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Interactive Comment

Interactive comment on "HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability"" *by* D. Koutsoyiannis et al.

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1. INTRODUCTION

Montanari (2008) concludes his review stating "I understand it is difficult to see my text above as a classic review". Indeed, his review is not of classical type and we are happy for this. We viewed it as thought-provoking and constructive discussion of the issues analyzed in Koutsoyiannis et al. (2008a). He offers several well-targeted statements regarding the current trends in hydrology that focus on determinism and neglect stochasticity, which we were happy to read and adopt. Moreover, he offers additional useful information and argumentation on the ideas we discuss in Koutsoyiannis et al.

(2008a). These include his example of the experiment of dropping balls into a spiked sieve, as well as his examples of the Probability Distributed Model and the HyMod model. Following his example, we will not provide a "classical" reply to his review but rather we will discuss further some of his ideas and statements in the next sections. While we agree with most parts of his review, naturally our discussion is focused on the points of disagreement (except in Section 2). In a separate document we discuss Montanari's criticism of our thesis that water and hydrology can play a more important role, related to energy, in the future.

2. ARE STOCHASTIC MODELS BLACK BOXES?

We agree with Montanari (2008) when he states "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". This gives us the opportunity to provide a rebuttal for this widely accepted idea that stochastic models are data-driven approaches. We argue that the framework provided by probability, statistics and stochastic processes is not at all a collection of blind tools. Rather it is a very insightful theory and very efficiently applicable to practical problems. We will start with a few examples.

(a) Even the humble binomial distribution can give insightful answers to physical problems. For example, in the question, what is the number of molecules in the front third of a 40 mˆ3 otherwise empty room, the probability theory combined with elementary physics could easily calculate that this number is 3.3 * 10ˆ26 and, more impressively, that the relative standard error of this estimate is almost zero (more precisely, of the order of 10ˆ-14; the interested reader can easily check the calculation having in mind that 22.4 L of air contain about 6.022 * 10ˆ23 molecules and assuming that the probability of a molecule to be in the front third of the room is 1/3). This is an example of how probability can give an insight into "invisible" systems and derive precise results for systems ruled by uncertainty. Those who may rush to characterize this example as silly or useless are notified that such an example is just a first step in the path of statistical mechanics, which next develops important concepts such entropy and provides justification of thermodynamic laws (e.g. Stowe, 2007). Furthermore, this example signifies the macroscopic viewpoint of probability and statistics. Another approach would be to assign an identification number to each molecule and trace its trajectory. Some may find the latter approach more insightful but they may stub onto limitations of quantum reality, which may hinder the ability to assign identification numbers and to determine the location of each molecule – not to mention that this approach may take quite some time.

(b) Some tend to regard the normal distribution as a blind algorithmic procedure to be applied to a data set. However we should have in mind that this distribution is a result of the central limit theorem (i.e., a deductive and thus general result rather than a data-based one) and this justifies why it constitutes a very powerful law, applicable in a great diversity of phenomena, natural, biological and social.

(c) The principle of maximum entropy is a very powerful probabilistic principle for logical inference (Jaynes, 2003; Papoulis, 1991) which, inter alia, can derive the normal distribution (independently of the central limit theorem), can explain the linear statistical relationships that often appear in random variables representing a natural process at consecutive times (not to be confused with deterministic linearity, which is hardly met in complex systems) and can help in parsimonious modelling in problems involving very many variables (e.g. Koutsoyiannis et al., 2008d).

(d) A Markovian process or autoregressive process of order 1 (AR(1)) is typically introduced as a black-box algorithm to simulate natural processes based on an observed time series. However, it can be well introduced in a more physical or conceptual manner, emphasizing the storage function in several natural processes. Thus, a linear storage system (a system whose input and output are related through a linear differential equation of first order), whose input is white noise, produces an output with Markovian dependence. Furthermore, if the time is discretized and the output is taken as a sequence of random variables at equidistant times, this output forms precisely an

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AR(1) process (Koutsoyiannis, 2000b). This provides some insight on the otherwise black-box model.

