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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.

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1. INTRODUCTION

We are honoured by Sivapalan's (2008) review – or "response" as he calls it – given his role in the Predictions in Ungauged Basins (PUB) initiative (the role of "leading [it] in its formative years"). However, we wish to clarify that our criticism of some of the formulations in the PUB "official" science plan (Sivapalan et al., 2003) is not the key issue of our paper (Koutsoyiannis et al., 2008a), nor does it imply a criticism about PUB itself.

Our paper, as manifested in its title, tries to take two central issues in the current scien-

tific and technological scene, that is climate and energy, and relate them to water and hydrology. The reasons why climate and energy are central issues are both objective (particularly in energy) and subjective or political (particularly in climate). These two issues are closely interlinked (cf. the Panel Discussion on "Climate Changes and Energy Challenges" of the 2008 Meeting of Nobel Laureates at Lindau on Physics) {Endnote 1}, regardless of our successful or unsuccessful presentation of the linkages in our paper. In addition, we tried to present our ideas of current and future linkages of water and hydrology with climate and energy focusing on (a) a regulating role that water could play in a future landscape of energy management, in which renewable energies would become the norm, and (b) the adverse influences of current climate research to hydrology, both on its orientations (the aspiration of a deterministically predictable future) and its role (a subservient to the needs of the climate change enterprise, as testified for instance in the European Union's Framework Programmes).

Some of Sivapalan's (2008) comments are similar in essence with those of Blöschl (2008) to which we wave already responded in Koutsoyiannis et al. (2008b). This particularly concerns the discussion of deterministic vs. stochastic world view, which we have tried to put on more logical and philosophical grounds, instead reproducing the stereotypical technicalities. Nonetheless, in the following two sections we give our responses to some of Sivapalan's key comments classifying them in two categories: points of agreement and points of disagreement. We tried to quote his original formulations to avoid any type of misrepresentation of his statements.

2. POINTS OF AGREEMENT

2.1 Climate change impacts

Sivapalan (2008) states: "I do agree with the authors that some of the research that goes on in the name of climate change impacts, especially in hydrology and water resources, is pseudo-scientific and does considerable disservice to our scientific discipline". We endorse this statement.

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2.2 Foundation of hydrology

Sivapalan (2008) states: "In spite of my disagreements with the authors, I do believe that hydrology does need to move on from its 20th century foundations"; "Hydrologic practice too has demonstrated remarkable success in being based on data and observations and a good appreciation of both causality and probability"; "Hydrology must extend the mechanistic worldview that it is currently founded on to embrace an evolutionary perspective, in which every aspect of the system is changing albeit at different rates." We endorse all these statements (even though we would not present causality and probability in contrast).

2.3 PUB initiative

Even though PUB is not the central issue of the paper, we are happy that it stimulated the discussion by Blöschl (2008) and Sivapalan (2008). We trust that this is also appreciated by Sivapalan and his colleagues as they had explicitly stated "New thinking and discussions, particularly on a conceptual level, are required that allow the reevaluation of current assumptions and paradigms in light of new requirements for our field" (Wagener et al., 2004) {Endnote 2}.

What we found most surprising in Sivapalan's review is his current position about PUB, with which we are pleased to agree, despite the fact that Sivapalan diagnoses "a fundamental misreading of the vision and goals of the PUB initiative" in our paper. We do not find this misreading in Koutsoyiannis et al. (2008a) because: (a) We do not discuss the PUB initiative in general (our appreciation of which we have clarified in Koutsoyiannis et al., 2008b) nor the "many PUB-related publications" (which indeed indicate a plurality of approaches) but we only refer to Sivapalan et al. (2003); and (b) We avoided to provide interpretations of Sivapalan et al. (2003), which could potentially give away our misreading, but essentially we quoted key concepts that were also depicted in figures which we reproduced, thus giving more emphasis on the pictorial representations rather than on verbal formulations. We believe that visual artistic depictions are more

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powerful and perhaps more honest and faithful in communicating specific messages. As the proverb says, "A picture is worth a thousand words" {Endnote 3}.

