WATER RESOURCES RESEARCH, VOL. 22, NO. 9, PAGES 177S–188S, AUGUST 1986

Dilettantism in Hydrology: Transition or Destiny?

V. KLEMEŠ

National Hydrology Research Institute, Environment Canada, Ottawa, Ontario

The unsatisfactory state of hydrology is, in the final analysis, the result of the dichotomy between the theoretical recognition of hydrology as a science in its own right and the practical impossibility of studying it as a primary discipline but only as an appendage of hydraulic engineering, geography, geology, etc. As a consequence, the perspectives of hydrologists tend to be heavily biased in the direction of their nonhydrologic primary disciplines and their hydrologic backgrounds have wide gaps which breed a large variety of misconceptions. This state of affairs often paralyzes hydrologists' ability to differentiate between hydrology and water management, hydrology and statistics, facts and assumptions, science and convenience, etc., with consequent dangers both to scientific development of hydrology and to its practical utility. The danger increases with the proliferation of computerized "hydrologic" models whose cheaply arranged ability to fit data is presented as proof of their soundness and as a justification for using them for user-attractive but hydrologically indefensible extrapolations. These points are illustrated, among other things, by discussion of flood frequency analysis. The paper concludes with some thoughts concerning minimum standards for the testing of hydrologic simulation models that would ensure at least a modest level of credibility, and with a few suggestions for ingredients of a long-term cure that can prevent hydrology from joining alchemy and astrology in the annals of dilettantism.

INTRODUCTION

Almost two decades ago, a stimulating paper by *Yevjevich* [1968] appeared in this journal. It dealt with misconceptions common in contemporary hydrology and with their consequences. The identification of misconceptions did not make them vanish, nor did it prevent emergence of others, nor did the revelation of their consequences have a strong retarding effect on their proliferation. Fighting them has been as difficult and frustrating as combating the legendary Hydra: as soon as one of its heads is struck off, two shoot up in its place. Effectiveness in this struggle seems to require a more general approach which perhaps may be called ecological. We should first try to understand the causes, roots, and conditions favorable to the spreading of the pest before deciding what measures to adopt to control it. In this, hydrologic misconceptions may be similar to mosquitoes; it may be necessary to drain the swamps rather than merely continue killing individuals. To carry the analogy one step further, this may require some heavy and expensive equipment where traditionally a fly swatter has been the standard tool.

In this paper, some of these issues are discussed from the point of view of an hydraulic engineer who, a quarter of a century ago, turned to hydrology not to question its concepts but to learn them in good faith. Thus it can be said that the views offered here are based on about 25 years of hydrologic observations. Consequently, they are more relevant in regard to central tendency than to occurrences of rare events.

WILL THE HYDROLOGISTS PLEASE STAND UP AND BE COUNTED!

Few of those who consider themselves hydrologists or whose occupation has been so designated, or even those who have taught hydrology at the highest academic levels, would qualify for such designation under conditions that are routinely required for qualification as a chemist, mathematician, biologist, economist, geologist, etc. One generally cannot make hydrology the main focus of undergraduate academic training, one cannot leave a university as a hydrologist. This is not a new discovery. It was pointed out, for example, a decade ago by *Dumitrescu and Němec* [1974] as one of critical problems concerning the future of hydrology as a science. By their academic background, hydrologists are foresters, geographers, electrical engineers, geologists, system analysts, physicists, mathematicians, botanists, and most often civil engineers. With the first degrees to our credit, many of us regarded hydrology just as one of those rather dull subjects which one had to take and was glad to let disappear into oblivion; for many this process was then already successfully completed in "junior high" with the ubiquitous sketch of the hydrologic cycle in the geography textbook; and certainly the vast majority of us did not at those important crossroads in our lives entertain hydrology as a strong candidate for a career. Many if not most have become hydrologists by default, i.e., as a result of a current job market situation, an opportunity to obtain research support while working toward a graduate degree, a repeated necessity to consider hydrologic aspects of engineering projects, being stuck with teaching a course on it, etc. In the process, one gradually "developed an interest in it," started to be identified as a hydrologist, and, most unfortunately, began to believe that he really was one. The quotation marks above do not indicate a doubt in the interest itself; rather they are meant to make one pause and consider carefully how much of the interest is really in the science of hydrology, in learning how it works, and how much of it is an interest in elaborating some pet concept from one's primary discipline which seems capable of performing a hydrologic trick.

Despite the loud protests that such a suggestion might provoke and despite the sincerity with which they might be raised, at heart most hydrologists are not hydrologists at all but engineers, geographers, geologists, etc. This becomes clearly apparent when one tries to bring hydrologists of different backgrounds together, for example, in a university environment by proposing the formation of a Hydrology Unit, Department, etc. As a rule the response is lukewarm even among the most dedicated "hydrologists." One will argue that he would miss "the stimulating interdisciplinary environment of the Geography Department," another would miss "the stimu-

Paper number 5W0750.
0043-1397/86/005W-0750$05.00

lating problem-oriented atmosphere of the Civil Engineering Department," the third would miss "the stimulating intellectual climate of the Geology Department," etc., as the case may be. To put it in plain words, the first cannot be stimulated by hydrology extending beyond rainfall-runoff correlations, the second beyond flood routing, and the third beyond a leaky aquifer. As for hydrology as a whole, we are dilettantes who "toy with the subject or study it lightly" [*Sykes*, 1978].

This is not entirely our fault but to a large extent merely a result of the present stage in the development of hydrology. Hydrology still is in a state of transition from being recognized to being a science in its own right. However, this transition is already becoming rather long considering, for example, the fact that the International Association of Scientific Hydrology (IASH) was founded more than 60 years ago. If nothing is done soon there is a danger that the transition will be petrified and the dilettantism that necessarily accompanies it will become the norm. Signs of this tendency can already be seen in rather schizophrenic attitudes often displayed by hydrologists; on the one hand, they recite the dictionary definition of hydrology as an independent science and, on the other, try to defend the status quo of its dependence by claiming that it is "an integral part" of their own primary discipline. Of course, hydrology is an integral part of geography, of water resource management, forestry, etc., but only as one of their building blocks, a component of their scientific basis, but not as a part of their main mission and emphasis [*Klemeš*, 1982*a*].

Hydrologists do not seem to be able to break free from the grip of their primary disciplines (on the contrary, they consider this grip a virtue), and to see hydrology as their main focus and object of study. Neither is this a new observation. For example, the perception (by hydraulic engineers) of hydrology as an appendage to hydraulics and hydraulic engineering was the last item in *Yevjevich*'s [1968] list of misconceptions in hydrology. However, this is more than a misconception in hydrology; it is a misconception about hydrology, one of its distorted images which are at the root of the problem and which themselves are the result of the fact that hydrology as a distinct natural science so far exists only as a dictionary defition.

