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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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INTRODUCTION

Having already received several reviewers' criticisms of our style in our opinion paper (Koutsoyiannis et al., 2008a), including diagnoses of "strong opinions", "dogmatic view" and "scatter-gun approach", we welcome Koussis (2008) strong assertion that "the only opinions worth stating in a public forum are the strong ones, those that can stir up controversy". Encouraged by this and being grateful for his intelligent, clearly-articulated and positive critique, we will discuss some of his points (three major topics in the following three sections), with which we either disagree or feel that they need

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clarification, also considering his statement that "scientists are not diplomats".

CLIMATE CHANGE

Referring to climate change, Koussis (2008) states: "the great inertia of the earth-system makes me think that precautionary measures are sensible as an insurance policy for the future". Admittedly, this seems to be a reasonable (and common) argument, but we do not support it. Precautionary measures for what? For the symptom? Politicians do not want to touch the real problems of our civilization and economy. In our opinion detailed in Koutsoyiannis et al. (2008a), the real problems can be summarized, in the "trinity" Unsustainable energy use (fossil fuels) – Demographic change (overpopulation, overconsumption) – Environmental change (urbanization, deforestation, pollution). Carbon dioxide emission is just a symptom of the unsustainable energy use and economy. Those who believe that exponential growths in energy use, economy and demography can be sustainable and ultimately feasible in the long run in a planet with finite resources, and that the only problem is the excess emissions of carbon dioxide that induce climate change, may feel free to disagree with our assertion of the potential of a "real risk of severe socioeconomic crisis in the not-too-distant future" (Koutsoyiannis et al. 2008a). They may feel confident to characterise the current global economical crisis just as an unfortunate surprise and to attribute the problems to increased temperature on the planet. We prefer not to follow this thinking and we wish to reiterate our statement that "The role of science is to deal with the true cause of problems before a crisis appears, leading developments and providing society with the ability to react promptly and in an informed way" (Koutsoyiannis et al. 2008a). We understand that this may be difficult as funds are granted to research that complies with the political priorities, which in this case does not favour seeking of real causes or even diagnosing of real problems. Perhaps we demand too much from our communities (scholars, academics, researchers), who allowed an (unprecedented) subordination of their institutions to the laws of a greedy market (cf. Xanthopoulos, 2003, 2005).

Further, Koussis (2008) suggests that he does not give credence to "to press report

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of ice sheets melting everywhere." We certainly endorse this statement: Press reports present an ex parte and biased picture of events related to climate. The most characteristic example is the melting of sea ice at the polar areas. When the ice melts, that is, every summer, the press reports present dramatic stories of the melting (e.g. The Time, 2008), upon which propaganda is built (cf. the timely message "Save Santa Claus"; Greenpeace, 2008). When the ice builds up, every winter, there is silence. As a result, people, believe that ice is consistently melting in the polar areas. This includes also scientists, who live in an unprecedented weird "climate" of science mixed with political and financial interests, inaccurate and biased press reports and propaganda. The answer is in the data. For example, the truth about Antarctic sea ice extent, as seen from the data (US National Snow and Ice Data Center, 2008a), is that there is an annual cyclical fluctuation with a slightly positive overyear trend (build-up rather than melt of ice; Koutsoyiannis, 2008, p. 16). In terms of news, one has to "construct" his own news based on the data, rather than rely on press and media reports. For instance, today's news about sea ice, extracted from US National Snow and Ice Data Center (2008b, c), could be that the Antarctic sea ice extent is 22% above the 1979-2000 average, whereas the same quantity is 11% below average in the Arctic. Fortunately, the Internet has made possible the rapid availability of data and also the discussions in blogs, which provide more balanced reports than traditional media (e.g. Goddard, 2008).

On the other hand, while we too "give credence to the physics of the greenhouse effect", in our opinion this physics involves significant uncertainty and we have been presented a biased image of the contribution of carbon dioxide in climate. The mainstream hypothesis, reflected in global circulation model outputs, is that carbon dioxide drives climate (without increasing of carbon dioxide concentrations, the models produce virtually a static climate). In our (and others') opinion carbon dioxide is just one of a diversity of human and natural forcings of the climate system (e.g. Pielke 2007, 2008a, b; US National Research Council, 2005). Again, the answer is in the data rather than in invalidated hypotheses. Figs. 1 and 2 in Koutsoyiannis et al. (2008a; the lat-

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ter taken from Koutsoyiannis et al., 2008c) indicate inconsistency of the mainstream hypothesis with actual data.

