KEEPING TECHNIQUES, METHODS AND MODELS IN PERSPECTIVE

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Introduction

Recently I gave a lecture in Prague on the pitfalls of contemporary frequency analysis of hydrological extremes. In the ensuing discussion, one participant asked me in a rather reproachful way why I was now criticizing the same statistical and probabilistic methods whose use in storage design I had pioneered in Czechoslovakia some forty years ago. I replied that it was because, in those days, I was doing engineering while today I was lecturing on hydrology. The audience took my reply as a tongue-in-cheek evasive manoeuvre, had a polite laugh and, apparently not wanting to embarrass the guest lecturer, did not pursue the matter further. I thought it was a pity because my reply was entirely serious and the response suggested to me that the audience did not appreciate the significance of the distinction I was alluding to. The occasion reminded me once again how deep, universal and persistent the problem has been, how it has transcended continents, cultures, political regimes, generations, and how the mathematization and computerization of technical and scientific work has reinforced it just when it seemed that - at least in the context of water resources engineering (or, more generally, water management) and hydrology - it had a good chance to be clearly recognized and relegated to history.

The problem I am talking about is a lack of clarity about the dual nature of a mathematical model which, depending on its purpose and use, can be either descriptive (synthetic) or investigative (analytical). The outward appearance of a model, its form, may often be similar or identical, and the difference which assigns it to one of the two categories usually resides in the way it has been constructed. Investigative models reflect some essential features of the physical/causal theory of the phenomenon being modelled and can be labelled 'theoretical' or 'scientific'. Descriptive models, on the other hand, are most often empirical in nature, or may be constructed as a simplified synthesis of the results of an analytical model, usually if the latter is too difficult to use in applications.

It follows that the form of a model, the type of a method, the algebraic structure of an equation, etc., does not have any absolute intrinsic value, is neither 'good' nor 'bad' apriori - this can only be judged from the perspective of its deeper nature on one hand, and its intended use on the other. This is well understood in established sciences like fluid mechanics and hydraulics where it is quite clear, for example, that an empirically constructed rating curve for a specific spillway may be adequately represented by a best-fit smooth curve within the range of the actual measurements but one could not use it with confidence for extrapolation or for any generalizations - for this, one would have to resort to a curve which, in addition to fitting the measurements, is based on the theory of fluid mechanics describing the particular configuration.

In the relatively young disciplines like hydrology and water resources management, the question is much less clear for a number of reasons. The main, in my opinion, is the fact that the two disciplines have common roots in 'hydraulic engineering' and have not yet clearly defined the demarcation line between their respective roles. Some of the other factors are, for example: The two disciplines are usually within

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J Water Resources Planning & Management ASCE one administrative unit in most organizations and the professional staff often switch from one discipline to the other, sometimes even without realizing it themselves; these professionals come from a large variety of academic backgrounds since universities often do not provide clearly structured specialized programs in either hydrology or water resource management - hence they enter either discipline without a genuine 'feel' for it. These issues have been discussed in detail elsewhere (Klemeš, 1982, 1986, 1988a,b, 1994, 1997).

The object of this article is to discuss the issue of models from the point of view of the business they are supposed to serve. In regard to the disciplines of hydrology and water resources management, the awareness of the distinction started to crystallize in the 1930s-50s, the issue seemed to be all but clear and settled by about 1975, but then a reverse trend set in and the waters started to get murky again, not so much because of ignorance but rather, it seems, because of calculated cleverness: for murky waters can hide many a profitable enterprise that feeds on the confusion and could not flourish in a transparent environment. The unfortunate thing is that there are many innocent victims of this state of affairs: the unsuspecting engineers, planners, hydrologists and other professionals looking in good faith to 'experts' for guidance and in no position to challenge the increasingly convoluted schemes, often devoid of substance, which they are being offered because "It seems clear that some models are pressed because professors are promoted and consultants are consulted in proportion to their generation of sophisticated, mathematically oriented models" (Fiering, 1976).

What only seemed to be the case two decades ago, has now become a well established reality. A colleague who faces this reality in his everyday work has matter-of-factly noted in his recent letter: "There are too many models now with nice-looking interfaces but no contents whatsoever. The planners believe that these will solve all their problems" (since he is on payroll of these planners, it may be in his best interest if I preserve his anonymity).

While most of what follows applies to modelling in general, the emphasis here is on statistical and probabilistic models.

The key issue: To know what your business is.

