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Hydrology and Earth System Sciences Discussions

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

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ON ALTERNATIVES TO PROBABILITY

I thank Alberto Montanari for his kind and thoughtful commentary. He is so kind that, while his own contributions to probabilistic and stochastic approaches to hydrology are very well known, he tries to change perspective and praise non-probabilistic approaches for the sake of a better dialogue. He tries to find weak features in my paper and he asserts that in some instances I am too strong when I criticize some common practices in hydrology. After reading the review by Montanari (2010) and after thinking of the points he raises, I became more confident that the points he criticizes as weak are strong enough and that I may not be strong enough in my own criticisms.

Montanari states “In my opinion, the implicit classification of approaches in the two

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categories considered by Koutsoyiannis (2009), namely, 'good' (determinism) and 'evil' (randomness), was only rarely done in hydrology." Perhaps Alberto I and live in different "hydrological worlds". Perhaps he has not received reviews suggesting that his stochastics papers are simply useless mathematical exercises offering no insight, not improving understanding, having no relationship with physical mechanisms. I have. Perhaps he was not criticised for using unnecessary mathematics in his papers. I was. Perhaps he has not read hydrological papers aspiring elimination or radical reduction of uncertainty, as if uncertainty was an "enemy" or "evil". I have, and I certainly regard uncertainty a positive quality, without which our world would not be liveable. Perhaps he has read different hydrological books from what I have. For in their majority, the books I have read promote the dichotomous logic and reductionism that I describe in Koutsoyiannis (2009). I believe that such logic is so common and widespread in our education that has become our "second nature". Even Montanari (2010, in the third line of his review) speaks of "stochastic and deterministic hydrology". As implied in Koutsoyiannis (2009), I think that such terms as "stochastic hydrology" and "deterministic hydrology", which are in common use, convey an incorrect message of dichotomy. For example, I would propose replacing "Stochastic Hydrology" with "Hydrological Stochastics" or the periphrastic term "stochastic methods in hydrology" – and for the same reason I applaud the term "Hydrologic Statistics" used by Maidment (1993) in his celebrated "Handbook of Hydrology".

Montanari states 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" and continues "I do not think hydrologists generally failed in perceiving the role of physical understanding for reducing the uncertainty". If he thinks that physical understanding can reduce uncertainty, then his view that I am biased is understandable. However, I am trying to show by means of calculations with the toy model, and not speculations, that physical understanding may not help reduce uncertainty. I show that, even in a simple model of a ridiculously simple system with fully known deterministic dynamics, uncertainty is not reducible. Thus, I believe that, in fact, uncertainty is underrated in hydrology in the worst possible manner: by thinking that it is reducible and that it is a negative quality. I state in Koutsoyiannis (2009): "Apparently, what makes the system alive is the same agent that creates the uncertainty. Only dead systems are certain – and this might be useful to recall when thinking to eliminate uncertainty". Perhaps this was not strong enough to express the fact that Change and Uncertainty are tightly related to each other and to Life, and thus uncertainty is a positive quality. I think, thus, it is appropriate to add a paragraph in the final version of my paper describing the positive character of uncertainty – and I also thank Soon (2009) who in his review pointed this out.

But I agree with Montanari that hydrology, in contrast to other geophysical disciplines, has had an important contribution in stochastic methodologies, which enhanced stochastic world views (cf. Hazen, Hurst, Matalas, Wallis, Mandelbrot, Klemes, to name a few of the older contributors). However, statistically, the recent trend, as far hydrological stochastics is concerned, is negative, and we should not "hide the decline"... The most impressive regression is the blind following of the climate industry by the majority of hydrologists, and the adoption of controversial deterministic future climate predictions. This, despite using some statistical type of downscaling approaches, is in my opinion a full departure of stochastic thinking. Today's hydrological community has less knowledge of stochastics than two decades ago, whereas investment has been done on alternative approaches, as discussed in Montanari (2010) and below.

