Hydrol. Earth Syst. Sci. Discuss., 5, S1749–S1753, 2008
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Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability"" *by* D. Koutsoyiannis et al.

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Received and published: 6 November 2008

Koutsoyiannis et al. (2008) give strong opinions on a great number of subjects. I will only comment on section 3 as it is the section that is most relevant to the hydrological sciences.

Let us be frank. Koutsoyiannis et al. (2008) have good will and great intentions. However, they got carried away in a zeal of scientific exuberance. Their discussion in section 3 revolves around the need for hydrology to "reinvent itself within a new paradigm and radically rethink its fundaments, which are unjustifiably trapped in the deterministic

myth of the 19th century and the illusive promise of uncertainty elimination (p. 2937, l. 8)." I don't think we should throw the baby out with the bath water. There are some good features of deterministic models that I think we should retain.

Koutsoyiannis et al. (2008) seem to have a dogmatic, binary world view and it is in this world view I believe they are trapped. A binary world view of EITHER physically based deterministic models that allow a "full description of the detailed physics of the hydrological cycle using mechanistic model structures and first principles" (p. 2934, l. 18), OR fully probabilistic models that mimic the behaviour of chaotic dynamical systems based on data, with no attempt to represent cause-effect relationships. The world is more diverse than that. There are so many shades and hues in representing nature by models. The shades and hues make modelling an art and they make models really useful.

We have had the discussion on physically based hydrological models before and, from my vantage point, this discussion has been resolved at the end of the nineties. Here it comes again - the fundamentalist philosophical versus the pragmatic positions. The classical exchange of thoughts is probably Konikow and Bredehoeft (1992): "the terms validation and verification have little or no place in ground-water science; these terms lead to a false impression of model capability" versus De Marsily et al. (1992): ".. using the model in a predictive mode and comparing it with new data is not a futile exercise; it makes a lot of sense to us. It does not prove that the model will be correct for all circumstances, it only increases our confidence in its value. We do not want certainty; we will be satisfied with engineering confidence." These discussions have made enjoyable reading for myself and many others, and have certainly contributed to a more careful use of models (Refsgaard and Henriksen, 2004).

I happen to have taught a course on hydrological modelling today, and when presenting deterministic flood forecasting models and statistical flood frequency methods, one student enquired why there is such a redundancy - why are there two alternative methods for the same problem of flood estimation? My response was that deterministic models

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represent cause-effect relationships which is what is often needed, both in science and engineering. If one is interested WHERE the water flows, the assertion that the flow paths are fractal is hardly useful. If one is interested whether flood levels in 12 hrs are such that evacuation measures are necessary, the annual exceedance probability of floods is not helpful either. Deterministic models are needed for this. The reasoning also goes the other way round, and statistical methods are much better suited for other purposes such as risk management and probabilistic design. There are horses for courses and I would strongly recommend the authors to have a broader look at the plethora of methods that exist in hydrology. Model choice depends on the problem and the data, and the choice of deterministic vs. probabilistic models is no exception.

I can agree with the authors' view that the uncertainty of hydrological models is often understated and the validity of climate impact studies is often oversold. Also, there is a clear need to represent large range dependence in hydrological behaviour and many models do not have the ability to do this. But, notwithstanding the practical usefulness of statistical methods, the understanding that comes from them may be limited - the notion of randomness indeed precludes cause-effect relationships. And identification of "cause-effect relationships" is usually what is considered as understanding. The use of quantum physics, incompleteness results and similar notions as METAPHORS (p. 2936, l. 5) does not sound like cause-effect relationships to me.

It is unfortunate that in their zeal of promoting one particular subset of statistical methods Koutsoyiannis et al. (2008) seem to misinterpret some of the literature. On p. 2935, l. 3 they state that "the aspiration of achieving pure DETERMINISTIC modelling .. has been "officially" formulated in the framework of the IAHS Decade on Prediction in Ungauged Basins (PUB; Sivapalan et al., 2003)." It would be interesting to know what makes them think so. The word "deterministic" does not appear in Sivapalan et al. (2003). In fact, since its inception the PUB decade has sparked development of a wide variety of new methods, including statistical methods.

The aims of Koutsoyiannis et al. (2008) are noble and the misinterpretation of the

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literature is clearly unintentional but nevertheless obvious. Carried away by a binary world view, they suggest that, in the PUB decade, uncertainty is to be ELIMINATED (p. 2935, l. 13) while the aim of PUB is that uncertainty is to be REDUCED: "The PUB scientific programme focuses on the estimation of predictive uncertainty, and its subsequent reduction, as its central theme." (Sivapalan et al., 2003, p. 857).

It is ironic that conclusion nr 5 of Koutsoyiannis et al. (2008) is almost identical with the aspirations of the PUB decade. Koutsoyiannis et al. conclude that the ".. uncertainty in all scales will necessitate new theoretical and methodological approaches to allow for the design and management of the engineered systems" as well as the "development of a new hydroclimatic theory that will recognize the structural character of uncertainty" (p. 2938, l. 8, 11). The PUB science plan has very similar wording on p. 878: "The PUB activities … , it is hoped, will lead to new predictive approaches based on a combination of current and new theories, and of existing and potentially new data sets .." Sounds as if Koutsoyiannis et al. had something to contribute to PUB.

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