The above examples have hopefully demonstrated that probabilistic and stochastic models are not necessarily black-box models, unless we introduce them as such. Unfortunately, several hydrological texts have presented stochastic models as black boxes and this also extends beyond hydrology. Even the famous book by Box and Jenkins (1970) is not free of such misrepresentation of stochastic models. In fact, the large families of AR, MA, ARMA and ARIMA models it proposes, cannot stand all together as conceptual or physically-based models but only as algorithmic manipulations of data, i.e. data-driven black-box models, generally not parsimonious in parameters. Such an approach is not necessary even in an algorithmic level (see alternative algorithms in Koutsoyiannis, 2000a, which separate physically meaningful parameters from algorithmic coefficients).

Here we must be cautious and explicitly state that understanding within a probabilistic context needs familiarization with essential concepts of probability and their differences from their deterministic counterparts. A good example that demonstrates this is the regression theory. Regression can provide insights in the study of a relationship between different variables or phenomena, but can also lead to incorrect interpretations if we are not familiar with the mathematical content of the concepts involved. Specifically, the regression theory has been linked from the outset to the so called regression fallacy (http://en.wikipedia.org/wiki/Regression_fallacy). The fallacy originates from failing to account for natural fluctuations, from misinterpreting random variables as "sharp" variables and relations among them as deterministic relationships, and from giving incorrect physical interpretations to mathematical concepts. Ironically, even the very term "regression", from Sir Francis Galton's study "Regression toward Mediocrity in Hereditary Stature" (in 1885) just reflects this fallacy as it manifests a false biological interpretation of a probabilistic concept. Today we know that the fallacy is related to the mathematical fact that the correlation coefficient is less than 1 and that it is symmetric

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in time. Thus, if r is the correlation coefficient between the parent's height X and the child's height Y, then the two should be related as Y = r X + (1 – r) m (assuming that X and Y have the same mean, m, and the same standard deviation). This relationship, however, if solved for X, is not written as X = Y / r – (1 – r) m / r, as in standard calculus, but rather as X = r Y + (1 – r) m (more precisely the two relationships should be written by means of conditional expectations, i.e. E[Y|X] = r X + (1 – r) m and E[X|Y] = r Y + (1 – r) m). The example demonstrates the additional difficulties in understanding probability, random variables and the notion of estimation, which differ from relevant notions in standard calculus.

The "regression" example also demonstrates the importance of good terminology in conceptualizing and understanding the probabilistic concepts. Continuing this discussion, we think that "Time Series Analysis" is a bad term giving emphasis to algorithmic processing of the data in a time series. A better term (due to Kolmogorov) is "Stochastic Processes" and gives emphasis to the abstract process (triggering a connotation on insights) rather than the data (pointing to data-driven approaches). For the same reason, we do not support the common name "Statistical Hydrology" (also used by Montanari, 2008) but we prefer the term "Hydrological Stochastics". "Statistics" is the branch of probability that deals with inference from data and thus "Statistical Hydrology" gives the focus on the data part. "Probability" is a mathematical theory firmly founded in Kolmogorov's axioms and serves as the rudder and compass of statistics. (According to Leonardo da Vinci, "He who loves practice without theory is like the sailor who boards ship without a rudder and compass and never knows where he may cast"). The term "Stochastics", introduced by Jakob Bernoulli in his book Ars Conjectandi, written more than 300 years ago, has revived recently to describe probability and statistics together (e.g. Barndorff-Nielsen et al., 2001). Its direct linkage to "Stochastic Processes", along with the fact that stochastics processes include in their descriptions deterministic controls (e.g. periodicity, dependence implied for instance by storage or other mechanisms, etc.) makes the term "Stochastics" ideal in communicating a message of an insightful representation of processes rather than a mere description of data.

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Of course data-driven approaches have their merits and we should not depreciate them collectively. Hydrology needs a diversity of approaches and research paths. But the characterization of an approach as black-box or data-driven has no relationship to the nature of the approach, whether deterministic or stochastic. For example, the current explosive development of the so-called "Artificial Neural Network" models (another bad term in our opinion; see Koutsoyiannis et al., 2008d) in most of the cases provides deterministic black-box and data-driven models, in which we cannot even write the governing mathematical equations. Yet such models have proven to be very useful in several hydrological tasks (including prediction) although in some cases their application seems to be an abuse of a modelling exercise (e.g. Koutsoyiannis, 2007a).

