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Hydrology and Earth System Sciences Discussions

Interactive comment on "High resolution reconstruction of monthly precipitation of Iberian Peninsula using circulation weather types" *by* N. Cortesi et al.

Anonymous Referee #2

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1 General Comments

In the manuscript "High resolution reconstruction of monthly precipitation of Iberian Peninsula using circulation weather types" N. Cortesi and colleagues develop a statistical description of monthly precipitation sums at more than 3000 stations located on the Iberian Peninsula (IP). The model is based on linear regression with the occurrence frequencies of a set of circulation weather types (CWT) as predictors. The weather types are based on daily values of mean sea level pressure (MSLP) evaluated at sixteen grid-points with their centre over the IP. The selection of the relevant CWTs

for a given station is based on a forward selection procedure. Model validation is carried out with a cross-validation experiment. The result is interpreted with respect to the station location and the synoptic interpretation of the CWTs. Based on long series of daily MSLP, hindcasts are made exemplary for three stations and compared with observations. The authors suggest that the model developed can be used to obtain reconstructions of monthly precipitation sums from MSLP fields. Furthermore, a similar model has been set up for river discharge for three selected rivers on the IP.

The general idea of the paper is interesting and the approach given is straight forward and certainly viable. The aim to reconstruct precipitation from MSLP fields is very tempting and thus the paper is certainly of interest for the HESS readership.

Unfortunately, the paper is not easily understandable. Without thorough experience in this area, the work would not be reproducible from the information given in the manuscript; crucial technical steps are missing and some problems with the correspondence between tables/figures and the text hinder comprehension of the work. There are furthermore technical aspects which seem not optimal. From my point of view the main problems are (see detailed comments below): a) use of CV (name and concept), b) constraining effects to positive values, c) insufficient explanation of the use of CWTs in the predictor, d) missing interpretation of the constant term in the model (seasonality), e) verification of the Gaussian assumption.

The relatively small part on river discharge at the end does not benefit the paper. An introductory part and the results are present in one chapter and does not contain sufficient information on the actual work which was done. I suggest to either leave it out (and maybe write a second paper on it) or extend it appropriately and weave it into the manuscript in a coherent manner.

Overall, the dense network of stations for the IP is certainly interesting, the modelling approach is viable as such but it has not been carried out in a technically sound manner. This raises doubts regarding the results. I recommend major revisions before

HESSD

9, C3712–C3720, 2012

publishing in HESS. Additionally the language is not easy to read, I strongly suggest proof reading the manuscript by a native speaker.

2 Specific Comments

2.1 Title

Is the reconstruction the main topic of the paper? "High resolution" reminds me on a grid but the approach is station-based and for the approach, it does not matter if you have three or three thousand stations. Reading the paper, I would have entitled it something like "Modelling monthly precipitation sums with circulation weather types for a dense network of stations ..."

2.2 Abstract

- giving the number of stations is not really a "resolution"
- "stepwise linear regression model with forward selection" does not well specify the model. The crucial information needed is the Gaussian assumption for the response (monthly precipitation sums) and the frequency of CWTs as predictors, as well as the independent treatment of stations and month.
- the coefficient of variation (CV) is commonly defined as $CV = \sigma/\mu$, the ratio of the standard deviation and the mean for a random variable. Without reading the rest of the paper and understand that you redefine this concept in your work using a relative error, I could not understand the abstract.
- is it fair to promise a monthly precipitation field when having only station-based data?

HESSD
9, C3712–C3720, 2012

2.3 Introduction

In the introduction as well as in the rest of the text, some sentences are difficult to understand, e.g.

- "This explains the generalized recommendation of high density precipitation which requires a database for regional analyses."
- "The model selected was a forward stepwise linear regression derived from that of Trigo and DaCamara (2000) and of Goodess and Jones (2002)." Forward selection is the approach you followed, right? Standard linear regression with CWTs as the predictors is the model, if you wish.

The discrepancy between daily CWTs and monthly precipitation is not resolved in the introduction which is probably puzzling for many readers at this stage.

You might want to consider another paper modelling precipitation with the CWTs, not for the IP, however [Maraun et al., Extremes 13:133-153 (2009)] or as an overview for statistical modelling of precipitation [Maraun et al., Rev. Geophys. 48:RG3003 (2010)].

2.4 Precipitation data

Some information on the gaps in the series would be helpful, or if gaps have been filled this would also be interesting to know.

2.5 Circulation weather type classification

If one does not know the approach used here, it is not easy to understand. Instead of pointing to a plethora of literature using the CWTs, you might want to spend more time

HESSD

9, C3712–C3720, 2012

on explaining them. A set of equations and some classification criteria (exemplary if you wish) additionally to Fig.2 could be very helpful here. Furthermore the occurrence of the individual CWTs for the individual months as a bar plot could be helpful. You promise to give similar information in Tab. 3, where I did not find it.

2.6 The model

Even if it is seemingly simple, you should explicitly write down the model as a regression equation, mention that the response is the expectation of a Gaussian random variable, namely the monthly precipitation sums and explain what is actually used as predictor (I suppose it is the days of occurrence of the certain CWTs relative to the length of the month). This makes clear that the predictors are dependent, they should sum up to one (or total days in the month).