Now Sivapalan (2008) focuses on the "plurality of models to fit different places and accommodate the diversity of applications" instead of the earlier "convergence of a plurality of approaches ... with a single-minded focus" or the promised replacement of the "cacophony of noises" (existing models) with a "harmonious melody" (new innovative models). He also states: "The reality is that the world is poorly determined and understood (and will remain so forever)" and "...our judgments no doubt clouded by increases in computer power and improvements in process understanding to the extent that we have lost the art of learning from careful observations" in lieu of the promised "sharp reductions of predictive uncertainty" and "paradigm change – from models based on calibration to models based on increased understanding". The new formulations are much closer to our views. In addition Sivapalan (2008) seems to try to weaken the weight of the "wedge diagram" in Sivapalan et al. (2003) that we criticised, in favour of "a simpler and more elegant diagram" in Wagener et al. (2004). This we regard as a positive gesture but, in our opinion, even the latter diagram does not differ significantly from the initial one. It too promises, during the PUB initiative timeline (i.e. up to 2012), a sharp increase of understanding (starting from almost zero at the beginning) and a sharp reduction of uncertainty (falling to almost zero at the end) {Endnote 4}. We welcome the newer Sivapalan's thoughts and focus but we regret that the PUB initiative has been expressed with these illustrations, promising a utopian future of hydrology.

3. POINTS OF DISAGREEMENT

3.1 Deterministic vs stochastic approaches

Sivapalan (2008) seems to adopt a balanced approach in saying "It is true that over the past 25-30 years we hydrologists have invested a lot on ever more deterministic models, without satisfactorily addressing the difficult problem of model identification

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and parameter estimation" but "... the authors seem themselves fixated on so-called stochastic models as the panacea against the evils of deterministic models, and in doing so revive old arguments about the merits and demerits of stochastic versus deterministic approaches."

The balanced approach would perhaps be optimal if the current views of deterministic and stochastic approaches were balanced themselves. This is not the case. We have tried to demonstrate in the article that the deterministic view has dominated in such an extent that has obscured the structural character of uncertainty in Nature and the limits of predictability. This dominance does not provide good service to science. Therefore the issue we wish to discuss is not the "merits and demerits" of deterministic and stochastic approaches, but the adverse effects and the regression of science due to the dominance of determinism. We think that a provocative style of discussion better serves this purpose.

We do not aim to survive old arguments. Rather we think that our arguments and views are new or at least are not identical to dominant views in our community. For example, we do not share Sivapalan's view of stochastic models as "purely data-based" models. This view, that probability and stochastics provide only black-box or data-based approaches, is also quite common in hydrological texts but it is just a misrepresentation of what probability and stochastics really are. Perhaps a better idea of what probability and stochastics really are could be obtained by reading texts out of the hydrological literature. We could mention lots of such texts and we have already cited some in Koutsoyiannis et al. (2008a,b). Here we wish to add the books by Papoulis (1991) on probability and stochastic processes (for which we doubt if it contains even a single data-based example, despite its engineering orientation) and Jaynes (2003), with the fascinating (and absolutely precise and successful, in our opinion) title "Probability Theory: The Logic of Science".

In this respect, we disagree with Sivapalan's opinion that the "so-called Blue Book edited by Professor Peter Eagleson" (US National Research Council, 1991) offers a

