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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrology of the Po River: looking for changing patterns in river discharge" *by* A. Montanari

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1 Premise

I am very much grateful to the Editor and the Referees for the professional management of the review process of the discussion paper. I found the remarks of the Referees accurate and very constructive. I am indebted to them for their help and support. In the revised version of the paper I am acknowledging their useful advice.

In my response to each of the Referees' remarks the text from the reviews is quoted in italic.

2 Reply to Anonymous Referee 1

I am grateful to Referee 1 for the careful assessment of the paper. My reply to his/her comments follows here below.

1) p. 6691, lines 15-17: What is being defined here as a flood in the Po River basin? Is there a threshold approach which determined the definition of a flood event or a geomorphic feature which defined the floods in this basin that are referenced in this paragraph?

In the revised manuscript I am providing an assessment of the severity of the recorded floods. Accordingly, the following new sentence is introduced: "The history of the Po River floods is well known. In fact, starting from the middle age the lands surrounding the river were intensively cultivated. As a consequence, high river stages significantly impacted local communities since that time and were thus recorded. The magnitude of the river flows for ancient events is not known and therefore it is difficult to assess the severity of the recorded floods. By observing their frequency, one may assess that events with about 5-year return period were recorded".

2) p. 6692, lines 7-8: The authors may want to consider referencing Cohn and Lins (2005) Geophysical Reseearch Letters paper titled, "Nature's style: Naturally trendy" (Vol. 32, L23402, doi:10.1029/2005GL024476).

I thank the Referee for the suggestion. It is indeed appropriate. The above reference is included in the revised paper.

3) Section 3.1: Although an earlier description of the anthropogenic influences in the basin is given for the Po River, please provide some additional detail of the anthropogenic influences in the tributaries that were examined as part of this study.

The following sentence was introduced in the revised paper: “The human impact along the tributaries is mainly due to urbanisation, river training, artificial reservoirs and water withdrawal for irrigation and civil use. River training and water storage/withdrawal may have a significant impact on the occurrence of floods and droughts. However, a visual inspection of the river flow data did not provide any clear evidence of the presence of non-stationarity during the observation period. Further information on the above tributaries can be found in the Water Resources Protection Plan of the Italian administrative regions Piemonte (available online at http://www.regione.piemonte.it/acqua/pianoditutela/tutela.htm, in Italian) and Valle D’Aosta (available online at http://www.regione.vda.it/, in Italian)”.

4) p. 6695, lines 9-17: The text in this section is a bit awkward. Could you report the p-values associated with these tests? I had to reread these sentences several times to understand the point that is being made here. Consider moving lines 19-26 to the beginning of the paragraph.

I agree with the Referee that reporting the p-values provides a more comprehensive information and therefore I amended the manuscript accordingly. In particular, the above sentence now reads: “However, it should be noted that the above trends are scarcely relevant from a statistical perspective. In fact, assuming that the data are independent and assuming that the null hypothesis of no trend is true, the p-values for the slope of the linear regressions are 11% and 26% for annual maxima and minima, respectively”.

5) Section 3.2, last paragraph: This is a very important point that is made by this manuscript.

I thank the Referee for appreciating one of the original remarks that are made in

the paper.

6) p. 6699, lines 1-4: I would think that there would be memory associated with streamflows on the same day of previous years, particularly if there is a strong seasonal signal in the data. I wonder if the author could add more explanation as to why one would expect no memory.

In the interannual analysis, memory is computed on the observations collected in the same day of previous years. We agree with the Referee that, in the presence of a strong seasonal signal, one would expect that the average of the observations collected in each calendar day follows the signal itself. In other words, one would expect that data collected in the same calendar day are similar. However, memory is more than that. Memory means that, for a given period of the year, a significant departure from the seasonal value implies the likely presence of an analogous departure in the same period of the next years. In other words, if one observes an unusually (with respect to the seasonal value) high sequence of flows during, say, the month of April of a given year, then the same outcome would be expected in the month of April of the next years. In my opinion, such an effect is not expected and it is not easy to explain. However, I agree that the above sentence might be misleading and therefore in the revised paper I changed it to: “Intuition suggests that anomalouos events observed in a given day of the year should not impact what will occur in the same day in subsequent years, ...”.

7) p. 6693, line 14: Remove “an”‘. The error was corrected.

3 Reply to Prof. Demetris Koutsoyiannis

I would like to thank very much Prof. Demetris Koutsoyiannis for the careful and thoughtful review of the paper. My replies to his comments follow here below. Quotes from the review are reported in italic.

