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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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Comments on Koutsoyiannis et al. from M. Sivapalan

I suspect that the authors' opinions, in one form or other, may be shared by many members of the scientific community. For them to be credible, however, these opinions must be articulated logically and convincingly, so readers can decide where they agree, where they disagree, and where they can agree to disagree. Unfortunately, this paper has adopted a scatter-gun approach, throwing up a number of diverse opinions, without a coherent thread connecting them. Here I try to distill what I see as the key issues behind the authors' opinions and provide my responses to them.

A. Deterministic vs Stochastic Approaches

The main complaint appears to be that current approaches to predictions are somehow fixated on deterministic models. 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 and parameter estimation; 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 and thereby constraining the predictions of such models, to which we need to return. It is understandable therefore that there is natural reaction against these increasingly sophisticated and complex deterministic models.

In their response, it appears that 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. 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). The earth system is changing and changing fast; neither the stochastic method based on past observations, which ignores physical and biological laws (i.e., cause and effect relationships), nor the deterministic method, which ignores the uncertainty, unknowability and perhaps unpredictability of many aspects of this complex system, is the right way.

The reality is that the world is poorly determined and understood (and will remain so forever). 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, especially in the changing environment we live in, by combining the knowledge and understanding of the physical (and biological etc.) system of interest, with explicit ac-

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knowledgement of the lack thereof (which means uncertainty).

Clearly this calls for a combined stochastic-dynamic approach that pays attention to not only physics but also biology and chemistry, and also human behavior, that govern not just system response (at the present time), but also the system evolution in the past and into the future. 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. The more we know and understand, the more recognition we have of what we do not know and understand.

I believe this is the correct way, and apparently unrecognized by the authors, this approach is already well reflected in much of the research we are already doing and in scores of publications that one can find in the literature. It is also well established in hydrologic practice (e.g., Australian Rainfall and Runoff, UK Flood Studies Report). Nowhere is this outlook more clearly articulated than in the research themes outlined in the Predictions in Ungauged Basins (PUB) initiative that I had the fortune of leading in its formative years. The wedge diagram that the authors reproduced from the PUB Science Plan (Sivapalan et al., 2003) calls for a new paradigm, based on models and predictive approaches that rely less on calibration and more on improved understanding. A subsequent publication by Wagener et al. (2004) articulated this needed paradigm change much more clearly using a simpler and more elegant diagram. I disagree with the authors' notion that the stated goal of PUB to reduce predictive uncertainty is somehow tantamount to an unstated subliminal vision of a utopian, deterministic world where everything will be known and understood, and hence perfectly predictable. This is a fundamental misreading of the vision and goals of the PUB initiative: it is quite the opposite of what was intended, as articulated in many PUB-related publications. Indeed, a careful reading of the PUB Science Plan will show that its framers clearly understood that there is no one "gospel truth" about the physical system we hydrologists study, namely catchments, and instead articulated a broader vision of "plurality", i.e., indeed, plurality in everything (e.g., applications, models, research programs). In fact, they did deliberate seriously on the viability of going after just one "community model", abandoned it as being unachievable and unworkable, and instead went for the notion of a plurality of models to fit different places and accommodate the diversity of applications.

B. Usual diatribe against the IPCC and GCMs

The main points in the authors' argument are: (i) the IPCC predictions have not been tested, or that predictions have been proved wrong, (ii) the IPCC predictions are based on models that are deterministic; that the mechanisms are so complex and unknown that only stochastic predictions will be valid, or no predictions are possible (e.g. chaos); and (iii) IPCC focuses too much on the symptoms, i.e. the effect of elevated CO2 concentrations resulting from use of fossil fuels, since fossil fuel use will taper off soon as we run out of them, and so why bother. I am not an expert on climate change and I do not have strong opinions on the climate change debate, some of which is political and social.

However, one needs to give more credit to the atmospheric science community than the authors have. On the one hand, it is not true that all of them routinely and dogmatically rely on (deterministic) GCMs for their predictions. There is great diversity of methods being used, including simpler (essential) models that help to trace back the evolution of climate in the past and then project into the future, and there is considerable open debate within the climate change community about the models and model predictions. The IPCC projections merely represent a consensus view based on current understanding and model capabilities. I do not understand how consensus can be confused as dogma. 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

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examples and arguments that are presented in the commentary by Koutsoyiannis et al. (2008) fall into this category.

The reality is that the climate change community is fully aware of the limitations of the models (e.g., GCMs) they use. The solution to these acknowledged limitations with these models is to continually improve them, and not abandon them in favor of so-called stochastic approaches, as the authors seem to advocate. I look at the enormous improvements that that the atmospheric science community has made in weather prediction in the last 25 years. This has come about through both improvements of both models and data. The same holds true for climate models. To deal with predictions in a changing world, where the past cannot be a sufficient guide to the future, we have no choice but to rely on models that embed cause-effect relations, i.e., models with a physical (and biological, even social) basis. The models we use in the future must increasingly factor in the capacity of human beings, vegetation and other living and non-livings to adapt to the rapid changes that arise due to environmental changes. Perhaps, these adaptations are not well represented in current models, but to abandon these because these are difficult, in favor of purely data-based stochastic approaches, does not seem to me like a viable option. As in hydrology, a diversity of models is needed, including data-based approaches.

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. I am talking about the practice of taking the outputs of GCM predictions of temperature and precipitation, combining these with hydrological models to predict outcomes for water resources and hydrological extremes (e.g., floods and droughts). Given the uncertainty in GCM projections, the uncertainty in hydrological model predictions and the fact that such models do not incorporate the likely adaptations that natural systems undergo, these studies are no more than sensitivity analyses, a mere scratching of the surface of a complex problem. There is much more we can learn about the impacts of climate change by

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comparing the structures and responses of hydrologic systems across a climatic gradient than by the use of uncertain GCM predictions with uncertain and inappropriate hydrological models.

C. New Paradigm for Hydrology

In spite of my disagreements with the authors, I do believe that hydrology does need to move on from its 20th century foundations. Hydrology did indeed start well, and people like Robert Horton and John Dalton showed remarkable success in providing the early progress, founded on observations and insightful data analysis, combined with appropriate models. Hydrologic practice too has demonstrated remarkable success in being based on data and observations and a good appreciation of both causality and probability.

However, the successes of the past 50 years are insufficient to deal with the rapid changes that are happening to the hydrologic system in the context of human-induced environmental change (e.g., land use change, climate changes). 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. Hydrology should, by necessity become broader, and increasingly inter-disciplinary. Water is connected to so many different aspects of the earth system, including humans, so much so that hydrology is no longer just the study of water alone, but must embrace everything water touches during its circulation around the earth. The foundations of this thinking have been articulated in the so-called Blue Book edited by Professor Peter Eagleson (NRC, 1991), which acknowledges the role of water as the "life blood and lymphatic fluid the earth, the planet's thermostat, resource to be managed, potential human hazard, and enabler and sustainer of civilization." This list goes beyond the role of water in energy sustainability, which the authors exclusively focus on as the foundation of their paradigm shift.

D. Summary

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The authors are entitled to their opinions. I do hope, however, that they will take note of my critical comments. Even if they do not modify their views, they should at least streamline their arguments so that their article becomes a reasonable reflection of the opinions held by a significant segment of the hydrological community.

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