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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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ADDITIONAL REMARKS ON THE COMMENT BY STEVEN WEIJS

I have already posted a first reaction to the thoughtful Comment by Weijs (2009). As this one is my last reaction to all reviews, I wish to say that it was a very positive experience, perhaps an exception to the routine, to receive such constructive and insightful discussions within the peer review process. I wish to thank Steven Weijs for offering this discussion. I am glad that the other reviewers also discuss some points of Weijs's review, which also demonstrates the usefulness of the transparent and public character of the peer review process in HESSD. I am also glad to hear that Weijs is currently working on a paper further exploring the problems of deterministic predictions (from the last statement of his Comment).

I view most parts of his Comment (sections 5 and 6 in particular) as complementary to my paper (Koutsoyiannis, 2009a) and I agree with the spirit expressed in these (e.g. the definition of understanding, the contrast of prediction vs understanding, with the importance given to the former, and statements such as "understanding is often overrated"). I also think Weijs is right in his statement "Maybe we just have to conclude that randomness and determinism can emerge from each other and which one dominates depends on scale in general and not just on time". Therefore, in the revised version of I will rephrase the relevant statement to indicate that time scale of prediction is also relevant and I will include a discussion about how determinism emerges from randomness (I will come back to this below).

Naturally, my reply will be focused on points of disagreement. I will start with some minor disagreements, which refer to phraseology or terminology. Thus, in Weijs's statement "D. Koutsoyiannis discusses determinism and randomness as two coexisting components of natural processes in time, identifying stochasticity with unpredictability", I would not use the word "components", which points to parts that constitute something. Rather, I would replace it with "qualities". Also, I would avoid saying "uncertainty is not a physical property of the system but just reflects lack of knowledge". While the statement is not mistaken, in my opinion, it may convey an incorrect message that uncertainty may be eliminated by improving knowledge. However, there are barriers that do not allow elimination of uncertainty (in prediction), as demonstrated in the paper, even if we have all knowledge that is possible to have about the system.

Furthermore, while I fully endorse Weijs's statement "I would say that prediction is the fundamental goal and understanding is just a means", I have a problem with what it precedes it, i.e. "I therefore think that even the statement [in Koutsoyiannis, 2009a] that 'prediction ... is a crucial target in science-with even higher importance in engineering' ... does understate the importance of predictions in science". For I do not think that I understate the importance of predictions is science by emphasizing its greater importance in engineering. I would insist about this greater importance in engineering, because failure of a prediction in science may result in falsification of a hypothesis. Actually this happens all the time and thus failed scientific predictions are a driver of the scientific progress. On the contrary, failure in engineering predictions (and hence engineering designs) may have more tragic consequences and even cost human lives.

A final slight disagreement is related to Weijs's statement "it is important here to make a formal distinction here between quantification of uncertainty, which is putting a number to the amount of uncertainty, and 'probabilization' of uncertainty, which is specifying the distribution." I would formulate it in a somewhat different manner, i.e. probabilization of uncertainty is the representation of an unknown quantity as a random variable; thus in an approach assuming probabilization, the quantification of uncertainty can be done in terms of the distribution function of the variable.

One point of a more essential disagreement concerns the "cause" of unpredictability, i.e. high dimensionality or nonlinear chaotic dynamics. Weijs (2009) states "To my opinion, the example given is not describing the main cause for uncertainty in natural systems we as hydrologists usually deal with. For this kind of systems, the high dimensionality is a more natural explanation of uncertainty than chaotic interaction in low-dimensional systems." This I have already discussed in my first reaction (Koutsoyiannis, 2009b) but I wish to add a couple of thoughts. First, Weijs seem to understands that high dimensionality alone does not result necessarily in (macroscopic) unpredictability, e.g. when he says "... I would rather see hydrological systems as high-dimensional complex systems, with surprising predictable macroscopic behaviour" (this will be further discussed below). Second, as I show using the toy model, chaotic dynamic alone can result in unpredictability (I remind that the toy model is 2-D only). Perhaps Weijs felt that I made a logical error, asserting the inverse, that unpredictability implies that the system is low dimensional – but I did not. (Similar types of logical errors are met in some hydrological applications of chaos, e.g.: a simple low-dimensional nonlinear deterministic system, can have a complex, random-appearing evolution; hydrological processes appear complex or erratic; hence hydrological processes are low-dimensional

chaotic). I have no doubt that hydrological systems are high-dimensional (I state it in Koutsoyiannis, 2009a) and I have challenged some claims for the opposite (Koutsoyiannis, 2006). But it is important to show that even low dimensional systems can be unpredictable and demand probabilistic descriptions. I strongly believe that that the recent regression of stochastics in hydrology is a regression for hydrology (see also Koutsoyiannis, 2010, i.e. my reply to Montanari, 2010) and that is why I insist that probabilistic/stochastic descriptions are necessary in hydrology, irrespectively of low or high dimensionality, and simplicity or complexity of the system.

