**Hydrology and
Earth System
Sciences
Discussions**

Interactive
Comment

Interactive comment on "Copula-based statistical refinement of precipitation in RCM simulations over complex terrain" *by* P. Laux et al.

P. Laux et al.

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We would like to thank the referee for this constructive comments and suggestions to improve the quality of the manuscript. The comments are addressed in detail below and the manuscript is revised accordingly.

REFEREE #3: This manuscript introduces a copula-based approach for downscaling of regional climate models. The subject matter fits the scope of Hydrology and Earth System Sciences and, in my opinion; it sustains the interest of a relatively large audience. I believe there are several issues that should be addressed before the manuscript is ready for publication. I recommend publication pending a major revision. The Authors can find below more specific comments:

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Comments: -My main concern is that some of the discussions and key arguments are not backed up by appropriate analysis/references. For example, the analysis provided in Section 4.3 Dependence on Altitude and Distance does not support the conclusions. The authors already acknowledge that "there are just three grid cells available for this inspection which is clearly not significant". This is true and, in my opinion, there are far too few data to gain traction on a possible link between dependence, altitude and distance. On the other hand, 11 out of 14 stations are in quite similar altitudes. I strongly urge the authors to eliminate Section 4.3, or at least, not to go beyond their own statement that "No clear functional dependence between the altitude of the stations and the Copula parameter θ exists".

ANSWER: We agree to the comments of the reviewer. Although we already analyzed more grid cells we decided to cancel section 4.3 as suggested by the reviewer. The whole manuscript is revised accordingly. A more comprehensive study about the dependence on altitude and the distance will be necessary and will be subject of a more detailed study in future.

REFEREE #3: -Section 4.4 Dependence of large-scale weather situation. In this section the implications of the results are not discussed in enough details. For example, the manuscript reads: "The empirical CDFs of observed precipitation in Garmisch-Partenkirchen based on a given WT and certain groups of WTs are illustrated in Fig. 13." Please note that the results should follow a discussion on implication of the results. Otherwise, readers have to interpret the results by themselves. The same comment applies to the following statement: "Both, the wet and the dry Copula density is similar to the unconditional Copula densities (compare with Fig. 8)".

ANSWER: Further comments and interpretations of Fig. 8-10 and Fig. 13 (now Fig. 12) have been inserted.

REFEREE #3: -The manuscript will be improved by just eliminating Sections 4.3 (Dependence on Altitude and Distance) and 4.4 (Dependence of large-scale weather situ-

ation).

ANSWER: As already mentioned we agree to delete section 4.3. However, we think that section 4.4 is very crucial for the paper. The results have been further elaborated, and the implications of the results are discussed in more details (see comment above).

REFEREE #3: -Page 3018, 1st paragraph: I believe that the correlation coefficient should not be the only measure to gauge performance, since the results may be misleading. The correlation coefficient should be followed by other quantitative measures such as RMSE or MAE.

ANSWER: We agree to the reviewer and think that further performance measures are necessary. Therefore, further measures such as e.g. RMSE and MAE are calculated. A subsection "Performance of simulations" (subsection 3.4) is inserted. The results are presented and discussed in the manuscript.

REFEREE #3: -Page 3019: The manuscript reads: "The objective weather pattern classification method of the German Weather Service (Bissolli and Dittmann, 2001) shows only moderate potential to further constraint the model. Including information about the humidity of the troposphere can slightly increase the skill for bias correction compared to the Copula-based stochastic simulations without using large-scale information". This discussion overstates the implications of the results and does not acknowledge the limitations of the study (too few data to make such conclusion). I suggest omitting this statement or providing a more conservative one.

ANSWER: The original weather pattern classification could not be used without preliminary grouping due to the partially small sample sizes of single WTs. However, this problem is addressed by a grouping strategy using three different classes (see section 3.3). We checked the sample sizes of the different subgroups (having on average ∼400 members) and found them large enough for our statistical analyses. However, as the section "Discussion" could be misleading with respect to this aspect, we clarified this briefly and additionally refer to section 3.3 to gain further information.

REFEREE #3: -Table 2 and the corresponding discussion: Given the tail of the data, Clayton copula is not the right type of copula family to be tested here. Instead, the Survival Clayton should be used which has a quite similar tail to that of Gumbel.

ANSWER: We think that the three different Copula models considered are sufficient in the context of this study. The main goal of this study is to prove that the methodology works and provides an alternative to "traditional" methods. Sure, more theoretical Copula models could be tested. However, this is far beyond the scope of this "proof of concept" paper. The authors decided to insert a critical statement in the manuscript (sections section 4.2. "Analysis of the empirical and theoretical Copula models" and "Discussion") about the tested Copula functions and potential improvement of using different models such as Survival Clayton. (see also comment to Referee #1)

REFEREE #3: -Page 3015: "Figure 7 (bottom) illustrates the composite of the three piecewise CDFs for modelled and observed rainfall residuals." Again, the results should be followed by a comprehensive discussion. What does the figure mean? Why is it presented?

ANSWER: The results of Fig. 7 are described briefly. Furthermore, it's implications are discussed in the section "Discussion".

REFEREE #3: -This methodology can be applied on positive pairs (both RCM and observed precipitation > 0); please provide a brief discussion on the drawbacks and limitations of the approach. Note that a significant part of bias may be because of RCM false and missed precipitation.

ANSWER: A new paragraph at the beginning of section "Discussion" is inserted and is devoted to explain the treated cases (1,1). The limitations of our approach, still lacking of the remaining cases are critically discussed in brief. Typical errors in observation and RCM model outputs are addressed.

REFEREE #3: -Page 3003: "For the mid-latitudes, large-scale stratiform events can be

represented well by climate models resulting in a relatively good agreement between modelled (grid cell) and measured rainfall amounts (point scale)." Please provide a reference.

ANSWER: First part of this statement is backed up by two further references; the second part is just the logical implication.

REFEREE #3: -The order of figures is chaotic; please note that figures should be placed in the order they are discussed in the main document.

ANSWER: The order of the figures has been corrected.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3001, 2011.

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