DETERMINISM

Koussis (2008) correctly points out that "The ability to describe a physical phenomenon deterministically fascinates us, perhaps, because such a mechanistic thinking has dominated the education in the natural sciences of many of us". However, the validity of the continuation of his argument, "But determinism exerts a strong attraction also because it explains in a straightforward logic how nature operates, in a broad sense, and is thus invaluable", is doubtful. Indeed, determinism provides a straightforward logic, which, given the dominance of the mechanistic thinking in education, may help us understand the mechanisms of the studied phenomenon. The problem is that the mechanistic understanding may not be consistent with the way Nature operates. In this respect, determinism may result in oversimplification of the system, overconfidence to our ability to model it and, eventually, misunderstanding of the system operation. All these may be manifested in the climate change perception discussed in the previous section. Only simple systems work in a straightforward manner, which can be described in sequential cause-effect relationships with one-to-one correspondence of cause and effect. In natural systems, such as the climatic system and a hydrological system, the cause-effect relationships may be circular (due to feedbacks) and many-to-many (due to complexity). Such causality can be better described and understood in probabilistic and stochastic terms.

Koussis continues that "first-principles based deterministic description of a hydrologic process satisfies because the parameters involved have physical meaning, are thus observable and can be estimated statistically through field tests (e.g., pumping tests) or sampling (e.g., inference of streambed roughness from sieve curves of streambed material)". It would be good for the discussion if he had clarified what he means with "first principles". Does he restrict "first principles" to Newton's laws? Then he can hardly derive deterministic descriptions of hydrological systems. Could he expand the scope of the "first principles" to include at least the laws of thermodynamics? Then he must admit that he may have departed from pure determinism (entropy is a probabilistic concept). Furthermore, since he essentially identifies "observable" (parameters) with "estimated statistically", he tacitly abandons deterministic aspiration and even the notion of "first principles" as the latter normally should not need fitting based on experimental data. What he calls "parameters ... with physical meaning" we view as statistical parameters with relatively small variation, so that we could estimate with a few measurements and in some problems we could regard as constants.

While Koussis (2008) believes that "the stochastic/indeterministic paradigm advocated by [Koutsoyiannis et al., (2008a)], may create confusion", he himself proposes a "deterministic and probabilistic viewpoints mix". Even though practical needs may necessitate combining of deterministic and probabilistic tools for a certain problem, we think that a "viewpoints mix" is more likely to create confusion. We do not believe in "viewpoints mixes" and we think that a stochastic viewpoint is general and self-sufficient enough so that it does not need any mixing. Thus, in a search for a consistent framework of thinking, the stochastic paradigm is more appropriate and generic: while a "sharp" outcome can be viewed as special case of an "uncertain" outcome, in which the variation has become (or has been assumed) very small, the opposite cannot hold. Cases with extremely low variation are not uncommon. As a simple example, we reiterate the result in Koutsoyiannis et al. (2008b) in which we found that in a 40 m^3 room, the variation of the number of air molecules occupying the front 1/3 of the room is as low as 10^-14. Thus, the number of molecules is known with virtually full certainty; continuing this we could infer the air pressure (given the temperature) with virtually full certainty. But it is important to realize that these are probabilistic results and not deterministic laws. It is important to comprehend that probabilistic/stochastic models can result sometimes in almost certain results (that are not deterministic in principle but do not differ from deterministic answers in effect). Most frequently, though, stochastic models will yield uncertain results and will quantify the implied uncertainty. In all cases, a stochastic approach is adequate to faithfully model reality: to uncover and explain

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certainty when there is certainty and to quantify uncertainty when there is uncertainty.

INFINITY

It seems that Koussis (2008) expresses a common misperception of the probability theory in the hydrological community, when he asserts that "one may miss the target looking through only one eye (for example, the probability distribution of the ages of a population leads to the conclusion that we have a slight, but finite chance of living almost indefinitely)". We like very much this example, which is common in discussions among hydrologists. We wish to point out, though, that this argument on the life span of a person has been vitiated more than half a century ago by the famous mathematician Feller (1950). In his own words:

"The question then arises as to which numbers can actually represent the life span of a person. Is there a maximal age beyond which life is impossible, or is any age conceivable? We hesitate to admit that man can grow 1000 years old, and yet current actuarial practice admits no bounds to the possible duration of life. According to formulas on which modern mortality tables are based, the proportion of men surviving 1000 years is of the order of magnitude of one in 10ˆ(10ˆ36) a number with 10ˆ27 billions of zeros. This statement does not make sense from a biological or sociological point of view, but considered exclusively from a statistical standpoint it certainly does not contradict any experience. There are fewer than 10ˆ10 people born in a century. To test the contention statistically, more than 10ˆ(10ˆ35) centuries would be required, which is considerably more than 10ˆ(10ˆ34) lifetimes of the earth. Obviously, such extremely small probabilities are compatible with our notion of impossibility. Their use may appear utterly absurd, but it does no harm and is convenient in simplifying many formulas. Moreover, if we were seriously to discard the possibility of living 1000 years, we should have to accept the existence of a maximum age, and the assumption that it should be possible to live x years and impossible to live x years and two seconds is as unappealing as the idea of unlimited life."