Indeed, the main point in Montanari's (2010) review is about approaches alternative to probability. Initially, Montanari points out that "[my] paper is not easy to follow in some sections and this may prevent its complete understanding." and wonders "Do we really need the complicate probabilistic description that Koutsoyiannis (2009) excellently summarises in the Sections 3, 4 and 5?" While I tried to make the paper as easy to follow and self-contained as I could, I must accept that he is right that it is not easy to follow. His review gives an indirect proof for that: Montanari proposes an "alternative" approach that he calls GLUE (p. C3043), i.e. "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." Perhaps he overlooked that this is more or less the same with what I have done in fact in the paper. I refer to the following part of Koutsoyiannis (2009): "The Monte Carlo method is very powerful, yet so easy that we may fail to notice that we are doing numerical integration and that there is some concrete mathematical background (Eq. 3) behind our simulations. . . . It is so very simple that it even bypasses the calculation of Sˆ−1(A). Results for the density function f_i(x) of the system state x (soil water) for time i = 100, in comparison with that for time i = 0, are shown in Fig. 7. The Monte Carlo integration was performed assuming f(x_0) to be uniformly extending 1% around the value x_0 = (100 mm, 0.30) and using 1000 simulations.".

Evidently, then, I do not propose anything novel or anything different from what Montanari has in mind. But I try to show that the Monte Carlo method, which we may intuitively use in our every-day problems, has a strong theoretical background. The Monte Carlo method is au fond a numerical integration method and it has been originally proposed as such (Metropolis and Ulam, 1949). In the example in Koutsoyiannis (2009) it is used for the solution of an integro-differential equation, which is derived by some reasoning, by reformulating the dynamics from its deterministic version to a stochastic version, which explicitly derives the probability density of future states.

One may wonder, then, why derive the integro-differential equation, or why mention it at all, if one can just obtain the same result by intuitive use of the same method. My view is that intuition and theoretical reasoning are both important and if the two are in agreement, as happens in this case, we may feel happy (although I must note that even this case may have some points to worry about our intuition, which need further study; cf. the discussion in William M. Briggs' blog http://wmbriggs.com/blog/?p=1269&cpage=1#comment-12955). The problem is when

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the two are in disagreement, whence certainly we should prefer the theoretically correct over the intuitive answer. In all cases, the final word is given by theory. A strong theoretical background makes a strong science. Being conscious of the theoretical background, results in most efficient use of the method and in an acceleration of scientific progress. For the toy model example, one could readily exploit several theoretical results mentioned in the paper, i.e. that (a) the characterization of uncertainty demands calculation of probability; (b) the latter calculation is au fond a high-dimensional numerical integration; (c) the speed of Monte Carlo integration does not depend on dimensionality; and (d) for a number of dimensions $d > 4$, the Monte Carlo integration method is more accurate (for the same total number of evaluation points) than classical (grid-based) numerical integration. By making use of these results one would immediately decide to use a stochastic method as the best integration method for this case, avoiding unnecessary search for improper methods. Also, being conscious of the theoretical background is a great help to avoid confusions and errors. I could mention several errors (even in hydrology) obtained by lack, ignorance or misuse of theory, but it is not my indent in this paper to demonstrate the importance of theory. Rather, I take it as a presupposition that theory is important.

Montanari seems to disagree with my statements “Some tried to banish probability from hydrology, replacing it with deterministic sensitivity analysis and fuzzy-logic representations. Others attempted to demonstrate that irregular fluctuations observed in natural processes are au fond manifestations of underlying deterministic dynamics with low dimensionality, thus rendering probabilistic descriptions unnecessary. Some of the above views and recent developments are simply flawed because they make erroneous use of probability and statistics, which, remarkably, provide the tools for such analyses.” Montanari wonders “Would not be a sensitivity analysis preferable? I am convinced that the final result would not be much different” and later on “The interested reader may refer to Zadeh (2005) for a description of more alternatives like fuzzy set theory, possibility theory and many others.”

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Apparently, this discussion could be a long one and I do not wish to include it into the paper; I think what I have already mentioned (as quoted above) is enough for the scope of the paper (I will only add a reference to Koutsoyiannis, 2006, about the misuse of probabilistic and statistical tools to derive flawed conclusions about deterministic dynamics in hydrological processes). I think that the debate between supporters of probabilistic and non-probabilistic approaches to uncertainty has been broadly presented, mostly in the so-called "artificial intelligence" literature, rather in hydrological literature. Since Montanari cites the one "camp" (Zadeh, 2005), I would add a couple of references to those who defend probability on mathematical and technical grounds (Cheeseman, 1985) and even on philosophical grounds (Piscopo and Birattari, 2008). As evident in my article, my personal position emphatically favours probabilistic approaches. Instead of using controversial (albeit fancier and more trendy) concepts, I prefer to "stand on the shoulders of giants" such as those I have quoted in my article: Bernoulli, Laplace, Maxwell, Boltzmann, Gibbs, Kolmogorov, Metropolis, Ulam, Jaynes, including Popper as far as philosophical implications are concerned.