1. The long-term variability may be more accurately described by the term "fluctuations" rather than the term "cycles" often used in the paper. Rigorously speaking, these are not cycles, because the time length of fluctuations varies.

I fully agree and changed the wording in the revised paper accordingly.

2. The term "memory" used throughout the paper is common in the related literature and has become a standard. However, I believe it is a misnomer and a misleading term. As explained in Klemes (1974) and Koutsoyiannis (2002), this behaviour, whose effect (not cause) is the high autocorrelation, could be interpreted as "change" that results in "absence of memory" or "amnesia", rather than "memory". In my view, terms better than "memory" are "persistence" and "dependence", while terms better than "negative memory" (an expression which looks absurd) could be "negative dependence" or "antipersistence". But this is just my view and I do not insist (noting, though, that the kind reference to my papers with the term "remember" in the phrase (p. 6692) "river flows may remember their past for a very extended period (Mudelsee, 2007; Koutsoyiannis, 2003, 2010)" may not be accurate).

I fully agree with Referee. In the revised version of the paper, I changed "memory" to "persistence" and "negative memory" to "antipersistence". I also changed the terminology in the sentence that is mentioned by the Referee.

3. I think that the water balance of the catchment, as summarized in Fig. 3 and

pp. 6692-6693 needs some clarification: (a) Some references would be useful. (b) I would suggest replacing the unit "10^9 m^3" with the equivalent and simpler "km^3". (c) I guess the "discharge" of 47 km^3 shown in figure is the "surface outflow" (to the sea). (d) For the control volume shown in the figure, the sum of outflows is 47 + 22 = 69 km^3; it seems the "groundwater withdrawal" is already included in the "civil and industrial use" and in the "irrigation", whereas the latter two seem to be parts of "evapotranspiration" and "discharge". If this is the case, then there seems to be a deficit of 78 – 69 = 9 km^3 considering the entire control volume, which includes the surface and subsurface water. Is perhaps this deficit a subsurface outflow to the sea or to adjacent catchments? Also, I am not sure if the partial balances of the two parts, surface and subsurface, close to zero.

The concerns of the Referee are well motivated. In the revised paper, I provided a reference for the information summarised in Figure 3. I also replaced the units as suggested. The term "Discharge" in Figure 3 is an abbreviation for the terminology "River discharge at the outlet" that is used in the text. As for the hydrological balance, in the revised manuscript I am clarifying that the input of 78 km^3 is compensated by the discharge (47 km^3), groundwater recharge (9 km^3) and evapotranspiration from vegetation (20-25 km^3). The groundwater withdrawal is compensated by evapotranspiration after irrigation and by civil and industrial use (part of it evaporates and part of it originates return flow to the river and therefore is included in the 47 km^3 of discharge). There is a term of deep percolation (about 1 km^3) and some direct groundwater flow to the sea which is not quantified. In the revised Figure 3, which is included below for illustration purposes (herein it is indicated as Figure 1), I am including explicitly the percolation term, but I am not including the direct groundwater outflow to the sea which is not quantified.

The above volumes are approximate estimates, as I clarified in the text. As for the hydrological balance, one should note that the uncertainty of the estimates does not allow one to reach a perfect closure (see, for instance, Montanari, A., Interactive comment on “Bringing it all together” by J. C. I. Dooge, Hydrol. Earth Syst. Sci. Discuss., 1, S29–S30, 2004). Moreover, the groundwater balance should not necessarily close to zero, as there might be some depletion of groundwater resources. In the revised manuscript I am explicitly allowing some uncertainty in the evapotranspiration estimate, which is given in the revised text and Figure 3 as 20-25 km^3. The revised sentence reads:

“In detail, the aforementioned average volumes of annual precipitation feeds the river discharge at the outlet, the annual inflow to the underground aquifer (approximately 9 km^3) and evapotranspiration from vegetation (approximately 20-25 km^3). The withdrawal from the acquifer is about 6.5 km^3, thus revealing that groundwater resources are close to overexploitation (deep percolation is about 1 km^3 and there is some groundwater flow to the sea). Thus the margins to ensure future sustainability of groundwater resources are limited, especially during years with lower than average rainfall. The annual water withdrawal for irrigation, which contributes to evapotranspiration, is 17 km^3, while water withdrawals for industrial and civil use amount to 5 m^3, 80% of which being withdrew from groundwater. Most of this latter flux evaporates and part of it contributes to the return flow to the river‘‘.