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balanced foundation of hydrology. Certainly the book (also cited in Koutsoyiannis et al., 2008a) has greatly contributed in highlighting promising research topics in hydrological sciences, but we do not recognize the role of stochastics in it. It rather offers general statements such as "The dichotomy in the current state of the art between the stochastic approaches and the deterministic approaches is unsatisfactory." Furthermore, it seems to us that the book, in its section "Stochastic-Dynamical Analysis of Hydrologic Time Series" and beyond (pp. 199-207) invests hopes to deterministic dynamical approaches based on strange attractors, instead of stochastic descriptions. Lots of studies that have followed this path and have "discovered" such low-dimensional chaotic attractors may be severely flawed (Koutsoyiannis, 2006a). With all respect to the colossal Eagleson's work and offer to hydrology, we do not agree with several of his views on stochastics, for example with his reductionist view of climatic and hydrological processes (or variables), which he regards as composing of identifiable separate parts, that is a deterministic periodic part, a deterministic aperiodic part (trend) and a stationary random part (Eagleson, 1970, p. 155-156). Nor do we agree with some of his related statements such as "The spacing and sizing of individual [rainfall] events in the sequence is probabilistic, while the internal structure of a given storm may be largely deterministic" (Eagleson, 1970, p. 184), in which he presents epistemic features (deterministic or stochastic approaches) as ontological properties of natural processes. These views are dominant even today, almost 40 years after publication of this book (see Koutsoyiannis, 2006b, for a detailed critical analysis of such views).

3.2 Past, future and uncertainty

Sivapalan states: "However, we must recognize that the days that we can completely rely on past observations (i.e., as a guide to the future) are way past considering the enormous changes that humans are making to the environment (and climate change is just one manifestation of this)." This opinion was also pronouncedly promoted lastly by others (e.g. Milly et al., 2008). However, we must recognize that any type of model, which is supposed to assess the hydrological regime under changed future conditions,

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needs data of the past to be calibrated and validated. Models that are not validated in the past cannot be reliable for the future. Changing background conditions may need nonstationary approaches, but we should not forget that nonstationarity is clearly a stochastic concept and hence stochastics is the proper mathematical tool to deal with it. Finally, if past observations were not a useful guide to the future, the huge efforts to obtain historical or proxy climatological and hydrological records to trace back the past (whose value Sivapalan recognizes) would be pointless. A precious contribution of all (recorded and proxy) information of the past is that it has allowed a better knowledge and understanding of the huge variability of the natural processes and the implied structural (not eliminable) character of uncertainty. Given this magnitude and character of uncertainty, we fear that those who reject the past as a guide to the future and seek to sharply reduce uncertainty, they may not find any other sufficient guide.

The perception of uncertainty itself constitutes another difference in our and Sivapalan's views, as testified in his statement "... by combining the knowledge and understanding of the physical (and biological etc.) system of interest, with explicit acknowledgement of the lack thereof (which means uncertainty)." In our view, uncertainty is not lack of knowledge or lack of understanding but simply lack of certainty. Thus, we think that uncertainty, whose structural character we have tried to emphasize in Koutsoyiannis et al. (2008a,b), is a property of Nature, which we should try to "know" and "understand" as such. For instance, entropy is a measure of uncertainty and the Second Law of Thermodynamics, according to which uncertainty increases in time, constitutes an important knowledge about Nature.

3.3 Climate and its predictions

In his section entitled "Usual diatribe against the IPCC and GCMs" Sivapalan criticises our discussion of climate research using several arguments, some of which we have already discussed above. It seems that he may have misunderstood our perception of climate or even what climate really is. Specifically, he states: "However, one needs to give more credit to the atmospheric science community than the authors have" and "I

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look at the enormous improvements that that the atmospheric science community has made in weather prediction in the last 25 years". First, we were not able to find in our paper anything that discredits the atmospheric community. Second, we do not agree with confusing atmospheric sciences and weather on the one hand and climate on the other hand. Apart from the difference in the time scale of application (climate is the average state of the atmosphere at a long time scale, e.g. 30 years, where "average" emphasizes the statistical basis of the concept), there is another, more fundamental difference of weather and climate. While weather can be modelled and predicted based on the atmosphere alone – and apparently the predictions are meaningful for a horizon of a few days – the climate cannot. The climate system is much broader than the atmosphere as it also includes land, the oceans, the cryosphere (ice-covered regions of the world), and the terrestrial and marine biospheres (e.g. Pielke, 2008). In addition, the climate is influenced by other factors, such as solar and volcanic activities. Thus, the improvement in weather prediction (at a time scale of days) does not necessarily imply an improvement in climate prediction (at a time scale of decades).