However trivial this statement may appear, the fact remains that it often is far from clear what the business of a particular business is, especially in businesses like hydrology or water management. For example, as recently as 1987, a Canadian information brochure on hydrology insisted that the business of hydrologists is to 'manage water' and explained how they do it in a chapter entitled "How Do Hydrologists Manage Water?" (ACH, 1987; cited from Klemeš, 1988a). While I had been aware of the problem in these two disciplines, I was rather surprised when I learned that - significantly in the present context - it existed (exists?) even in statistics itself. In a paper with a catchy title "Probability, statistics and the knowledge business" authored by an eminent American statistician we read:

> "How does a *consulting statistician work? Does he say to the scientist:* You must do this. You must collect data in this way. You must analyze the data in this way. And so on. You must make a t-test ... The answers are: Of course not! How idiotic can you be? And yet, the great bulk of workers on foundations of statistical inference seem to think in terms of such answers ... I have met scientists who have been brainwashed by statisticians to the view that their problems amount to the calculation of a linear discriminant and the scientists want to know how to do this" (Kempthorne, 1971; cited from Klemeš, 1994).

However, blessed are the scientists who can blame statisticians for this kind of confusion about their business! - Hydrologists and water resource professionals have no such luck and must blame their own kin for exactly the type of brainwashing Kempthorne was talking about. For, to paraphrase him, I have met hydrologists brainwashed by other hydrologists to the view that their problems amount, say, to the calculation of Linear moments; and water managers brainwashed by other water managers to the view that their problems amount to mastering the linear or quadratic or other programming technique; etc., etc. And, most important, I am not talking now about a 30-year old history addressed in Kempthorne's paper but about the present practice - I am reporting 'live from location', so to speak!

In view of this, a scheme shown in Fig.1 may be useful in the further discussion. It attempts to bring out the fact that unless one has a clear idea about his own business, it may be difficult to recognize what the business of others is, be clear about his own priorities, responsibilities and opportunities, avoid duplication, unnecessary frictions, appreciate the mutual dependencies and need for collaboration, as well as many other important issues - including the object of our main present interest, i. e. the difference in the usefulness and legitimacy of the two types of models mentioned earlier in the context of "My business" on one hand and the "Others' business" on the other.

It should be emphasized that the scheme in Fig.1 has been simplified in the extreme in order not to detract from the basic logic of the 'distribution of labour' and specialization needed for deeper understanding and advancement in specific fields of human endeavour as well as in their integrated effects on nature and society. A slightly more detailed picture, including the main feedbacks, for Water Resources Management in the position of "My business" is shown in Fig.2 adapted from an original prepared to help focus the agenda for a Water Resources Management Group formed at the University of Toronto in February 1971, of which I served as frustrated chairman until my resignation a year later.

Against the background of the preceding discussion, the logic of my answer to the Prague question may perhaps be seen in a sharper focus:

When I entered the engineering profession in the mid-1950s, the concept of probability distributions of hydrologic variables was already a standard feature of hydrology textbooks. I was neither qualified to question its legitimacy, whether methodological or substantive, nor was it my business to do so. My task was not to contribute to the science of hydrology but to use its results to make a contribution in my own business which was the planning and design of dams and storage reservoirs. The probability distribution of inflows was just a 'hydrological input', one of many different inputs I as an engineer had to rely upon. And once accepted as such, a transformation of this input by a reservoir was, conceptually at least, a rather straightforward matter. While such a transformation was not yet a standard practice, I thought it produced insights enhancing reservoir design (e.g., quantification of the reliability of reservoir yield) and promoted it vigorously - this I thought *was* my business as an engineer.

A reinforcing argument was the fact that the final products of 'my business' (e.g., numerical values of various reliability measures) were, because of the integrating effects of reservoir storage, quite forgiving to differences in the (tails of) probabilistic models of the inflows, as the 'theory of storage' predicted (Moran, 1959); for example, the 'error' in annual reliability due to a 'wrong' distribution model seldom exceeded 2% within the usual range of skewness of annual flows (Klemeš, 1971), and the 'error' due to a 'wrong' persistence structure was in the same range (Svanidze, 1964; Klemeš et al., 1981). Moreover, the differences between various models fitted to the data, small as they were in the body of a distribution, were largely lost in the coarse discretization which was satisfactory to yield stable results (Klemeš & Jones, 1969).

When, on the other hand, I am engaged in the business of hydrology - e.g. when investigating the nature of the probability distribution of a particular hydrological entity - then it is not only legitimate but imperative that I look into the ways its probabilistic model is structured, into the question whether a given model can be regarded as theoretical (explanatory) or merely empirical (descriptive), that I understand that a method adequate for the construction of an empirical model may be inappropriate for the development of a theoretical model, etc.

