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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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My first reaction to the opinion paper by Koutsoyiannis et al. (2008) [henceforth, K-op] was, "these people grabbed the bull by the horns". Regardless of whether one agrees with the views of the authors, or not, I believe that this kind of paper truly contributes to any field, for it prompts re-thinking about "accepted truths". Just for this service the authors ought to be congratulated. K-op address many complicated, and important to society scientific issues. I wish to add a few comments, mainly related to hydrology, to an already stimulating discussion.

On climate change and sustainability:

Whereas I share the scepticism of the authors about the reliability of the results of climate change research, the great inertia of the earth-system makes me think that precautionary measures are sensible as an insurance policy for the future. Figure 1 in K-op shows a slightly downward trend of global temperature in the short-term, but I also give credence to the physics of the greenhouse effect (not to press report of ice sheets melting everywhere). On the other hand, the discrepancies between observations and hindcasting GCM results for temperature displayed in Fig. 2 are so alarming that it is reasonable to question the uncertainty-laden predictions of future climate. In my view, the diverging climatic simulations by various GCM models do not attest to reliability. Climatic models should not be simply calibrated, but also validated with data not used for calibration (the "falsification" of Popper, often cited by Koutsoyiannis and his co-workers).

Uncertainty is inherent in any prediction and should be estimated. In climate change, we are far from the "engineering confidence" of de Marsily et al. (1992) (cited by Blöschl, 2008). We may not be in a position to gauge the uncertainty involved, but we should get a sense of it. K-op argue convincingly that the best way to explore this, generally admitted, uncertainty is in probabilistic terms by "statistical methods admitting stationarity along with long-term persistence". We should study how uncertainty propagates from the climatic forcing to the hydrologic response. We will then learn if, e.g., the natural variability of streamflow is greater (due to long-term persistence, as K-op argue) than that induced by climate change, or not. Yet, other uncertainties may be greater still, and may involve parameters characterising the IPCC scenarios, e.g. demographics (implied by K-op) and technological adaptation.

Counting on your tolerance, I will risk a social comment. At the same time that we are concerned about our future as a fossil fuel based society, we should grapple with sustainability as a conflict-causing moral issue. Sustainability may not be used as an argument for imposing upon our fellow men living in developing countries to be satisfied with less because, in our greed, we took more than our fair share, damaging the Earth (not that China, India, Brazil and other countries would let this happen, mind you).

On hydrology, uncertainty and risk:

The ability to describe a physical phenomenon deterministically fascinates us, perhaps, because such a mechanistic thinking has dominated the education in the natural sciences of many of us. But determinism exerts a strong attraction also because it explains in a straightforward logic how nature operates, in a broad sense, and is thus invaluable (the milestone book by Eagleson, Dynamic Hydrology (1970), is still important today).

Instead of attempting to decipher whether stochastic models link cause and effect in the same way as deterministic models do, I would like to approach model characterisation and selection heuristically, using examples. My focus here is on an appropriate model for a particular problem. Such a model is parsimonious. For example, it suffices to consider that light propagates as rectilinear rays in conventional optics, whereas the wave nature of light is necessary to explain refraction; Newtonian mechanics suffices for football, safely ignoring relativistic effects, whereas the latter are essential in elementary particle physics. K-op and all discussers agree that the use of diverse models is justified in hydrology. Perhaps, then, semantics caused apparent rather than actual disagreements.

From a practical viewpoint, a sharp, first-principles based deterministic description of a hydrologic process satisfies because the parameters involved have physical meaning, are thus observable and can be estimated statistically through field tests (e.g., pumping tests) or sampling (e.g., inference of streambed roughness from sieve curves of streambed material). Hence, in my view, determinism is not limited to, e.g., the Navier-Stokes equations, in which appear the material properties density and viscosity. It also embraces, for instance, the 1-D equations of St. Venant or the equations governing groundwater flow, which have composite derived parameters with physical meaning

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(a streamflow resistance coefficient, hydraulic conductivity and a measure of aquifer storativity).

Nature is, of course, heterogeneous, for which reason the specification of the requisite parameters often hits on insurmountable obstacles that one may choose to overcome by applying stochastic methods, with statistical tools; stochastic theories of subsurface solute transport is a relevant example. However, does the choice of a problem-solving approach define the character of a process? For instance, the phenomenological laws describing diffusion and dispersion rely on probabilistic concepts, yet their form is deterministic. Again, we are able to estimate their parameters a priori only within a coarse interval of uncertainty; for example, the value of a stream‘s dispersion coefficient can be estimated roughly from macroscopic hydraulic quantities, and better via tracer experiments. In their reply to Montanari (2008), K-op classify the formula of Manning (for a natural stream) as a statistical/stochastic law because it has theoretical shortcomings and is experimental, i.e., empirical (yet empiricism is the origin of many scientific results later embedded in theory). I believe that such a classification, within the stochastic/indeterministic paradigm advocated by K-op, may create confusion. The goal is better appreciation of uncertainty. This can be achieved by promoting the reality of uncertainty in water resources education, linking it, among other things, to natural heterogeneity, and through that to the notion of scale.

I concur with K-op that models represent reality incompletely. Therefore, their results are inherently inaccurate and uncertain, and become more so when model parameters are ill defined or poorly estimated. Science aims to reduce that uncertainty. Scientists and engineers do so by using either better models (a reductionist would turn, for instance, to a higher-dimensionality model) or more and better data (stochastic-statistical preference), or both. A main advantage of the stochastic approach is that it is inherently capable of quantifying uncertainty. Yet, by driving deterministic models with stochastic inputs or by using sensitivity analysis, we can also quantify uncertainty, albeit indirectly. In that sense, deterministic and probabilistic viewpoints mix, and may

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complement each other.

I share K-op's reservations about investing too heavily in "deterministic descriptions and models" as well as their trust in data (and use of probability and statistics). However, one may miss the target looking through only one eye (for example, the probability distribution of the ages of a population leads to the conclusion that we have a slight, but finite chance of living almost indefinitely). In my view, in the absence of data, the combination of a simple deterministic-conceptual model with a data regionalisation technique offers a better chance of getting reasonable answers than either element alone.

I wish to close with a personal note on style. I am not disturbed that K-op "give strong opinions" (Blöschl, 2008). Indeed, I welcome it and submit to you that the only opinions worth stating in a public forum are the strong ones, those that can stir up controversy. One is then in the excellent company of the great pre-Socratic philosopher and founder of the dialectic method Heracleitos (6th-5th century BC), who wrote in one of his, lamentably few, surviving Fragmenta "polemos pater panton", "war is the father of all" (war is to be interpreted as controversy, dispute, challenge, conflict etc.). After all, scientists are not diplomats; what matters in science is the strength of clearly articulated arguments. Demetris Koutsoyiannis and his co-workers excelled on this count. I enjoyed this opinion paper.

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