As we stated above, we do agree about the usefulness "to trace back the evolution of climate in the past and then project into the future". Backtracing of climate is served mainly by paleoclimatology (rather than models), whose findings are impressive and help us know and understand the huge natural climatic variability and uncertainty. Paleoclimatological methods heavily rely on statistics and, thus, lack of proper statistical knowledge and methodology can result in serious shortcomings (Wegman et al., 2006; see also a compelling story in a popular science journal by Crok, 2005).

A final Sivapalan's comment that we wish to respond is this: "In my view it is some of the climate-change skeptics who display ill-informed and half-baked opinions on the basis of anecdotal evidence, or based on misunderstanding and mis-representation of what the IPCC is all about. Some of the examples and arguments that are presented in the commentary by Koutsoyiannis et al. (2008[a]) fall into this category." It seems here that Sivapalan puts us the label of climate "sceptics", for which we do not have

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a particular problem, given that the term is etymologized from the Greek "skepsis" meaning "thought". Nor do we have any problem if he finds our opinions "half-baked" because we believe that scientific progress is better served by "half-baked" outlooks than by "settled" fallacies. On the other hand, we do not think that our opinions are ill-informed and based on anecdotal evidence. A simple look at the reference list, which includes some of our own research contributions, suffices to show that this is not the case with our paper.

4. FINAL REMARK

Sivapalan's critique offered us the opportunity to see the difficulties in communicating some ideas that may depart from those in everyday discussions. We thank him for this as well as for his time and effort, and we wish that his estimate that our opinions may become "a reflection of the opinions held by a significant segment of the hydrological community" comes true.

ENDNOTES

{1} This meeting was held in 28 June-4 July 2008 and several Nobel Laureates (Deisenhofer, Giaever, Michel, Osheroff, Rubbia, von Klitzing, Steinberger – not to be confused with Gore and IPCC who were awarded the Nobel Peace Prize) contributed in this Panel Discussion, from which the importance of climate and energy issues in research and its societal implications, as well as some relevance with the ideas discussed in our paper can be verified (http://www.lindau-nobel.de/PublicMeetingProgram.AxCMS?Meeting=105; http://www.lindau-nobel.de/MediaContainer.AxCMS?type=lectures&meeting=105&elementID=22). However, this meeting was not the inspiration of our paper. In fact the paper is (verbatim, except for the illustrations) part of our (rejected) research proposal to the IDEAS programme written and submitted to the European Research Council in February 2008.

{2} Most of our criticisms were known to Sivapalan et al. (2003) from the outset as

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they were contained in a review by Koutsoyiannis, for which he was thanked in the acknowledgments. Some of our points were also presented by Koutsoyiannis publicly in the last IAHS/IUGG General Assembly in Perugia in 2007 (Nalbantis et al., 2007) and were discussed with much interest. Apropos, we wish to praise HESSD for archiving and making public all discussions and reviews. In our opinion, eponymous public reviews serve science (as well as scientists, for the accountability it provides) in the best way – as this discussion demonstrates.

{3} Related to this discussion is a paraphrase in Nalbantis et al. (2007), "A measurement is worth a thousand models".

{4} We found that the article is openly available on line in cee.uiuc.edu/people/kumar1/Reprints/Wagener%20et%20al.%202004%20PUB%20EOS.pdf.

REFERENCES

Blöschl, G.: 'A binary world view': Interactive comment on "HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability"" by D. Koutsoyiannis et al., Hydrol. Earth Syst. Sci. Discuss., 5, S1749-S1753, 2008.