In hydraulic engineering (or, more recently, in water resources engineering into which the former has been gradually transformed) the proprietary attitude towards hydrology has always been strongest. This is understandable, since it was the practical needs of hydraulic engineering which brought hydrology into existence. However, in being born from practical needs, hydrology is not unique. Probably every specific science has its roots in some practical human need and activity resulting from it. Even the purest of the sciences, mathematics, has developed from the need for counting in trade and commerce. Perhaps a few thousands years ago, somebody suggesting that mathematics should be treated as an independent science would have been accused of trying to drive a wedge betwen mathematicians and merchants, in the same way as I was accused by a respected colleague at a recent meeting of the American Geophysical Union of "always trying to drive a wedge between hydrologists and engineers." I would rather call it trying to cut the umbilical cord between them, which I see as inevitable and eventually beneficial to both. For example, one can speculate whether today we would know about the vast reserves of oil in the continental shelf and of large deposits of coal hidden from the prospector's eyes if a "wedge" had not been driven between geology and mining and geologists were kept away from such esoteric pursuits as stratigraphy when there was urgent need to improve the efficiency of mining and prospecting in order to get more coal, the lifeblood of early industrial development, to markets.

However, the greatest obstacle to progress in hydrology is not the varied nonhydrologic background of hydrologists per se, despite the fact that it narrows their respective windows on hydrology. After all, these windows collectively could eventually be coalesced to offer a relatively uninterrupted view of the whole field. The most negative aspect of the situation is this: the disciplinary specialists are so attached to their respective windows that, in trying to obtain the whole picture, they don't move to the other windows but rather try to reconstruct even the most remote scenes from the distorted perspective in which they see them from their own vantage points.

Typically, hydrologists with a hydraulic-engineering background, conditioned to dealing with water flowing over spillways, through pipes, conduits, and river channels, or seeping through dikes and under foundations of dams, tend to see a river basin, and, indeed, the whole hydrologic cycle, as one big hydraulic machine where all the water is driven by the forces of gravity and friction. Whatever does not fit into this framework is treated in a cavalier manner as "losses" or "errors," is settled by "assumptions" usually involving surrogate hydraulic mechanisms which seem to give "reasonable" results. *Hall* [1971] has pointed out that about 80% of hydrologic activity in the basin, the evapotranspiration, which is driven largely by radiation energy, is treated in one or two percent of a typical hydrology textbook and the remaining 98–99% is devoted to the 20% of the activity governed by gravity and friction. What else can one expect when at least 90% of hydrologic textbooks have been written by hydraulic engineers for hydraulic engineers, and about a discipline considered to be, to use Yevjevich's words, an appendage to hydraulic engineering?

By analogy, hydrologists looking through the wide-angle optics of the geography window are attracted by the broad outlines captured by multiple regressions, while those gathered around the systems skylight from which they can see nothing at all have no choice but to conjure up a completely new world of synthetic hydrology composed of linear black boxes assembled from normal components and embellished with Bayesian lining.

In summary, the lack of progress in hydrology including the proliferation of hydrologic misconceptions can be traced to the diverse nonhydrologic disciplines from which hydrologists come, which they do not want to leave, and which are responsible for their respective hydrologic vacua. Hydrology has not yet acquired a perspective of its own; the hydrologic window in the house of science, although appearing in the plan, has not yet been installed. As a result, hydrology is as yet lacking a solid scientific foundation needed for its development as a natural science. Nobody else can build this foundation than universities by granting hydrology the status of a primary discipline for which the student would be prepared by mastering its broad scientific basis rather than by learning only about techniques for its practical applications as is the current practice.

MODELS THAT WORK WELL—THE GREATEST DANGER TO PROGRESS IN HYDROLOGY

For a good mathematical model it is not enough to work well. It must work well for the right reasons. It must reflect, even if only in a simplified form, the essential features of the

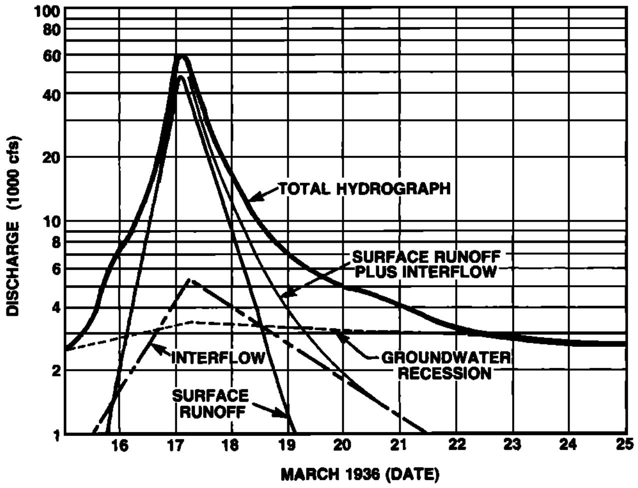

Fig. 1. Example of hydrograph analysis (*Linsley et al.* [1949, 1975]; reproduced by permission of McGraw-Hill Book Company).

physical prototype. The many wrong reasons why models may work well were given elsewhere [*Klemeš*, 1982*a*] and will not be repeated here. My favorite example is the difference between the Ptolemaic and Copernican planetary models [*Klemeš*, 1974]. The first worked well despite a profound misconception, the second because it was based on a scientifically correct principle. In the present context, the most important thing is that the difference between the two models, while paramount for astronomy (and theology), was of little consequence to the user, the sailor or ship owner. For him, to navigate his boat, the important thing was the result, the number, which both of the models could supply. It is my guess that the Copernican model eventually prevailed mostly because it was computationally simpler. Had computers been available at the time so that a more refined fitting of the Ptolemaic model could easily be implemented and adding another epicycle would mean just going once more through a DO-loop, this model could well still be with us today and space exploration would still be only a fantasy. This would be even more likely if astronomy research were funded by ship owners rather than the nobility.

Going back to hydrology, we can readily substitute the big water resource management institutions for the ship owner, the hydraulic engineer for the sailor, and the hydrologist for the astronomer. The trouble is, however, that our hydrologist is most likely to be the same hydraulic engineer whom we already have substituted for the sailor. Our hydrological "astronomy" is done mostly by sailors and paid by ship owners. Unfortunately, unlike 16th-century astronomy, it is richly endowed with ever more powerful computers so that adding a few more epicycles presents no difficulty. That is why in hydrologic modelling we concentrate on refining the computation of various hydrologically irrelevant trivia while evading the difficult problems, "... our technological successes have simply made us more efficient at being stupid" [*Welles*, 1984], and why hydrologic models make such ideal tools for the preservation and spreading of hydrologic misconceptions. The computerized modelling technology, besides strengthening the temptations of the sailor/hydrologist to improve the number rather than the concept, has also been a godsend to our hydrologic shipowners because of its tremendous potential to divert talent and resources into the pursuit of the irrelevant. The point is that genuine progress in hydrology might embarrass the big water management institutions by exposing the pitiful scientific basis of their past decisions and undermining their hard-won power. Hence rather than encouraging real research, they channel their support of hydrology into the acquisition of new hardware and the development of new models based on old ignorance, ostensibly to aid the solutions of urgent practical problems, improve technology transfer, satisfy client needs, or whatever the current cliché may be.

Being (mostly) engineers at heart, hydrologists are concerned not about the validity of their hydrology but of their numbers and, what is really tragic, they often do not even see the difference between the two things. I first realized this when about 20 years ago, dissatisfied with the arbitrariness of "probability distribution fitting" to streamflow samples, I tried to find out whether the occasional negative skewness encountered in these samples may have some hydrologic reasons [*Klemeš*, 1970] rather than be only a sampling artefact to be disregarded as was common practice. At various times I raised the point with several preeminent hydrologists only to be deeply shaken by some of their responses. For example, one suggested that I "solve" the problem by fitting a flipped-over Pearson III distribution in such cases, another proposed that I could easily "get rid" of the problem if I used the square of the coefficient of skewness as a parameter!