Thus, probabilistic thinking does not imply "living almost indefinitely". Rather, provides a quantitative answer to any related question, assigning a probability value to any event. The answer is always consistent with intuition. The probability that life span exceeds any positive number x decreases toward zero as x decreases, becomes inconceivably small for very high x and becomes precisely zero for x = infinity. According to Karl Popper (1982, p. 133) probability is the extension (quantification) of the Aristotelian idea of "potentia". Furthermore, Feller's counter-argument is an implementation of the Aristotelian idea of "potential infinite" (Aristotle, Physics, 3.7, 206b16) that "exists in no other way, but ... potentially or by reduction" (and is different from mathematical complete infinite).

It is very disappointing that these Aristotelian notions in general and Feller's counter-argument in particular, have not been widely known to our hydrological community and to meteorologists, who still use the non-scientific concept of probable maximum precipitation (PMP) and flood (PMF). Amazingly, such concepts have been called "physically based" but such a physical basis has never been provided. Given the popular dichotomy of probability and statistics vs. physics, some may view as "physics" a mere opposition to probabilistic arguments. For example, according to Horton (1931; from Klemes, 2000), "It is, however, important to recognize the nature of the physical processes involved and their limitations in connection with the use of statistical methods . . . [a] Rock Creek cannot produce a Mississippi River flood any more than a barnyard fowl can lay an ostrich egg". In our opinion, adopting today such an argument reveals just an incorrect logic and perhaps ignorance of probability rather than any type of physics.

Having in mind that the PMP/PMF concepts try to put a physical (deterministic) upper limit that is feasible for precipitation (see definition in WMO, 1986) and involving elementary logic we easily understand that even the terminology is self-contradictory, and thus not scientific. Namely, the word "probable" contradicts the existence of a deterministic limit. In fact, this does not need a great deal of philosophical penetration.

The contradiction has been more practically expressed as "conceptually, we can always imagine that a few more molecules of water could fall beyond any specified limit" (Dingman, 1994, p. 141). Yet the linkage to the Aristotelian potential infinity may make us more sensitive in seeing the logical inconsistency and more confident in rejecting the PMP/PMF concepts.

Furthermore, separate examination in depth of each of the specific methods devised to determine PMP will reveal that they are all affected by additional logical inconsistencies. While they are all based on the assumption of the existence of a deterministic upper limit, they determine this limit statistically. This is obvious in the so called "statistical approach" by Hershfield (1961), who used 95 000 station-years of annual maximum daily rainfall belonging to 2645 stations, standardized each record and found the maximum over the 95 000 standardized values. Naturally, one of the 95 000 standardized values would be the greatest, but this in not a deterministic limit that could be called PMP (Koutsoyiannis, 1999). If one examined 95 000 additional measurements, one might have found an even higher value. Thus, the logical problem here is the incorrect interpretation of an observed maximum in precipitation as a physical upper limit.

The situation is perhaps even worse with the so-called moisture maximization approach of PMP estimation (WMO, 1986), which seemingly is more physically (hydrometeorologically) based than the statistical approach of Hershfield. In fact, however, it suffers twice by the incorrect interpretation that an observed maximum is a physical upper limit. It uses a record of observed dew points to determine an upper limit, which is the maximum observed value. Then it uses this "limit" for the so called "maximization" of an observed sample of storms, and asserts the largest value among them as PMP. Clearly then, this is a statistical approach and a very bad one, because (a) it does not assign any probability to the value determined and (b) it is based only on one observed value (called in statistics the highest order statistic), rather than on the whole sample, and thus it is enormously sensitive to one particular observation of the entire sample (Papalexiou and Koutsoyiannis, 2006; Koutsoyiannis, 2007).

The solution to this problem is extraordinarily simple: Abandoning of the concept of PMP and the improper statistical methods used for its determination. Proper probabilistic methods which can replace PMP do not result in logical inconsistencies. Applying a probabilistic thinking to Horton's example of Rock Creek vs. Mississippi River flood, we may see that, while the upper limit of both floods is regarded to be infinite (and this is common in the two cases), for any finite value of flood discharge the probabilities of exceedence in the two cases differ by a vast number of orders of magnitude (as in Feller's numbers above). Thus, one will never be tempted to consider any specified flood discharge value equally probable or equally improbable in the Rock Creek and in the Mississippi River.

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