Hydrol. Earth Syst. Sci. Discuss., 11, C5679–C5686, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C5679/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatial extremes modeling applied to extreme precipitation data in the state of Paraná" *by* R. A. Olinda et al.

F. Serinaldi (Referee)

francesco.serinaldi@ncl.ac.uk

Received and published: 15 December 2014

General comments

The manuscript under review presents an application of models for max-stable processes to rainfall data recorded by a raingauge network operating in the state of Paraná (Brazil). The paper does not propose methodological improvements compared with the existing theoretical results on max-stable processes. On the other hand, the application could be of interest. However, in my opinion, the overall quality is quite poor. The methodology is described in a very superficial and unclear way; it is not as detailed as in theoretical papers but also not plain enough as should be required in an applied research paper. In this respect, several papers overlooked in the bibliography (and

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

mentioned below) provide better examples of technically sound but accessible presentations of max-stable processes and models. The application is rather superficial, with vague descriptions of data and results, as well as a questionable setting in a hydrological context. Some references are also misspecified and out of the context. Finally, the very poor language (syntax, grammar, typos) and text editing make the reading rather difficult. To summarize, my opinion about this paper is not positive. As much better examples of application of max-stable processes can be found in the hydrological literature, I refer the Authors to these papers as a guide. Some specific remarks are provided in the following.

Specific comments

Pag. 12733, line 5-10: "A natural approach to such a modeling is the theory of extreme spatial and the max-stable process, characterized by the extension of infinite dimensions of multivariate extreme value theory, and making it possible then to incorporate the existing correlation functions in geostatistics and therefore verify the extremal dependence by means of the extreme coefficient and the Madogram."? Maybe it could be better something like "A natural approach to such a modeling is the theory of spatial extremes and the max-stable processes, characterized by the extension to infinite dimensions of multivariate extreme value theory. This allows the incorporation of correlation functions widely used in geostatistics, and the use of the concept of extremal dependence". More generally, the abstract takes for granted the familiarity of the reader with tools such as Madogram, extremal coefficient, etc. The abstract should be readable, informative, and should mention the overall rationale, key issues (e.g. max-stable processes, in this case), and a summary of the main findings. Using sentences such as "This method is based on the theorem proposed for de Haan and on the models of Smith and Schlather." requires the introduction of the full references in the abstract, which should be avoided if not strictly necessary. Some technicalities should also be avoided as they can be understood only after reading the "Methodology" sections.

P12733-L15: "proposed for" -> "proposed by".

HESSD

11, C5679–C5686, 2014

P12734-L20: “adjusting the data” ? -> “modelling”?

P12734-L25: “Kojadinovic and Yan, 2012” refer to GoF tests not to spatial extremes.

P12734-L26: “Max-stable processes are the well-founded methodology for that Schlather and Tawn (2003)” ? Please, reword.

P12734-L7-10: Please reword. Graeler et al. (2013) do not deal with max stable processes for rainfall (and even for runoff). I’m almost sure because the rainfall data used in that paper were analysed and modeled by myself using the methods described in Serinaldi (2009) and Serinaldi (2010), which do not apply EVA asymptotic concepts and max-stable processes at all, but focus on the whole rainfall process. On the other hand the Authors missed some pertinent (applied and methodological) references such as Shang et al. (2011), Westra and Sisson (2011), Erhardt and Smith (2012), Shaby and Reich (2012), Gaume et al. (2013), Ribatet (2013), Robert (2013), Thibaud et al. (2013), Bacro and Gaetan (2014), Papastathopoulos and Tawn (2014), among others.

P12735-L13-16 is identical to P12734-L2-7: Please avoid copy-and-paste.