While the fixation on getting a number, a fit, or simply on problem solving rather than understanding, may be typical for engineers (although it probably has been brought to perfection by systems analysts), the deeper problem is the inability to cross one's own shadow so to speak. In modern parlance it would be called the breaking up of a selfreference system [*Hofstadter*, 1979]. For the hydrologist this system is his primary discipline. He should realize that his hydrologic efforts are often similar to trying to raise himself by his own bootstraps, and should be aware of the dangers involved, in particular of the danger that a selfreference system tends to evolve into a selfrighteous ideology which can recognize, and communicate with, nothing but itself, and be proud of it. This selfrighteousness is quite typical among "engineering hydrologists" whose attitude sometimes reminds me of Stalin's response to a criticism that Marxism-Leninism cannot jump out of itself: "To jump out of oneself may suit to wild goats but would be unbecoming of a scientific theory of Marxism-Leninism" (quoted from memory).

An essential prerequisite for breaking the vicious circle of a selfreference system is the art of knowing "the difference between knowing the name of something and knowing something" [*Feynman*, 1983]. In hydrologic modelling (as elsewhere), this dividing line is often blurred by mathematics. The reason is that its x's and y's can be easily arranged in formally correct algebraic structures which can describe equally well valid hydrologic concepts as well as misconceptions. Combined with the aforementioned possibility that a wrong concept may well produce a reasonable number, this ability of mathematics provides an inexhaustible source of hydrologic misconceptions.

Whatever mathematical (geometrical, statistical) procedure has led to a number with a plausibly sounding hydrologic name, it automatically becomes a "hydrologic model" whether it contains any hydrologic substance or not. This may happen even in spite of explicit disclaimers of the authors of such a model. "Hydrograph separation" (Figure 1) provides a good example. *Linsley et al.* [1949] caution that "... except for special experimental techniques on small areas, the source of water passing a gaging station cannot be identified," that "no reliable check on the adequacy of any of the [common] procedures is possible," that the common rules are "arbitrary," etc. They further state that the purpose of the exercise is merely to estimate the amount of direct runoff in which regard the procedures "do not differ excessively" so that "the selection of a particular method is not so important as is its consistent application throughout the study." A quarter of a century later [*Linsley et al.*, 1975] they reiterate even more clearly that the method of hydrograph separation which they propose for the division of a hydrograph into direct and groundwater runoff is arbitrary "... since there is no real basis for distinguishing between direct and groundwater flow in a stream at any instant." It is quite obvious that the aim here has been to get a volume-related number rather than a scientific model of the hydrologic subprocesses such as "baseflow," "interflow," etc., defined on the basis of "separation techniques." Yet, these caveats have been generally disregarded or forgotten and the hydrograph separation techniques in question have become the cornerstone of many "conceptual" hydrologic models advertised as physically based and suitable for application to analysis of such subtle problems like the effect of climate change on streamflow, changes in water quality in streams due to manmade causes, etc.

Another example is the Thomas-Fiering model which has become a stochastic hydrologic model par excellence despite *Fiering*'s [1966] warning 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."

In both cases, as in many others, the mathematics describes a rather arbitrary rationalization for obtaining a "hydrologically plausible" number (i.e., a number satisfying some nonhydrologic need of the modeller, typically a need for a design parameter), not an actual physical process involved; in this regard it can be completely irrelevant. Thus for instance, following the standard analytical procedures of hydrograph separation cited by *Linsley et al.* [1949, 1975], one can easily identify "surface runoff," "interflow," and "groundwater flow" in hydrographs of outflow from a simple nonlinear reservoir such as a kitchen sink or a bathtub. Such "flow components" and other "parameters with real physical meaning" often are nothing more than hydrologic epicycles, usually regression coefficients with hydrologic names [*Klemeš*, 1982*a*].

For nonmathematicians, mathematics has an irresistible seductiveness (D. Berlinski, unpublished data, 1980). It is quite typical that once they have formulated an empirical fact or a scientific concept in a mathematical form, they are no longer capable of thinking about that fact or concept but only about the mathematical description itself and about the manipulations suggesting themselves by the algebra. This "mathematical" thinking takes over most easily where the "physical" thinking is weak, when the scientific knowledge available to the analyst does not lead him to new insights, when his scientific intuition runs out of steam. The vacuum created by an inability to see patterns in the substance is then spontaneously filled with an increased attention to patterns in the form, and the reality, instead of being described by mathematics, is stretched or lopped to fit the Procrustean bed of its various formalisms.

Hydrology, having no solid foundations of its own and moving clumsily along on an assortment of crutches borrowed from different disciplines, has always been an easy victim of this practice. Every new mathematical tool has left behind a legacy of misconceptions invariably heralded as scientific breakthroughs. The Fourier analysis, as was pointed out by *Yevjevich* [1968], had seduced the older generation of hydrologists into decomposing hydrologic records into innumerable harmonics in the vain hope that their reconstitution will facilitate prediction of future hydrologic fluctuations (fortunately, few computers were available at the time so that the Fourier fever did not become an epidemic); various statistical methods developed for evaluation of differences in repeatable experiments have been misused to create an illusion of a scientific analysis of unrepeatable hydrologic events; linear algebra has served to transform the idea of a unit hydrograph from a crude but useful approximation of a soundly based concept into a pretentious masquerade of spurious rigor now exercised in the modelling of flood events; time series analysis has been used to remake inadequate 20-year streamflow records into "adequate" 1000-year records, or even more adequate 10,000-year records; and the theory of pattern recognition is now being courted in the vain hope that it will lend scientific legitimacy to the unscientific concept of mindless fitting that dominates contemporary hydrologic modelling. In all these cases, mathematics has been used to redefine a hydrologic problem rather than solve it. *Box* [1976] calls such use of mathematics "mathematistry" and laments:

> In such areas as sociology, psychology, education, and even, I sadly say, engineering, investigators who are not themselves statisticians sometimes take mathematistry seriously. Overawed by what they do not understand, they mistakenly distrust their own common sense and adopt inappropriate procedures devised by mathematicians with no scientific experience.

(It is ironic that much of the oversophisticated mathematistry currently practiced in hydrology is being advanced under the banner of Box-Jenkins modelling.)

Box obviously takes it for granted that, in the same way as the mathematician has a sound mathematical common sense, the physical scientist has an equally sound physical common sense for his own discipline. However, the parallel is not so straightforward as it may seem. The mathematician is at an advantage because the object of his common sense is, so to speak, the common sense itself, i.e., the logic of the human thinking process. In contradistinction, the physical scientist's common sense must also embrace the "logic" of the external physical world, i.e., an intuition for the "thinking process" of nature which often seems strange and contrary to what our own logic expects. It has been said about the physicist Niels Bohr: "He never trusted a purely formal or mathematical argument. 'No, no,' he would say, 'you are not thinking; you are just being logical' " [*Frisch*, 1979]. A scientific common sense simply cannot be developed solely by reasoning as a mathematical one can; for example, we could not have discovered the Reynold's number just by analytical thinking as we did the prime number.

