Author's response

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The author is grateful and thanks Dr Hall for his valuable comments. The author's paper is meant to be a 'discussion' paper—that is clear from its content. The discussion and exchange of opinions and experiences are essential in such context and Dr Hall's contribution to such exchange is most welcome.

The author agrees completely with Dr Hall's comment concerning terminology. Indeed, it is certainly more appropriate to call the models based on physical principles '*physics-based*' or '*mechanistic*' rather than '*deterministic*' models. Consistent use of the term *deterministic* in the paper is due to the customary jargon of practitioners. The author feels that proposed terms, i.e. '*physics-based*' or '*mechanistic*' should be used and their use recommended in scientific and technical publications as well as in professional reports. The author wishes to mention at that place that the problem of terminology is important but not easy to solve. The gap between universally meaningful and logical terminology (Abbott, 2002*a*) and everyday practitioners' jargon is very difficult to bridge in a consistent way.

Dr Hall makes several remarks in his comments referring to the author's opinion that 'mechanistic *models* should be compelling, appealing, simple and related to physics'. He also points out that the generality of such models 'will . . . always be bounded, so . . . it could be argued that there is nothing to distinguish physics-based and data-based models other than the range of their generality'. The author would like to make his idea explicit. When the author wrote 'compelling, appealing, simple and related to physics', he meant *theories* (laws that express simple physical principles, formulation of laws, equations), not *models* (alas, again a problem of terminology). Consider that up to now the researchers in the sediment movement domain were not able to produce such physical laws that would be 'compelling, appealing, simple and related to physics' and on the scale of interest to engineers. With the result that modelling of sedimentation and morphology is a shaky business with uncertain results.

As for generality, obviously, the results of physics-based models are limited to the range of validity of the physical laws (equations) on which they are built. But they *are* general in the sense that these laws will be satisfied within the full range of possible applications of a model. Thus a 1-D model of a river instantiated with a software that respects the equation of momentum conservation in unsteady flow will always conserve the momentum, whatever the range of discharges is and whatever modifications (such as dams, dykes, groynes, etc.) are introduced to the modelled system. This cannot be said about data-based models.

The author indeed 'fails to acknowledge that data driven approaches have a respectable tradition of being used to describe the long-term characteristics of weather-related phenomena'. There is a reason for that: he omitted from the scope of the paper the practice of extreme value statistics because it is not related to the problem. In the author's paper models are understood as they are defined by systems theory. A model (a transfer function, or a system) is supposed to produce an output when an input is introduced. Such a model is physics-based or correlative data-driven. It is supposed to be predictive to be useful. The author feels that Dr Hall refers in this part of his discussion to different domain. Indeed, the estimate of probability of extreme events (off-shore waves, project floods, etc.) is a very different problem from the prediction of events as a result of given inputs. It is in this context of probability estimates that Dr Hall raises the question of extrapolation beyond observed data. There is a long and respectable history of developments of the statistics theories of extreme events, from Adolphe Quetelet, through von Bortkiewicz-Corrado Gini controversy and until the Gumbel contribution that is so currently used in hydrology. Estimate of probability of occurrence of extreme events, however, has little to do with 'training' of ANN or GA or ARIMA-like correlative transfer mechanisms that are supposed to produce output responses to input forcing. In case of *models in terms of systems theory* the author feels very uneasy about the practice that consists of applying data-driven trained models to the events out of the range of training sample or when such trained models are applied to *modified* systems. The word extrapolation and its criticism has been used by the author in this context. It is one story to extrapolate beyond past-observed training data concerning *modelling system* (*or transfer function*) that remains unchanged and very different story to do it to a modified system. Whatever respectability there is attached to such meteorological models on 'long-term', it may be put in question with climate change and green-house effect. As for short-term, if there had been any respectability, it is now replaced by the notorious incapacity of prediction of weather extreme events (e.g. storm of December 1991, 2001 and 2002 flash floods in Southern France, etc.).

To present in favour of extrapolation approaches arguments such as 'Data-driven approaches may be unavoidable . . .', 'statistical methods . . . [are] . . . universally used . . .'; or 'yet there is seldom a physics-based alternative', is not convincing. The author does not anathemize such practice but tries to point out that the results may be wrong.

