Hydrology and Earth System Sciences Discussions

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

D. Koutsoyiannis

dk@itia.ntua.gr

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REPLY TO WILLIE SOON'S REVIEW

I thank Willie Soon for his congratulations, for reading my paper several times, as he says, for offering constructive comments and for asking interesting questions in his review (Soon, 2009). I also thank him for pointing to recent literature that I was not aware of, for offering interesting quotations (Liao, 2009; Lighthill et al„ 1986), which are not far from what I try to say in my paper (Koutsoyiannis, 2009a), and for making public our discussion about the uncertainty as a positive quality. I will include a paragraph about the latter issue in the revised version (see also Koutsoyiannis, 2010a, i.e. my reply to Montanari, 2010). I will also include the reference to Laskar (1999), modifying the text accordingly, as suggested.

First I wish to discuss Soon's concern expressed as follows: "My main concern is that the connection may not be as direct or clear as our current knowledge would permit. To illustrate a practical barrier or problem, there are serious questions even about the reliability of the computed 'chaotic' solutions from discrete dynamical equations as newly discussed in Lorenz (2006), Teixeira et al. (2007) and Liao (2009)". I looked at these papers and I think they refer to discretization, for numerical purposes, of continuous time differential equations (see also Koutsoyiannis, 2010b, i.e. my reply to Koussis, 2010). My "toy model" is different because the dynamics is expressed in discrete time by construction, so it is accurate by definition (as a toy model of course and nothing more than this) and there is no numerical error due to artificial (numerical) discretization. Obviously, though, a more realistic model would use continuous-time dynamical equations. However, even the discrete-time dynamics may have some physical realism at the annual scale in the hypothetical system examined, given the annual cycles of hydrological processes as well as the annual cycles of the vegetation growth.

I found it interesting to read in Soon (2009) that "[his] enthusiasm for the claim that there exists a finite bound to the dimension of the global attractor of atmospheric circulation . . . quickly died off when [he] learned that that dimension, with additional assumptions, is about 10ˆ19". But I wonder, if it were, say, 10ˆ2, or even 10, would this indicate that atmospheric circulation would be predictable? The toy model I study has a dimension of 2, yet is well unpredictable.

Coming to Soon's questions, I think that a couple of them are rhetorical and do not need a reply by me. I refer to the questions: "Seriously though, who are we trying to kid? How to achieve an accurate prediction of climatic variables?" Perhaps the statement that follows, "It has been nearly impossible to accurately predict temperature and rainfall for more than a season ahead" is somewhat exaggerated. A season ahead? What about Tennekes's estimate from his recent "Farewell Message to the Dutch Academy" (http://pielkeclimatesci.wordpress.com/2010/02/12/henk-tennekes-resigns-from-dutch-academy/): "I told my audience that the prediction hori-

zon, in 1950 estimated by John von Neumann at 30 days, in fact is only three days on average". What about the recent news that the Met Office will stop publishing seasonal forecasts, after some recent loud failures? (see "Met Office seasonal forecasts to be scrapped", http://news.bbc.co.uk/2/hi/uk_news/8551416.stm).

But the next question perhaps needs an answer: "Let me start my discussion with a question upon seeing DK's November 29's first reaction to the comment/review posted by Steven Weijs: What does the latest 'Climate-Gate' incident have to do with DK's paper or Steve Weijs's review?" Of course, the discussion paper was written and published before the Climategate incident. I thought, though, that my "random walk" approach allowed me to refer to Climategate, taking advantage of the random coincidence I mention in Koutsoyiannis (2009b; i.e. my first reaction to the review by Weijs, 2009). I referred to it for the additional reason that I found in one of the Climategate emails a kind of unexpected support for one of my theses regarding the futility of geoengineering. My thought was to stop the discussion there. But after reading this question and after seeing that Koussis (2010) also refers to Climategate in his review, I started to wonder if there is a connection other than the random coincidence I mention, e.g. at a philosophical level. I think there indeed is, and this connection also emphasizes the positive character of uncertainty in Nature and Society. Were future predictable, would it not also be controllable? Would, then, this not give an enormous power to an elite of scientists and technocrats, developers, users or owners of a supermodel for which the future has no secrets? Would this elite not present itself to the society as saviours? But I think that uncertainty is the real saviour from such Orwellian conditions. In my view, Climategate serves as a "toy model" to demonstrate the development of such conditions (elitism, power, saviour attitude) as well as the detrimental implications of such conditions to Science. Interestingly, these developments were based upon the aspiration and speculation that future is predictable, rather than any proof that it is so. At the same time, the Climategate incidence per se supports the unpredictability argument: were the future predictable and controllable, the incident would not happen at all. However, while I believe that these thoughts may sketch an answer to Soon's