Eq (1): Please, change y with another letter to avoid confusion between the series of random variables Y and the maxima of such a series; otherwise, P12736-L16 could not make sense. Moreover, the sentence in P12736-L16 seems to me a bit obscure, if not meaningless: According to EVT, the distribution of the monthly maxima, (Y1,. . ., Yn), and that of the maximum of monthly maxima, max(Y1,. . ., Yn), are equal except for an affine transformation, if we assume GEV holds true (and vice versa). From a practical point view, the distribution of the monthly maxima Yi is what we estimate (as the max of monthly maxima is just a single value in the record period). I guess that the Authors mean that the distribution of monthly maxima (and thus of max of monthly maxima) can be assessed without knowing the parent distribution of all (extreme and not extreme) daily rainfall values (assuming that such a parent distribution exhibits a suitable behaviour). If it is so, please clarify. In the present form, the text seems to me a crasis of results concerning block maxima and max-stability postulate.

HESSD

11, C5679–C5686, 2014

P12736-L17: Eq. 2 describes the cumulative distribution function, and not the probability density function

P12736-L22-23: Why putting an applicative reference here? Please reword, or better, move in the introduction or somewhere else, where some examples of application of univariate EVT can be listed. Moreover, in this context it could be better to mention some more general papers such as Katz et al. (2002), or the recent book "Extremes in a changing climate" (2013, Springer), or Salvadori et al. (2007).

P12737-L2: "...be a sequence of i.i.d. random variables of the maximum monthly rainfall at K-dimensional observation from a distribution function F, in our study, K represents the number of meteorological stations, i.e., K = 232"?? Please reword (e.g. "be a K-dimensional random vector of n i.i.d. observations; in this study, K represents the number of meteorological stations, i.e., K = 232, and n the number of years")

P12737-L5.13: please reword this section. Referring to degeneracy is not useful in this context. My point is that the presentation is not deep enough to justify technicalities (which do not help the overall understanding), and simultaneously it is not plain enough to provide an informal but informative introduction to key concepts. The Authors should make the effort to avoid to simply shorten standard presentations (already available in theoretical papers), and try to communicate the rationale in a plain language. In this respect, the Coles' book is a nice example of blending between theoretical stuff and a plain presentation accessible to no statisticians. Dealing with max-stable processes, Thibaud et al. (2013) provide a good example.

P12737-L15-18: Please, reword paying attention to grammar, syntax and punctuation, which is used quite randomly throughout the text, thus making it not very easy to read.

Section 2.3: As for Section 2.2, the presentation is almost uninformative. If the Authors choose a "statistical paper" style they should provide all elements to make the discussion understandable. Otherwise, if they opt for a plain presentation suitable for the HESS audience, they cannot simply shorten statistics-oriented introductions removing

HESSD

11, C5679–C5686, 2014

key elements without which the discussion is not informative at all.

Sections 2.4-2.5-6: As for the previous sections, these presentations are really poor. Concepts are introduced superficially with no links to each other. In the present version, it is not possible to understand how the different parts of max-stable theory and modelling framework are put together. The papers mentioned above provide examples of more suitable introduction of such a theory in applied research. In particular Ribatet (2013) provided a very nice introduction of theory and inference methods. The other papers give shorter treatment but suitable for hydrological journals. Moreover, as already mentioned, the low quality of grammar and syntax does not help.

P12742-L19: "...=E...=Var"? E[·] and Var[·] of what? See e.g. Thibaud et al. (2013) or Ribatet (2013).

P12743-L13-17: It seems that the Authors use 34 years of monthly maxima (4 months from January to April); which years? Please, pay more attention to the presentation of the data.

P12747-L1-6: Please, reword.

P12747-L1-6: From an agricultural standpoint, in my opinion, information about daily rainfall maxima over monthly time windows is not very informative. What matters in agriculture is monthly and seasonal rainfall accumulation (or average). This is why, for instance, drought indices have time scales larger than one month, and seasonal forecast is a key issue in water management to set up irrigation policies, reservoir scheduling, water abstraction, etc. Modelling daily maxima is a nice exercise from a statistical point of view but with little application, at least in the context discussed in this study. Concerning raingauge network design, the reported statements are a bit too vague. I suggest having a look to Rietsch et al. (2013) (among others), for instance, just to have a more grounded idea of the real-world problems posed by network design and corresponding technology. Moreover, concerning the effect of space-(time) correlation on the information available from a network, sometimes things are not so

HESSD

11, C5679–C5686, 2014

straightforward (see e.g. Matalas and Langbein, 1962).