It follows that I attach little meaning to learned arguments about 'best models' and 'best methods' to fit models, be they models of distributions or of stochastic structures of hydrologic phenomena, unless it is first clearly stated what a given model is for. In the context of the 'business of hydrology', my criticism has always been directed to the common misconception that vigorous mathematical polishing of a purely empirical model somehow transforms it into a theoretical one capable of contributing to the science of hydrology. As explained earlier, 'theoretical' here means based in some fundamental and irreducible way on the dynamics of hydrological processes - not, say, on computer theory, decision theory, extreme-value theory, distribution theory, number theory and the like. In other words, if no knowledge of hydrology is required for the construction of, say, a given distribution model of annual flow totals or flow maxima, if all the know-how needed is some method from page 78 or 87 of a statistical manual, then the model is not 'theoretical' in the hydrological sense, regardless of the status of that method in the context of other theories such as listed above. Such a model may still be useful for applications in somebody else's business but not for contributing to the science of hydrology which is the hydrologist's business.

Recently, I reproduced from Feller (Klemeš, 1997) a graphic example of the usefulness in 'somebody else's business' of an empirical distribution model known to be theoretically wrong in the parent discipline - anthropology. The distribution being modelled was that of human life expectancies, it was fitted with a model with no upper bound, and this intrinsically wrong model has been successfully used in the actuarial practice for decades despite the fact that, say, for a life span of 1000 years it yields a rather meaningless probability of 'one in a number with 10^{27}(not 1027 as misprinted in the cited paper) billions of zeros'.

A few historical snapshots.

To my knowledge, the problem of 'empirical-versus-theoretical' models was first clearly stated by the incomparable Robert Horton in 1931 whose argument I have summarized in Klemeš (1997).

Just recently, I have come across records of a vigorous discussion on the topic in the USSR in the period 1951-52, initiated by A.D. Savarenskii (Savarenskii, 1952). A hard-line Stalinist that he was, Savarenskii conceived the discussion as a highly politicized campaign promoting 'progressive' *genetic methods* (theoretical models in the language of this essay) 'based on dialectical materialism' against 'reactionary' over-reliance on *formalistic-statistical methods* (empirical models). Since, despite his political zeal, Savarenskii was a competent scientist nevertheless, he inevitably made a number of pertinent points in the process (e.g., that, in view of the changes in land use and climate, hydrological processes cannot be regarded as stationary). However, as I now have reasons to believe, he politicized the issue not out of ignorance but deliberately in order to have a pretext for denouncing S.N. Kritskii and M.F. Menkel of whom he was extremely jealous because of the popularity in the Soviet 'applied hydrology' school of their 3-parameter power-gamma distribution model for empirical distribution fitting, and of their graphical reinterpretation of his own matrix storage model of 1940 (equivalent to the Moran model of 1954) which - in the absence of efficient computing techniques at the time - did not catch on in its original numerical form. It may have been just the timing of the death of Joseph Stalin in 1953 that saved Kritskii and Menkel from falling into disgrace or worse.

On this continent, the confusion about the difference between the 'business of hydrology' and the use of its products in engineering crept, for instance, into the 1965 ASCE Committee on Surface-Water Hydrology definition of stochastic hydrology as "the manipulation of statistical characteristics of hydrologic variables to solve hydrologic problems on the basis of the stochastic properties of the variables" (cited from Klemeš, 1978). Obviously, what was meant were *empirical* stochastic models - but these were meant not to *solve* 'hydrologic problems' but to *describe* some observed properties of hydrologic data for use in solving engineering or other applied problems. This was made clear by Fiering (1966) who introduced the term *synthetic hydrology* for empirical stochastic hydrologic models, "in an effort to remind the user that ***hydrologic sequences generated by recursive models, of whatever sort, are meaningless unless transformed into some metric and then ranked to aid and abet in the exercise of a decision***" (emphasis added).

Another attempt to draw attention to the distinction between the business of hydrology ('scientific truth') and that of engineering ('concepts of expediency') was made in a memorable paper by Yevjevich (1968): "There is a substantial difference between the needs for practical and expedient engineering concepts of safety and the scientific truth. They do not necessarily coincide. ... Hydrology has developed slowly because it has been considered an appendage of hydraulic engineering rather than a natural science." The latter, of course, was the consequence of the fact that, in the absence of hydrology as a full-fledged science, engineers themselves had to supply the hydrological inputs they needed and they did so by constructing simple empirical models from the data - after all, it was not their business to do 'scientific hydrology'. The slow progress Yevjevich was referring to was then exacerbated by the fact that these engineers, while keeping to their 'expedient' techniques, started to think of themselves as 'hydrologists' engaged in the science of hydrology, merely because they were embellishing their techniques and empirical models with increasing doses of mathematical polish.

