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Hydrology and Earth System Sciences Discussions

Interactive comment on "HESS Opinions "The art of hydrology"[1]" by H. H. G. Savenije

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Models and Theories

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The discussion paper raises a most timely and important issue. Its importance, as we aim to argue in this comment, transcends beyond hydrology to all of the environmental science and science in general. What should be the most productive approach to un-

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derstand our environment and to solve the environmental problems that are avalanching on the planet at all spatial and temporal scales? This is not a question of setting an opposition between different professions (e.g., scientists versus engineers, as mentioned in the discussion). Rather, it is a question of finding the best way towards a common goal.

Here so far a dichotomy of bottom-up (reductionist) versus top-down (artful) modeling has been discussed. In the discussion paper the bottom-up models were characterized as "one-size-fits-all" and "physically based" "models of everywhere", i.e. models "based on upscaling physical laws" from the laboratory case with well-defined boundary conditions to a real hydrological problem, where boundary conditions differ greatly from the laboratory ones. The dichotomy was further sharpened in the comment of Murugesu Sivapalan, who referred to the bottom-up models as to models "founded on universal theories of catchment response and associated governing equations" with the remaining problem being to tie the models to reality by estimating model parameters from calibration. To these, the author of the comment opposed the "top-down" models that, according to the text, are derived solely from the data (p. S2013). Does it mean "without any physically based equations"?

Returning to the discussion paper, we can see that, rather, the top-down approach is meant by Hubert Savenije (2008) to look for "links with fundamental laws of physics" (p. 3162), that applying this approach "we try to find physical laws that describe" the emerging patterns known from empirical data (p. 3162). Doing so requires "imagination, inspiration, insight, field experience, creativity, ingenuity and skill" (p. 3166). These qualities, according to Hubert Savenije, belong primarily to the field of art. This is disputable, but we return to this issue later.

At this point it is essential to note that the naked dichotomy consists not in the priority of physical laws over data or vice versa (which would be a rather strange opposition), but in the fact that the bottom-up modeling presumes and even dictates that **all fundamental physical laws are already included into the existing models.** Somewhat

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sharpening the edges, one can say that there is no longer any need to think creatively in terms of big theories or looking for fundamental discoveries, these qualities of the hydrology student can be safely shelved for good. The only problem is the one of calibrating the existing universal models with the data. Apparently, the more data are becoming available, the easier the problem resolves. What Hubert Savenije instead proposes is, to our understanding, that observing real world and learning from it one should not be afraid of thinking creatively and involving physical (biological and ecological) mechanisms and laws that might have been missing from the "well established models" or even counteract them to replace some of the older propositions! Rather than trying to fit the real world into the Procrustean bed of once established set of mathematical equations.

Here it is useful to recall the notion of physical theory. What is the difference between a model and a theory? In brief, model seeks agreement with the data in the first place. Theory seeks agreement with the fundamental laws of nature first and then explains the data. Theories and laws of nature are not statistical correlations, but exact relations between strictly defined measurable variables (Brillouin, 1956). The agreement between theory and data improves infinitely with increasing accuracy of observations. For example, the more accurate the determinations of the distance power in the Newtonian gravity law became, the more precisely it was shown to be equal to 2.

The validity of the fundamental laws of nature and of good theories based on them has been tested on such a great amount of empirical data that it is a good theory that can tell you whether the empirical data are of good or bad quality rather than the data tell you something about the theory. For this reason, good theories can be used for making predictions, like the existence of many elementary particles was predicted in theoretical physics prior to their actual discovery. How justified is the use of models for making predictions?

During model development the priority is given to reaching a satisfactory agreement between the data and the mathematical structure of the model. On the basis of the

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available sets of data points taken from the general ensemble of all empirical evidence the modelers determine linear and non-linear correlations between the chosen measurable variables, including their temporal changes. The resulting time dependence of model variables allows one to make a forecast for the future. Such a forecast, however, is nothing but a limited extrapolation of what has been observed in the past. With changing the empirical datasets the model structure and forecasts change. With inclusion of ever growing amounts of observations the models become more and more complex, while their agreement with the available observations naturally improve. Thus, an ideal model ultimately comes as an exact and convenient, i.e. mathematically formalized, representation of all the available data. **However, to the degree the model is a model and not a theory, it lacks the predictive power. Because of the obvious fact that it cannot be expected that the calibrations made on the basis of the *known* data will remain valid in the domain of *predicted* (i.e. still unknown) data. This is a conceptual, fundamental problem with the modeling approach.** The universal laws of nature predict things. The data-specific calibrations are valid for description of the data they were derived from. (In other words, a dependence calibrated like $Y = aX$ based on the known set of data, may well become $Y = bX^3 + c$ in the unstudied set of data.)

Remarkably, the few well established laws of nature (laws of energy and matter conservation, momentum and angular momentum conservation, second law of thermodynamics, Newton's laws of motion, ideal gas equation) represent a very modest minority among the thousands of calibrated relationships that are incorporated into modern complex computer-powered models. Logically, therefore, even if the modelers did not know those laws of nature at all, they could have easily replaced the corresponding equations with but a few additional calibrations. We thus come to the conclusion that models do not logically need to use the laws of nature.

