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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.

A. Montanari (Referee)

alberto.montanari@unibo.it

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I am enjoying this discussion that promises to deliver significant inputs regarding hydrology, climate change and sustainability. I think launching this new type of publication for HESS, namely the HESS Opinions, was a very good idea and I wish to congratulate with the Editors for taking this initiative. I also believe the provoking paper of Koutsoyiannis et al. (2008), hereafter denoted as K2008, is a very good start! This contribution well matches the focus of HESS and is clearly aimed at triggering a discussion. I hope hydrologists will take this opportunity to express their view and I highly

recommend publication of K2008 on HESS.

The only marginal problem I noticed with this discussion, after also consulting with some readers, is that the sum of the papers is quite long to read and my review is also going to be long, given the amount of things to discuss. It would probably be better to fix a page limit for the future HESS Opinions. With a short paper the reviews and comments would be also short and the message delivered by the discussions would be immediately understandable. Of course this suggestion does not apply to K2008.

1 Short review

My review is quite long and therefore I am providing here below a short summary in the form of an itemised list, to make the reading easier. Any of the items here below is subsequently dealt with in my extensive review that follows hereafter.

- Main contribution of K2008. I enjoyed reading this paper. I believe it delivers an extremely interesting vision about the role of the statistical approach in hydrology. I recommend publication on HESS. The authors may consider to address/discuss in the revised version the criticism by Blöschl (2008) and Sivapalan (2008), and may be some of my remarks here below.
- One of the main contribution delivered by K2008 is that statistical model are potentially useful in hydrology. They can incorporate cause-effect models (which we are used to call deterministic) and can allow one to gain a better comprehension of the underlying physical processes. I think this message is interesting.
- In my opinion deterministic models should not be rejected. I believe the integration of the two approaches is the way forward, on the basis of a better understanding of the physical system. Within this respect I agree with Sivapalan

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(2008). In my view this also what K2008 are supporting. There is probably the need for K2008 to provide a definition of statistical and deterministic model.

- I agree with K2008 (as well as Sivapalan (2008)) that the prediction of the future availability of water resources, in the possible presence of climate change, is affected by a significant uncertainty.
- I agree that the problems related to water will become much more important in the future. I am not expert within this respect, but I believe that water will hardly be able to play a central role within energy production.

My extensive review follows here below.

2 Extensive review

2.1 Main contribution of K2008

In my opinion the contribution of K2008, as well as their reply to Blöschl (2008), delivers an extremely interesting message about the role of the statistical approach in hydrology. I really enjoyed reading the papers and I appreciated the efforts the authors did to motivate with historical and philosophical considerations the current opinions and fashions about hydrological modelling (although the philosophical part in the reply to Blöschl (2008) is perhaps too long). What I really appreciated in this discussion is that two different schools of thought are emerging, although I believe the different views have much in common. I think it is extremely important that the role of statistical hydrology is clarified and explained, because I believe there is still much to gain from it in the future development of hydrological modelling.

It is indeed curious that statistical hydrology is clearly declining, as the current trend in

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the published literature clearly show. I think the current focus on determinism, at the expense of statistical hydrology, is not justified by the results in the real world practice and the progresses in processes understanding. It seems that hydrologists are getting convinced that statistics implies refusing a better understanding (no cause-effect relationship), while determinism stimulates the comprehension of the system dynamics. This opinion is clearly diffused. On the contrary, K2008 well point out that a statistical description of a deterministic system can provide a very useful cause-effect relationship. I think K2008 did a better job in explaining this concept in their reply to Blöschl (2008). I therefore suggest that the text of the opinion paper is revised by incorporating the further clarifications and nice examples reported in their reply to Blöschl (2008).

In detail, I identified three major contributions in the opinion paper by K2008: (1) the discussion about the emphasis currently being given to climate change and the credibility of future climatic scenarios delivered by GCMs; (2) the discussion about the sustainability of the current economy based on the exploitation of the oil fuels; and (3) the discussion about the new role hydrology should assume to cope with the current unsustainable use of oil and fossil fuels. I believe the first two issues are interrelated, while item number (3), which includes the discussion on the perspectives of statistical hydrology, could probably deserve a stand alone consideration. In fact, I believe the possible need for rethinking hydrology's fundaments is independent of the new role that hydrology itself should assume in the face of the unsustainable economical development. I am not sure my interpretation above is correct, but I believe the authors should better clarify the logical connection (or independence) between issues (1)-(2) and issue (3) above. In fact I got a bit confused to initially try to find the motivation for rethinking hydrology's fundaments by going back to the discussion about climate change assessment. I think this marginal change in the presentation would make the message of the paper clearer.