It is because they are overawed by the apparent impenetrability of the logic of nature that scientists are drawn to mathematistry in a subconscious hope that nature can be cheated and the simple logic of mathematical manipulations can be substituted for the hidden logic of the external world. Only the most powerful scientific minds occasionally are able to transcend this tendency and subordinate mathematics to their scientific intuition. Thus Newton developed calculus to be able to formulate mathematically his physical concepts about

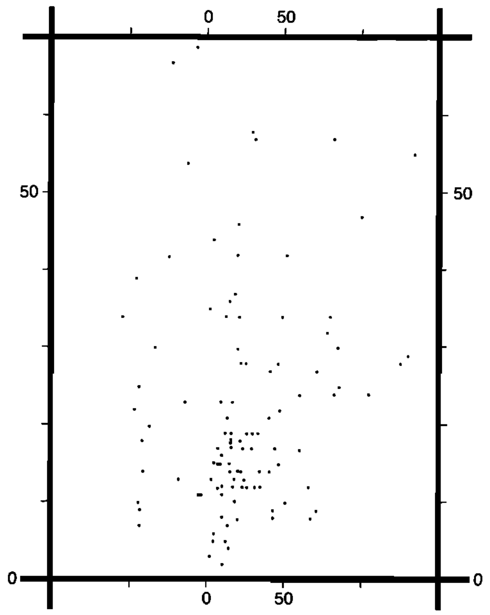

Fig. 2. Net monthly inflow in mm (abscissa) versus runoff in mm for the Rainy Lake Basin (adapted from *Klemeš* [1983]).

moving objects; Einstein searched for years for a proper mathematical tool suitable for expressing the concept of general relativity until he finally found it in Minkowski's geometry; Dirac had to invent the delta function to translate into mathematics an aspect of the true but peculiar logic of the behavior of elementary particles. However, the important thing is not that hydrology has had no Newtons, Einsteins and Diracs, and only seldom a Horton, but that hydrologists be aware that the logic of hydrologic processes cannot be deduced from algebra, and that credibility of hydrologic models can rest only on at least approximately correct rendering of the true dynamics of these processes.

Thus at the present stage of hydrologic science, hydrologic modelling is most credible when it does not pretend to be too sophisticated and all inclusive, and remains confined to those simple situations whose physics is relatively well understood and for which the modeller has developed a good "common sense" within his primary discipline. This applies, for instance, to those cases where hydrology is clearly dominated by hydraulics and fluid mechanics and is modelled by a specialist in this area who consciously limits his inquiry to what he knows and avoids what he knows only the name of (this often implies that he refers to himself as a hydraulic engineer, a hydrogeologist, etc., rather than a hydrologist). Examples include overland flow models, channel and reservoir routing models, and models of water movement in porous media with simple structures. Their combinations sometimes can reasonably portray some simple real-life situations such as the movement of storm water in urban areas, i.e., situations where the water moves mostly over impervious surfaces and through impervious and regularly shaped conduits, where the area is small so that the rainfall is relatively uniformly distributed over it, and where the evaporation during the storm episode is negligible. While these models are often labelled by the somewhat pretentious misnomer "urban hydrology," many authors, mostly those who consider themselves hydraulic engineers rather than hydrologists, still stubbornly refer to them by their proper name as storm water management models.

As the modellers venture further away from the well-lighted grounds of their primary disciplines into the shadows of hydrology where a name is often the only light (or a will-o'-the-wisp) in sight, hydrologic misconceptious find it progressively easier to hide and flourish in the thickets of mathematistry of their hydrologic models. While this danger is serious and universal, it remains dormant until the models take the shape of easy-to-use "software packages." At that stage the unholy trinity of good intentions, ignorance, and efficiency closes the circle and a hydrologic misconception becomes a virtually insurmountable obstacle to progress in hydrology.

HYDROLOGIC MATHEMATISTRY AS A BASIS OF INDEFENSIBLE EXTRAPOLATIONS

The muddled thinking which makes it difficult to differentiate between the pursuit of hydrology as a science and the pursuit of a number as a convenient basis for water management decisions not only leads to bad science and hinders the progress in hydrology but eventually also leads to bad water management. This is most likely to happen when hydrologic models based on mathematistry are used for extrapolation; and extrapolation is, in fact, the main application of hydrologic models in water resource management.

If a wrong model is used for interpolation, the error is usually small or at least it does not exceed the difference between the numbers being interpolated. Yet, even here dangers lie in wait if one does not realize the difference between a mathematical model and the prototype which it is supposed to describe. For it may well be that no physical entity corresponds to the interpolated number. For example, if it takes 4 hours for one man to load a truckload of soil, 2 hours for two men, and 1 hour for four, the task certainly cannot be completed in 3 hours by one and one third of a man despite the fact that a mathematical model may suggest it. However, the dangers of extrapolation are much more subtle because its results may look plausible in the light of the analyst's expectations. Hence extrapolation does not have a good reputation among scientists. Their attitude to it has recently been well expressed by *DiFrancia* [1981]: "Sometimes, lacking better information, one provisionally assumes that a law, verified inside class *P*, also is valid outside class *P*. This is called an extrapolation. Extrapolation often has considerable heuristic usefulness. But every physicist knows that it is not a rigorous procedure. Extrapolation may sometimes be used carelessly by writers, philosophers, historians, politicians, and so on; not by physicists (at least not by good ones!)." Given the emphasis on, and the cavalier lack of care exercised in, extrapolation in hydrology, one is left to wonder whether DiFrancia would find hydrologists eligible even for this group of what he obviously considers only casual offenders.

Both interpolation and extrapolation are extensions of a pattern. However, in extrapolation one must be doubly aware what kind of a pattern one wants to extend. For the same information or data may conjure different patterns in different minds depending on the mode of thinking to which the particular mind has been conditioned. For example, to a mathematician, Figure 2 may suggest the pattern of a random field while to the Good Soldier Švejk [*Hašek*, 1974] it could well suggest the pattern of a physical process involving an interaction of flies with a picture of His Highness Emperor Frantz Josef II in an old pub in Prague. Needless to say, both would be wrong since the plot shows a monthly "rainfall-runoff rela-

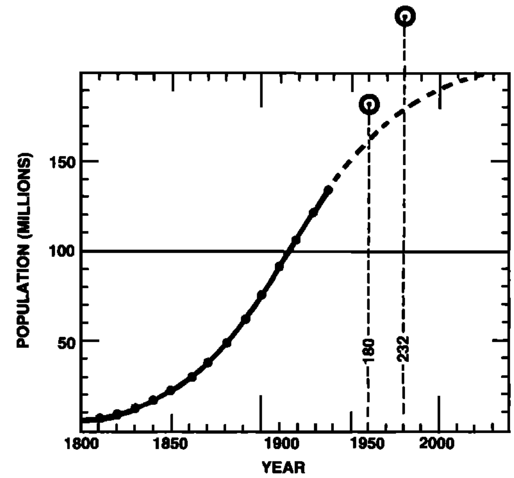

Fig. 3. Conjectural population of the United States [*Thompson*, 1942]; double circles show the actual population in 1960 and 1980.

tion" and hydrologic patterns should be invoked in its interpretation.