In conclusion, the clichés that determinism provides understanding, insights, and descriptions of cause-effect relationships and that stochastics provide data-driven models, are mistaken and should be fought and hopefully abandoned.

3. SHOULD DETERMINISTIC MODELS BE REJECTED?

Montanari (2008) states: "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 (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" (he uses K2008 for abbreviating Koutsoyiannis et al., 2008a). He also writes: "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."

We think that rejecting deterministic models would be a crazy idea and we would not support it. On the other hand, we do not fully agree with Montanari's idea of "the integration of the two approaches", or the idea "of the optimal mix between a stochastic and a deterministic description". We understand the practical purpose behind Montanari's

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statements, but we do not fully agree with his formulations. Integration of approaches may not be feasible, particularly if we regard them incompatible with each other. Mixing of approaches does not necessarily lead to optimal problem solution. Rather, an approach that more faithfully represents the processes and interactions between system elements is more likely to lead to an optimal solution. We think that for complex hydrological systems a stochastic approach is more general than a deterministic approach (it encompasses the deterministic approach as a special case, when e.g. variables are assumed to have zero standard deviations, or when expected values of variables are taken) and therefore a stochastic approach possesses this integrating character intrinsically. As we detailed in Koutsoyiannis et al. (2008b), we do not support binary world views and dichotomic or reductionist logics. Our view subscribes to a stochastic/indeterministic paradigm and we think that this is more powerful and consistent with natural behaviours than a deterministic view.

This discussion is not the appropriate place to give definitions of statistical and deterministic models, as Montanari urges us, but we will clarify our view using examples. We would characterize the conservation of mass, momentum and energy as deterministic laws, because they are exact and do not involve any parameter to be estimated from data. Such laws are able to fully describe very simple systems. For instance, for a Newtonian fluid with viscosity mu and density rho flowing in a circular pipe with diameter D with laminar flow condition, we can calculatte the flow discharge Q using the laws of conservation of mass and momentum and the definition of a Newtonian fluid, as Q = pi rho g Dˆ5 J / (128 mu), where g is the gravity acceleration and J is the energy gradient (e.g. Noutsopoulos and Christodoulou, 1996). This we would call a deterministic law, too. However, the usefulness of this law is minimal. In real world problems the flow is turbulent rather than laminar and in natural systems the geometry is not as simple as in a circular pipe.

For turbulent flows in natural systems typically we would replace this law with Manning's formula, i.e. Q = (1/n) A Rˆ2/3 Jˆ1/2, where n is Manning's roughness coefficient, A is the cross-section area and R is the hydraulic radius. People are tempted to call this a deterministic law too, but is this justified enough? First, this formula describes a turbulent flow, which, almost by definition, involves a stochastic description. Second, the formula is inexact. Even its more accurate counterpart in circular pipes, the Darcy–Weisbach/Colebrook–White equation, is known to be accurate only to 15% (e.g. Koutsoyiannis, 2008). The Manning formula applied to circular conduits partially filled involves an error up to 30% unless n is appropriately corrected according to the ratio of the flow depth to the diameter (WPCF and ASCE, 1976; Koutsoyiannis, 1990). The error is even greater in more complex cross sections (e.g. compound flood plains with double orthogonal or double trapezoidal cross sections), where the use of the Manning' formula is not recommended unless a special procedure is applied for the compound cross section. Those errors reflect the fact that the equation does not faithfully describe the physics. Third, Manning's formula has not been established solely by theoretical reasoning and deduction but is by large a result of several laboratory and field experiments. For all these reasons, we would not classify it as a deterministic law but rather as a statistical or stochastic law. Some colleagues may feel more comfortable to say that the original writing of the equation (as above) reveals a deterministic model and that we could modify it to a statistical equation by adding an error term. However, it is better to keep the writing as is and just imagine one or more of the variables involved as random variables. Have we really gained anything by saying that the equation is stochastic rather than deterministic? In our opinion yes, because we are more conscious of the error or uncertainty the equation implies.