This having been said, the author agrees with Dr Hall that data-driven approaches may be useful and necessary in many circumstances, although he feels that the word 'unavoidable' used in the discussion is awkward because following an 'unavoidable' path may well lead to wrong results. It is of engineer's responsibility to design safe solutions. To declare *a posteriori* that he followed an 'unavoidable' path or 'good practice' is no excuse for the error. The author certainly agrees with Babovic *et al.* (2001), cited by himself in the main paper; he also agrees that there is a middle ground, common in engineering practice, between data-driven and physics-based approaches. Beven's (2002) argument, however, that there will always be sub-grid-scale processes impossible to measure and, hence, purely physics-based approach is impossible, is inappropriate. Simply because we are discussing civil engineering, hydraulics and hydrology domains and the 'grid' is defined in adopted laws of physics that describe the phenomena of interest on the scale of engineering interest concerning these domains.

At this point the author would like to stand by what he wrote about new modelling paradigm and avoidance of calibration. In his discussion Dr Hall makes a remark on what he calls 'that mysterious "engineering experience" ' used to make an estimate of parameters. While 'engineering experience' is by definition *terra incognita* for purely academic researchers, it is not that mysterious for practitioners. Consider in particular the case of Manning's *n*. Dr Hall suggests that such values of the latter that an experienced hydraulician can estimate entered his mind from calibration processes. This is not an accurate perception of the situation. Originally these values have been *measured*, *not calibrated*, precisely in order to supply engineers with estimates. See for example the collection of photographs and tables existing before anybody thought of 'calibration': indeed, they go back to 1920s–1930s (Chow 1959; Barnes 1967). These things are first taught in engineering schools and then practising engineers can later, in professional life, compare them with their own observations on the field. And, of course, when running numerical models, they compare their estimates with the results of computations. All this sums up in experience that is not that mysterious. One can compare the situation to driving of a car: the difference between a driver that has just been licensed and somebody who has been driving every day for the last ten years. There is nothing mysterious in talking about 'driving experience' of the latter, except for a holder of the licence who has never driven a car since. In our field, however, the important point is to make the distinction between what can be calibrated and what must not be calibrated. Definitely one must not calibrate arbitrary parameters that represent in reality dynamic processes at the scale of engineering interest. What can be calibrated are parameters that are well defined: Manning's *n* can (should) certainly be calibrated for a 1-D model of nearly straight fixed bed river reach without singular head losses. When boundary conditions are given and there is a gauge recording free surface elevation at a section of the reach then indeed, in such a (rare) case we have the data that specifically allows for calibration of *n*. What about, however, a 2-D model of an estuary, based on equations in which intervenes 'constant horizontal turbulent diffusivity coefficient, to be calibrated by the user' (the citation comes from a user manual of a software produced and marketed by a well known institution)? Specific data allowing for calibration of this quantity is never available. In considered modelling software there are a number of parameters that can be varied (calibrated?) with the purpose of making coincidental computed and observed values. Among others there is this diffusivity coefficient. If we 'tune' the model using only this coefficient, what will be the predictive value of the model? The same remark can be made when the constants of *k-e* turbulence model of a 3-D coastal area modelling software are concerned: here again these constants are supposed to be 'chosen and calibrated' by the user. Obviously, there is no way to obtain specific measured data that would allow for calibration of the constants that intervene in *k-e* theory and are specific to the model that is considered. Thus, while thanks to new technologies (incidentally, the author is grateful to Dr Hall for references concerning SAR observations) there are more and more possibilities to obtain the data for validation of models (in the sense of 'new paradigm') the specific data necessary for calibration of parameters are scarce. And this difference opens the way for *tuning* (one is tempted to call it *tinkering*) of some parameters that eventually may render the results of *prediction computations* false.

As the author wrote at the beginning of this response, Dr Hall's contribution is highly appreciated. It would be most interesting if his discussion could raise more contributions and more criticism from other colleagues. Engineering, hydraulics, hydrology are not highly exact sciences. The consensus among practitioners and researchers, engineers and teachers on what is correct engineering practice is extremely important and can only be achieved by open discussion and exchange of views. The *Journal of Hydroinformatics* should be thanked for making such exchanges possible.

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