It is chiefly in connection with extrapolation where the scientific common sense mentioned by Box, in our case an ability to see hydrologic patterns, is important as a guard against the danger of seductiveness of a mathematical pattern. A telling example of this danger is supplied by *Thompson* [1942]. Figure 3 shows the growth of the population of the United States between 1800 and 1940, and its excellent fit by the logistic (growth) curve. Thompson was so impressed with the accuracy of the fit that he did not hesitate to believe that even the future reality would follow the mathematical pattern of the curve. Both his enthusiasm for, and his confidence in, the logistic curve are truly enviable:

> ... the Malthusian expectation of a doubling every twenty-five years ... continued through five decenia; but it ceased some seventy-five years ago, and a retarding influence has been manifest through all these seventy years. It is more recently, only after the census of 1910, that the curve seemed to be finding its turning point, or point of inflection; and only now since 1940, we can say with full confidence that it has done so. ... Wars and financial crises have made their mark upon the curve; manners and customs, means and standards of living, have changed prodigiously. But the S-shaped curve makes its appearance through all of these, and the Verhulst-Pearl formula meets the case with surprising accuracy.

Today, almost half a century later, we can see how these statements stand up to the reality. The U.S. population reached 180 million in 1960 and 232 in 1980 (plotted as double circles in Figure 3), which shows that Thompson's full confidence in the curve having found its turning point was premature, the appearance of an *S* curve in the data was a delusion, and the surprising accuracy which the formula showed for interpolation was a very poor indicator of accuracy in extrapolation.

This example is instructive not only because it shows how a good fit of data can be misleading as far as extrapolation is concerned but because it shows how a well-chosen name combined with mathematics can conspire to set a perfect trap for a mathematical amateur. For were it not for the name "growth" curve and the exactness implicit in the mathematical formulation, Thompson's confidence in it would have been hardly so strong. The irony is that both the name and the mathematics of the curve had been originally chosen because the curve was found to fit growth processes which do converge to an asymptotic state (a typical example is the growth of an autocatalytic monomolecular reaction decelerating through exhaustion of reagents. This process, which is responsible for yet another name of the curve—autocatalytic, " ... is often fitted by the equation $x = a/(1 + be^{-kt})$ where a, b, k are constants. However, ... various other relations fit available growth data equally well ... !" [*Needham*, 1962]). Thus to postulate from the mathematics of the curve that a given growth process whose initial stages it fits well will reach an asymptotic state stands the logic of the use of the curve on its head. There are many growth processes that have no inherent tendency to stabilize and not even the most perfect fit of their observed states by the growth curve guarantees that they will. Here belong, for instance, the development of science and technology, human craving for an ever higher standard of living, lust for power, budget deficits, "progress" in general, and, of course, cancer.

Before dismissing Thompson's attitude to the growth curve as naive, it should be realized that the attitude of many hydrologists to hydrologic models is essentially the same. One only needs to recall how a decade ago hydrologists took seriously a postulate of infinite memory in hydrologic processes for no other reason than an appearance of an infinite lag in the mathematical structure of the fractional Gaussian noise model which happens to provide a good fit to some hydrologic time series. However, this case has been discussed in detail elsewhere [*Klemeš*, 1974] and will not be repeated here. An example closer to Thompson's use and interpretation of the growth curve is the use and interpretation of probability distributions in hydrology. Nowhere has this example been brought to a higher degree of perfection than in flood frequency analysis which epitomizes all what has been said before; it therefore deserves closer scrutiny.

Extrapolation of flood frequency curves to obtain estimates of the customary 100-, 500-, 1000-year, etc., flood (I have been approached recently with a request for an estimate of a 1,000,000-year flood; a request for an estimate of a flood exceeded on the average once since the Big Bang may soon follow and should not pose a serious difficulty for a good flood frequency theorist) has neither a sound empirical basis nor a theoretical one [*Klemeš*, 1986*b*].

From an empirical point of view, a 2- or 5-year flood may be a sound concept on the assumption that 50 years or so of data are available, the historic record does not look conspicuously different from a random series, and physical conditions during that period are known to have been approximately stationary. In such a case it makes sense to talk about a flood that was exceeded, on the average, once in 2 or 5 years, and to interpolate past exceedances within this frequency range using some smooth curve provided that the ordered sequence itself is sufficiently smooth. However, to extrapolate this empirical concept to, say, a 10,000-year flood does not make sense for most rivers in the northern hemisphere. A flood which was exceeded, on the average, once in 10,000 years can only be defined if there were at least a hypothetical possibility of a streamflow record of the order of 100,000 years long. But there is no such posssibility in most cases, since as recently as 10,000–20,000 years ago most of this hemisphere was covered with ice; there may have been very large floods indeed since the ice sheet retreated but there certainly was no flood that could have been exceeded on the average once in 10,000 years as only one or two such periods existed.

Turning now to its theoretical basis, extrapolation of the concept of average return period can often hardly be justified

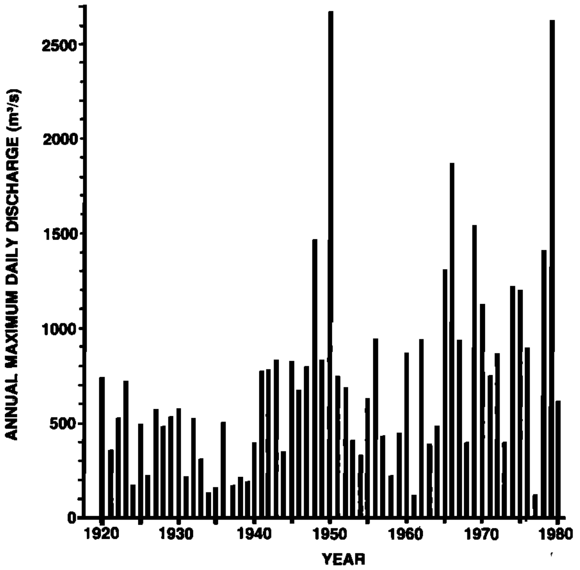

Fig. 4*a*. Annual maxima of daily discharge for Red River at Emerson, Manitoba, Canada.

even beyond a 10-year flood or so because of nonstationarity of physical conditions over periods longer than 50–100 years. There are many known causes of nonstationarity ranging from the dynamics of the Earth's motion to manmade changes in land use, and as yet unknown causes can be discovered. In this context, the notion of a 100-year flood, which has acquired such a pivotal position in flood frequency analysis, has no meaning in terms of average return period. Even some relatively short hydrologic records like those shown in Figures 4*a* and 4*b* indicate that the concept of an average return period, of whatever length, may be flawed. And a probabilistic interpretation suggesting that a flood with an average return period T can be exceeded with probability $1/T$ in any given year (or any given time) is, if anything, even more dubious. Apart from disregarding nonstationarity for mathematical convenience, it implies an ergodic stochastic process where only one time series exists and redefines the uncertainty about the actual dynamics of flood occurrences as a certainty that they are random events. From the physical point of view, an equal probability of an extreme event in all time intervals is extremely improbable even if the time series of the related process does look stationary and random. In the physical world, in contrast to the theory of random sampling, specific events have specific causes even if we are ignorant of them. And as far as the really extreme events are concerned, conditions for them may be developing for a long time and once the event has occurred its immediate repetition may be virtually impossible. An earthquake may release forces that have been building up for decades or centuries; a glacier whose sudden melting results in a flood may have grown for decades and, once melted, the probability that it will cause a similar flood drops to zero; it may take many days or weeks for a frontal system or a hurricane to develop and once the water it carries has been precipitated, there is no immediate danger of another similar event occurring, etc. Suppose for instance that the deluge was a historic flood resulting from a climatic perturbation caused by a catastrophic volcanic eruption in the Aegean Sea which in turn may have been a consequence of pressure build-up along the boundary between the Eurasian and African tectonic plates. From the geological point of view, it is quite probable that just after the Flood the probability of another similar event in the area sharply decreased and has been increasing ever since.