Also, I do not accept Weijs's comment on real numbers vs. quantized variables, i.e., "It is true that it is fundamentally impossible to calculate real numbers with infinite precision, but I doubt if the example states given in the caricature system are really real numbers.... The amount of soil water x_i can be quantized by the number of molecules present in the discrete volume and the flux vi is also quantized by the number of molecules leaving the volume in one discrete timestep." Here Weijs misses the fact that, if one goes to such quantized descriptions, one should know the position and momentum of each molecule (and, interestingly, Weijs mentions the positions of molecules in a following comment) in order to predict whether or not the molecule would stay in the liquid phase or change to gaseous phase (because this exactly interests us in the toy model). Not only are the position and momentum real numbers (rather than integers) but they also imply additional uncertainty due to Heisenberg uncertainty principle. This would enhance what I call in the paper the premise of incomplete precision. For simplicity, I will keep in the paper the continuum mechanical description and the premise of incomplete precision as they are now, noting here that they would hold (a fortiori) in a quantum mechanical description, which apparently is out of the scope of my paper.

I found Weijs's comments with regard to entropy and its maximization very interesting. Weijs states "I am curious if there would be some explanation for the constraints on mean and variance, which reflect the information available about the state at long lead time" and later on "If we do not have any information and maximize entropy, we end up with the a uniform distribution on (-infinity, infinity)". Weijs is absolutely right in this; evidently, in the absence of constraints, maximization of entropy would not be of great help (expect in very simple systems, e.g. in throwing of dice). In statistical thermodynamics the constraints on mean and variance represent the laws of preservation of momentum and energy, so there is no question here. But I too am curious what they exactly represent in a more generalized setting, like in the case of the toy model. In an earlier study (Koutsoyiannis, 2005a, b) using these constraints I was able to reproduce/explain important statistical properties including the general shape of the marginal distribution of hydrological variables and the Hurst-Kolmogorov dependence structure. However, while I believe that the entropy concept is very relevant to hydrology and geophysics and very promising both in explanatory and prediction level, I think that very little has been done for its exploration and that there is a long way before we can have concrete results. Therefore, I look forward to see important contributions in future research about it – in particular which the appropriate constraints are and what they represent.

Another related remark of Weijs that I wish to discuss – and express my disagreement – is this: "Another point regarding the maximum entropy principle is the difference between the tendency to thermodynamic equilibrium and the principle of maximum entropy as a method of inference based on limited information. Although links exist, which are not always straightforward, they are different concepts." This opinion, that the thermodynamic entropy maximization in natural systems and the entropy maximization in inference are two different things, is dominant in science. However, in my opinion they must be the same thing. In my view, entropy is a measure of uncertainty. Nature maximizes uncertainty (this is my view of the Second Law), so following Nature, we could use the same principle for inference about natural systems. The following quotation from a great book by Robertson (1993) sheds some light on this issue:

"Suppose a system of particles is cooled to our lowest convenient temperature, and

thus to low entropy. Now allow this refrigerated system to move past at some velocity v, where the speed is not so excessive that one must be concerned with relativistic formulations of thermodynamics. The macroscopic motion of the system does not change its temperature or entropy. Our monitors allow us to keep track of this system, and we expand our endeavors by adding several more such frigid systems, all moving at arbitrary but known velocities. We monitor all of them, and we continue to regard them as being cold, low-entropy systems. Our model, so far, is no worse in concept than that existing all day long in the control tower at any major airport. In principle, we can increase the number of these systems substantially, and they continue to be cold. Also, in principle, since there is no specified size for these systems, we can allow them to be single atoms, each moving at its own velocity. And there we have it. We have devised a system of atoms at very low temperature, with non-negligible kinetic energy, each moving along a path that we can follow. The entropy is low because of our information about the positions and momenta of the individual atoms. Our description of the system is in dynamic terms, not thermal ones. How do we cool such a system? There is no effective or meaningful way to do so. The individual systems are already as cold as we can conveniently make them, and there is no reason for us to ascribe a temperature to the known dynamical motions of the system. If our monitors fail, and we lose the ability to describe the system dynamically, then we must regress to a thermal description, with consequent temperature in terms of the mean kinetic energy of the atoms (if this is the appropriate measure), and increased entropy as a result of our loss of information. (Think of what could happen at a major airport if all monitors in the control tower fail.) Clearly, then, the entropy of a system does depend upon our information about it. If two observers have different descriptions of the state of a system, they will ascribe different values to its entropy, as well as to other variables. However, if two observers perform equivalent experiments on equivalent systems, they should obtain equivalent results, including their values for the entropy. Laplace's demon should see a zero-entropy universe (except for quantum effects), and as a consequence a zero-temperature universe."

I will conclude my reply with a discussion of the nice remarks of Weijs about the emergence of determinism from randomness. Indeed it was my omission not to refer to this more explicitly in the paper, which I will correct in the revised version. For I agree with Weijs comment that “However, the macro-states, which are for example sums or averages such as the water storage and vegetation cover, are far more predictable than the micro-states, such as the position of all water molecules and the activity of the individual stomata in the vegetation leaves.” But I do not agree when he says that “the macro-states of a large number of molecules interacting is surprisingly predictable.” There is no surprise here and the emergence of macroscopic determinism involves no magic. It is predicted by the laws of probability (law of large numbers, central limit theorem, principle of maximum entropy). Weijs states “Sometimes this predictability simply follows from the calculus of probabilities”. I wonder if “sometimes” is justified here, because I am not aware of any other type of calculation, except probabilistic, to derive macroscopic determinism from microscopic randomness. For this reason, I endorse Weijs’s quotation from Grandy Jr (2008) and, in particular, the word “only” in it: “Effects of the microscopic dynamical laws can only be studied at the macroscopic level by means of probability theory”.

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