

Interactive comment on “River flow forecasting with Artificial Neural Networks using satellite observed precipitation pre-processed with flow length and travel time information: case study of the Ganges river basin” by M. K. Akhtar et al.

Anonymous Referee #1

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This paper presents some sound and interesting attempts at exploring and progressing the long awaited transition of neural network hydrological forecasting and prediction into the more challenging domains of "spatial modelling" and "distributed catchment processing". The authors report a novel process that uses a number of pre-processing mechanisms and stages to convert two important distributed spatial datasets into a set of lagged neural network input drivers: the different datasets comprising a digital elevation model of their catchment and a set of satellite-derived computed rainfall input

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grids. The trialling and testing of their reported methodological developments is nevertheless applied to a single catchment such that the uniqueness of their established experimental findings remains open to question: to what extent can their case specific results and conclusions be generalised to other situations; would similar results be produced if their method was applied to another catchment of a similar nature or perhaps transferred to one that had different properties? It could be the case that more studies are planned: perhaps the authors could comment on such matters. If not, trialling and testing of the proposed method under different scenarios might indeed be considered as a natural "next step" forward. The method is however applied to a large river catchment and this accomplishment is in itself useful since such explorations extend the limited traditional scope and range of most published neural network rainfall-runoff forecasting applications.

The authors in attempting to discover the best individual model would nevertheless appear to have been lured into the trap of not pursuing the strongest and most pertinent directions of scientific probing or investigative reporting. The potential merits of their rainfall input method in the reported material is overshadowed due to the "prisoner effect" of including two previous observed discharge values as inputs: something that is of particular concern in the case of shorter forecasting horizons since it introduces a phase shift error, that is not easily detected with traditional hydrological metrics such as the ones that were adopted, or can in fact be resolved by means of standard neural network training algorithms - as stated in the text. The authors present a strong focus on delivering superior neural network forecasting models but from a scientific viewpoint might perhaps instead have developed and implemented a more comprehensive set of experiments for assessing the potential pros and cons of their rainfall input method: for a more detailed discussion on such matters see Koutsoyiannis (2007). This suggested expansion of reported material to provide a more balanced account of operational issues would hopefully turn out to be more constructive than destructive in supporting and encouraging interested parties to consider the potential uptake of such novel concepts in a broader methodological or subject domain context. The rainfall inputs for

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example could be tested as independent drivers in their own right so as to establish their real potential contribution, or usefulness, over and above that arising from their implementation in combination with past discharge input drivers. Full model runs up to and including 10 steps ahead might likewise be reported across the board so as to ensure that interested scientists are provided with a reasonable portrayal and one that is based on a rational set of both good and bad effects related to each particular method. This matter is of particular importance in the recommended investigation of "rainfall without discharge" modelling, since it might well be the case that past discharge inputs over longer forecasting horizons are depressing the level of model output skill. The innovative approach that is reported in this paper could also be benchmarked in a more stringent manner: a multiple linear regression equivalent might perhaps be used to depict a set of neglected nonlinearities and in so doing demonstrate the overall extent to which the problem that is being modelled represents a complex nonlinear situation (Abrahart & See, 2007); and, if at all practical, perhaps some sort of traditional quasi-physical based model of the river might also be developed such that it would be possible to distinguish if one or other of such approaches offers a superior product for operational forecasting purposes. The authors did not discuss past neural network modelling strategies for developing and incorporating lagged rainfall inputs or in fact consider including for assessment some equivalent numerical counterparts: one or several sets of weather station rain gauge records are often employed, sometimes as an assortment of independent input records, but at other times amalgamated and used in some combined manner as a single individual driver. Most past scenarios used a single rainfall input: Campolo et al. (1999) however used distributed rainfall measured at several rain gauges; whereas Dawson et al. (2006) used a set of peripheral catchment weather station records. The relevant modelling advancements are thus presented "out of context": such that the quantified testing of their reported procedures against past published methodologies will remain outstanding and is a matter that would still need to be addressed at some later point. I am also left wondering if it was logical or sensible to include a full set of simultaneous inputs that covered ev-

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everything up to and including a period of 50 time lags since signal attenuation over time and space could well introduce a lot of noise into the associated drivers such that the neural network modelling process might get confused or is perhaps overwhelmed. It might be more logical to instead develop a series of tests either adding or removing inputs in some sensible order - as occurs in traditional statistical "stepwise multiple linear regression". I would also recommend the inclusion of assessment metrics that can detect phase shift error such as the Persistence Index (Kitanidis & Bras, 1980). It is possible to calculate this metric and a suite of other relevant hydrological metrics in a standardised manner and on a simultaneous basis for multiple model outputs using HydroTest (www.hydrotest.org.uk: Dawson et al., 2007; 2009).

Some additional information would also be helpful. The authors state that low flow records are not modelled. How are high and low defined in such cases? The authors also state that their training and testing datasets had good agreement: please provide a full set of supporting traditional statistical descriptors for the input and outputs variables contained in each subset. The temporal plots are a bit confusing since continuous records are depicted but as reported in the text all low flow records were removed - so are the gaps in each series bridged over?

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