P12748-L8-23: In my opinion, this discussion about the properties of observed rainfall should be moved at the beginning of the section, where the data are presented (please, consider to create a sub-section describing the dataset). Please, clarify which maxima are shown in Fig. 7 (annual maxima at daily scale? Monthly scale? The maximum value over each decade? Please, be clearer in presenting data and results... Please, use measurement units in every figure's key and/or in captions. Some lack of accuracy is allowable in journals where the application is just an example, but a bit less in engineering and applied research journals, where the application has a more important role.

P12749-L10-29: In my understanding, Fig. 9 shows the empirical process Z* versus the modelled Z* (accounting for spatial dependence); however, I cannot see where the independent process described in the text (P12749-L17-20) enters this comparison. Please, provide a more accurate description.

Section 4. Conclusions are too vague and do not reflect/describe the findings. Conclusions should summarize facts (reported in the main text) based on new theories (if any) and results (supported by tables, diagrams, or logical reasoning). A conclusive paragraph with perspective and future developments is fine, but is not sufficient by itself as a "Conclusions" section.

Technical corrections

As mentioned above, in my opinion, the overall presentation needs a deep revision concerning grammar, syntax, typos, materials' organization and description, as well as a better setting in the hydrological context.

References

Bacro J.-N., Gaetan C. (2014) Estimation of spatial max-stable models using threshold exceedances, Stat Comput, 24, 651–662

HESSD

11, C5679–C5686, 2014

Erhardt R.J., Smith R.L. (2012) Approximate Bayesian computing for spatial extremes, Computational Statistics and Data Analysis, 56, 1468–1481
Gaume, J., N. Eckert, G. Chambon, M. Naaim, and L. Bel (2013), Mapping extreme snowfalls in the French Alps using max-stable processes, Water Resour. Res., 49, doi:10.1002/wrcr.20083
Katz, R.W., M.B. Parlange, and P. Naveau, 2002: "Statistics of extremes in hydrology", Advances in Water Resources, V. 25, pp. 1287-1304
Matalas N.C. and Langbein W.B. (1962) Information content of the mean, Journal of Geophysical Research, 67(9), 3441–3448
Ribatet M. (2013) Spatial extremes: Max-stable processes at work, Journal de la Société Française de Statistique, 154(2), 156-177
Rietsch, T., P. Naveau, N. Gilardi, and A. Guillou (2013), Network design for heavy rainfall analysis, J. Geophys. Res. Atmos., 118, 13,075–13,086, doi:10.1002/2013JD020867
Robert, C.Y. (2013) Some new classes of stationary max-stable random fields, Statistics Probability Letters, 83(6), 1496-1503
Salvadori, G., C. De Michele, N. T. Kottegoda, and R. Rosso (2007), Extremes in Nature: An Approach Using Copulas, Springer, Dordrecht, The Netherlands.
Shaby B.A., Reich B.J. (2012) Bayesian spatial extreme value analysis to assess the changing risk of concurrent high temperatures across large portions of European cropland, Environmetrics, 23(8), 638–648
Shang, H., Yan, J., Zhang X. (2011) El Niño–Southern Oscillation influence on winter maximum daily precipitation in California in a spatial model, Water Resources Research, 47, W11507, doi:10.1029/2011WR010415.
Papastathopoulos I., Tawn J.A. (2014), Dependence properties of multivariate max-stable distributions Journal of Multivariate Analysis 130 (2014) 134–140
Thibaud, E., R. Mutzner, and A. C. Davison (2013), Threshold modeling of extreme spatial rainfall, Water Resour. Res., 49, 4633–4644, doi:10.1002/wrcr.20329
Westra S., Sisson S.A. (2011) Detection of non-stationarity in precipitation extremes

using a max-stable process model, Journal of Hydrology, 406, 119–128

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12731, 2014.