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Hydrology and Earth System Sciences Discussions

Interactive comment on "Interannual variability of winter precipitation in the European Alps: relations with the North Atlantic Oscillation" *by* E. Bartolini et al.

E. Bartolini et al.

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We greatly appreciate the extensive comments from the referee.

The main point he raises concerns representativeness of interpolated grid data. This topic would deserve more attention in the scientific literature, as it is crucial for understanding the impact of large scale climatic phenomena. In this reply, after a general introduction to the topic, we will extensively discuss the reviewer's criticisms and warnings, also with reference to some new small parts added to the paper.

In general, for interpolated databases consistency and smoothing can affect the reconstructed data. Consistency has been addressed in the paper by comparison with station data (see specific comment below) but no mention has been made on the smoothing effects. We can group into the "smoothing effects" the variability of station density in time and in space. The first problem, station density variability in time, could affect the coherence of the reconstruction and can become important when there are necessities of volume preservation across spatial scales. We believe this smoothing effect does not have particular impact on our results, because we are not addressing any downscaling issues. In the interpolation approach used for the database CRU TS 1.2 each value in the time series is estimated based on the number of stations available at that moment. The number of available stations has increased until the 80's and then has slightly decreased, somewhat affecting the interpolation. Nevertheless, in the database CRU TS 1.2 this effect is minimized by using the anomaly approach: the time and space components of the station data are separated firstly constructing a mean climatology (1961-1990) and then deriving and interpolating monthly anomalies. For both the interpolations the thin-plate spline technique is used as a function of latitude, longitude and elevation; this method has the advantage of being robust in areas with sparse or irregularly spaced data point (New et al., 1999). We do not see serious problems of temporal coherence in this approach, unless one would refer to a check on conservation of rainfall volume, that does not seem addressed in the procedure. In our approach, however, temporal consistency is more important than volume consistency, and the temporal coherence appears to be good, specially observing station vs grid data time series. In addition, the comparison of grid rainfall and station rainfall time series, available in Bartolini (2007), now cited in the paper, demonstrates that the quality of correlation does not change from the early to the most recent periods.

With regard to the anthropogenic effects, the limited check performed on rainfall stations does not show this kind of influence, as the trends are negligible. The wide-scale nature of our analysis, however, makes this issue not particularly relevant.

The reviewer raises also a problem of grid-data representativeness for high altitude regions. To comment on this topic it can be useful to separate two different aspects

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of the problem. On the one hand it can be considered that the original precipitation measurements are influenced themselves by several factors, like the ratio of solid to liquid precipitation and wind conditions. Using station data would then maintain the underestimation problem, well known in the literature. On the other hand, it is important to consider the effect of the orographic configuration of the study domain on the interpolation procedure. In fact, the interpolation is also based on a representative cell elevation. In complex terrain this simplification will produce inevitable uncertainties on the reconstructed cell precipitation. However, we do not see how the use of original rain gauge time series could be much more representative of the entire alpine area, except if recurring to specific high-resolution geo-regression approaches. Given the Europe-wide perspective of our work, this would be really out of the scope of the paper.

SPECIFIC COMMENTS

It should also be said that the authors are comparing here station point precipitation with areally (grid) averaged precipitation, which on a monthly basis is probably not bad, but in principle is not correct.

As anticipated in the first part of the answer, the main idea behind this analysis was to verify if gridded data can at least capture the main patterns of variability existing in the measured precipitation time series. In this sense, even when the comparison shows evident bias (scatterplot deviating from the 1:1 line) the dispersion around the straight line is limited, and this allows us to preserve the variability of the original data, as the coefficient of variation removes bias by definition. This has been stated more clearly in the paper.

Although the fits are generally quite good, it is noteworthy that they are worst in the two highest laying stations.

In fact, the worst fits are equally shared by high-elevation stations (Sonnblick) and low-elevation stations like Alessandria (lying in the Padana plain) or Domodossola, which

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is a valley-bottom station. It is hard to find a rule for dispersion (low R) and bias (slope different from 1) of grid data as compared to station values.

Note that the correlation coefficient is not a good measure for identifying bias.

This is true. It has been clarified in the paper

Perhaps the authors could also add the slope of their constrained regression to Table 1 to identify the level of over or under estimation.

In the revised paper we added, as suggested, the slope of the regression in Table 1 and axes titles for Fig. 1.

It is of course very unlikely that a strong relationship between precipitation mean or variance (summer or winter) and altitude at the scale of the European Alps can be found. The authors make reference to that on page 2051. Nevertheless I think it is illustrative to show that to readers of HESSD.

We prefer not to include this figure because it comes out really not significant. We just found a cloud of points demonstrating the lack of any interesting relation between these variables.

In interpreting the spatial maps in Figures 2-7 the authors often refer to the European Alps. Is it possible to include a polygon outlying the area they refer to as the European Alps in those Figures (or at least some of them).

We did not include in the maps a polygon outlying the area of the European Alps just because the figures are not large enough to include additional information. Moreover, even presenting borders in form of a contour line would not produce a clear geographic border as suggested by the reviewer. In the text, the reference to corners of a rectangle containing the Alps does clarify how we have delimitated them (e.g. for aggregating rainfall shown in Figure 2).

As the authors correctly point out, the mountain range does generally lie in the statistically insignificantly correlated area. However it is quite relevant that there are areas, such as the southern slopes that the authors refer to, which do show some significance.

A short comment has been included in the paper. A discussion on "higher resolution" effects could be the subject of future work.

The study is heavily based on the interpretation of the spatial fields in Figures 2-7. The colour schemes cannot be read printed black-and-white. Is it possible to recolour the figures?

We understand that the colour scheme of the figures cannot be easily read if printed black-and-white but, after various tests, we have concluded that colour shades are the only way to represent the whole variability in the correlation coefficients.

The fact that the authors found statistically significant trends in the gridded data in the Eastern section of the Alps but not in the station data (page 2054, line 16) should be a cause for concern. Is it not possible that the trend is a by-product of the spatial interpolation and changes in the station density there?

The referee suggests that the trend detected can be a by-product of the spatial interpolation used for the creation of the CRU TS 1.2 and the changes in the station density occurred in the most recent part of the century. We cannot exclude that this is indeed the case, but we have provided some clarification to the qualitative statement put in the conclusions of the previous version of the paper. Getting back again to Bartolini (2007) it was concluded that there is in fact a good agreement between the trend detected in the stations and those referred to the gridded time series. In both time series Trento shows a negative significant trend, while all the other time series do not present any temporal tendency, except for Sonnblick, which has a negative significant trend only in the grid cell. However it must be considered that the important point of our analysis is that the trend detected is weak and not really spatially extended, so it is not possible to relate it to the interannual variability of winter precipitation, which remains unexplained.

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I would not refer to a decrease in precipitation as a drought (page 2054, line 21) unless you first define what a drought is.

We agree. Drought is substituted with "dry anomalies"

Do not use precipitation in plural (page 2047, lines 5 and 12).

Amended

REFERENCES

New, M. G., Hulme, M., and Jones, P. D.: Representing twentieth-century space-time climate variability. Part I: Development of a 1961-90 mean monthly terrestrial climatology, J Clim, 12, 829-856, 1999.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2045, 2008.