

The Cryosphere Discuss., referee comment RC1
<https://doi.org/10.5194/tc-2021-363-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on tc-2021-363

Anonymous Referee #1

Referee comment on "Synoptic control over winter snowfall variability observed in a remote site of Apennine Mountains (Italy), 1884–2015" by Vincenzo Capozzi et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-363-RC1>, 2022

Capozzi et al analyze a more than century long series of snowfall from the southern Apennines in Italy, and relate it to synoptic weather types and teleconnection indices. The analysis involves a newly digitised series of snowfall, and adds to the understanding of snowfall in a Mediterranean regime. Since there are little studies on snowfall, especially with such long series and from non-Alpine regions, it is a valuable contribution to the field.

The manuscript is generally well written, even though at times the language is flowery. However, the composition of the manuscript needs to be improved. A lot of methods can be found in the results. The discussion is missing completely (see also point 4 below). And the methods contain too much and too little (point 1 below).

The manuscript focuses a lot on "visual inspection" of time series, as well as relating different time series, but again mostly visual. The manuscript could be significantly improved if the authors would conduct some statistical analyses by relating time series in a bi-variate way, instead of visually comparing time series across pages. This would require only simple correlation analysis, which the authors conducted in other parts of the manuscript. However, extending this type of analysis would make the conclusion and results from the paper much stronger (see also comments below).

Major comments

- Methods description Sec 2.1 and 2.2 are both very detailed and at the same time miss critical information. The information on data collection and processing is only interesting for very specific readers. The authors could consider moving large parts of this into an appendix. (also the Pettitt and CUSUM are standard tests, so no such detailed description is needed). On the other hand, key information is missing: Which data did you use, daily or subdaily? How did you arrive at monthly values? How did you deal with gaps in the series?
- L309ff and Table 1: How did you define these periods? Just taking 23yr periods? Why exactly these years? I do not think it's a good idea to create these groups, since they might or might not include and exclude relevant points in the time series. If you want to discuss interannual variability and long-term changes, I suggest employing moving window averages (for long-term changes) and moving window standard deviations (for interannual variability). A period of 20 or 30 years would make sense. This would not have the "problem" of arbitrarily defining year groups.
- Figure 8 and related text: Besides the issue with year groups (see comment above), I think it would be much easier if you showed scatter plots of STx versus HNS, instead of trying to compare time series across pages. You could also calculate correlations between these two to give more weight to what you identify from "visual inspection". This would make it easier for readers to see your points.
- Discussion is missing completely. Regarding the snowfall series: How do your results compare to other long-term series from Italy, such as Parma or Torino? If I remember correctly they have been published, but possibly not in international journals.

Minor comments

- L11: mismatch of period wrt to title. OK, later I understood. The snowfall series ends 2020, but the reanalysis 2015, right? You should clarify this and be clearer.
- L37ff the literature review is a bit random. It's mixing snow cover parameters (depth, fresh snow, SCD) and it's not clear why the authors chose the specific geographic limit. Btw, there are many other studies from Italy and other countries in the Alps. Also more with century long series. It's not necessary to mention all, but maybe the authors could make their point better.
- Introduction: Long series are great, but snowfall has extremely high spatial variability, so many short series can identify different aspects than one long time series. Maybe the authors could elaborate more on this topic.
- L64ff: At this point in a paper, there should not be a summary of what is being done, but the aims of the paper (high-level understanding, hypothesis, etc.)
- Figure 5: why did you choose a lowess smoother? Would a simple moving average (10/20/or 30 years) be easier? For the lowess, you also need to supply the degree and the weights, not only the time span.
- How are "snow days" (NSD) defined? This would belong in the methods. (related Table 1, Figure 5, ...)
- L409: how did you determine statistical significance of trends?

- L452: How does the correlation table look like for all values, not only the upper and lower quartiles? Maybe for the supplement.
- Section 4: have you considered also correlating teleconnection indices to the HNS series?
- Figure 11: Hovmöller plots do not work for a discrete x-axis, where you have the five teleconnection indices. Would a simple correlation analysis not work better here, too?