But let us consider a less esoteric example represented by the flood record shown in Figure 4*b*. It is an example of a better-than-average flood data base in southern Ontario since it is almost 30 years long, the river is not regulated and no drastic change in land use took place in the basin during the period of record. However, the flood regime is hydrologically very complex. While most of the maximum annual flows occur during the snowmelt season in March and early April and are related chiefly to the accumulation of snow on the ground, some occurred in the middle of winter due to a sudden rise of temperature which caused a short-lived thaw, another occurred during a flash flood caused by a local storm in early summer, but the largest flood of all was caused by a hurricane which hit the region in October 1954. Thus the record represents a collection of hydrologically nonhomogeneous events some, or perhaps all, of which may in addition be subject to a climatic trend. Yet, for the purpose of flood frequency analysis, it is routinely regarded as a random sample from a homogeneous population, is fitted by some simple probability distribution and the fitted curve is boldly extrapolated to provide estimates of a 100-, 500-, even a 1000-year flood. Figure 5 shows one such fit (curve A) as produced with the aid of a standard flood frequency analysis program of the Water Resources Branch of Environment Canada which is no worse than similar programs used by other organizations (out of sheer pity for the data, I excluded the hurricane flood although this often would not be done). Now, if, for some reason, the three lowest maxima in the practically floodless years were slightly lower or higher than the record indicates, the corresponding best fits produced by the program would be as shown by curves B and C, respectively. The legitimacy of these curves would hardly be questioned by a hydrologist. It should! For it is by no means hydrologically obvious why the regime of the highest floods should be affected by the regime of flows in years when no floods occur, why the probability of a severe storm hitting this basin should depend on the accumulation of snow in the few driest winters, why the return period of a given heavy rain should be by an order of mag-

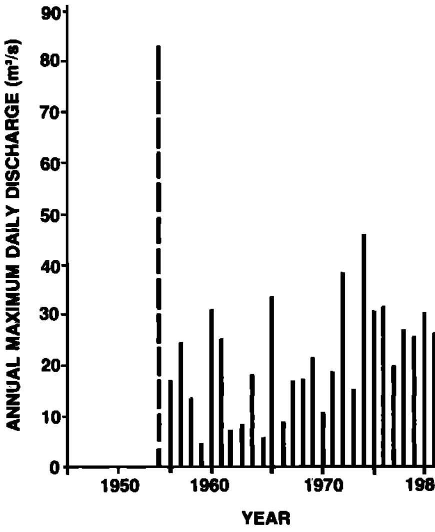

Fig. 4*b*. Annual maxima of daily discharge for East Humber River near Pine Grove, Ontario (at the northwest edge of Metropolitan Toronto). Hurricane Hazel hit the area in October 1954, shortly after the station was put in operation; the Hazel flood peak was estimated at 83.3 m³/s.

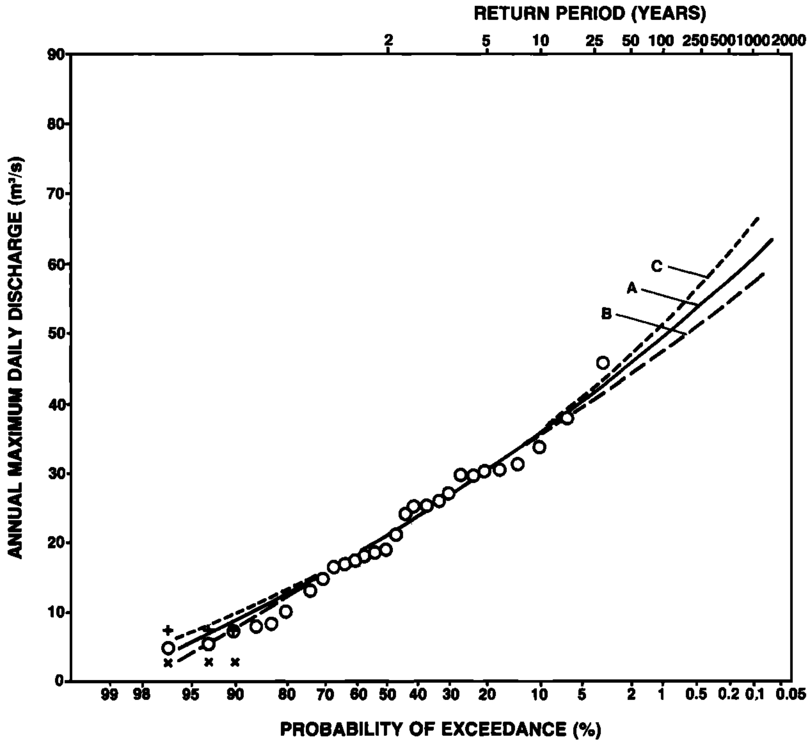

Fig. 5. Results of flood frequency analysis for data shown in Figure 4*b* (excluding the hurricane Hazel flood of 1954) obtained by a maximum likelihood fit of a three-parameter lognormal distribution using a Flood Frequency Analysis Package developed and operated by Water Resources Branch of Environment Canada, Ottawa. (*a*) Fit to historic data. (*b*) Effect of an arbitrary reduction of the three lowest records to 3 m^3/s. (*c*) Effect of an arbitrary increase of the three lowest records to 7.5 m^3/s.

nitude different depending, say, on slight temperature fluctuations during the melting seasons of a couple of years.

To believe in legitimacy of the effect of the three lowest points on the upper tail of a probability distribution of floods, as is shown in Figure 5, presupposes that a belief in the proposition that maximum annual flows are a random sample from an a priori specified homogeneous probability distribution is much stronger than a belief that they are hydrologic events. And this is indeed the impression which one gets from publications on flood frequency analysis. From the bulk of "scientific" literature on the subject one cannot but conclude that the existence of some a priori given probability distributions of floods and of perfectly random sampling mechanisms that generate flood chronologies from them are self-evident hard facts, which not only need no hydrologic evidence but override any such evidence to the contrary; for everything is based on these two assumptions, they are never questioned and no hydrologic, climatic, geologic, or other physical conditions are invoked in the analysis. The floods are stripped of all hydrologic context down to bleached skeletons of numbers giving their peak flows and these numbers are then subjected to the most rigorous treatments regarding plotting positions, Box-Cox transformations, maximum likelihood (sic!) parameter estimates, goodness-of-fit testing, etc., apparently in an unshakable belief that the amount of this rigour determines the degree of hydrologic relevance of the results.