However we call the equation, we may all agree that it is an extremely useful formula in the design of hydraulic constructions such as canals and sewers. More caution is needed if we apply the equation in natural river cross sections. Its application should involve greater uncertainty due to heterogeneity of the cross section and roughness in any river reach. The greater uncertainty is naturally expected in a stochastic world view. A hydrologist with a deterministic world view and reductionist thinking (here we borrow the exceptionally successful term from Savenije, 2008) may be tempted to get

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rid of the heterogeneity by dividing the river reach into several segments assuming that each one has almost constant geometry and roughness. This logic, despite seeming to reduce uncertainty, is fundamentally flawed. For, a basic presupposition behind Manning's formula is the one-dimensional flow, which means that the length of the channel should be orders of magnitude greater than the width (and depth) to hold. Thus, the reductionist must abandon the use of Manning's equation and replace it with three-dimensional differential equations (the Navier Stokes equations). If that was possible for complex natural systems, Manning's equation would be totally useless even for the simpler artificial canals and pipes (a fortiori). Thus, the deterministic reductionist logic heads to a deadlock.

In contrast, a stochastic view is more pragmatic and useful. We can use the Manning's equation with the consciousness that it implies uncertainty and error. Due to its simplicity, its use is absolutely relevant to hydrology (we embrace Dooge's, 1997, argument that hydrology should search for simplicity) and, more generally to the Scientific Method, in which parsimony of descriptions is absolutely desirable (the Occam's razor; e.g. Gauch, 2003). A stochastic approach, in addition to accepting uncertainty, will also try to quantify this uncertainty. And, in certain feasible cases, it will attempt to reduce uncertainty. Here, however, lies another big difference between a deterministic and a stochastic world view. As described above, the determinist will try to reduce and perhaps eliminate uncertainty by a reductionist approach, where reductionism concerns both the separation of the problem space into small pieces and the reduction of the mathematical description to first principles (which supposedly implies better understanding). In a stochastic world view, the reduction of uncertainty generally requires new data. In our example of a river cross section, the stochastic view calls for a programme of simultaneous measurements of the river stage and discharge plus topographic data of the river. Using these measurements we would no doubt be able to establish a relationship between river stage and discharge, which should be much more accurate (i.e. with lower uncertainty) in comparison to the one derived by Manning's formula. The disadvantage of this relationship is its local applicability to merely the river section for which it was constructed – and nowhere else. This should be well understandable and compatible with a view of the structural character of uncertainty in Nature. One should not expect that by conducting some measurements at a particular place he would be able to reduce the uncertainty all over the world.

Obviously, there is nothing new in the "measurement technique" that we described above as a means to reduce uncertainty. Stage-discharge relationships have been routinely constructed for decades in numerous rivers all over the world. Engineering hydrologists and water professionals know very well that this is the means to reduce uncertainty. So what we are attempting here is to show that the logic of measurements is consistent with a stochastic view of nature and, at the same time, more recent logics that envisage the uncertainty reduction by building very detailed physically-based distributed models that increase the understanding and render measurements not necessary, may have negative effects for several reasons. First, they head science toward deadlocks, as described above. Second, they present understanding in contradiction to observation and measurement of natural processes, which is not scientific. Science cannot be divorced from observation. Third, they lead (and actually have led in the last two decades) to regression of stochastic methodologies in hydrology, which is very negative. And fourth, they give an alibi to current trends to suppress hydrological measurement programmes in several countries. Certainly, "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" (Montanari, 2008). Certainly, modern satellite data provide useful secondary information on hydrological processes, particularly in areas that are not covered by ground measurement networks. However, ground measurements are and will remain indispensable both in modelling and understanding of the natural systems.

4. PUB AGAIN?

In Koutsoyiannis et al. (2008b,c) we have already provided detailed clarification of our views of PUB, a subject that proved to be the most sensitive among the reviewers. Yet

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it seems that we have not been understood enough. Thus, Montanari (2008) states "On the other hand, I do not agree with K2008 when they say that PUB is excluding a stochastic description.... I never had the feeling that PUB was excluding the statistical approach". We do not think that we said that PUB excludes stochastic descriptions. Nor did we criticise Montanari's feeling about PUB, which in fact we share. We did not even refer to PUB per se. We only criticised a paper (Sivapalan et al., 2003) which in our opinion expressed a deterministic view of hydrology (including PUB) and Nature that may have adverse effects (see also Koutsoyiannis et al., 2008c).

