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

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INTRODUCTION

Blöschl (2008) correctly notes that in our paper (Koutsoyiannis et al., 2008a) we give strong opinions and he responds with strong characterizations such as that we "got carried away in a zeal of scientific exuberance", that we "seem to have a dogmatic, binary world view", that we "are trapped" in it, that we have a "zeal of promoting one particular subset of statistical methods" and that we "seem to misinterpret some of the literature". It was our deliberate choice to express our opinions in a strong way in an attempt to "stir the waters" and thus we appreciate that Blöschl adopted a similar, if not stronger, tone in his review.

Dogmatism is a negative qualification in science. Perhaps an antidote is to try to understand others' views. In this respect we tried to understand Blöschl's views and locate the differences with our views. In doing so, it became obvious that several of our points were not understood and hence his review gives us the opportunity to further clarify our position and hopefully improve the discussion – with the risk of being pedantic for which we have to apologize in advance.

BINARY WORLD VIEW?

A central point in Blöschl's (2008) review is the concept of "binary world view", a concept that provided the title of his contribution. Initially his definition of this concept relies on categorization of models, in "EITHER physically based deterministic models that allow a 'full description of the detailed physics ...', OR fully probabilistic models that mimic the behaviour of chaotic dynamical systems based on data, with no attempt to represent cause-effect relationships." However, in his next statement, "The world is more diverse than that", he seems to try to put his concept on an ontological basis.

We think that a world view is not directly related to the technicalities of models used in several practical engineering or other applications. Such models have indeed been stereotypically classified in deterministic and probabilistic (or stochastic), and this binary classification appears also in Blöschl (2008) – but hopefully not in Koutsoyiannis et al., (2008a). Rather a world view is a philosophical issue and it should be discussed as such. Perhaps some professional or even research hydrologists may dislike the idea of involving philosophy in hydrology. However, according to Gauch (2003) a proper foundation of science and technology, should follow the hierarchy Common Sense > Philosophy > Philosophy of Science > Scientific Method > Scientific Specialities > Technology. The venerable tradition, which served previous generations well, of philosopher-scientists has been discontinued in the late 20th century, only to survive as a reminder in the title "Doctorate of Philosophy". Gauch argues that a return to this

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tradition would be beneficial for science.

Given the philosophical context of the issue, instead of exposing our own humble views, here we prefer to quote a great philosopher of the 20th century with Austrian (Viennese) origin, Karl Popper. Ironically, he is the same philosopher whose ideas provided the philosophical background of the debate by Konikow and Bredehoeft (1992) and De Marsily et al. (1992), which is cited by Blöschl (2008). However, our context here is entirely different as it refers to a world view, rather than science's premises. Clearly, Popper's world view is indeterministic:

"My thirteenth and last thesis is this. Both classical physics and quantum physics are indeterministic" (Popper, 1982, p. 83).

Popper (1982) uses the metaphor of a film strip, which we attach to a certain given instantaneous state of the world (a given time slice) and we use to represent all past and future time slices. In a deterministic world an instantaneous state or time slice is sufficient to represent all past and future states. At the other extreme, a purely random world, in which no prediction is possible, all film strips leave open all possibilities and would be exactly alike. In that case, there would be no time change, but there would be a unique and time invariant complete catalogue of possibilities. An indeterministic view is none of the above, yet it contains catalogues of possibilities (or propensities), to which a probabilistic, time-varying measure or weight can be ascribed. Thus, close to the time slice to which the film is attached, most weight is given to the possible states that are very similar to the states of the "deterministic" film strip whereas as we move farther away from this instant into the past or future, increasing weights are given to other, dissimilar states. In the latter case, physical laws connect probabilities of different states, rather than the states themselves. The interested reader may find this discourse in Popper's (1982) own words in the Appendix accompanying this comment.

This indeterministic world view naturally calls for a stochastic world description. And this is the world view we try to advocate. Clearly, this view is different from classical

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deterministic and classical probabilistic models, and does not support the classical dichotomy of probabilistic and deterministic models. The "particular subset of statistical methods" that according to Blöschl we try to promote (p. 2936 and Fig. 7) is a departure from what Popper calls "the other extreme", the classical probabilistic model that admits pure randomness without dependence in time.

It is unfortunate that the motto that our manuscript contained in the beginning of the Introduction to support our indeterministic view was omitted in the HESSD typesetting (and it is our fault for omitting to check this). The motto is a quotation from a Greek philosopher, Heraclitus, which reads (transliterated into Latin characters) "Aeon pais esti paizon pesseuon" or "Time is a child playing, throwing dice". Apparently, there have been opposite views, including those of Laplace and Einstein (cf. his famous saying, 2500 years after Heraclitus, "He [God] does not throw dice") which were criticized by Popper and other modern thinkers (see citations in Koutsoyiannis et al., 2008a, p. 2935).