I myself entered the discussion on the instigation of the late Professor P.A.P. Moran, the eminent Australian statistician and probabilist, with whom I had discussed the lack of appreciation in hydrology and water resources engineering of the distinction between theoretical and empirical probabilistic models. Recognizing the seriousness of the problem, he asked me to prepare a paper about it for the 1971 upcoming symposium on statistical hydrology in Tucson, Arizona of which he was co-chairman. I complied and illustrated with several specific examples the difference formulated in the introductory statement of my paper as follows (Klemeš, 1971):

> "Pure hydrology is concerned with hydrological processes as such, should strive for explanations of how things happen and why they behave as they do, and its methods should be independent of any eventual practical use of the acquired knowledge. In applied hydrology, on the other hand, the major concern should be to know to what extent our findings about hydrological processes are relevant to the practical decision-making process in water resource management, to what extent a more precise knowledge can make the decisions more rational, the results more predictable, and the means of achieving them more economical. Logical as this concept seems to be, it is far from being implemented in hydrology in general and in statistical hydrology in particular".

However, the sentiment illustrated by these few examples was already gaining ground and the early 1970s saw a burst of activity in the implementation of the above 'logical concept' the slowness of which I was complaining about.

Thus Fiering's clear formulation of the utility in the 'water management business' of (descriptive empirical) stochastic hydrologic models was brought to life by a number of ground breaking papers. For example, one (Slack et al., 1975) demonstrated that, if the 'business' is a water management task of 'minimization of expected opportunity design loss' for some flood protection measure, then "nothing is gained in reducing [it] if the underlying distribution ... is defined over and above simply using the normal as the assumed distribution", despite the fact that the latter definitely is hydrologically (Savarenskii might have said 'genetically') wrong. Other studies (e.g., Su and Deininger, 1974; Jettmar and Young, 1975; cited from Klemeš, 1977) showed, for example, that optimum reservoir operation (operating rules) is rather insensitive to the stochastic structure of the inflow model.

In short, by the time the decade was half over, the fact seemed to be clearly established that mathematical sophistication and polish, even the hydrological soundness itself, of statistical and stochastic models had no absolute intrinsic value for the 'water management business' and mattered only insofar as "different choices lead to different designs" (Slack et al., 1975). It also has become obvious that the effect of the differences in 'input models' mattered progressively less with the extent of averaging (integration), whether physical (by reservoir storage, say) or computational (in the calculated 'expected' values of economic parameters), to which the models were subjected (Klemeš, 1977).

Parallel to these developments in the understanding of the role of probabilistic hydrologic models in the business of water management were attempts to move from empirical to theoretical probabilistic models in the science of hydrology itself ('pure' hydrology, in the language of the times) where their hydrological soundness did have an intrinsic value since it aided the 'knowledge business' of hydrology. These included Yevjevich's explanation of the higher persistence in annual runoff series as compared to that in the series of annual precipitation, Kalinin's hydrological rationalization of the tendency of annual runoff to be gamma-distributed, Eagleson's pioneering 'dynamic approach' to flood frequencies, my own work on possible hydrological causes of negative skewness of some flow distributions and of the 'Hurst phenomenon', and some other studies. This early work I have summarized in Klemeš (1978). To emphasize that the issue here is fundamentally different from mere 'manipulation of statistical characteristics of hydrologic variables' which underlies empirical models, I called it 'physically based stochastic hydrology'. At the time, I deliberately avoided the labels 'empirical' and 'theoretical' stochastic (probabilistic) models, using instead the terms 'operational' and 'physically based'. The reason was that, in the context of probabilistic hydrologic models, the term 'theoretical' was used whenever the theory of probability or statistics was used in their construction, even when, from the point of hydrology, the model was purely empirical and involved no hydrological theory whatsoever; my present usage betrays my (cautious) belief that this danger is no longer too large.