You mention "The forward selection criteria". To my understanding, forward selection is an approach to predictor selection, i.e. choosing those factors which are relevant for describing the response (precipitation). I would NOT call this a "criterion". In order to perform this selection, you do however need a *criterion* to measure improvement of the model due to addition of a new predictor; generic criteria are Information criteria as the AIC, BIC, etc. or a likelihood-ratio test or even a cross-validation experiment. All these criteria somehow involve the model complexity, i.e. the number of parameters (predictors) used. The idea is to find an optimal trade-off between "model error" and model complexity (Occam's Razor). The before mentioned criteria do this. I do not see how your criterion the "CV" does that since it is not clear what a significant improvement of the model is. Your ad hoc value of 0.01 seems arbitrary to me and without any theoretical foundation. I browsed through the new edition of Daniel Wilks text book (I think you referred to a previous edition) and could not find a hint to your criterion, please cite the chapter you are referring to. A short explanation on why you are using this measure of relative error would be helpful. If it is only to make the RMSE comparable

HESSD

9, C3712–C3720, 2012

between stations, this is already a valid reason.

As far as I understand, you constrain all model parameters to positive values. Although your reasoning for that seems plausible (positive contribution of rain by a CWT) it is not meaningful in the regression context. There is a constant offset for each month and the parameters (coefficients) associated to the CWT presence modify this constant offset either towards larger values for wet CWTs bringing rain or towards smaller values (negative coefficients) for dry CWTs. This artificial constrained might mess up the whole modelling approach.

To my understanding, you use a constant offset for each month, thus there is a seasonal signal modelled by this constant, I think this interpretation is worth mentioning. Another efficient approach could be to consider only monthly precipitation sum anomalies and combine the data of three or four month to one season.

What about the residuals? Are they sufficiently close to the Gaussian assumption made? A plot (e.g. QQ-plot) would be nice to illustrate that. The distribution of precipitation, at least on a daily scale, is usually skewed and assumptions other than Gaussian are used, e.g. Gamma for a daily scale [Ambrosino et al., J. Climate 24:4600-4617 (2011)].

Leave-one-out cross-validation is not the same as jack-knifing, the latter is commonly used to estimate the bias of an estimator, while the former is used for model validation (cf., e.g. D. Wilks textbook) .

“measure model validation” sounds strange to me. In the context of model validation one can measure the model performance, fitness, quality, etc.

2.7 Model validation

It is not clear to me how the cross validation experiment was performed. Please add a sentence explaining that briefly. Something like: for each station and each month a

HESSD

9, C3712–C3720, 2012

leave-one-out cross-validation experiment was carried out. One data point (i.e. one month) has been separated, the model parameters are estimated for the remaining data points and the model performance for the data point left out has been calculated using ... This procedure is repeated until all data points have been left out once.

An interesting piece of information would be the bias of the model, since the linear correlation coefficient does not report this. It is, however, included in the RMSE and thus in your "CV". Significance for the correlation coefficient should be reported.

Recall that you model the mean of the response variable and you expect a dispersion around this value (residual standard deviation). This dispersion should be reported, at least in Fig. 9 as error bars.

You mention that the residuals are "normally distributed around the null value" but you also say that "the width of the left half is usually twice the width of the right half". This is contradictory. If the last statement is true, your modelling assumption is not even approximately fulfilled.

You refer to column A in Tab. 3 which is missing. Column B (not indicated as such) shows "the percentage estimation of precipitation by WTs over the total observed monthly precipitation." A more consistent approach in my eyes would be to divide by the total predicted precipitation to obtain the contribution of a WT.

2.8 An example of reconstruction of long term monthly precipitation in the IP

As far as I understand, you "reconstruct" (hindcast) monthly precipitation for three stations as an example. I could not find further reconstructions presented in this manuscript. Reconsidering the title in this light, I find it not being well chosen.

Furthermore, I would expect confidence bands/error bars for the "reconstruction".

HESSD

9, C3712–C3720, 2012

2.9 River flow modelling

Just after the results of your main point and just before the discussion, there is a new chapter. It starts in the tone of an introduction which I found very irritating here. Since the river flow modelling part is not sufficiently described but only briefly, I recommend to take it out.

2.10 Discussion and conclusion

Here, you start with: “The circulation weather type classification devised by Trigo and DaCamara (2000) has been successfully applied to reconstruct and validate monthly precipitation for the 56 yr of the period 1948–2003 at 3030 Iberian site locations ...”. As mentioned earlier, I found the reconstruction for only 3 sites in the paper.

3 Tables and Figures

Some results presented in the form of tables could be as well presented as figures (e.g. bar plots) which I consider as preferable. For example Tab. 3 and the missing “Column A”.

Label text is too small in Figs. 3a and 3b.

How is the spatial interpolation realised in Figs. 6 and 7? Why is that used here? You could have presented it in the same way as Figs. 4 and 5. I don’t see added value.

Figs. 9 and 10 have too small labels and there are no confidence bands. Helpful for a validation would be a scatterplot (predicted vs observed).

HESSD

9, C3712–C3720, 2012

4 Language

Please consider letting a native speaker proof read your manuscript. Not being a native speaker myself, I refrain from pointing out semantic and syntactic problems.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6935, 2012.