Comments on the re-revised version of Present and future synoptic circulation patterns associated with cold and snowy spells over Italy, *by M. d'Errico et al.*

January 27, 2022

Remarks

The authors have taken many of the earlier comments by the reviewers seriously. Again the paper has seen big changes. The SST-run has disappeared, and new scenario runs have appeared. Some figures are gone, many new have appeared. Section 4 is new material, showing some analysis with CMIP5 data. I appreciate the extra effort but have difficulties to understand them. I think most of the changes work out well and the paper has improved. However, I still have several points/remarks. My suggestion to the editor is that the paper still requires some revisions prior to acceptance.

Points (mostly remarks to the authors' response document)

1. My previous point 3 ("too large size of the clustering domain") has not been used. Instead, arguments are given why the authors think their large domain (120degree wide in longitude) is necessary. I can follow their arguments, but still hesitate as to its justification, especially if the final target area is that small. Large-scale embedding is essential of course, but in my view the large domain now leads to flow fields not tied very strongly to the region of interest. This implies that the results are perhaps less clear than would otherwise have been possible, in the sense that for the target region, the response fields likely regress to the mean. But in the end, it is the responsibility of the authors to squeeze the best possible results out of the data. *My suggestion* would be to include a brief discussion on how experimenting with domain-size would impact the results. This would also help to address issues raised below under 4 of my minor points.
2. I can agree to the authors' reply to my Point 5 ("significant differences"). If one can argue that the flow is different outside the target area, this should also be sufficient. as it points to different types of air masses involved.
3. It is appreciated that the figures have been improved. Yet I have some further suggestions (see below).
4. On my point 8 ("alarming results of PlaSim"): I am pleased that an error was discovered and corrected for and am happy with the authors' response and putting these results into a better context of existing results/debate. Still the huge increase in frequency is difficult to grasp.
5. On my point 9 ("alternative suggestion"). I am pleased to see that the authors have followed up my suggestion.
6. On my point 11 ("4K ocean"). I think it is good that the authors removed that part in the re-revised version. The additional simulations with different scenarios are a good replacement.

Other comments (mostly minor, except 4).

1. In Section 2.1 (line 53) the Delta-SST-run is mentioned. I think it is not used anymore (see my above point 6). This sentence can be removed.
2. Cluster 2. The authors refer to it (sec 2.2, line 124) as a "Scandinavian" blocking. However, when I look at the MSLP pattern, it doesn't look quite like a

Scandinavian blocking, as the high pressure is much too far south. Maybe the authors could rephrase it into "resembles/is more like a Scandinavian blocking pattern". It could help to show MSLP also as an anomaly. Generally, I think also Figure 3 would benefit from taking an anomaly perspective, with fewer colors. The color-scale for precipitation is not well chosen (I'd use the conventional BrBG option in an (relative)-anomaly framework, again with fewer colors).

3. Table 1: the results for Cluster 2 for RCP8.5 (bottom-right numbers) seem inconsistent. The number between brackets should be 53.4% higher than the number outside the bracket (or, likely, the number outside the bracket should be 53.4% lower than the one inside). Can I assume this is a typo?
4. Figure 4: It is not instructive to start with this figure, as you mainly see the increase of the Z500 level with increased warming levels. Figure 5 is the key figure. It is strikingly similar under all 4 scenarios (as they should I think, since you based your analogs on these patterns). I first wondered why these patterns do not agree to the ones obtained from observations... Similarly for figure 6: These MSLP fields don't look at all like the patterns obtained earlier based on the observations (Figure 2e-f)? Based on these figures I *would call neither of them Scandinavian blocking*. The most pronounced features in Z500 (fig 5) are the low's rather than the highs (The highs are far away over the Atlantic). The reason that the patterns are different of course is that you now search for analogues in a much smaller domain, more connected to the flow in the region of the target region. I do appreciate this shift, but it somehow conflicts with your earlier arguments (of choosing a wide domain for the clustering). Why did you first construct cluster centroids for a larger domain (making them not very different in the target area) but subsequently use a smaller domain for the analogues? I would rather use the same domains for both exercises. The current choice makes the story much more complex than needed.
5. Figure 7-9: Again, using an anomaly framework would be much more instructive. Now, upon visual inspection one can see hardly any difference between Cluster 1 and 2.
6. Section 4: I cannot really follow what is being done here based on the information presented. It is stated that the "We then embed these observed events into historical simulations...". What does this mean? Do you project all days in CMIP5 for each winter and model on the observed cold spells, and compute their Euclidean distance? If so, why would a average reduction of the distance between the observed (cold-spell) and modelled (CMIP5) circulation be an indication that the cold-spells become more frequent? Or do you perhaps select cold days in some way from CMIP5 first? In its current formulation I cannot see how the information in Figure 10 helps us.