question and are relevant to the paper's topic, I am thinking not to include them in the paper in order not to distract attention from the scientific issues to those that are more philosophical and ethical, or even political.

Another interesting question is the following: "In other words, how can we be assured that there will be no cancellation of positive and negative tendencies even given the greater degree of freedoms and nondeterministic noises in the real world?" I think cancellation of positive and negative tendencies is meaningful when moving to larger temporal or spatial scales. In my view the problem can be described well in stochastic terms. The statistical thermodynamics serves of a nice example of how this can be done. It also demonstrates the path of the emergence of determinism (in macroscopic sense) from randomness (in microscopic sense), which is discussed by Weijs (2009). There are some hints about this in section 5 of the paper, but I think the problem needs to be better addressed. So, I will include a paragraph to discuss the conditions under which we expect the cancellation of tendencies, or else reduction of uncertainty. Roughly, this depends on the presence of antipersistence or persistence, where the former (naturally) results in cancellation of opposite trends and thus to higher predictability, whereas the latter results in higher uncertainty.

The last set of questions are quite crucial: "But perhaps a more fruitful approach would be to ask the most direct question on how the formulation of GCMs can add to our totally inadequate handling of both the deterministic and stochastic aspect of hydrologic variations on local, regional to hemispheric scales? Should the GCMs may be deemed overly complex or too burdensome (in the sense it is being held hostage to the algorithmic complexity that its outputs are often difficult if not impossible to interpret)? What alternatives must be sought to replace GCMs?" I think this set of questions need several papers to be addressed, so I do not think I can include this discussion in this paper. But my general position, roughly contained in the paper, is that a sound alternative to current GCM-based climate prediction could be offered by a stochastic theory of climate. Such a theory would give the emphasis to evidence from data rather than

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unproved hypotheses and speculations. GCMs themselves could potentially have their room in a stochastic framework, e.g. in formulating prior information and/or assigning prior probabilities. But I am afraid that eventually, in the posterior predictions (i.e. after incorporating the evidence from data) the influence of GCMs may be negligible because of the huge departure of GCM outputs from observed data (Koutsoyiannis et al., 2008; Anagnostopoulos et al., 2009). Therefore, I think such a stochastic framework is more important to incorporate computationally simpler macroscopic (statistical or stochastic) relationships between several factors affecting climate (e.g. greenhouse gases concentration and temperature, perhaps extending the study by Soon, 2007). We see statements that climate sensitivity (i.e. the change in global mean near-surface air temperature that would result from a sustained doubling of the atmospheric CO_2 concentration) "is likely to be in the range of 1.5 to 4.5°C" (IPCC Third Assessment Report - TAR) later changed to read "Progress since the TAR enables an assessment that climate sensitivity is likely to be in the range 2 to 4.5°C with a best estimate of about 3°C, and is very unlikely to be less than 1.5°C" (IPCC Fourth Assessment Report). While such values are crucial in climate prediction, I wonder whether such statements can justify any type of deterministic modelling of climate, or they better suggest that a stochastic framework would be more scientific and more honest. Moreover, I wonder if the statements themselves are ultimately scientific, given their ambiguous vocabulary, such as "likely", "best estimate" and "very unlikely".

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