Reading Montanari (2010) I found two arguments favouring a non-probabilistic approach: (a) "... because a student cannot learn anything. Is the integration between deterministic and stochastic approaches feasible for a student?"; (b) "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." For the second argument, "I believe there are instances..." my counterargument is "I do not believe"... I have presented my ideas by means of mathematics and numerical examples, and I do not think one's beliefs are relevant. We need to have a similar detailed example to see whether or not a non-probabilistic approach "might be preferable".

But regarding the first argument, I am in agreement with Montanari that probability is a difficult path for a student – but at the same time I strongly believe it is feasible to learn. I always feel as a student (and I think we all are), which shows that probabilistic approaches and their integration with deterministic dynamics are feasible for students.

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Perhaps most criticisms of probability and most supporting of alternatives reflect the difficulties with probability. Eventually, probability demands a different way of looking at things – and that is why it is difficult. A random variable is a different object from a regular variable, and a stochastic process is different from a time series. If x is a regular (real) variable, then the expected value of, say, its square xˆ2, equals xˆ2 (i.e. E(xˆ2) = xˆ2) but if it is a random variable then this expected value equals a constant (i.e. E(xˆ2) = mˆ2 + sˆ2, where m is the mean and s is the standard deviation). A student may find it weird that an expression E(xˆ2) is not at all a function of x in the latter case. In most hydrological texts, no distinction between random and regular variables is done, and certainly this creates confusion to students.

But I really believe that the effort to conquer the probabilistic concepts is rewarding. Once a student has assimilated the difference of the mathematical objects in probability theory with respect to the common algebraic objects, and once he has attained to climb "on the shoulders of the giants" I mentioned, he achieves a broader view of nature and natural processes (an "overstanding" type of view, to refer to the relevant point I am trying to make in Koutsoyiannis, 2009). Also, he understands the scientific method, because, according to the quotations from Laplace, Maxwell and Jaynes I offer in Koutsoyiannis (2009), the logic of Science is probability. No more and no less than this. Obviously, I do not support the modern trend to follow easier and fancier paths in the education of students. Rather, I believe that modern reforms of education have had detrimental effects.

One last point I wish to discuss, to clarify my view, is about Bayesian approaches. Montanari (2010) seems to imply that Bayesian approaches might be one of the alternatives, which in turn implies that they are different from probabilistic approaches. He also seems to identify "Bayesian" with "subjectivist". Specifically, I wish to clarify that, in my view, Bayesian approaches certainly are probabilistic approaches and that they are not necessarily subjective (except its abused version known as "Bayesian beliefs" as I mention in the paper). At least they are no more subjective than any other scientific notion. May I remind that, even the coordinates of a point in a Euclidean space depend on the "subjective" selection of the coordinate system. Also, the velocity of a body depends on the "subjective" choice of the frame of reference: a car whose speed gauge displays 110 km/h, moves with a velocity of 10 km/h according to an observer in another car moving in front whose speed gauge displays 100 km/h, but it moves with a speed of 210 km/h if the observer's car goes to the opposite direction. This "subjective" velocity would be clearly reflected in a hypothetical crash of the observer's car with the observed car. The same car has a velocity of about 30 km/s for a hypothetical observer on the Sun, and so on. The Coriolis force, with which hydrometeorological models explain atmospheric motion, is "subjective" in the sense that it does not exist at all for the hypothetical observer on the Sun. In all cases, whenever the information that we have about the observer changes, the numerical value of the observed object, or even its very existence, changes accordingly. Why, thus, do we need to give emphasis to "subjectivity" just in Bayesian statistics and not elsewhere in Science? Jaynes (2003), in order to direct attention to constructive things and away from controversial irrelevancies, invents an imaginary being, a robot, which is given the available prior information and data, and reasons according to certain definite rules. Apparently, an assignment of prior probability in Bayesian statistics depends on the prior information (state of knowledge) that we have. Prior information is more subjective whereas data are more objective. The Bayesian framework is not about adhering to prior information, on highlighting "subjective expert knowledge", or on blending "Bayesian beliefs". Rather, it is about transforming prior information into posterior probabilities by using the evidence contained in the objective data.

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