Only the same inverted logic which made Thompson believe that the mathematics of the growth curve would guide the growth of U.S. population to a 200 million limit makes hydrologists believe in probabilities of extreme floods obtained by extrapolation of probability distribution functions fitted to a few peak flow numbers. It never seems to enter the minds of flood frequency theorists that the word "probability" in the name of these functions does not bestow a probabilistic meaning on data to which they are fitted, that their name has been derived from the fact that they usually fit well an ordered arrangement of numbers which are known to be random samples from probability distributions. As a matter of fact, a growth curve may fit an ordered sample of floods equally well as a probability distribution function may have served Thompson in fitting the growth of U.S. population.

The usual justification for engaging in the mathematistry of flood frequency analysis is that the engineer needs at least an estimate of probabilities of large floods to be able to optimize the design of various structures; that it is exactly because we do not know the answers to the difficult hydrologic, climatic, geophysical, and other aspects of flood probabilities that we must resort to mathematical and statistical simplifications underlying flood frequency analysis; that precisely because of our ignorance of the hydrologic truth and because of the shortness of hydrologic records, we must use the most efficient and rigorous mathematical methods in order to extract the greatest possible amount of information from the data. This argument (and no other can be offered in defence of flood frequency analysis) is just another example of the muddled thinking which cannot differentiate, on one hand, between engineering concepts dictated by expediency, and scientific truth (as Yevjevich put it is his 1968 paper when referring to the misconception of the "maximum probable precipitation") and, on the other hand, to paraphrase Feynman, between "something and the name of something," because the information that our rigour extracts (or rather extorts) from the numbers, while pertaining to some hypothetical probability distribution from which these numbers would be a random sample, does not become information on probabilities of floods merely because we use that name.

To an outsider it may be difficult to understand the motives behind the ongoing arguments about the fine mathematical points of flood frequency analysis which are about as relevant to probabilities of extreme floods as they would be to the number of angels that can dance on the tip of a pin. If the flood frequency theorists were (good) engineers they probably would adopt the simplest procedures and try to standardize them in view of the fact (1) that the differences in things like plotting positions, parameter estimation methods, and even the distribution types, may not matter much in design optimization [*Slack et al.*, 1975], (2) that there are scores of other uncertain factors in the design that must be settled in a rather arbitrary manner so that even the whole concept of optimization must be taken as merely an expedient design procedure, and (3) that flood frequency analysis is just one convenient way of rationalizing the old engineering concept of safety factor rather than a statement of a hydrologic truth. If they were (good) hydrologists, they would readily realize that the whole underlying concept to which the technique is being applied is hydrologically badly flawed and they could not but see that the rigour in the technique is spurious and that by pursuing it they are being lured away from hydrology into playing hydrologically irrelevant games. And if they were (good) statisticians they would first try to establish the validity of the underlying assumptions because "it is inappropriate to be concerned about mice when there are tigers abroad" [*Box*, 1976]. Thus the most feasible explanation is that the flood frequency theorists are engineers at heart, hydrologists by self delusion, statisticians by ambition, and dilettantes by historical circumstance; the mathematistry they practice is the only "hydrology of floods" they were ever taught by their teachers, themselves hydrologists only by default.

Flood frequency analysis has been discussed in detail not only because it so well exposes the fallacies of hydrologic extrapolation but also because it is a good example of the most serious obstacles to progress in hydrology and has perhaps the best survival potential of all hydrologic misconceptions. First, unlike Thompson's extrapolation, it is in no danger of being proven wrong by observation of the reality because the many hundreds of years of flow records necessary to assess the correctness or otherwise of a 100-or-more-year flood will not be available soon. Second, it is in no danger from engineers whom it is supposed to serve not only because it provides them with a needed number but also because it often does not matter much what number it is [*Slack et al.*, 1975]. Third, it requires no hydrologic knowledge whatsoever while providing a solid basis for a successful career in hydrology. Fourth, it needs just about as much mathematics as is covered in undergraduate engineering or science courses. And fifth, it offers a virtually inexhaustible variety of games that one can play on even a small computer with nothing more than 30 or so numbers.

The danger from flood frequency extrapolation to water management is that it gives an appearance of scientific knowledge where there is ignorance. An "optimal" decision based on probabilities which are presumed known and approximately correct but are in fact unknown and may be indeed unknowable or ever undefined, can easily be much worse than a decision made in full awareness of the lack of knowledge, although this may not be immediately apparent. The danger to hydrology from extrapolations based on mathematistry is that they lead it on the path of bad science.

Science can use extrapolation only as a tool for probing the limits of existing knowledge, as a hypothesis to be tested. This distinguishes science from applied disciplines where extrapolation or an untested (or even untestable) hypothesis must often serve as a basis for decision and action. Such disciplines are sometimes called "arts" to emphasize their extrascientific components; e.g., medical arts, the art of design. A truly rigorous medical diagnosis which does not employ extrapolation can probably be made only on the basis of a patient's autopsy; a correct scientific answer regarding the magnitude of a 1000-year flood (or even the soundness of the concept itself) can probably be obtained only long after the ruins of the structure for the design of which it was needed will have been declared a historic monument. Herein lies the difference between hydrology and water managemet, between science and engineering expediency.

SAFEGUARDS AGAINST MISUSE OF EXTRAPOLATION IN HYDROLOGY

The danger from extrapolation of wrong patterns increases with a diminishing possibility of checking the results by observation. It is therefore in this direction in which the need for a sound scientific basis of hydrologic models increases. This order is roughly as follows: (1) short-term forecasting and prediction, (2) hydrologic simulation, and (3) long-term forecasting and prediction. Unfortunately, this is the same order in which also the difficulty of the problem increases and our understanding of the relevant physical mechanisms decreases. In the first group, we can often rely on the laws of fluid mechanics and hydraulics and sometimes the task of extrapolation of a theoretical hydrologic pattern can be recast as a problem of interpolation or short extrapolation of a geometric or statistical pattern known to be consistent with past empirical evidence. In the second group, at least an indirect testing on analogous empirical data is often possible, but in the third group the only basis of credibility is a hydrologically sound theory, since an opportunity to correct a wrong extrapolation by comparison with the reality will always come either too late or never.

The diminishing credibility of hydrologic models in the direction indicated above, and an increasing caution in their use which one would expect as a result, are, however, not evident in current attitudes. A number obtained by extrapolation of a flood frequency curve based on no physical or empirical evidence seems to be taken with the same seriousness as one obtained, for example, by extrapolation of a flow rating curve based on hydraulic computations using known physical properties of a stream channel. This is because hydrologists are usually unable to see the difference between hydrology and water resource management, between hydrology and statistics, between hydrology and the mathematics of curve fitting, between facts and assumptions, and, as a result, between hydrologic concepts and misconceptions. Improvement in this situation can be achieved by the adoption of higher standards for verification of hydrologic models.

