Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2020-61-AC2, 2021

Earth System Dynamics
Discussions
Open Access

Interactive comment on "A dynamical and thermodynamic mechanism to explain heavy snowfalls in current and future climate over Italy during cold spells" *by* Miriam D'Errico et al.

Miriam D'Errico et al.

miriam.derrico@yahoo.fr

Received and published: 18 January 2021

Anonymous Referee #2

The paper discusses the topic of heavy and extreme snowfall in Italy in current and future climate. This is a scientifically interesting and societally relevant subject. The starting point of the analysis is a set of 32 extreme historic cases with significant snowfall in at least one of two Italian cities of Bologna and Campobasso. The authors go at length in describing these cases (in the Appendix), which vary from relatively short outbursts, to long-lasting episodes involving cold spells in large parts of Europe. This is followed by an analysis of snowfall under similar circulation types, occurring in 500 year

Printer-friendly version

Discussion paper

simulations conducted with an intermediate complexity model (PlaSim). It is concluded that extreme snowfall may increase or decrease, depending on whether or not future climate change will express in more than average warming of the Mediterranean. The paper provides an interesting set of observed cases, along with some interesting analysis of simulations in a coarse resolution intermediate complexity model. However, as the paper is presently formulated, it lacks to provide a convincing story that connects the two. There are moreover serious shortcomings in the current description and presentation of the results, which I will try to motive in more detail below. Based on this, however, I recommend to reject the paper in its current form.

We understand the extended criticism expressed by the referee and we are willing to take all necessary steps to provide a coherent presentation of our results. This includes, as also suggested by reviewer #1: i) refocusing the results obtained with PlaSim on the role of atmospheric circulation and on the abundance of patterns simulated with future conditions, ii) better underlying the strong limitations in the representation of thermodynamics of these events in PlaSim. A detailed answer to the specific comments is provided below.

Major remarks 1.

Event definition. In the Introduction the authors argue, that while there is general consensus that temperature is increasing and mean snowfall is decreasing, knowledge of the changes of extreme "snowy" cold spells is inconclusive, because of inconsistencies in their respective definitions. From this statement I had anticipated that the paper would start with such a definition. However, it is absent. Instead the authors implicitly "define" the case by means of the observed large-scale circulation that accompanied the (start of the) events. Despite the circulations being "very similar" as the authors write on p5 L140, there is apparently enough variation to allow the huge differences in the observed snowfall amount (Fig3). The correlation figures, though only briefly described, also seem to hint in this direction (rather low correlations).

In the new version of the manuscript we will provide a clear statement on the definition of the events, namely that we consider documented events which have produced at least a record of minimum temperatures and/or a record snowfall amount (or snow at locations where snowfall has never been observed before) at one or more locations in Italy. Then we have not considered the starting day of the cold/snowy spells, but rather the day where most of the records are documented as the central day of the cold/snow spell for the search of analogues. We will also remove figure 8 and just comment on the value of the autocorrelation function at lag0. As observed by the referee, we will underline that , despite the similarity in the large scale patterns of SLP and Z500, there is a large variability of the position of local pressure minima. Due to the complex geography of Italy, small changes in the position of the cyclonic minima can drive precipitations on the Adriatic, or the Thyrrenian sides of the peninsula thus explaining the different recorded amounts. This also suggests, as stated by the referee, to center our results on the occurrence of large scale circulation patterns associated with these events.

Snowfall/depth in intermediate complexity models. The way in which the study attempts to address its main question, involves the use of an intermediate complexity model. While there is nothing wrong with using such intermediate complexity models, it can be questioned whether they are suitable for the problem at hand. Cold spells, especially when defined with respect to a fixed temperature, and in particularly snowfall, will depend sensitively on a lot of parameters, microphysics, precipitation, the representation of the underlying orography and much more. Since for snowfall to occur, the temperature has to be around freezing point, biases in temperature will all too easily imply biases in snowfall. To the knowledge of this reviewer, intermediate complexity models are relevant to the real world mostly because of their reasonably well resolved "dynamics", not so much because of the details of their resolved thermodynamics / surface parameters / precipitation, let alone snowfall. As a consequence, I think the results in this paper should be treated with extreme care, and can basically only be interpreted within the limited validity of the intermediate complexity model itself, and not as a direct proxy of what may happen in the real world at a local scale, such as, in this case, in Italy.

