways, and thermodynamic evolution of air masses involved in heavy snowfall episodes, via novel methodologies based on tracking trajectories of air masses as those introduced in ~\citet{papritz2017lagrangian}, and by using convection permitting models to study sea-air-snow interactions~\citep{bartolini2019convection}."

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, I have performed an analogs analysis, to detect trends in the patterns associated with the occurrence of heavy snowfalls (typically Atlantic ridge). As analogs, I select the 5% closest patterns to the average sea-level pressure patterns identified, for the two periods, during snow events. Note that the results do not depend on the threshold used for the selection in the range 0.25% to 5%. This analysis shows that, besides a significant time decreasing trend for analogs of the typical situations leading to snowfall in Switzerland, there are no long term trends in analogs for the other countries examined.

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 have better explored the possibility of using the Clausius Clapeyron relationship but I have not found, in the literature, a common agreement on the possibility of applying it locally in space and time, as it would be needed to study snowfall extremes. For example, PA O'Gorman, CJ Muller - Environmental Research Letters, 2010 find deviations from the relation that can be both attributed to physics or to model capabilities. I would avoid going into this slippery argument. However, if the reviewer has a convincing argument and a practical suggestion to undertake this analysis, I would be happy to change my mind.

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 but 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. Following the approach of Kawase et al. 2016, I have performed both a weather regime analysis and analogs search of the atmospheric circulation patterns associated with heavy snowfalls. This analysis is useful to show that there is a prevalence of Atlantic Ridge patterns associated with extreme snowfall events.

Technical corrections:

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

I have added: "We show results at two different levels, regional (NUTS2) and national (NUSTS0). These subdivisions are commonly used by stake-holders to assess impacts of climate variables on economy and society and are the reference adopted by Copernicus for its products (see, e.g.~\citep{brandmueller2017territorial})"

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

corrected

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

The reference to block maxima has been dropped as it was confusing.

<u>ANSWERS TO REVIEWER 2</u>

###General comments:
The article investigates extreme snow depth trends in Europe in the last 40 years and attempts to explain these trends in light of global warming and changes in atmospheric circulation. I find the topic interesting and definitively of scientific interest for WCD.

I thank the reviewer for the positive comment. In the new version of the manuscript, I will take into full account the comments raised by the reviewer to improve the presentation of the paper.

1) I'm puzzled by the data. I'm not familiar with ERA5 and E-OBS but reading the data section, it seemed to me that the author actually analyze SWE, not snow depth. It may only be a vocabulary issue.

Indeed I am using snowfall (sf) data and not snow depth. I have corrected this issue through the paper.

2) Figure 5 shows that applying a linear regression to annual maxima is not robust since it may be much influenced by 1-2 largest points. Therefore 2 subperiods are considered in Figures 6 to 9, which I support. But then wouldn't it be more consistent to consider in Figures 2-3-4 differences between the two subperiods rather than linear trends? This is not anecdotal since the regions with largest increase/decrease might partly change (e.g. ITF1). A t-test, e.g., could be applied to test differences in means.

As suggested by the reviewer, the new version of the paper contains differences between the two sub periods (with a T-test for significance) instead of the linear trends computation.

Note that another way to get more robust trends in annual maxima is to fit a nonstationary GEV distribution but it may be unnecessarily complicated here.

I have tried the GEV fitting but the results show a very large sensitivity to data. This means that by considering 21 or 19 years, the estimation of the parameters largely fluctuates, whereas it is more stable when considering differences between sub-periods

3) I find the idea of comparing atmospheric fields during extreme events excellent . However I'm puzzled by several interpretations (see below) and I'm not sure that the conclusions are supported by the analysis. First I'd like to see the average Z500 fields for period 2 because I don't think one can interpret anomalies without the mean field (or at least I'm not able to). In Figure 6 the author shows that decreasing trends are mainly associated with negative anomalies over eastern Europe. I see the correlation but is this causality? In particular if one considers a neighboring region with positive trends, don't we have the same pattern (i.e. negative anomalies over EE)? Idem for the positive trends.

Thank you for the encouraging comment. In order to progress in the interpretation of the results, in the new version of the manuscript I have largely extended the analysis. First of all, I moved from Z500 to the sea-level pressure fields to track the circulation. This choice is motivated by the fact that Z500 presents a thermodynamic trend superimposed to the information about the circulation (Jézéquel, Aglaé, et al. "Trends of atmospheric

circulation during singular hot days in Europe." ***Environmental Research Letters*** **13.5 (2018): 054007.). As suggested I now plot both the absolute fields as well as the anomalies, so the patterns are more recognizable and directions of the atmospheric flow can be justified. This analysis (L227-240) allows us to identify cyclonic structures such as the Genoa Low or the Cyprus Low associated with extreme snowfalls. The anomalies show an enhancement of those patterns, possibly enhancing extreme snowfalls.**

4) More generally, looking at the quite noisy map of Figure 4d), is there good hope to be able to explain trends from atmospheric circulation? For example in Italy I can see quite positive, null and negative trends within a few km of a quite flat region. I expect all these regions to be influenced by the same atmospheric circulation, therefore differences in trends are either due to regional characteristics or this is merely rainfall variability (or data issues). Please consider analyzing larger regions to be able interpret smoother maps.