DETERMINISTIC AND STOCHASTIC MODELS

Models try to represent real world but are always approximations of reality. We agree with Blöschl (2008) that model choice depends on the problem and the data. Clearly, there are problems in which a purely deterministic model is a virtually perfect approximation. Such problems involve rather simple systems and small prediction time horizons.

On the other hand, it has been a widespread stereotype that physics (including physical hydrology) is something separate from probability, statistics or stochastics: physics is related to understanding and description of cause-effect relationships (this is also implied by Blöschl) while stochastic tools are just mathematical/algorithmic models. In our opinion, which we tried to explain in Koutsoyiannis et al. (2008a), this is a common fallacy. Among other things we mention (p. 2935) the "statistical physics, which used the purely probabilistic concept of entropy (which is nothing other than a quantified

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measure of uncertainty defined within the probability theory) to explain fundamental physical laws (most notably the Second Law of Thermodynamics), thus leading to a new understanding of natural behaviours and to powerful predictions of macroscopic phenomena". Which of the following two concepts provides better understanding of the natural phenomena related to heat? Lavoisier's deterministic/mechanistic theory (persisted in scientific literature until the end of the 19th century) of the caloric fluid, a hypothetical fluid (a weightless gas) that flows from hotter to colder bodies (passes in pores of solids and liquids)? Or the 20th century's statistical thermodynamics?

It is interesting to note that the latter statistical theory, applied in systems with huge numbers of molecules, provides, in effect, "sharp" laws (that could be called deterministic) of macroscopical phenomena, such as the law of ideal gasses. This however should not mislead us to characterize the statistical thermodynamics as a deterministic model. It is a probabilistic model, and its results highly rely on quantum physics considerations, in which probability is a fundamental concept. Thus, statistical thermodynamics has formed a sound paradigm entirely based on probability as a tool both for explanation (or understanding) and mathematical description of natural behaviours. Furthermore, the Second Law of Thermodynamics essentially shows that Nature works in a way that maximizes uncertainty in complex systems. Following Nature's behaviour and applying the principle of maximum entropy (maximum uncertainty) to any type of system we can infer useful knowledge about the system's behaviour. This knowledge, however, is no longer expressed in terms of certainty about the sharp states of the system, but rather in terms of probabilities of these states.

As we tried to demonstrate in the depiction of Fig. 6 in Koutsoyiannis et al. (2008a) there are significant differences between typical thermodynamical systems (e.g. a mass of gas or liquid) and a hydrological system. The latter is much more complex and necessarily implies a more complex probabilistic behaviour. Yet it seems that the principle of maximum entropy can be applicable in hydrological systems (e.g. Koutsoyiannis, 2005a, b, 2006b; Koutsoyiannis et al., 2008b).

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Another important result of the 20th century science, this time coming from the study of chaotic dynamical systems, is that uncertainty/unpredictability emerges even in simple nonlinear fully deterministic systems for long prediction horizons. It is very easy to demonstrate (e.g. Koutsoyiannis, 1995, 2006a) that, beyond a certain time horizon, a probabilistic model may have better prediction skill, even in a fully deterministic system with fully known deterministic dynamics, than the deterministic model based on this dynamics. Thus, it is meaningful to use stochastic models even for fully deterministic systems.

If the latter statement sounds to be an exaggeration or exuberance, we can provide two additional examples where fully deterministic problems are effectively resolved by stochastic methods. The first is the numerical integration of a completely known (deterministic) vector function on a high-dimensional space. It is very easily shown (e.g. Niederreiter, 1992, pp. 1-5) and also implemented in mathematical software (e.g. http://mathworld.wolfram.com/MonteCarloIntegration.html) that a Monte Carlo (stochastic) integration method (in which the points of the function evaluation are taken at random) is faster/more accurate than a deterministic method (in which the function evaluations are done at an equidistant grid) and thus preferable over classical numerical integration (for dimensions higher than 4). The reason for this is that stochastic simulation is not affected by "the curse of dimensionality". The second example is global optimization at nonconvex surfaces. It is well known (and also implemented in water resources problems including hydrological model fitting, e.g. Vrugt et al., 2003) that evolutionary and genetic methods, all of which rely on stochastic techniques in searching the feasible space, are in effect the only feasible solution in complex problems involving many local optima.