We will completely rephrase the manuscript and present our approach as a possible way to detect compound extreme events and then analyse the role of atmospheric circulation in their occurrence via a simple model capable of producing several hundred thousands of large scale circulation patterns in relatively short time. We will take particular care in clearly stating the thermodynamic limitations of our study.

Reanalysis. The principal source of reanalysis data is well known for its shortcomings, of especially its surface variables. Some reasons are given in https://journals.ametsoc.org/bams/article/77/3/437/55258/The-NCEP-NCAR40-Year-Reanalysis-Project. As such it is questioned whether the snowfall, t2m temperature and consequently snow depth are variables that can be meaningfully used. Upper-level air temperature, and Z500, as well as possibly mean sea-level pressure can be safely used.

In the new version of the manuscript, as suggested by the reviewer, the focus will mostly be on the large scale circulation patterns associated with the identified extreme events. We will therefore carefully report results on the physical variables (especially snowfall) associated with the events detected.

Unrealistic SST+4K simulation Three different simulations are carried out with PlaSim. In one of them the global SST is increased uniformly by 4 degrees. By not changing atmospheric forcing, this leads to an unrealistic situation. The situation of lakeside snow effects might be an important aspect of snowfall changes in the future, but it is likely that some sort of compensating effect occurs in reality. As a consequence, the statements in this paper are likely over-confident. Without doubt there is a role for both circulation and thermodynamic processes. It is worthwhile to lookup some recent literature by e.g. O'Gorman on this subject.

The +4K SST is an idealized simulation, taken from AMIP runs, that we use to push to an extreme set-up in order to observe clear thermodynamic changes in PlaSim, mostly to study the possible lake effect snow in the Mediterranean. We will stress furthermore that this simulation is just used to understand the possible thermodynamic feedback of warmer Mediterranean sea during events whose atmospheric circulation matches cold and snowy spells. The comments of both the referees made us rethink to the conclusions that can be drawn from this analysis: the fact that the convection potential is enhanced with warmer seas can i) produce snowfalls in some cases where the temperatures remain below the melting threshold ii) transform the snowy events in events where large amounts of convective precipitation falls on the ground in liquid or mixed phase, with important consequences for hydrology and winter tourism. iii) Clearly states that snowfall can disappear in cold spells in RCP8.5 scenarios.

Statistical significance. The study starts with a description of the 32 cases (or in fact the description is only given in the appendix). Reading through this interesting and expansive list I get the conclusion that there is a substantial difference between the historic cases, both in scale, in duration, in extremity, etc. As exemplified by Fig3 the variance in local snowfall accompanying these events is huge. Despite this variance, the authors state that the underlying T850/SLP or Z500/SLP fields are quite similar. Why then, do the users restrict themselves to use only 32 cases from the simulations? To me this is unclear. It basically means that for every historic event, only the closest C3 single model event is selected, whereas already from the observations it becomes clear that there is a huge variability within these cases. In other words, there must be many similar circulations where no snowfall occurs. I could imagine that more robust (model) results could be obtained by considering a larger subset of similar circulation types.

We definitely agree with the reviewer that there are other possible ways to define

the analogues. For example, one could take the N closest fields for each event having 32xN events in the database. However, our choice is motivated by the will to have, in the simulations, exactly the same statistical sample as in the reanalysis and being able to directly compare the statistics obtained in the different datasets.

Given my comments above, it is my feeling that the paper could benefit from a radical change of viewpoint. By letting the simulations of the model of intermediate complexity form the heart of the paper, and providing context from observed cases in an added discussion, the claims could be made more specific to what is achievable with such a model. For example, how do cold spells change in such a model, and can these be used to examine extreme snowfall. Because you run a simple model, you can afford to run as many long simulations as are required to achieve at least significant results with respect to the circulation changes. The thermodynamic changes will be hard given the limitations of the model, but perhaps some knowledge can be squeezed out, if results are considered at larger spatial scales. I do not think PlaSim can be reasonably expected to give realistic results at local scale.

