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Hydrology and Earth System Sciences Discussions

Interactive comment on "Why hydrological forecasts should be evaluated using information theory" *by* S. V. Weijs et al.

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GENERAL COMMENTS

I really enjoyed reading the interesting paper by Weijs et al. (2010a). It aims to "shed some light" on the evaluation of hydrological forecasts from an information-theoretical point of view. By means of a clever decomposition of the Kullback-Leibler divergence proposed as a proper scoring rule for forecast quality (Weijs et al., 2010b), the authors first show how the use of deterministic forecasts increases uncertainty to infinity, then, they give two interpretations (information interpretation and utility interpretation) to explain and analyze the possible paradox due to the current wide use of deterministic forecasts in the society. In this context, the paper is part of the topical debate on the

uncertainty analysis in hydrological modelling (Pappenberger and Beven, 2006; Montanari et al., 2009; Koutsoyiannis, 2010). The paper ends with a section explaining why information should be preferred to utility as a model calibration objective.

I think the paper by Weijs et al. (2010a) is relevant to the topics covered by HESS, and the research presented is important and innovative for the hydrological community. However, I believe that more appropriate examples and evidence to support the topic presented in the paper should be needed, as stated (C2244: 8-10) also in the thoughtful Comment by Anonymous Referee #1 (2010), which I consider almost entirely appended to my Comment.

SPECIFIC COMMENTS

In this section I would like to ask the authors some questions and make some comments, which could be useful to clarify some points of the paper by Weijs et al. (2010a). What does the statement in (4660: 7-10) imply? Do you mean that Consistency is an inherent property of forecasts? This seems to be in contradiction with what is correctly affirmed in (4677: 20-22). In fact, as stated by Brier (1950): "(. . .) one of the greatest arguments raised against forecast verification is that forecasts which may be the "best" according to the accepted system of arbitrary scores may not be the most useful forecasts. (. . .) This may lead the forecaster to forecast something other than what he thinks will occur". Please, clarify.

In (4660: 17-19) the authors state: "In meteorology, the evaluation of quality is called verification (Latin: veritas=truthfulness). This term is somewhat misleading, because establishing that a model simulates the truth is impossible (Oreskes et al., 1994)". I thank the authors for citing that very interesting paper and I totally agree that the term verification could be misleading. But, in my view, this is only because the truth of a model cannot be demonstrated, not because "establishing that a model simulates the truth is impossible". In fact, Oreskes et al. (1994) also state: "(. . .) A model, like a novel, may resonate with nature, but it is not a "real" thing", which means that a model can

simulate (Latin: similis=similar) the truth but cannot be true. The simulation capability of a model can be established “if it is consistent with our experience of the natural world”. Otherwise, why do we have to mind about the evaluation of a single forecast using observations?

Anyway, this is not a rhetorical question. In fact, what we can find also in the paper by Oreskes et al. (1994) is that “Models can only be evaluated in relative terms, and their predictive value is always open to question. The primary value of models is heuristic”. This is a very interesting issue that the authors raised by citing that paper. Do not the authors think that a comparative evaluation of weather forecasting systems (e.g., Ehrendorfer and Murphy, 1988) should be of greater use instead of evaluating single forecasts by comparison with observations?

Finally, about the question raised by Anonymous Referee #1 (2010) in (C2246: 17-21), could the Jensen–Shannon divergence (Lin, 1991) in place of the Kullback–Leibler divergence be of use?

TECHNICAL CORRECTIONS

In the line (4659: 5), replace “(...) a decomposition we developed recently (Weijs et al., 2010[b]) (. . .)” with “(...) a decomposition recently developed by Weijs et al. (2010[b]) (. . .)” because of an improper use of “we” since the authors of the two papers (Weijs et al., 2010a, b) are different.

For the same reason, in the line (4663: 10) replace “As we showed in our recent paper (. . .)” with “As showed by Weijs et al. (2010[b])”.

In the line (4667: 12) citation is needed, the term “surprise” meaning self-information was coined by Tribus (1961).

Citation needed in the line (4674: 14).

In Table 1, the uncertainty term of the Brier Score needs a superscript T after the first vector, and the number 1, as a vector, should be in bold notation.

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