All above indicate the power of stochastic approaches to all types of problems, even the deterministic ones. In addition, it is completely untrue that stochastic methods have no relation to cause-effect relationships and that, as put by Blöschl (2008) "randomness precludes cause-effect relationships". Quite the opposite. Methods to detect such rela-

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tionships based on available data (i.e. on induction) are mostly probabilistic/stochastic in nature. These include regression methods, spectral methods to detect periodicities, and entropic representations to detect non-periodic deterministic controls and reconstruct the topology of attractors. Once such deterministic controls are detected, stochastic processes can incorporate them in their structure. And once stochastic models are built, they enable us to get insight into complex systems and solve difficult problems, through Monte Carlo simulations. All in all, stochastic processes have enabled views, understanding and modelling of the world that are much richer than mechanistic/deterministic views.

Obviously, a catchment is a hydrological system that transforms rainfall to runoff and this transformation implies a deterministic control. Purely deterministic hydrological models utilize this control to predict river flow, a prediction which very often gives satisfactory answers to real world problems. However, a stochastic view of the same problem is more consistent and more powerful. Such a view will fully utilize the deterministic control and simultaneously would admit (a) a probabilistic distribution of catchment properties (such as the distribution function topographic index in TOPMODEL; Beven et al., 1995) to represent catchment inhomogeneity, (b) a probabilistic representation of model parameters (such as in the equifinality approach, Beven and Binley, 1992, or in Bayesian approaches, e.g. Vrugt et al., 2003), and (c) a stochastic description of the total predictive uncertainty.

With the above discourse we aimed to show that the classical dichotomy of deterministic vs. stochastic models as two irreconcilable approaches, which is also the view expressed by Blöschl (2008), is not part of our thinking. Rather, we regard such dichotomies (and other related ones such as signal vs. noise) as part of those notions that should be rethought and perhaps abandoned in the "new paradigm" we propose. We do propose that the stochastic thinking should replace the deterministic thinking. This is not equivalent to replacing deterministic models by models based on pure randomness or by strange concepts such as "the assertion that the flow paths are fractal"

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(Blöschl, 2008, p. S1751) – which, by the way is incorrect, except perhaps in a limited range of scales, because otherwise it would imply an infinite amount of time for a water particle to be transported (Veneziano and Langousis, 2008). Rather, it is equivalent to enriching the deterministic approaches with all those aspects related to uncertainty, thus enabling more complete views and more pragmatic approaches. Also, what we propose is not equivalent to abandoning cause-effect relationships. Rather, it is equivalent to generalizing the concept of deterministic causality in a stochastic context, following earlier ideas by Suppes (1970).

THE PUB INITIATIVE

A major point of Blöschl's (2008) critique, in which our paper is accused of misinterpreting (some of) the literature, is related to our reference to Sivapalan et al. (2003), the "official" paper of the IAHS Decade on Prediction in Ungauged Basins (PUB).

Indeed, in Koutsoyiannis (2008a) we criticised some of the formulations in Sivapalan et al. (2003) and discussed their implications. We wish to stress that our criticism does not imply a negative position against PUB or a depreciation of the specific document as a whole. On the contrary, we think that PUB is a very important and strongly needed initiative and that the document provided a considerable input to the hydrological community, to make our research effort more coherent and structured. PUB has inspired many researchers and professional hydrologists and generated many conferences and significant research, whose methods and results, nevertheless, may not be in line with those formulations of Sivapalan et al. (2008) that we criticize. In this respect, we offered our criticism, which again starts from philosophical grounds, as a discussion for a potential correction or adaptation of aspirations, premises and methodology. The need for adaptation is not uncommon in science plans and does not imply depreciation of the plan.

Again, Blöschl's reaction to our criticism offers us the opportunity to clarify it. Blöschl discovers that, "ironically", our statement that concludes with our emphasis to "recog-

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nize the structural character of uncertainty" has very similar wording with Sivapalan et al. (2003), i.e., "The PUB activities ..., it is hoped, will lead to new predictive approaches based on a combination of current and new theories, and of existing and potentially new data sets." Admittedly, we have been unable to recognize any similarity of wording or reference to the notion of the structural character of uncertainty in Sivapalan et al., a notion that is central in our views. Furthermore, Blöschl states that, "Carried away by a binary world view, [we] suggest that, in the PUB decade, uncertainty is to be ELIMINATED (p. 2935, l. 13) while the aim of PUB is that uncertainty is to be REDUCED". Here we have to admit that our use of the term "elimination" may not correspond literally to the term "sharp reduction" of predictive uncertainty that Sivapalan et al. (2003, p. 874) have used. However, we reproduced their original Fig. 5 in our Fig. 5, retaining the original caption verbatim. This figure is absolutely suggestive of what they mean by "sharp reduction" and gave us the impression of being far away from recognizing any structural character of uncertainty. Clearly, we do not agree that such "sharp reduction" is feasible.