Following the suggestion of the reviewers, we will completely reorganize the manuscript as suggested. First we will give more space to the reconstruction of the 32 events, which was one of the major challenges of this study. Then, we will specify the use of intermediate complexity climate simulations as a tool to study the atmospheric circulation associated with the detected extreme events, through the use of analogues. Finally, the +4K simulation will be exploited only as an additional tool to understand the possible role of a warmer mediterranean sea in determining a thermodynamic feedback to the events. We will underline the limitations and suggest our approach (documentary sources + analogues search) as a way to investigate compound extremes.

Minor remarks:

Note that I will not comment on all minor textual and graphical aspects, since I believe the paper should first be rewritten. The other reviewer has already commented on some of the figures.

1. On page 4, it is stated that five sigma levels are used. However, on page 3, the model is introduced with ten vertical levels.

We thank the reviewer and the sentence "the vertical, five non-equally spaced sigma (pressure divided by surface pressure) levels are used" will be deleted from the manuscript.

2. Figures 1,2,4 can be left out. Graphics of the snowfall panels in Figure 5-7 should be improved. Currently, they make a rather unconvincing case of why you would analyze the snowdepth in central Italy.

We will remove Figure 2 and 1. Figure 4 will be replaced by the boxplots shown in Figure A1 at the end of this review where boxplots of the spatial average over Italy of SLP (a), T850hPa (b), Geopotential Height HGT (c) and Snow depth (d) for all winter days (grey) and for the analogues of cold spells (blue), are presented. The boxplots make a convincing case that the analysis over Italy makes sense.

3. Figure 8. It is unclear over which domain the correlations are computed. Furthermore, it seems totally irrelevant to consider a lag running up to +/- two (!) months. A pointwise correlation between 0.2 and 0.3 in the observations suggest to me that there are huge differences between the fields. If anything, the larger correlations in the PlaSim C4 simulations suggest that the simpler model is not at all able to capture the variability as observed. *

We remove Figure 8 as suggested by the reviewer. We will rephrase the sentence to account for the large variability.

4. Figure 9. These are already more meaningly lags, but here the significance of the results are questioned. Furthermore, it is not clear whether deviations from REF

climatology are used, or from each simulations' own climatology. The mean snow-depth anomalies are also rather small âĹij O(cm), suggesting that the events are not as extreme as the text suggests.

To answer this question, we have produced Figure A1 at the end of this review. IT will substitute Figure 4 in the previous version of the manuscript. Indeed the RCP8.5 scenario is about 10 degrees warmer in winter, over Italy, than the CTRL scenario. Interestingly, the 4SST scenario produces about the same temperatures as the CTRL scenario. This explains why in the RCP8.5 scenario we barely observe snowfalls while we do observe it for the 4SST one.

5. Fig 11., I don't understand the units of convective precipitation (âĹij10ËĘ-8 m/s), neither do I understand whether this is a composite over all winter days, or only over the 32 selection. Moreover, the blue area may indicate enhanced precipitation, but over Italy the signal is predominantly red indicating a decrease.

We agree with the reviewer that the convective precipitation rate P_c as expressed in Plasim has a non-common expression. Indeed, P_c is computed using the following step: first, the distribution of temperature T_{cl} in the cloud is found by lifting the air dry adiabatically and corrected due to condensation of water vapor. Then, the temperature tendency $(\Delta T)_{cl}$ is the temperature difference between the environmental heating and cloud temperature of each cloud layer $(T_{cl} - T_e)$. Cumulus clouds are assumed to exist only if the environmental air with temperature Te is unstable stratified with regard to the rising cloud parcel: $T_{cl}) > (T_e)$. The top of the cloud σ_{Top} is then defined as $\sigma_{Top} = \sigma_{l+1/2}$ if $(T_{cl})_l < (T_e)_l$ and $(T_{cl})_{l+1} > (T_e)_{l+1}$. Then, the final temperature $\partial T/\partial t$ which appears in the diabatic leap frog time step is given by $(\Delta T)_{cl}/2\Delta t$, where $2\Delta t$ is the leap frog time step of the model. The convective precipitation rate $P_c[m/s]$ of each cloud layer

is therefore given by the expression

$$P_c = \frac{c_p \Delta p}{L g \rho_{\mathrm{H_2O}}} \frac{(\Delta T)^{cl}}{2\Delta t} \tag{1}$$

where Δp is the pressure thickness of the layer and ρ_{H_2O} is the density of water. Note that, in the previous expression, the larger the convection, the more negative is the P_c value because of the definition of $(\Delta T)_{cl}$ which is itself negative. Therefore, the interpretation given in the manuscript is correct : large negative values of P_c correspond to heavier convection. In the new version of the manuscript this will be specified together with the computation of the composite that is over the 32 events. We will include all the terms needed for the computation of P_c so that the readers will find it easier to understand figure 11.