Following the suggestion of the reviewer, in the new version of the paper, I also use the NUTS0 level (country) to investigate trends. NUTS-0 is the scale of European countries. This scale allows us to identify that the increase in maximum snowfall is a stable feature in the Balkans. Four countries showing positive trends are then analysed in more detail than in the previous version, focusing on both thermodynamics and dynamics aspects. For thermodynamics, I analyse atmospheric stability via convective available potential energy (CAPE). This analysis shows an increase of instability in the second period with values that, in previous studies (Olsson et al. 2017) have been identified as sufficient to trigger snowfall convective precipitations. Furthermore, I perform both a weather regime analysis and analogs search of the atmospheric circulation patterns associated with heavy snowfalls. This analysis is useful to show that there is a prevalence of Atlantic Ridge patterns associated with the extreme events with a tendency to increase in recent times. The new section 4.2 (and Figures 9-11) is entirely dedicated to the connection between heavy snowfalls and weather regimes. The combination of these analyses show that there is an interplay between dynamical and thermodynamic factors in determining the changes in snowfall maxima.

###Specific comments

L5: “coherent with the mean global warming and previous findings”: I’m not sure to understand to which of your results you refer to here.

I will rephrase this sentence as: “is coherent with previous findings and caused by global warming”

L6: “discrepancy between trends in average and maximum SD”: to investigate this, wouldn’t be interesting to look at the regions with the largest discrepancies between means and extremes? Introduction: please consider referring to Beniston et al. 2018, The European mountain cryosphere: a review of its current state, trends, and future challenges, which gives a good overview of changes in the European mountain crysophere.

Thank you for the suggestion. Indeed I analyse, on one hand, two countries in the Balkans because they have positive or zero trends also for total yearly SF and, on the other hand, Switzerland and Turkey because they show opposite trends between mean and extremes (the largest discrepancy). Thank you for the additional reference that will be added to the new version of the paper.

L95: “large SD amounts correspond to snow to be removed“: I’m not sure about that. The weight of the snow (SWE) is much more important than the depth.

As answered in comment 1, there was a problem in naming the variable used in the first version of the manuscript. SD will be changed to “snowfall” and references to SD will be dropped.

L 100: “total amount of water”: does ER5 really give you a total amount of water? Then this would be a SWE (mm of water), not a depth. Or do you mean “total snow depth”?

Again, thanks for pointing out the problems with this variable. In the new version of the manuscript it will be changed to snowfall.

L 108: “from daily total precipitation”: Idem I don’t understand how you get snow depth from water amount.

Also here I will change the description to point out that from total daily precipitation we can get a proxy of snowfall (and not snow depth) variables.

L113: where does this 2/3” coefficient come from? Figures 2-3-4; please consider exchanging colors since later on red=decrease, blue=increase. Please consider merging Figures 2 and 3 (e.g. by crossing out the significant regions)

The coefficient 2/3 has been dropped in the new version of the paper, which also uses the newest EOBS dataset. I prefer to keep red and blue in Figure 2 and 3 .

Figure 4: are you sure these are NUTS-2 regions? It seems to me they are much larger. L 146-147 “Indeed . . . trends” : actually this was also the case in Fig 3a)

According to wikipedia, the NUTS-2 regions used in the paper are correct:
https://en.wikipedia.org/wiki/Nomenclature_of_Territorial_Units_for_Statistics

The level of “department” or “province” is NUTS-3 while the level of states is NUTS-0. NUTS are now better referenced.

L 164 “due to the two outliers” : I guess these two outliers occurred at the end of the period

That is right and it is specified this in the text of the next version

Figure 6: please consider showing the average field of period 2. Also the windows are much too large. Please consider showing smaller windows centered on the considered locations.

I have followed the suggestion of the reviewer and show absolute fields, together with the anomalies. Focus on countries, instead of regions, improves the visualization of changes. A more focused window on the interested countries has been selected.

L 178 “weaker cyclonic structure” : I understand that geopotential heights are higher (positive anomalies) but don’t you need the mean field to interpret it as a “weaker cyclonic structure”? L 179 “an anti-zonal of a blocked pattern”: I’m not an expert in atmospheric circulation but I don’t understand where you see that Figure 8: is the scale the same for all panels? L 182 “the surrounding . . . events”: is tis particular to CZ03? Actually I see that in all panels. L 187 “negative SD anomalies . . . viceversa”: I don’t see that (or I don’t understand) L 195 “tend to suggest a stronger meridional flux” : I don’t understand this interpretation L 196 “deeper cyclones” : I don’t understand why negative anomalies imply deeper cyclones. L 220: “we observe more anticyclonic conditions” : where do you show that? I’m not sure that this kind of conclusion can be drawn from a few events.

All this part will be completely rewritten. First of all geopotential height has been substituted by sea-level pressure, which allows to visualize better cyclonic structures (that are indeed present). All of these comments are therefore answered in the new version of the paper

###Technical corrections:
L 63: Luthi et al: commas
L 101: “higher” → larger
L 120: “tend coincide”
L 143: “NUTS2” is “NUTS-2” above
L 153: “could hep”
L 155 Altman: commas
Figure 5: NUTs2. Also I guess a) is positive and b) is negative
L167: “atmospheric” → meteo? L 182 “positive anomalies” → negative?
L 183 “positive SD anomalies” → negative?
L 193: CH5 → CH05

All the technical corrections will be taken into account