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# ***Interactive comment on “Large scale atmospheric forcing and topographic modification of precipitation rates over High Asia – a neural network based approach” by L. Gerlitz et al.***

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Received and published: 10 November 2014

## **1 General Comments**

This manuscript presents a new estimation of monthly precipitation rates generated for a large target region of High Asia (1989–2011). The very high resolution (1km) gridded precipitation data are obtained by training an artificial neural network, using large-scale dynamical fields from reanalysis data and local geographical features as input. The downscaling method is described, validated, and a few examples of its output are shown.

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The study is timely and is a welcome application of downscaling approaches in this data-scarce region. However, I think that the manuscript could be improved substantially by shortening some less relevant text parts and by adding new figures and analyses. Two major issues have to be addressed before publication:

### 1.1 Validation strategy

The authors use 157 meteorological stations for calibration during the period 1989 to 2000, and further 18 “independent” stations for validation during the period 2000 to 2011. Little is told about what motivated the selection of these 18 stations, and a couple of them have long/frequent spurious data gaps, making them unusable. Furthermore, I would like the calibration/validation strategy to be better explained:

- the calibration period starts in 1989, but ERA-Interim starts in 1979. Why?
- the considerable database of 157 stations is not used at its full potential if the calibration is done for 11 years only. Statistical downscaling approaches can take full advantage of all available data by using cross-validation techniques, in which data samples of data are used in turn for calibration and for validation (i.e. k-fold cross validation). This approach would have the advantage that a real estimation of the skill of the ANN can be estimated at all stations’ locations, not only on a fixed sample of validation stations for which the true representativeness cannot be assessed.
- the 18 stations used for validation are not *truly* independent, since they are also used to tune the degrees of freedom of the ANN (Fig. 5). As discussed by e.g. Elsner and Schmertmann (1994), it is crucial to define a truly independent data sample for validation, that have not been used for either training or tuning of the model.

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To summarize, I suggest the authors to better explain the rationale of their validation strategy as well as their reasons for the choice of the calibration/validation/application time periods.

## 1.2 Objectives of the study

I believe that the study could be improved with more clearly stated objectives. The authors correctly discuss the advantages / drawbacks of dynamical and statistical downscaling methods, as well as the reasons for the need of new methods to estimate precipitation in High Asia. However, little is done to really discuss the added value of their new precipitation estimates.

Since they do not seem to wish to make their dataset freely available, more emphasis could be given on the question raised in the title of the study: "atmospheric forcing and topographic modification of precipitation rates". The analyses proposed in Fig. 7 and 8 are not really conclusive with respect to the title. With selected examples (for example "zoomed" target regions), they could discuss their results with regard to orographic precipitation more effectively. Currently available gridded datasets (TRMM, HAR) are too coarse for comparison, but the 5km TRMM rainfall climatologies from <http://www.geog.ucsb.edu/~bodo/TRMM/index.php> could be a starting point.

Another axis of research could be to continue further the discussion started in the sensitivity analysis (Fig. 8). Would it be possible to select the most important predictors based on Fig. 8 and then realize a distributed evaluation of their relative importance? (for example, a map of the 0.1, 0.5 and 0.9 percentiles of the influence of elevation or the wind index)

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As a whole, I find the study interesting and hope to see it published in a more elaborated form (see also specific comments below).

Best regards,

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## 2 Specific comments

Throughout the manuscript:

**The ratio text/figures is not really balanced.** The text can be sometimes repetitive. As an example: P1277 L20 → P1278 L5 contains several sentences repeating more or less the same information and could be shortened. Chapter 2 is a comprehensive review of knowledge about precipitation in High-Asia, but it could be skipped almost entirely without much lost for the rest of the paper. I am *not* asking for an entire suppression but I suggest the authors to consider shortening their manuscript as a whole.

