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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.

Anonymous Referee #2

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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 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.

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).

2. 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.

3. 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-NCAR-40-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.

4. 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.

5. 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

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.

6. 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.

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. 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. 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 point-wise 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

simulations suggest that the simpler model is not at all able to capture the variability as observed. 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 $\sim$ O(cm), suggesting that the events are not as extreme as the text suggests. 5. Fig 11., I don't understand the units of convective precipitation ($\sim$10ˆ-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. 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.

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