It is interesting to note that the attention of Blöschl (2008) and Sivapalan (2008) was caught mainly by item (3) above. For this reason, I will start my review by myself

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discussing item (3) first. My comments about items (1) and (2), which I believe are very important as well, follow hereafter.

2.2 Statistical model are potentially useful in hydrology

Blöschl and Sivapalan have a clearly a different view on hydrological modelling with respect to K2008, although I would like to emphasise once again that I believe the different opinions are actually not so much divergent. In fact, I believe much of the incomprehension would be solved if a definition of stochastic model was provided. I recommend Koutsoyannis et al. to consider this issue when revising the paper, because there is clearly a difference among the notion of stochastic model of the different authors. For instance, it seems to me that K2008 consider TOPMODEL as a stochastic model, and I agree with this view. However, a large part of our colleagues consider stochastic models accordingly to the classic definition, that is, as purely black-box, data-driven approaches where the knowledge of the physical behaviours of the system is not exploited.

K2008 deliberately adopted a provoking approach. I appreciate their dauntlessness, as their tone is clearly stimulating the discussion. Provoking messages have usually a emotional impact, which may be good but in some cases it makes the meaning of the message itself not easily understandable. In fact, I do believe that K2008 did not mean "...to throw the baby out with the bath water" (Blöschl, 2008), although their initial wording was giving this feeling ("Hydrology....must reinvent itself", line 15, page 1). It is now clear, after their reply to Blöschl (2008), that K2008 were deliberately provoking and did not mean at all that any model making use of cause-effect relationships is to be thrown away. On the contrary, their notion of stochastic model well includes cause-effect approaches.

Actually, I think it is clear that K2008 meant to say that in their view a fully deterministic description of hydrological processes will never be possible, for the rea-

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sons they well summarized. The same feeling is shared by Sivapalan (2008), who states that “The reality is that the world is poorly determined and understood (and will remain so forever)”. Indeed, there are so many complexities interacting in, say, the rainfall-runoff transformation that a fully deterministic description might be impossible, even if one completely understood the dynamics of the process. Koutsoyiannis, in his reply to Blöschl, provided excellent examples of how the trajectory of a fully deterministic systems, even if perfectly understood, can be modelled only statistically. There is another nice example that comes to my mind, which is the experiment of dropping balls into a spiked sieve (see, for instance, http://education.mit.edu/starlogo/models/library/GaussianDistribution). Here, the geometry of the system is perfectly known as well as the initial and boundary conditions. However, once a ball is dropped in the sieve it is impossible to predict deterministically its trajectory, because no one can predict which way the ball will follow after hitting a spike. However, the distribution of the balls at the bottom of the sieve is well known to be Gaussian. I believe this is another nice example showing that the full comprehension of the dynamics of the system not always allows one to set up a deterministic description, while a stochastic description can provide an excellent model. Could not the rolling balls be assimilated to drops travelling within a catchment? Could it be that modelling the water paths is impossible by deterministic means while a stochastic approach could provide a very satisfactory description?

The adoption of a stochastic approach would not prevent us to gain a better understanding. On the contrary, a meaningful statistical descriptions should necessarily be based on a satisfactory comprehension of the system (see the example of the balls above). I think this is the view of K2008, which is better expressed in their reply to Blöschl (2008) than in the original paper, where their provoking tone may induce the feeling that they are just supporting a data-based description. Actually what K2008 mean is well expressed by their statement in the opinion paper, at line 20, page 2936: “Hydrology has never been divorced from probability theory, but the state of the art in probabilistic, statistical and stochastic concepts in hydrology is far from satisfactory.

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This is mainly because these concepts have been based, to a large extent, on the classical statistical paradigm rather than on the study of natural behaviours...". And again, at line 3, page 2937, they note that the Hurst Effect can possibly be explained accordingly to the principle of maximum entropy, therefore denoting the constant effort Koutsoyiannis always did to explain with physical considerations what comes out from the statistical analysis. Within this respect, I think the opinion of K2008 agrees with that of Sivapalan (2008) who excellently stated (page S1782 and S1783): "The role of science is push the frontiers of our knowledge and understanding so that we can make continuous improvements in our ability to make predictions.....by combining the knowledge and understanding of the physical....system of interest, with explicit acknowledgement of the lack thereof....".