Furthermore, Montanari states "Calibration can be eliminated/reduced by using regionalisation, parameter transfer, expert knowledge and many others, in both deterministic and stochastic approaches." We could even agree with this statement if it were out of this context. But we criticized the context, in which the need for calibration would be eliminated (or sharply reduced) by increased understanding. Montanari implies that the need for (direct) calibration would be reduced by using data from elsewhere (regionalization and parameter transfer), with which we agree. These old techniques are indeed very useful in hydrology and will remain very useful. Undoubtedly, it is meaningful to study them further and refine them. However, the implied uncertainty would always be greater in indirect calibration (data or transferred parameters from other catchments) than in direct calibration (data from the basin of interest), as the above example of an in-situ stage-discharge curve demonstrates. In this respect, we agree with Montanari's next statement: "Of course uncertainty would remain there (probably increased with respect to a gauged situation)" adding our opinion that the word "probably" in his statement should rather read "with probability 1".

5. IS OUR DISCUSSION TOO PHILOSOPHICAL?

The view of hydrology brings us to the last part of our discussion of Montanari's (2008) review, related to the novelty or not of our ideas, the need or not to reinvent hydrology and the need or not to involve philosophy in this discussion. We understand Montanari when he states "I do not believe the approach K2008 are proposing is new. I think it is

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just less used than in the past and therefore one may get the feeling that the ideas are unusual and new." After all, he belongs to the Italian hydrological school, which has offered significant contributions in probabilistic thinking and modelling in hydrology. We have great respect for this school and its members (we will avoid to name them not to risk to miss some – but certainly he himself is one of them). Indeed, 30 years ago, the part of our article referring to deterministic vs. stochastic views may have had no meaning. It is the dominance of determinism in geosciences that makes our article timely. This dominance originates from disciplines other than hydrology, from climate research in particular, and serves well the aspiration of a predictable climate in the distant future, and in turn serves well the dominant climate change enterprise. This deterministic "climate" has severely influenced also hydrology and this is the reason for our strong reaction.

In this respect, our statement "Hydrology ... must reinvent itself within this new paradigm and radically rethink its fundaments, which are unjustifiably trapped in the 19th-century myths of deterministic theories and the zeal to eliminate uncertainty" targets the current trend, what we view as a scientific regression due to adverse deterministic and reductionist influences. Since Montanari seems to be annoyed by our term "reinvent" we will rethink it and perhaps change it to "rethink" or something similar (also we must change the term "fundaments" to "fundamentals" or "foundation" because a good colleague who is a native English speaker told us that the former term may imply a funny connotation).

Furthermore, Montanari seems not to welcome the involving of philosophical concepts in our discussions, when he says "the philosophical part in the reply to Blöschl (2008) is perhaps too long". However, we insist that philosophical concepts are absolutely relevant when we discuss scientific foundations. For, as we better explained in our reply to Blöschl (2008) (Koutsoyiannis et al., 2008b) philosophy is the natural foundation of the scientific method in general, and of scientific disciplines in particular (Gauch, 2003). In our opinion, revisiting philosophical concepts, the scientific method and the logic, is

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not a waste of time, nor is it just a general, useless and impractical discussion. Rather, it may have practical utility and may help us to be more productive and avoid failures. For instance we believe that the rejection of the concept of the probable maximum precipitation (PMP), which we regard as one of the biggest failures in hydrology, should be based also on philosophical grounds, i.e. for its logical inconsistencies and its misleading promise of risk-free constructions or practices, as we state in Koutsoyiannis et al. (2008a) (see also Koutsoyiannis, 2007b).

6. FINAL REMARK

Montanari's review offered us the opportunity to see several points of our paper that needed further clarification and to communicate some of our additional opinions and experiences that are related to the theme of the paper but are not contained in it, thus broadening this very interesting discussion. We frankly thank him for his constructive and thoughtful discussion as well as for his positive attitude and his congratulations.

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