While the work on theoretical probabilistic hydrologic models has not lost the perspective of what its business is and has been bearing fruit ever since (e.g., Muzik and Beersing, 1989; Kavvas and Govindaraju, 1991; Kavvas and Karakas, 1996), the work on empirical models seems to have lost the sense of direction outlined by Fiering and drifted into a new business of modelling for the sake of modelling. This development was undoubtedly stimulated by computer technology which has made 'manipulation of statistical characteristics' easy thus often substituting mechanical computations for thoughtful analysis of the purpose of the exercise. This danger was again first noticed by Fiering who, as early as 1976, offered a vision of the rather dismal state of affairs which has reached fruition in this decade. I have long been fascinated with his insight and quoted it on several occasions, last time in Klemeš (1997); however, it is worth quoting once more before the century is over:

"Fascination with automatic computation has encouraged a new set of mathematical formalisms simply because they now can be computed; we have not often enough asked ourselves whether they ought to be computed or whether they make any difference ... we build models to serve models to serve models to serve models, and with all the computation, accumulated truncation, roundoff, sloppy thinking, and sources of intellectual slippage, there is some question as to how reliable are the final results" (Fiering, 1976).

Indeed, while new models and methods are continuously cropping up, advertised as ever more powerful tools for water planning and management, they are seldom subjected to the scrutiny of being "transformed into some metric and ranked to aid and abet in the exercise of a decision" and analyzed "whether they make any difference". More typically, a statistical or probabilistic model or technique is entirely cut off from any specific hydrological or water management business and is massaged mathematically in the vain attempt to make it a 'better' model.

The truth is that this new type of model can serve neither hydrology in aiding its 'knowledge business' nor water management in any of its diverse specific tasks. Model building has developed into a 'modelling business' of its own, reversing the role of modelling from serving a given discipline to achieve a specific objective, to an enterprise to which various disciplines themselves serve merely as an excuse for engaging in the activity of modelling; cynic may say that at least in one area of human endeavour the goal of 'sustainable development' has been achieved.

I was brought up to date in these matters at a European Geophysical Society meeting a few years ago when, asking one author about the purpose of her model and how it was related to the dynamics of the process being modelled (precipitation, in the given case), she replied, obviously surprised by my lack of comprehension of the basics: "??? But this is the scientists' business - we are *modellers*!" I took this disarming clarity about the nature of one's business with a grain of salt, I must admit.

I realize that if the question put to me in Prague came from a modeller of such persuasion my reply must have seemed genuinely puzzling and irrelevant - and so, for that matter, may seem this article to those whose perspective coincides with the above example.

Postscript

Myron Fiering once sent me a preprint of a joint paper he had written with his colleague Peter Rogers, with a note saying, among other: "Peter and I are continuing to push on our campaign to re-humanize our brand of engineering, but in effect we are climbing on the bandwagon which you initiated some years ago ... acceptance by reviewers has been less than overwhelming because we take a characteristically unpopular position about the directions in which our profession has been heading ... Nonetheless I think you would like it, so I enclose a copy for your amusement" (M. B Fiering, personal communication, October 9, 1985). Yes, I did like the paper; but it was difficult then, as it is now, to be amused seeing one's profession drift into irrelevance.

Acknowledgement

This paper is dedicated to the memory of the late Myron B Fiering whose writings have been a source of inspiration for over 30 years.

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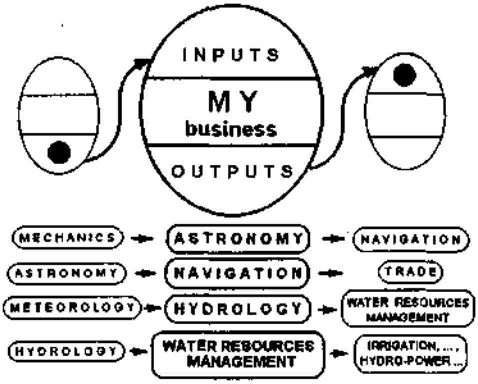

Fig. 1: A schematic illustration of typical information flow between different areas of human activity.

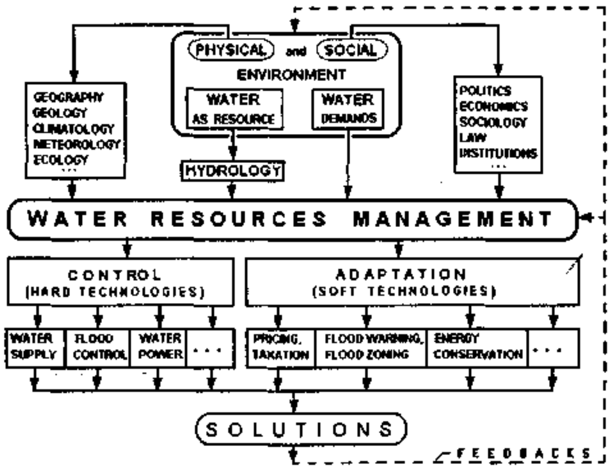

Fig. 2: Schematic relationship between Water Resources Management and its inputs and outputs.