Actually, there are numerous examples of statistical approaches in hydrology which take profit from the available information about the system. Koutsoyannins mentioned TOPMODEL; the Probability Distributed Model (PDM; Moore, 1984) and the HyMod model (Boyle, 2000) are examples where statistics is used to describe the distribution of the water storage within the catchment. At the end all these models end up with a cause-effect rainfall-runoff relationship and therefore they can well be used for flood forecasting or other applications. Within this respect, I do not fully agree with the distinction between statistical and deterministic models made by Blöschl (2008), which reads: "My response was that deterministic models represent cause-effect relationships which is what is often needed, both in science and engineering". Indeed, both PDM and HyMod represent a cause-effect relationship while being statistically based. I think this consideration justifies the need to better clarify the role of the statistical approach in hydrology and therefore supports one of the scopes of the K2008 paper.

In summary, I fully recognise that the theory advocated by K2008 is meaningful and potentially bringing very useful perspectives. I believe it is not in contrast with the philosophy of PUB, because a probabilistic description should necessarily be based on a better comprehension, as I stated above. On the other hand, I do not agree with K2008

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when they say that PUB is excluding a stochastic description. PUB indeed calls for a modelling framework based on a better perception of the underlying physical mechanism but I never had the feeling that PUB was excluding the statistical approach. It is indeed true that PUB seeks to reduce uncertainty and limit the need for model parameterisation (that can be hardly performed in absence of data). But I did not get the feeling of PUB assuming that a deterministic description is needed to reach this goal. Calibration can be eliminated/reduced by using regionalisation, parameter transfer, expert knowledge and many others, in both deterministic and stochastic approaches. Of course uncertainty would remain there (probably increased with respect to a gauged situation). Therefore I would not associate the elimination/reduction of calibration with the elimination/reduction of uncertainty, as K2008 seem to imply. I think this is not the aim of PUB.

That being said, I do not believe the approach K2008 are proposing is new. I think it is just less used than in the past and therefore one may get the feeling that the ideas are unusual and new. Actually, I do not see the need to say that we must reinvent hydrology, unless one wants to be provocative. We have a long history of statistical approaches in hydrology within the line of thought that K2008 well expressed. Therefore I would suggest K2008 to better clarify what they actually mean when they state that hydrology should change his fundaments. What I see in K2008 as new is the very coherent and well explained formalisation of the role that statistics should play in hydrology and the vision about how statistics and determinism could interact (this is well explained in the reply to Blöschl). May be this is the new paradigm they are proposing?

2.3 Deterministic models should not be rejected

While I fully agree with K2008 that statistical hydrology should play its proper role, I am not fully convinced that this should imply rejecting a priori a deterministic description. I believe the way forward is the integration of the two approaches. Why not describ-

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ing deterministically the processes that well lend themselves to this approach? For instance, I am thinking about flood wave propagation in open channels.

K2008 state at line 11, page 2934: “Engineering hydrologists understood early that the design of engineering projects based on deterministic projections is largely a hopeless task...”; and, at line 17 of the same page: “The trend towards the so-called “physically based models” allowing for spatial variations (Abbott et al., 1986) signifies this change of perspective. The hidden assumption behind these is that modern computational means would eventually allow the full description of the detailed physics of the hydrological cycle using mechanistic model structures and “first principles”, i.e. Newton’s laws and their particular formulations in fluid mechanics (Navier-Stokes equations)”. I do not agree with this. For instance, I am convinced that spatially distributed hydrological models are very useful, especially in the real world practice. See, for instance, Moretti and Montanari (2008), where the authors satisfactorily used a spatially distributed rainfall-runoff model to estimate the design flow at ungauged river cross sections, by calibrating the model parameters in a downstream gauged site. These models have well proven to be capable of providing a reliable spatially distributed simulation of the river flows in internal sections. Therefore the value of downstream observations can be exploited to derive information in ungauged sites (see also Brath et al., 2004). Actually, these models are not fully deterministic. Typically they are a mix of physically based and conceptual approaches. In many cases, being uncertain, they are associated with a stochastic model for estimating uncertainty (like the meta-Gaussian approach by Montanari and Brath (2004) and Montanari and Grossi (2008)). However, one may recognise that in these model the level of determinism is high and I am fully convinced they can exploit additional information thanks to their distributed behaviour. For instance, I am convinced the spatially distributed representation of the surface flow paths, that are derived from the digital elevation model of the catchment, allows one to efficiently constrain the flow routing parameters.

