Hydrol. Earth Syst. Sci. Discuss., 6, C3040–C3045, 2010
www.hydrol-earth-syst-sci-discuss.net/6/C3040/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "*HESS Opinions* "A random walk on water"" *by* D. Koutsoyiannis

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Received and published: 5 January 2010

I very much enjoyed reading the paper by Koutsoyiannis (2009) and the review by Weijs (2009). I think they provided an outstanding contribution to the subject of predictability and a very insightful interpretation of the role of stochastic and deterministic hydrology. I think HESS readers will greatly benefit from reading Koutsoyiannis (2009), in particular because the interaction and difference between deterministic and stochastic descriptions was not correctly interpreted in some recent hydrological contributions. The toy model Koutsoyiannis (2009) used is a great example for perceiving the role of uncertainty in hydrology. I therefore highly recommend publication of this excellent paper on HESS.

In compiling this review I made an effort to identify some critical remarks. It was not easy for me, because I am a convinced supporter of the use of probabilistic approaches for estimating uncertainty in hydrology and therefore I greatly appreciate the scientific contributions by Demetris Koutsoyiannis. However, I am also convinced that the authors' work greatly benefits from constructive reviews and therefore I identified what I consider two potential weak features of Koutsoyiannis (2009), which I try to describe here below.

I fully agree with the main message conveyed by the paper, while I have some doubts about the effectiveness of the way the message is brought forward. In some instances I think Koutsoyiannis (2009) is too strong. In my opinion, the implicit classification of approaches in the two categories considered by Koutsoyiannis (2009), namely, "good" (determinism) and "evil" (randomness), was only rarely done in hydrology. In fact, hydrologists were always aware about uncertainty and nowadays the concept of uncertainty is always associated to any hydrologic prediction, without being considered "evil" or something to eliminate. I agree that uncertainty was, and still is, frequently underrated in geosciences but not as much in hydrology and therefore hydrologists may find the above classification a bit biased. It is true that uncertainty is often associated to lack of understanding (but this is justified to some extent) and it is true that hydrologists are used to estimate uncertainty by also following alternative approaches to probability. However, this does not necessarily mean that uncertainty is improperly dealt with. We hydrologists certainly need a clarification of the meaning of frequentist probability, Bayesian probability and alternative approaches to estimate uncertainty (see also Montanari et al., 2009) but I do not think hydrologists generally failed in perceiving the role of physical understanding for reducing the uncertainty.

A second concern that I have is that the paper is not easy to follow in some sections and this may prevent its complete understanding. Do we really need the complicate probabilistic description that Koutsoyiannis (2009) excellently summarises in the Sections 3, 4 and 5? Would not be a sensitivity analysis preferable? I am convinced that the final result would not be much different. The question is very important for advisors and students, because a student cannot learn anything. Is the integration between deterministic and stochastic approaches feasible for a student? If a sensitivity analysis provided acceptable results, would not it be preferable? As a matter of fact sensitivity analysis makes use of the deterministic description without neglecting the theoretical and practical role of uncertainty. For what reason we should always prefer probability? I often stick on probability myself but I believe there are instances where alternative approaches might be preferable.

I would like to further explain my two concerns above by providing an example of how Monte Carlo Simulation and sensitivity analysis could be an effective alternative to probability for the toy model by Koutsoyiannis (2009). This implies that (a) the use of alternative approaches to probability is not necessarily equivalent to classifying randomness as "the evil" (this is related to my first concern above) and (b) a simple reiterated application of the deterministic approach would be a candidate substitute of the complex stochastic approach proposed by Koutsoyiannis (2009) (this is related to my second concern above).

As a matter of fact, the presence of uncertainty in the predictions provided by the toy model by Koutsoyiannis (2009) is clear to any hydrologist. As Koutsoyiannis (2009) rightly pointed out, uncertainty is related to model structure (which could include the initial conditions if they are not observed), model parameters and observed data (which could includes the initial conditions if they are observed). Let us suppose, as Koutsoyiannis (2009) did, that all the uncertainties can be neglected but the uncertainty in the initial conditions and let us suppose, coherently with Koutsoyiannis (2009), that uncertainty in the initial conditions is known. Therefore it is possible to apply the GLUE method (Beven and Binley, 1992), which I consider a special case of Monte Carlo simulation and sensitivity analysis. GLUE is frequently criticised for not being coherent with probability theory (Mantovan and Todini, 2006), but the criticism is related to choice of the formal likelihood therein used. When the formal likelihood is correctly specified GLUE is indeed coherent (Stedinger et al., 2009);

when the limits of acceptability approach is used (Liu et al., 2009; Winsemius et al., 2009) GLUE is coherent with a (subjectivist) Bayesian approach (see, for clarification, http://en.wikipedia.org/wiki/Bayesian_probability).

Application of the GLUE approach is carried out by (1) randomly sampling N initial conditions from the related probability distribution (uniform distribution over a specified range, accordingly to Koutsoyiannis (2009)), with N sufficiently large; and by (2) running the model for each of the N initial conditions therefore obtaining N outputs for any lead time. The range of the outputs would increase with increasing lead time as a consequence of the presence of uncertainty. Limits of acceptability can be fixed on the model predictions accordingly to expert knowledge if applicable, and model runs can be rejected as non-behavioural if they do not respect the limits themselves. The collection of the behavioural outputs would allow one to estimate a probability distribution for the model prediction therefore providing a quantification of uncertainty. Under the above hypotheses (the same assumed by Koutsoyiannis (2009)), if N is sufficiently large and limits of acceptability are consistently fixed the GLUE approach converges to the true probability distribution. The advantage of the GLUE approach is its simplicity and the possibility to explicitly incorporate expert knowledge through the limits of acceptability. This last opportunity is particularly useful when enough information is not available to reliably infer the uncertainty in the initial conditions. In this respect, the alternative approach could present some advantages with respect to probability. The disadvantage of the GLUE approach is related to its computing requirements and its subjectivity which implies a careful and informed selection of its subjective choices.

The above approach does not imply downgrading the role of uncertainty and does not imply classifying randomness as “the evil”. I think it is a convincing example that alternatives to probability are possible. The interested reader may refer to Zadeh (2005) for a description of more alternatives like fuzzy set theory, possibility theory and many others.

The selection of a simpler approach (in this case GLUE) is not to be thought as driven by laziness. In this respect, I very much appreciated section 5 ("Prediction vs understanding") in Weijs (2009), where the opportunity to select models with the smallest complexity is excellently justified. Also, that section clarifies the role of understanding in the context of physical predictions (and simulations) in a very convincing way.

I have a final and very minor remark on Koutsoyiannis (2009). The author seems to imply that the separation of deterministic and random dynamics as additive components is inappropriate. I believe it would be advisable to specify that under certain assumptions such disaggregation is justified. Actually, there are numerous examples (the author correctly cites the ARMA models) where this kind of separation is properly used.

Once again I would like to express my very high opinion about Koutsoyiannis (2009). I think it will be a highly cited paper.

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HESSD

6, C3040–C3045, 2010

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