4. Some more clarification is used for the construction of climacograms of Fig. 8. Were the series somewhat “deseasonalized‘‘ before estimating variances? If not then perhaps the low slopes for scales < 100 days reflect more the periodicity of the annual cycle rather than correlation. Also, it would be useful to see in comparison, in the same graphs, plots derived from the annual series (for time scales > 1 year) in which the effect of seasonality disappears.

I fully agree with the Referee that the presence of seasonality may have an impact on long term persistence estimation. Actually, the series were not deseasonalised before estimating variances. However, I believe that the annual series are too short (about 100 observations at most) in order to derive useful indications. In my opinion, it is better to include in the same graphs the analysis of the original and deseasonalised

daily series. Therefore, in the revised paper I changed the graphs by including, in the same picture, the diagrams for the deseasonalised series. Moreover, I reworded the paper to comment the new results. In my opinion the general conclusions of the analysis do not change much.

5. p. 6690 “the top observed values in Italy of minimum, average and maximum daily river flow, that are 275 m^3/s . . .‘‘. Could the author clarify that “top‘‘ means “largest‘‘ in all three.

I agree with the Referee. The wording was changed accordingly.

6. p. 6691. The definition of “hydro-ecoregions‘‘ is not clear enough. What does “limited variability‘‘ mean?

I substituted “limited variability‘‘ with “limited range of variation‘‘. I also provided the reference Wasson et al. (1996) for the definition of hydro-ecoregions.

7. p. 6691. “story of the Po River‘‘ -> “history of the Po River‘‘. Also, in “The 1705 flood is remembered . . .‘‘ is the author sure that “is remembered‘‘ is a suitable expression here? What about “registered‘‘? Furthermore, the author could consider changing the Roman numbering of centuries to Arabic (throughout the entire paper; a few readers may have some inability in reading Roman numbers).

I agree with the Referee and changed the wording accordingly.

8. p. 6693, “The overall situation depicted in Fig. 3 reveals an intense exploitation of water‘‘. Perhaps it could be mentioned that, since the major part of precipitating water outflows to the sea, the situation is far from critical and there is margin for further exploitation.

In the revised paper I am now mentioning the possibility to further reduce the river outflow to the sea. The revised sentence reads: "The overall situation depicted in Figure 3 reveals an intense exploitation of water resources that is currently sustainable on average, as we previously mentioned, but it is potentially problematic during drought periods. Increasing artificial water storage and water withdrawals for irrigation is an option that has been considered. However, concerns have been expressed about reducing the river outflow to the sea, which would exacerbate water quality problems along the river reach and the coastal areas.

9. p.6694, "loess" -> "LOESS" (this should be an acronym).

I agree with the Referee and changed the terminology accordingly.

10. p. 6695. Could the author clarify if the confidence levels given are calculated assuming independence or otherwise mention the assumption made.

In the revised paper I am not referring to confidence levels anymore, but rather to p-values as suggested by Referee 1. I specified that these latter are computed under the assumption of independence.

11. p. 6696, "25 and 10 days along the intra-annual and inter-annual direction". Is it meant "adjacent values" instead of "days" (i.e. 25 days and 10 years)?

The Referee is correct. In the revised paper I am using the term "adjacent values".

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 6689, 2012.

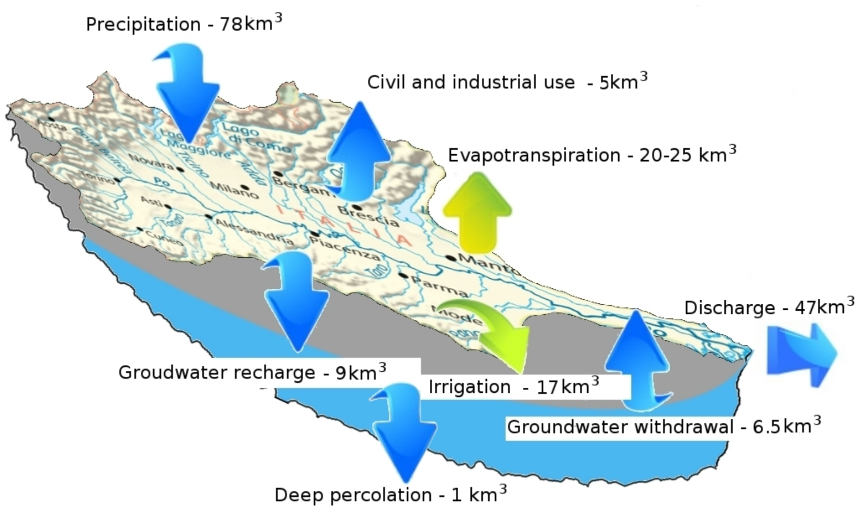

Fig. 1.