**Presentation of the results.** In their current form, the figures are difficult to interpret for the reader:

- the validation stations are marked in red in Fig.1 but there is no simple way for the reader to know which validation station is where
- while Fig. 6 allows a coarse evaluation of the model skill (which looks quite

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good), it is not really possible to assess an important aspect of the precipitation estimates: their capacity to reproduce observed inter-annual variability. More analyses/figures are needed here

- the choice of a continuous diverging color table and of topographical color-shading makes it really difficult to associate a pixel on the plot with a precipitation value (e.g. Stauffer et al., 2014). Fig. 7 (top-right) is a good example of this problem: I find it difficult to distinguish more than three zones of precipitation, and since the dataset is of very high resolution it would be good to be able to distinguish between topographical (artificial) shading and orographic (real) precipitation.

Since I expect the text to be modified substantially I will not make too many specific comments here:

P1227

L24: “precipitation-genetic” does not seem to be a very commonly used expression.

P1284

Fig 1: please explain the choice of the target area. Is there a specific reason for omitting the Karakoram/Hindu-kush? Doesn't the presence of very different settings (e.g. Indian lowlands VS central TP) make the job of the ANN considerably more difficult? The stations that are referred to in the text should be named in Fig. 1

P1285

L5: how do ANN handle missing data? How many gaps were found in the time series?  
L7: I am not sure if ERA-Interim is assimilating precipitation directly. In all cases, it is not relevant since ERA precipitation is not used as predictor.

L12: these other predictands are mentioned here, but they are not validated and almost not used afterwards (in particular number of precip days). Are they also influenced by orography?

P1287

L2: 4.4 percent: in Fig. 2, the number indicated is 0.044. I guess this is an error  
L10-L12: EOFs 4 to 6. Indeed, they explain less variance as the other ones but, as stated by the authors, a very large part of the variance is due to the annual cycle (EOFs 1 and 2). The remaining EOFs could become particularly important for inter-annual / intra-seasonal variability.

P1288

L17-20: your domain also includes lowlands. Discuss the choice of the 500hPa level in this case.

P1289

L11-12: does the strength of the wind field also influences the wind effect parameter? Would it be useful to also include wind speed as a predictor?

P1290

L7: are the predictands also normalized? This does not make much sense for the considered variables. How do ANN handle skewed distributions? Do they predict negative precipitation values as linear models would?

Fig. 5. It seems that the choice of the number of neurons is strongly influenced by one or two stations showing the largest errors (the other stations do not vary much between 2 to 8 neurons). This calls for the use of a much larger sample of data for this tuning procedure. Why not using the 157 stations during 2001-2011 to select the degrees of freedom and then use independent stations for validation?

This color scale has an abrupt change of color at 260 mm. This creates a completely arbitrary and non-physical “border” in the Himalayas, which makes any interpretation difficult.

Fig. 6. I suggest to ignore the stations with too many data gaps. The Y-axis range should be adapted to the precipitation sums, the first four plots are barely readable. Units are mm.month<sup>-1</sup>? Would it be possible to add annual precipitation as dots for example, to assess inter annual variability?

Fig. 7. I am concerned about the high amount of precipitation in in January and the spurious artefacts in July at the mountain ranges. But these could be related to the colorscales. Can you detect an east to west decreasing gradient at the Himalaya range as documented by e.g. Bookhagen and Burbank (2010) or Maussion et al. (2014)?

P1297 L16 and P1298 L25: Why not show the figures? 8 figures is not too many for a paper and especially the annual amounts would be very interesting to show (annual amounts are a variable people can evaluate more easily than monthly sums).

P1299, Fig 8. What motivated the choice of the four example stations? Without a location on the map it is difficult to assess these results. The difference between left and right panels is not explained in the legend. It could be better to scale the Y-axis in percentage of total amounts instead of mm.

P1303 L5: The HAR recently changed its name to "High Asia Refined analysis" (<http://www.klima.tu-berlin.de/har>). Maussion et al (2014) is the correct reference.

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Interactive comment on *Earth Syst. Dynam. Discuss.*, 5, 1275, 2014.

**ESDD**

5, C550–C557, 2014

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