

Interactive comment on “Hydrological response in the Danube lower basin to some internal and external climate forcing factors” by I. Mares et al.

Anonymous Referee #1

Received and published: 1 August 2016

Overview The paper presented here aims at investigating the influence of *natural* (this not emphasized in the title) forcing factors on Danube river flow: temperature, precip, a drought index, climate indices (internal climate variability), Earth's magnetic field and external solar forcing. The Quasi-Biennial Oscillation (QBO) is also used in order to make the link between solar forcing, internal climate variability and flow variability. Lots of results are presented which results in a very rich paper that is eventually properly organized. I am wondering, though, whether it would have been more appropriate to break down this paper in two distinct smaller ones: one dealing with finding predictors of Danube flow (provided this has not been realized before...), the other addressing the issue of solar and geomagnetic forcings + QBO potential impacts on weather regimes associated to hydrological variations. I have the feeling these two parts could have been better related to each other. Careful attention has been paid to statistical signif-

[Printer-friendly version](#)

[Discussion paper](#)



icance issues which is particularly important as the approach conducted is based on correlation only. The methodology used is overall rigorous, the research and results are interesting and certainly would deserve publication. Yet, the paper suffers from several flaws that in my opinion need to be considered before it can be accepted for publication in HESS. Below are some comments that hopefully will serve as a basis for improving the paper. Many suggestions and revisions are also provided in an accompanying PDF file (annotated version of the ms).

General comments: - Overall, the quality of the English language needs to be drastically improved in my opinion; as is, it is sometimes difficult to understand what the authors want to tell. Sentences are most of the time too long and complex; as a result they can be very often confusing (again, due to the English language). It also looks like the paper was not enough revised before submission, as lots of typo errors, missing (or sometimes extra-) words can be found in too many places throughout the text. This could have been avoided, whatever the english skills of the authors. Along with the amount of data processed and the difficulty to link the two parts (predictors, external forcing), language issues make the paper a little confusing overall.

- As their approach is only statistical, the authors should moderate their interpretations in terms of "response" of a climate index to solar forcing, or "impact" of solar forcing on hydrology or climate indices (e.g., finding a 10.7-14-yr peak in a power spectrum doesn't make a physical link with 11-yr solar cycle!). Comments in this sense are also provided below and in the annotated paper. If physical interpretations are to be provided, the discussion must be supported by a much more extended use of consistent references of the literature.

- Abstract would need one or two introductory sentences presenting the aims and hypotheses to be tested.

- Data: more details should be given on the geomagnetic signal used (data, why it is important to take it into account, recall its physical influence expected on weather

[Printer-friendly version](#)

[Discussion paper](#)



regimes and hydrological variables...). It should be explained more precisely why QBO is used and why it is considered solar/geomagnetic data (as QBO is presented in this section)?

- It would have been interesting to show the geomagnetic and solar activity time-series.
- Spectral analysis: the authors need to provide information about the choice of reference background noise, it is not enough to cite the Mares et al. 2016 EGU abstract. For example, what AR(1) process was chosen as background noise?
- Since there are many possibilities for smoothing data, it might be interesting to mention why Butterworth filters were preferred.
- Cross-correlation graphs in figures 9, 10 and 11: confidence levels should be represented as dashed lines (like on spectra graphs) instead of using small arrows. Cross-correlation functions should be represented over a longer period of lag time (e.g. 15 years or higher) instead of only 5 years, for instance between lag -1 or -5 and lag 15. Time units on figure 10b should be indicated (yr). From figure 10b it seems like either Q_ORS lags solar flux or solar flux lags Q_ORS: as a result the links emphasized are certainly not straightforward, as well as the causal relationship (the "response"). What is the contribution (in terms of discharge amount) of the 9-15-yr component of Q_ORS (here the series are normalized)? Also, MTM spectra of Danube discharge should be shown and discussed.
- In several places it is stated that the hydrological signal responds to geomagnetic or solar signals: I think this should be avoided as the study only deals with correlation, no matter the robustness of the statistical tests conducted. For instance, lines 472-485: I don't think it is correct to conclude to a response (i.e. physical and causal link, here) of a climate index to solar forcing just because 2 peaks arose surrounding the 11-yr solar activity periodicity. Again, it should be kept in mind that no physics is accounted for here, where all is just a matter of correlation.

Technical/minor comments: See annotated PDF.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-304/hess-2016-304-RC1-supplement.pdf>

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-304, 2016.

HESSD

Interactive
comment

Printer-friendly version

Discussion paper

