Comments on the revised version of Present and future synoptic circulation patterns associated with cold and snowy spells over Italy. By Miriam D'Errico[1,*], Flavio Pons[1,*], Pascal Yiou[1], Cesare Nardini[2], Frank Lunkeit[3], and Davide Faranda

Remarks
The authors have taken many of the earlier comments by the reviewers serious. The paper has improved considerably but also has seen quite a big change compared to the previous version. I am pleased with some of these, yet I have still difficulties understanding the results. Or perhaps I should say, new difficulties, different from before, because there is a lot of new material. New are a K-means clustering approach, and a focus on the frequency changes of the mslp analogs. Although there is potential in this new part, I have a number of concerns and remarks that may influence results and require new analysis. My main concern is that the most important new result (the "huge" increase in RCP85 cold-spell analog mslp conditions, at least for certain of the cases) is hard to digest without any suggestions as to the why of it. The paper does not offer any help with explanations. The same holds for the almost complete absence of effect on frequency, for the SST+4 runs where PLASIM global oceans are increased by 4 degrees.

Without a more proper interpretation of these results I cannot accept this paper. My recommendation based on this version, is revise with major revisions.

I list some of my main comments below.

1. Section 1 and 2 have not changed much. There still is a multipage long descriptive section 2 on the cases, making rather clear that they are quite different. To me providing the entire list with details on all cases is way too much given the amount of analysis that is undertaken subsequently. I leave the decision up to the editor, but I would be happy to see (some/most of it) put in an appendix.

2. To make some order in the chaos of all cases, the authors decided to conduct a K-means clustering analysis. This could be a useful thing sometimes indeed. They end up with two main clusters. However, there is no real argumentation for this. Figure 2, the scree plot, is poorly formatted with labels dropping off. It also doesn't tell anything as far as I can see, except that there is no favourable grouping. I would put this in supplementary material, but definitely tidy up the graphics!

3. I think the domain chosen for the clusters is *way* too large. Although the authors warn the reader that they do this for a reason, the cluster domain now covers 120 degrees in the zonal direction, which is a 3rd of the earth. Have the authors experimented with using a domain that is more compact, to zoom in slightly more on the actual situation over Italy? Although the subsequent PLASim simulations are of course also rather coarse I think it would help make the analysis more relevant useful.

4. Another basic question, has the clustering be performed on anomalies wrt to a climatology or to the full fields? Because of pre-existing large-scale pressure gradients, a full field framework is not recommended.

5. To augment my previous statement, it could help the authors to examine whether the differences in mslp between the cluster centroids are actually statistically significant over the prime region of interest: Italy. If not the authors have a problem with section 2.3.

6. The quality of figures 3-5 is poor and does hardly provide insight in the way they are presented now. They should be improved. My advice is to combine mslp and T850 in the same plot (or maybe even T2M as well), using shading and contours. And use an anomaly framework! So make these plot wrt a DJF or whatever climatology 1981-2010 or so. And use much tighter colour bands. It is almost not possible to make out differences in temperature at all this way and even for mslp it is hard to see how the flow is organised.

7. Then onwards from section 3, we turn to the model world of PlaSIM. I appreciate that the authors have brought some of my earlier suggestions into practice by focussing less on the thermodynamic aspects. However, the results that are produced by the analogon approach are quite surprising/disturbing/alarming, at least the one for the RCP85 scenario. In there we see spectacular increases in the frequency of cases. Although the world also warms, this might not yield extremely cold/snowy situations in the end (which the authors warn for already), but still.

8. To me, the huge increase seen in RCP85 raises an alarm bell. Why/how does this occur? Many existing climate model ensembles exists (e.g. CMIP5, CMIP6), but as far as I know, none does produce such extreme changes in the tails of the distribution. So the authors at least have to come up with a convincing story here.

9. An 11-step scheme is presented to obtain structures that are similar to each of the 32 events. However, by now focusing on each of them (in the table), the reader may wonder how strange/anomalous each of them was. The readers have no idea about the mslp fields underlying each case, and therefore have no feeling about what the numbers in the tables indicate. Why not simply use the two cluster centroids decided on in section 2.3 and use these to find analogs for?! One could even use these two cluster centroids and search for distribution changes in the way done e.g. in the snow paper by de Vries et al. Clim Dyn. DOI 10.1007/s00382-012-1583-x. (eg their figure 6).

10. The same question I had on the domain size applies here as well. First: is the analoging done on anomalies or full field, and have the authors experimented with the domain size? If the final interpretation is to hold for Italy specifically, it should (I believe) be demonstrated that a domain is chosen that at least for that region provides meaningful results.

11. The rationale for using a +4SST is that the MedSea warms faster than the rest, but in PLASIM the oceanwater is globally raised by 4 degrees. I am then surprised to see that this leads to no adjustment whatsoever.

12. Figure 7 is unclear what we see. Is it climatological mean snowcover? Units seem to be kg/m2, which is probably the same as cm snow. But then showing the snowcover up to natural logarithm values of -15 is rather small/meaningless..

13. Figure 8-9 same story as for figures 3-5. (see above comments)

14. Finally, in those figures RCP85 mslp analogs are combined with the T850 conditions. This reduces the number of cases accordingly.