Thus, my opinion is that a partial deterministic description is possible and useful and

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therefore we should not reject a priori this approach. Therefore, I agree with Sivapalan (2008, page S1783): “Probability or stochasticity plays an important part in this. Indeed, stochasticity and determinism are equal and opposite partners, i.e., two sides of the same coin, in this integrated approach, the only difference being that as the frontiers of knowledge and understanding advance, the boundaries between what we know (the deterministic part) and what we do not know or cannot predict (the probabilistic part) also evolve”.

In fact, I too believe that the way forward goes through the increase of our understanding of natural systems, as predicated by PUB. Our aim should be to set up a deterministic description whenever possible, without being tempted by audacious representations. When the bound of a meaningful/useful deterministic scheme is reached, depending also on the purpose of the analysis, the stochastic description, necessarily based on what we know about the physical process, should come in and be efficiently integrated. The “a priori” selection of an “ideal” modelling approach, either deterministic or stochastic, should be avoided, in favour of the “...so many shades and hues in representing nature by models. The shades and hues make modelling an art and they make models really useful” (Blöschl, 2008). The identification of the optimal mix between a stochastic and a deterministic description is part of the art.

2.4 Prediction of the future availability of water resources

I generally agree with the opinion of K2008 that the attention that is being paid to climate change is not comparable to what is done for other emerging environmental problems. I indeed believe water resources management will be a problem in the future, for the reasons that K2008 well summarised, and climate change is just one of the causes. It is clear that climate change is a very emotional issue and I agree that science should not be much influenced by emotions.

I think all of us, perhaps including K2008, recognise the advances made by our col-

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leagues dealing with atmospheric sciences in the last decades (Sivapalan 2008). However, one thing is to predict the weather, another thing is to predict the climate. When dealing with climate, stochasticity plays a much important role. I believe predicting the external forcings to climate, like the solar and volcanic activity, is still very difficult if not impossible. The results is that GCM predictions are still highly uncertain. This is clear if one compares the pattern of the future climate, as predicted by GCMs, with the past patterns. The comparison clearly shows that the variability of climate is underestimated. When one adds the hydrological piece to the above modelling chain, to obtain a picture of the future state of water resources, the uncertainty becomes much more relevant. We all know how much hydrological models are uncertain, even when working with the observed meteorological forcing. By contrast, other pressures to water resources in the future can be more easily predicted (K2008).

2.5 The role of water in the future

I too believe that the current economy, based on the exploitation of oil and mineral fuels, is hardly sustainable. We all know how much attention is being paid towards the exploitation of renewable energy. I agree with K2008 that the importance of water, within this context, is going to increase. However, I do not believe that water will ever be able to play a central role. First of all, the water use for energy production is far from being sustainable. We all know how many concerns the use of water raises for the preservation of the environment. Of course we need to devise clever systems for preserving the state of water while using it, but I think there is an upper limit to water exploitation for energy production and I believe such a limit is already very close. I am not an expert about these issues, but it seems to me that K2008 are overlooking the possible advent of new techniques. What about the possible role played by nuclear energy? What about solar and wind energy? I agree that the ability to produce solar energy and wind energy is related to the presence of water in the climatic system, but I do not think that hydrology is significant within this respect. Of course water always

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plays a central role because it is needed for life, but we should not overestimate the role of hydrological sciences.

3 Conclusions

I understand it is difficult to see my text above as a classic review. There are many points where I fully agree with K2008, while I have some disagreements that I hope I clearly expressed above. However, my overall opinion about K2008, and their reply to Blöschl (2008), is enthusiastic. I highly recommend publication on HESS. My main suggestion is to try to address/discuss in the revised paper what emerged by the comments of Blöschl (2008) and Sivapalan (2008). I believe they contain very useful hints. In fact, it would be a pity if in the HESS paper, that is what is finally known to the public, the criticism and the good suggestions by Blöschl (2008) and Sivapalan (2008) were not incorporated.

My review has benefited from a discussion with Francesco Laio, whom I wish to thank very much for the useful suggestions he provided.

I wish to congratulate with K2008 for their very interesting work.

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