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Hydrology and Earth System Sciences Discussions

Interactive comment on "Low-frequency variability of European runoff" *by* L. Gudmundsson et al.

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LOW-FREQUENCY VARIABILITY, LONG-TERM VARIABILITY AND HURST-KOLMOGOROV DYNAMICS

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I wish to add my contribution to the discussion about terminology. On the occasion, I also wish to add a few more comments about some other issues of the interesting and useful paper by Gudmundsson et al. (2011a). I offer these comments in a constructive attitude, and I wish the authors consider them as contribution to the discussion rather than as a formal intervention of the handling Editor.

Terminology is important in science and it is good that Reviewer 1 started this discussion. On the other hand, I agree with Gudmundsson et al. (2011a,b) that "between-

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year variability” may be misleading and that “low-frequency variability” is an established term.

Interestingly, the authors use the term «“low-frequency variability” to describe the general phenomenon of temporal variability at scales longer than one year». From their definition, in companion to their reference to atmospheric science, as the domain where “low-frequency variability” is used, one may incorrectly imply that the “general phenomenon” they speak about is something new to hydrological sciences. However, the truth is that the “general phenomenon” is first introduced in hydrology and is mostly known as the “Hurst phenomenon” due to Hurst (1951) who pioneered its observation and study. I was astonished not to see any reference to Hurst, who was a hydrologist, and to his breakthrough paper, which has been quoted in many disciplines including geophysics and atmospheric sciences, as well as electronics, informatics, economics and many more. Some of the references in Gudmundsson et al. (2011a), e.g. Mudelsee (2007) explicitly mention the “Hurst phenomenon”.

Another common name of this behaviour is “long-term persistence”. The authors propose as an alternative term the “long-term variability”. Persistence and variability are seemingly different notions but in fact they are the same thing (Klemes, 1974; Koutsoyiannis, 2002, 2003, 2006, 2010). Another name that is more recently being used (e.g. Koutsoyiannis, 2011) is “Hurst-Kolmogorov (HK) behaviour” or “HK dynamics”, to give proper credit also to Kolmogorov (1940), who was the first to propose a mathematical model to describe this behaviour (which he devised when studying turbulence) and also to demote “phenomenon” and a possible connotation that this very common and regular behaviour is “phenomenal”.

Since there is a large body of literature about the HK behaviour, formulations in Gudmundsson et al. (2011a) such as «However, little is known about the relative importance of long-term variability as compared to annual or sub-annual variability» are not accurate and should be replaced, I think, with a description of the HK context and references to relevant studies.

The fact that Gudmundsson et al. (2011a) used a formalization different from that typically used in the HK framework, in which the large-scale variability (or persistence) is quantified through the Hurst coefficient, H, does not change the essence that they study the same behaviour. Their "fraction of low-frequency variance" is an interesting metric and provides some useful information. On the other hand, I think it has some weaknesses, which I would advise the authors to discuss. The decomposition of a stochastic process into three additive components seems to me arbitrary and subjective. The seasonal component has a conceptual basis and a rather objective quantification. Yet the cyclic seasonality cannot be expressed by an additive component. One can see this by inspecting a time series Y, defined as the difference of the seasonal component from the original series, i.e., Y := X – X_Seas. The periodicity should still be present in Y; for example, one may readily see that the correlation between January and February in Y is equal to that in X, so that Y should be treated as cyclostationary rather than as stationary (i.e. the same as X). But the biggest problem seems to be the arbitrariness in the partitioning of Y into X_Long and X_Resid. One can imagine from Fig. 2 that an infinite number of smoother or rougher "red lines" could be found for X_Long and, accordingly, the "grey line" for X_Resid would be rougher or smoother, respectively.

I am not suggesting that the authors should change their formalization and necessarily adopt one of those typically used in Hurst analyses. I am just suggesting that they should discuss the weaknesses of their approach.

Another issue that confused me is related to the two hypotheses that Gudmundsson et al. (2011a, pp. 1708, 1709) set forth and eventually accept as valid. How can the temporal evolution of Q_Long depend on the atmospheric forcing, on one hand, and, on the other hand, the variance of Q_Long not depend on the atmospheric forcing but on catchment processes? If the atmospheric processes drive the long term variability of runoff, how can they not influence the long term variance? My questions concern also the analyses in section 4.2, 5.1, 5.2, and the Conclusions.

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With respect to Fig. 4, I have difficulties to believe that the percent of variance represented/explained by the low-frequency component in temperature is only 4% at most and is about 1% in Central Europe, as indicated in the figure. As a counter example, the authors could see an analysis of temperature in Vienna (Fig. 3 in Koutsoyiannis, 2011) where the long-term variability is prominent (Hurst coefficient 0.74). Perhaps the reanalysis data used in the study (instead of using gauged data) have distorted the long term variability? Perhaps a comparison of analyses with measured vs. reanalysis data at a few locations would be informative.

I found the section 3.3 (Spatial patterns of simultaneous variations) too brief and difficult to understand. The authors provide some references to other works, but I think it would be useful if they could explain in more detail the entire methodology and its particular elements (e.g. what is Procrustes analyses?). Explanation is provided only for the geodesic distance with the help of the illustration of Fig. 3. Again, I am not sure if I understood it correctly. What do the points in the figure represent? In reality, is the index set a set of discrete points, as in the figure, and not a continuous space? What happens for a continuous index set?

A final issue concerns data availability. I checked the two web sites linked on p. 1709 and I was able to locate some data from the Global Runoff Data Centre, which however are at monthly scale; Gudmundsson et al. (2011a) mention daily runoff data. Can these be accessed from the web and how? In contrast, I was not able to locate any data on the EU-WATCH web site. Could the authors provide a more explicit link to the data?

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