Reply to

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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Received and published: 8 March 2011

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 'recalibration' (Benestad, 2010).

We agree with Dr. Benestad that any method based on analogues is essentially unable to create forecasts not previously observed. However, in the particular application presented in our paper we also perform a second step (Random Forests and Multiple linear regression) on a subset of the observations used as analogues. Therefore, the output of our algorithm is never the exact value observed in any of the analogue synoptic situations historically observed. Conceptually, our method is similar to Dr. Benestad's one, in the sense that, after performing a search of analogs, we post-process the output from simple analogs. In general, it can be seen (Figs. 2 to 5) that the post-processing step clearly improves the performance of the analogue search. We agree that Dr. Benestad's approach seems interesting and it would be nice to inter-compare them in a future paper. We will add a paragraph making clear this in the revised version of the manuscript. Anyway, it is clear that there is no universally perfect downscaling method available. In this paper we wanted to test new routes. In this sense, it is interesting to note that, as far as we know, no previous paper uses random forests after an initial selection of analogues. It makes the paper interesting, as it explores new routes not previously explored by other authors before.

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)?

Since variables of different nature (Geopotential height, wind speed, temperature, pressure….) were to be combined, with the aim to define the EOF hyperspace in which to start the search of analogs, from the beginning all the ERA40-ERAInt. original variables were standardized to have variance= 1 and mean= 0. At a second step, EOF were computed for each variable on those standardized values. For each variable, a number of EOF were retained so that at least they represented for each variable percentages of variance above 80%-90%. Since the variances of the original variables are very different, unless they are initially standardized, the combined EOF will always point in the direction given by the leading EOF of the variables with the highest

variance.. Please read the ANNEX for a more in depth answer to this point.

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.

We agree with Dr. Benestad here. However, we do not imply otherwise in our text.

I also suspect that the results may vary with different choices of predictors and predictor domain.

We have carried out intensive additional calculations to address this point. The results show that changes in the predictor domain do not have a clear impact on results. Please read ANNEX. In this line, we could add some lines to the paper if given the opportunity to do so in a revised version of our paper.

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.

As many other research groups http://eca.knmi.nl/publications/index.php#papers we have used data from the European Climate Assessment and Dataset (ECA)project. For details on the nature of data, please check http://eca.knmi.nl/FAQ/index.php . In our opinion, an in-depth discussion on the quality of data from some such a widely used and accepted source like ECA, falls well beyond the scope of our paper.

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?

It is the moisture transport in the lowest level. If it is not clear, we would welcome any suggestion for a better denomination of this magnitude.

(what physical relevance would that have for climatological studies/konsequences?)

The reviewer is right when he states that a complete analysis of the moisture balance requires the vertical integration over the whole troposphere, like in Berbery and Rasmusson (1999).
However, long time series of rawinsonde are scarce and such an analysis would be oriented to a complete moisture balance. In this paper we wanted to check the feasibilty of getting realistic values of a quantity such as the moistue transport at surface level. This quantity involves a nonlinear operation of two quantities (wind speed and relative humidity/dew point temperature) and analyzing the feasibility of this downscaling will help us perform future studies over vertically integrated transports too. However, moisture transportation is maximum at low levels.
Similar non-linear quantities appear, for instance, during forecasts of wind speed for wind energy sector. Our main interest in this case relies in the study of the applicability of a new downscaling method to variable like surfae moisture flux. In future studies we can apply the same methodology to vertically integrated transports of moisture, but it is clearly out of the scope of the current study, which is basically oriented to the

intercomparison of the performances of different methodologies.

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

The reviewer is right but we would like to point out that this high resolution has only been applied to ECMWF model from January 26th 2010 onwards. Unfortunately, we cannot use historical records at this high resolution.

How sensitive are the results to predictor domain?

Results are not sensitive to predictor domain. Please read ANNEX.

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?

If fractions of the overall variance above 80-90% are to be retained, the results indicate that using mixedEOFs exhibit a slightly worse performance than using independent EOF. For details, please read ANNEX.

Did the bootstrapping take into account persistence and time structures?

We agree with Dr. Benestad that this is something that must be taken into account if using cross-validation. However, in our case, the training dataset and the test datasets are independent. Therefore, autocorrelation is not increasing performance measures, since persistence is not helping the forecast process.

For regression on non-Gaussian data -perhaps a better choice would be to use GLM rather than LM?

At this new stage, we have carried out nearly 66000 tests of normality. In our opinion, we think that using MLR instead of GLM is a perfectly valid choice. For details, please read ANNEX.

How do we know that precipitation is more intermittent and depending on very local factors than surface moisture fluxes? Is this even true?

When we say in our original paper that precipitation is more intermittent we do not refer to the meaning of "intermittence" that is commonly used in some fields such as turbulence or in chaos in dynamical systems. We specifically refer to the fact that sometimes it rains and sometimes it does not. Particularly, in the Mediterranean area, precipitation is very often of convective origin.
On the other hand, horizontal moisture transport at surface level cannot be strictly zero unless wind speed is 0, something that is not very common. If the editor ask us so, we can rewrite this part to make it clearer.

Perhaps the reason why ERAINT provides a better description than ERA40 is (partly) the higher spatial resolution?

This might be true or not. Despite the fact that ERAInterim model has a higher spatial

resolution tahn ERA40, it is also true that there are other changes as well, such as the 4DVAR assimilation in ERAInterim versus 3DVar in ERA40, new humidity analysis and so on. For details, please have a look at this link
http://www.ecmwf.int/research/era/do/get/era-interim

In general, I found the paper hard to read - very dense and packed with non-standard acronyms ('RF', 'FA2', 'RSD', 'D', 'D2').

Regarding Dr. Benestad's concern on acronyms, we have used those that to the best of our knowledge are the most widely used in the scientific literature (Hanna et al., 1991,Olensen, 1995b& 1997, Liaw and Wiener, 2002, Grömpig, 2009). In this sense, we welcome any specific suggestion that may contribute to a better identification of what the different acronyms used in this paper stand for.

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?

If the editor gives us the opportunity to prepare a revised version of this paper we will follow this suggestion and also include the additional bibliography suggested by Dr. Benestad.

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. 245255, 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).