6. At some places, strange formulations are used (e.g., in the Appendix, in one of the cases (p9, L284), it is stated that "The cold primates belong to Sweden and Finland", a sentence that is hard to understand. I would recommend to let an native speaker spell check the entire document upon resubmission

We will take care of checking the document for wrong expressions.

Please find figure A1 in attachment: Figure A1: Boxplots of the spatial average over Italy of SLP (a), T850hPa (b), Geopotential Height HGT (c) and Snow depth (d) for all winter days (grey) and for the analogues of cold spells (blue).

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-61, 2020.

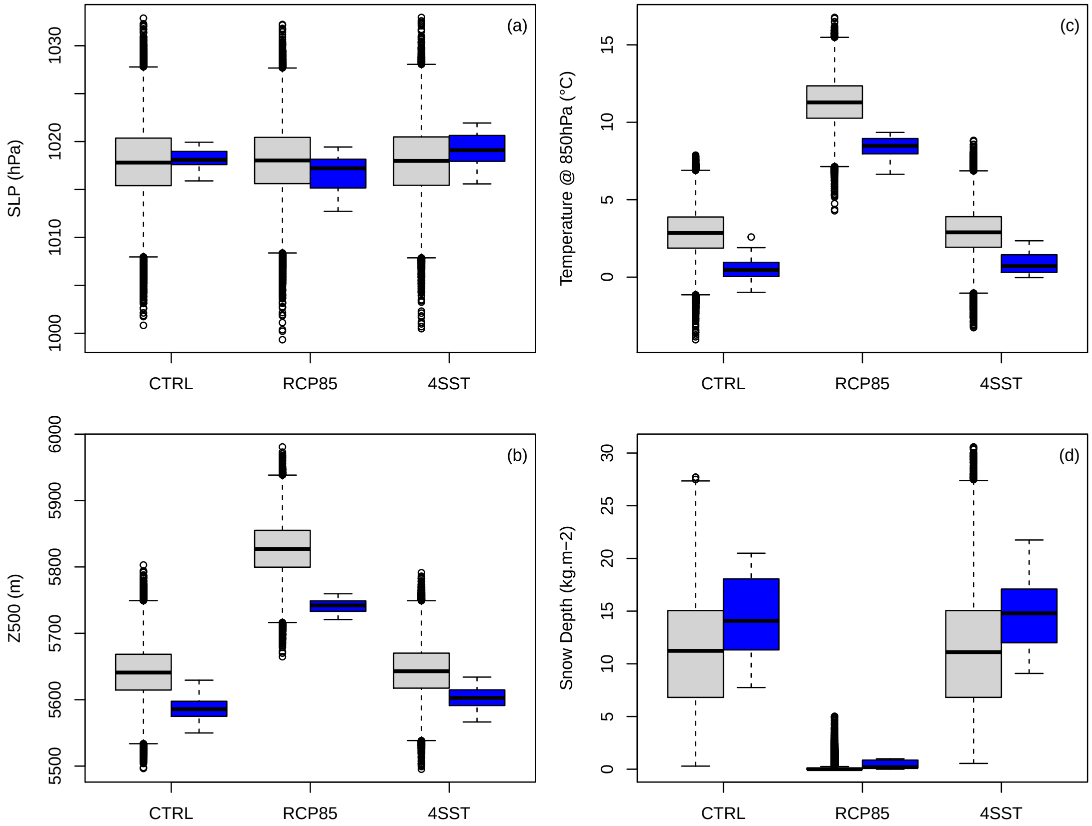
(a)
SLP (hPa)
1000
1010
1020
1030
CTRL
RCP85
4SST
(c)
Temperature @ 850hPa (°C)
0
5
10
15
CTRL
RCP85
4SST
(b)
Z500 (m)
5500
5600
5700
5800
5900
6000
CTRL
RCP85
4SST
(d)
Snow Depth (kg.m−2)
0
5
10
15
20
25
30
CTRL
RCP85
4SST

Fig. 1.