Based on our own scientific expertise, we can illustrate the above points with specific examples of models that were judged to be most successful based on their agreement with the data and claimed derivability from a "universal" theory, yet shown to confront the fundamental laws of nature. As one can see, the problem transcends across the natural science as a whole. The biological model of organismal growth (West et al., 2001) misinterpreted the energy conservation equation and replaced it with the one conflicting with the energy conservation law. Despite that, the model showed perfect agreement with the data. After the error was identified (Makarieva et al., 2004) it took the model's authors four years to explicitly admit it (Moses et al., 2008) and re-formulate the model. The re-formulated model re-calibrated using the same data as the original (wrong) one showed equally good agreement with the data and got equally well published (Hou et al., 2008). Thus, irrespective of conflicting with the energy conservation law or not, the model agreed with the data, was widely cited and raised little concern in the reading audience. Another example is the recently criticized model of hurricanes as Carnot cycle (Emanuel, 2003), which was argued to be based on the concept equivalent to perpetual motion machine of the second kind (Makarieva et al., 2008). The model also showed perfect agreement with the observed hurricane velocities and persisted unchallenged in the meteorological literature for over a decade. This leaves one to ponder seriously over the methodological problem of whether modern models need to be based on physical laws at all, to perform perfectly as long as the agreement with the data is concerned?

Science of atmospheric circulation is physically based on the Eulerian equations formulated together with the ideal gas equation in terms of the fundamental measurable environmental variables like mass or molar density, pressure and temperature. Eulerian equations can be used in a quantitative study of atmospheric circulation if one knows the physical nature and quantitative features of the observed pressure gradients that bring about motions of air masses.

It has been postulated in meteorology that the physical cause of the observed pressure gradients is the horizontal temperature gradient (differential heating). This paradigm rules in meteorology practically unquestioned for over three hundred years starting

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from the famous paper of Hadley (1735). At the same time, the most powerful processes of atmospheric circulation - hurricanes - develop over nearly isothermal surfaces. The attempts to explain monsoon-type ocean-to-land circulation by seasonal changes in the differential land/ocean heating logically contradict the observed existence of ocean-bordering deserts (like Sahara), where the horizontal temperature gradients are of the same sign and larger magnitude, yet do not bring about ocean-to-land air flow. Hadley circulation is modeled on the basis of differential heating by placing the unrealistic cap at the top of the atmosphere to set the otherwise unknown boundary conditions. Even in this case it does not appear possible to reach a satisfactory agreement with the data and explain the circulation intensity (Fang and Tung, 1999). If one discards the non-existent cap, one has to conclude that due to the larger atmospheric height scale over the warmer surface the temperature-gradient based circulation should have predominantly developed in the upper atmosphere with a rapid decline of wind speeds towards the surface due to increasing turbulent resistance. This is not supported by observations either.

To summarize, for a demanding student of atmospheric science there seem to be quite a few grounds to "challenge assumptions" and "view differently", as Geoff Pegram proposes (S2193). Instead, meteorology has been developing in the direction of enriching the empirical databases with use of ever progressing technological facilities and increasing the spatial and temporal resolution of computer models based, as discussed above, on the once (and for all?) established set of mathematical equations. The nature of the driving force of atmospheric circulation perceived as unclear and enigmatic by Lorentz (1967) has not been resolved or even partially elucidated during the following decades of climate modeling. In meteorology, models seem to have successfully outcompeted theory.

In our recent studies (Makarieva and Gorshkov, 2007; Makarieva et al., 2008) we proposed a physical mechanism of the evaporative-condensational force that can quantitatively explain all major phenomena of atmospheric circulation, including hurricanes and tornadoes, as their driving force. This force is based on the phase transition of water vapor; it is directly relevant for hydrology which, therefore, acquires a dominant role in the explanation of atmospheric circulation. The physics of the evaporative-condensational force can be interpreted and understood as a peculiar case of osmosis. In the atmosphere, the role of semipermeable membrane of a unique nature is played by the vertical temperature gradient - it selectively removes, via condensation, one of the gases from the mixture (water vapor) and does not allow it to propagate to the upper colder atmosphere in quantities sufficient for the restoration of gravitational equilibrium of water vapor in the gravitational field. At the same time, lacking material essence, this unusual "membrane", unlike the conventional osmotic membrane, is penetrable to the dynamic flow of mixture as a whole, sustaining continuous air circulation. We wish to hope that these results will be discussed by the scientific community from the standpoint of creative constructivism and further developed to yield insights into important climate problems.

Returning to the qualities attributed by Hubert Savenije to art, namely "imagination, inspiration, insight, field experience, creativity, ingenuity and skill", we would say that these are qualities that every scientist must possess (with field experience understood broadly as acquaintance with the empirical data). The fact that these qualities are today attributable to art (and not to science!) when discussing the trinity science-technology-art, is just another manifestation that they have been largely lost from the modern scientific enterprise. The discussion paper actually invites us to regain these qualities, to fearlessly challenge the cherished dogmas and not be afraid of free thinking. We add our voices to this call.

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