Crok, M.: Kyoto protocol based on flawed statistics (English translation by A. den Tex), Natuurwetenschap & Techniek, February, 2005 (http://www.uoguelph.ca/~rmckitri/research/Climate_L.pdf).

Eagleson, P. S.: Dynamic Hydrology, McGaw-Hill, NY 1970.

Jaynes, E.T.: Probability Theory: The Logic of Science, Cambridge University Press, 2003.

Koutsoyiannis, D.: On the quest for chaotic attractors in hydrological processes, Hydrological Sciences Journal, 51 (6), 1065-1091, 2006a.

Koutsoyiannis, D.: Nonstationarity versus scaling in hydrology, Journal of Hydrology, 324, 239-254, 2006b.

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Koutsoyiannis, D., Makropoulos, C., Langousis, A., Baki, S., Efstratiadis, A., Christofides, A., Karavokiros, G. and Mamassis, N.: HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability". Hydrol. Earth Syst. Sci. Discuss., 5, 2927-2949, 2008a.

Koutsoyiannis, D., Makropoulos, C., Langousis, A., Baki, S., Efstratiadis, A., Christofides, A., Karavokiros, G. and Mamassis, N.: 'Which Binary World View?': Interactive comment on "HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability"" by D. Koutsoyiannis et al., Hydrol. Earth Syst. Sci. Discuss., 5, S1761-S1774, 2008b.

Milly, P.C.D., Betancourt, J., Falkenmark, M., Hirsch, R.M., Kundzewicz, Z.W., Lettenmaier, D.P. & Stouffer, R.J.: Stationarity is dead: Whither water management?, Science 319(5863), 573, 2008.

Nalbantis, I., Efstratiadis, A., and Koutsoyiannis, D.: On the use and misuse of semi-distributed rainfall-runoff models, XXIV General Assembly of the International Union of Geodesy and Geophysics, Perugia, International Union of Geodesy and Geophysics, International Association of Hydrological Sciences, 2007 (http://www.itia.ntua.gr/en/docinfo/744/).

Papoulis, A.: Probability, Random Variables, and Stochastic Processes (third edn), 57. McGraw-Hill, New York, USA, 1991.

Pielke, R., Sr.: What Is The Difference Between Weather and Climate? Climate Science: Roger Pielke Sr. Research Group News, 2008 (http://climatesci.org/2008/07/02/what-is-the-difference-between-weather-and-climate/)

Sivapalan, M.: 'Response to Koutsoyiannis et al.': Interactive comment on "HESS Opinions "Climate, hydrology, energy, water: recognizing uncertainty and seeking sustainability"" by D. Koutsoyiannis et al., Hydrol. Earth Syst. Sci. Discuss., 5, S1781-S1787,

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2008.

Sivapalan, M., Takeuchi, K., Franks, S. W., Gupta, V. K., Karambiri, H., Lakshmi, V., Liang, X., McDonnell, J. J., Mendiondo, E. M., O'Connell, P. E., Oki, T., Pomeroy, J. W., Schertzer, D., Uhlenbrook, S., and Zehe, E.: IAHS Decade on Predictions in Ungauged Basins (PUB), 2003-2012: Shaping an exciting future for the hydrological sciences, Hydrol. Sci. J., 48(6), 857-880, 2003.

US National Research Council, Committee on Opportunities in the Hydrologic Sciences: Opportunities in the Hydrologic Sciences, National Academy Press, Washington, DC, 1991.

Wagener, T., Sivapalan, M., McDonnell, J. J., Hooper, R., Lakshmi, V., Liang, X., and Kumar, P.: Predictions in Ungauged Basins (PUB): A catalyst for multi-disciplinary hydrology. EOS, Newsletter of the Amer. Geophys. Union, 85(44), 451, 457, 2 November 2004.

Wegman, E. J., Scott, D. W., and Said, Y. H.: Ad Hoc Committee Report On The 'Hockey Stick' Global Climate Reconstruction, US House Committee on Energy and Commerce, 2006.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2927, 2008.