Blöschl wonders what makes us think that Sivapalan et al. serve the aspiration of achieving pure deterministic modelling. Our reasoning is this. Sivapalan et al. state "It is expected that PUB will herald a major change of paradigm in surface hydrology from one that is dominated by calibration to a new exciting one based on understanding". We have also reproduced in our Fig. 5 the original Fig. 3 and its caption. This figure seems to say that at the outset of PUB (2003) the existing models can only perform in gauged basins and are totally based on calibration whereas the existing understanding is at zero level (a state that seems they characterize a "cacophony of noises" in their Fig. 9 which we also reproduced in our paper). At the end of PUB (2012), new innovative models based on increased understanding will replace old models and the need for calibration will be eliminated.

We believe that all these reflect a deterministic thinking. Can any approach eliminate the need for calibration, except a fully-deterministic one that assumes almightiness of determinism? What does the aspiration to eliminate the calibration need indicate, except the almightiness of deduction (i.e. that logical method of inference which does not require data)? Apparently, the regionalization methods, which constitute a valuable tool in dealing with ungauged basins, are not free of the calibration need (the calibration is done in other catchments). Thus, regionalization does not replace calibration with understanding. Rather, it depends on data and is based on induction. And the dependence on data, as contrasted to understanding, is much stronger in regionalization than in the typical calibration in a single basin. In Koutsoyiannis et al. (2008a) we have tried to explain (including a brief discussion of induction vs. deduction in Fig. 6) why data and calibration are always necessary in complex heterogeneous systems, and consequently to stress the need for a departure from a deterministic thinking in which understanding (and deduction) would be able to replace data (and induction).

It would be interesting to assess the current (i.e. past half way of the PUB Decade) state in terms of the understanding which (as promised) would replace calibration. Where would the current state plot in Sivapalan's et al. Fig. 3? We fear that the most important progress in terms of understanding is the understanding of the necessity of data and the indispensability of measurements and calibration. This opinion seems also to be shared by others: Quoting K. Beven from Tchiguirinskaia et al. (2008), "we need those better measurements, and not necessarily better models", "the answer is in the data and a new theory alone would not be enough" and "the focus in the future should be oriented on new and more accurate measurement techniques".

Blöschl (2008) concludes: "Sounds as if Koutsoyiannis et al. had something to contribute to PUB." Indeed, we wish to believe that the paper and this discussion have something to contribute to PUB. We think that challenging the promise of "reducing predictive uncertainty" (in deterministic terms) by changing the focus "from calibration to understanding" is a contribution. To this aim we can add the following points summarizing our ideas:

(a) Deterministic thinking (which is related to a world view and its implications on science, and is not identical to using deterministic models) and mechanistic analogues may become obstacles when dealing with complex real-world systems. The understanding of natural behaviours necessarily relies on probability (as for instance in thermodynamics).

(b) Deduction (as in mathematical theorems, including those of probability theory), while effective in classical thermodynamics where systems consist of a large number of components with similar characteristics (e.g. the molecules of a gas), is ineffective in hydrological and geophysical systems where each component is unique (not identical to each other).

(c) Induction (inference from available data, using probability) is the scientific method of choice in hydrology and this requires data and obtains estimates based on data, i.e. calibration.

(d) Uncertainty is not just a result of imperfect understanding or deficient data and models, i.e. subjective, or a temporary set-back, but rather – and to a large degree – a structural, inherent property of Nature, i.e. objective.

(e) Convergence of scientific approaches (particularly at a premature stage) may not necessarily lead to progress. Rather, Nature herself emphasizes the importance of divergence and diversity, as well as the important role of indeterminism in evolution.

A CONCLUDING REMARK

The notion of the structural character of uncertainty in Nature (and the related indeterministic world view), along with its implications on science and technology, is discussed in Koutsoyiannis et al. (2008a) not merely with respect to PUB. Rather, it is put as an overarching idea for a number of issues related to climate, hydrology, energy and water. Our plea in the paper title for "recognizing uncertainty" should not be interpreted as a pessimistic invitation to concede a defeat in a battle against uncertainty. In our view, uncertainty is not an enemy to eliminate or to sharply reduce. Rather, we regard

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it as very "positive" property of Nature and Life, the mother of creativity and evolution, and the reason why Life is fascinating. This latter is well known to people working in the media, who spend big amounts of money for broadcasting news and sports games "live" (i.e. when the outcome is uncertain); if certainties and determinism were more exciting, they would broadcast recorded versions in the next day (e.g. when the score of the game would be known). Without uncertainty, life would be boring and concepts such as hope, will (particularly, free will), freedom, expectation, optimism, etc., would hardly make sense. A technocratic system where a few super-experts could accurately predict, using super-models, what will happen in the next hundred years and thus could have full control on the environment and the society, would not be livable, in our opinion. Fortunately, this will never happen because uncertainty is a structural property of Nature and Life.

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