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Hydrology and Earth System Sciences Discussions

Interactive comment on "Downscaling of surface moisture flux and precipitation in the Ebro Valley (Spain) using analogues and analogues followed by random forests and multiple linear regression" *by* G. Ibarra-Berastegi et al.

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One fundamental weakness of the analog model, namely that it is unable to predict new record-breaking values are neglected. This ought be addressed in every paper on the analog model. A simple iid-test (Benestad, 2008) can easily demonstrate that for any series one will expect to see new record-breaking events (in some sense) as the sampling goes on - it's usually a question about time. Hence, the analog model is likely to misrepresent the upper (lower) tails of the distribution, even if a mean trend is accounted for. One solution is to use apply local quantile mapping through a 're-

calibration' (Benestad, 2010).

Another issue concerns the search for analogs in EOF/PC space - are the patterns scaled by their eigenvalues or are the different dimensions 'normalised' (Imbert & Benestad, 2005)?

The comparison between different methods and strategies are probably not universially valid, but should be repeated from case-to-case. I think that the performance of a certain method is very region and season dependent. I also suspect that the results may vary with different choices of predictors and predictor domain.

Data on atmospheric moisture and surface moisture flux are notoriously unreliable, as they often are based on derivations rather than direct observations. The paper should provide more discussions about the data quality issue. I'm also a bit confused about the term 'surface moisture flux' and zonal/meridional components - is this not the vertical flow of H2O across the lower boundary (soil to air)? Or is it the moisture transport in the lowest level? (what physical relevance would that have for climatological studies/konsequences?)

Recent NWF operate with a spatial resolution as high as T1279 L91 (at ECMWF), with a 16 km resolution globally ($\sim$0.1 degree).

How sensitive are the results to predictor domain? How would the results be affected if the downscaling was based on a lower number of 'mixed EOFs' (Benestad et al., 2002) rather than many independent EOFs?

Did the bootstrapping take into account persistence and time structures? For regression on non-Gaussian data -perhaps a better choice would be to use GLM rather than LM?

How do we know that precipitation is more intermittent and depending on very local factors than surface moisture fluxes? Is this even true? Perhaps the reason why ERAINT provides a better description than ERA40 is (partly) the higher spatial resolution?

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In general, I found the paper hard to read - very dense and packed with non-standard acronyms ('RF', 'FA2', 'RSD', 'D', 'D2'). PErhaps keep more of that information in Tables and just make a more general reference to them, rather than being very detailed in the main text?

Relevant publications Benestad, R.E., E.J. Førland and I. Hanssen-Bauer (2002), Empirically downscaled temperature scenarios for Svalbard Atmospheric Science Letters Volume 3, Issue 2-4, doi.10.1006/asle.2002.005, September 18,p 71-93

Imbert, A. & R.E. Benestad (2005) An improvement of analog model strategy for more reliable local climate change scenarios Theoretical and Applied Climatology 82, p. 245-255, DOI: 10.1007/s00704-005-0133-4

Benestad, R.E. 'A Simple Test for Changes in Statistical Distributions', Eos, 89 (41), 7 October 2008, p. 389-390

Benestad, R.E. 'Downscaling Precipitation Extremes: Correction of Analog Models through PDF Predictions', Theor. & Appl. Clim, Volume 100, Issue 1, DOI: 10.1007/s00704-009-0158-1.(2010).

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