In this regard, models of the first category are in the best position, since the availability of test data and the ease of testing of model performance provide a relatively good safeguard against the spreading of at least those misconceptions which lead to the largest extrapolation errors. Here the modellers must be cautious, since "Pediction ... is a precarious game because any prediction can eventually be compared with the actuality" (*Aitchison and Dunsmore* [1975]; here the authors are using the term "prediction" in the sense in which "forecasting" is used in hydrology). For models in the third category, the possibility of testing is virtually nonexistent. As a result, the modelling game has been very safe here and it will remain so until its hazards are exposed and understood on theoretical grounds. The greatest immediate improvement is thus possible in the second category where modelling, while being a safe game now, can be made quite precarious.

If one defines a simulation model as a mathematical model whose objective is the synthesis of a record of some hydrologic variable Y_i (e.g., streamflow) for a period T from available concurrent records of other variables X_i, Z_i, $\cdots$, (e.g., precipitation, air temperature, etc.), then the model is useful to hydrology (as a tool for the testing of the plausibility of its underlying scientific concepts) via the degree of success in reproducing the record $Y_i(T)$. If the model is proven successful in several such instances, $i = 1, 2, \cdots, n$, where data on Y_i are available then there are reasons to believe that it will also be successful in simulating an unavailable record $Y_{n+1}(T)$ from available records $X_{n+1}(T)$, $Z_{n+1}(T)$, $\cdots$; this then will render the model useful to water management where the record $Y_{n+1}(T)$ may be needed in aid of some decisions.

Strictly speaking, this rationale (based on a transformation of a dynamic extrapolation in a single case into a statistical interpolation between several distinct cases) is applicable only if the simulation model can be developed without any recourse to the available records $Y_i(T)$, $i = 1, 2, \cdots, n$. In hydrology this is not yet the case and simulation models generally must be fitted to (calibrated with) available records of $Y_i(T)$. This makes them hydrologically rather useless (first, given a large enough number of its degrees of freedom, almost any model can fit a given record; second, no hydrologic purpose is served if the only virtue of a model is its ability to be fitted to an existing record) but, surprisingly, it does not seem to undermine their credibility in water management circles. The most plausible reason for this is that once the available record has been used for calibration, there is nothing left for verification, and consequently the adequacy of the model cannot be challenged by "comparison with the actuality." The model then qualifies by default and nothing can interfere with its "successful" application for filling in gaps in historic records, record extensions, etc.

However, for simulation models this freedom from challenge is not as perfect as it is, for instance, for flood frequency models because the adequacy of simulation models can be tested at least indirectly. The rationale described above can be modified to admit calibration at the expense of reducing the strength of the test to what might be called an operational level. At this level a model would be required to demonstrate only its operational adequacy by successfully simulating an available record which has not been used for its calibration and which has conditions hydrologically similar to those corresponding to the unavailable record which is the final purpose of the simulation. Thus a model with inputs X_i, Z_i, $\cdots$, can first be calibrated using an existing output record Y_i, and then tested by simulating another available output record Y_j using inputs X_j, Z_j, $\cdots$. Success in such a test would lend the model at least a modest level of credibility vis-a-vis its ability to simulate the desired unavailable record Y_k using existing records X_k, Z_k, $\cdots$.

This concept, the simplest example of which is the common split-sample test (which presently represents the highest level of verification of simulation models and is applied only occasionally, e.g., *World Meteorological Organization* [1975, 1985]), has been extended [*Klemeš*, 1982*b*, 1986*a*] into a systematic hierarchical scheme which can be used for operational testing of (1) the transposability of a simulation model within a region (a "proxy-basin" test); (2) the ability of a model to simulate streamflow at a given site for different climatic, land-use, etc., conditions than those for which it has been calibrated (a "differential split-sample" test); and (3) the ability of a model to simulate streamflow in a different basin than that for which it has been developed and for different conditions than those for which it has been calibrated (a "proxy-basin, differential split-sample" test). The somewhat esoteric labels used for these tests have been motivated solely by the author's experience that a simple name tends to undermine the scientific respectability of a concept (compare, for instance, the fate of the mass curve; *Klemeš*[1979]).

In reality, the above tests are nothing more than an attempt to systematize a rather self-evident procedure that would allow hydrologic simulation models to demonstrate their ability to perform the tasks which are expected of them. A recommendation for their mandatory use is hardly a revolutionary requirement and many a nonhydrologist may be shocked at a thought that meeting some kind of such tests is not a sine-qua-non condition for any real life application of a hydrologic model. Yet this is the way things are in contemporary hydrology. It is likely that lack of data for proper testing would be blamed for this situation, together with the "need to provide the engineer and planner with at least approximate results" even if, in the absence of testing, it is not known whether "approximate" is not merely an euphemism for "wrong." The real reason, however, is the same as the one given in connection with flood frequency analysis, the consequences being similar as well.

CONCLUSIONS

Hydrology has not yet consolidated itself as a science in its own right. The process of consolidation is difficult, since on one hand, hydrology has no clearly defined scientific base and, on the other, the formation of such a base is made difficult by the lack of consolidation. The net result of this vicious circle has been stagnation. The various nonhydrologic backgrounds from which hydrologists come make it difficult for them to cross the line and change their individual disciplinary perspectives of hydrology into a hydrological perspective of their primary disciplines, or even to see a difference between these two perspectives. The unsatisfactory status quo is strengthened by vested interests of large water management organizations which, through their policies for support of hydrology research, often encourage mediocrity and inhibit innovation (this is just a specific instance of a much wider long-standing problem of research management; see, for example, *Parkinson*, 1960; *Medawar*, 1969; *Braben*, 1985). The resulting misconceptions are then difficult to eradicate, since they are often used as standards by which progress is measured.

These are symptoms of transition through which probably every newly emerging science must pass. The inevitability of this transition state carries with it an inevitability of dilettantism. However, there is a danger that the transition is never completed and the dilettantism becomes the norm. To prevent this from happening, the hydrologic community must first recognize the situation for what it is and start taking their adoptive discipline seriously. This requires, in the long term, to give hydrology a chance to become a broadly based primary discipline in university curricula and, in the short term, to increase the emphasis on natural sciences in the teaching of hydrology while raising professional standards in practicing it, among other things by rigorous testing of performance of hydrologic models. If the present trend away from physical processes and toward mathematistry ("blackboard hydrology," J. E. Nash, personal communication, 1982) continues in hydrologic education and practice, hydrology will end up in a dead end as a science and become useless for applications. Practices of bad science in hydrology cannot be blamed on engineers and other decision makers who "need numbers." For if these numbers are not to be based on sound hydrologic science but only on manipulations of arbitrary assumptions and concepts, hydrologists are not needed. Engineers can do such a job much better themselves since they at least can tailor the assumptions to the particular projects and, not mistaking them for scientific truth, will treat them accordingly in the decision process.

What then remains for the hydrologist to do if we take away from him the curve fitting, model calibration, the chasing of systems responses, correlations, finite elements, kriging, etc.? Perhaps, his efforts expended on the fitting of flood and drought frequency curves could be better spent in acquiring deeper knowledge of climatology, meteorology, geology, and ecology, since many "hydrologic" problems transcend the framework of hydrology as we know it today. Instead of more refined calibration techniques and analyses of residuals, he should perhaps aim at the inclusion of other forms of energy into hydrologic models than the overworked kinetic and potential energy of water, since the latter two cannot drive, even if they wanted to, but a small fraction of the hydrologic cycle. Rather than extorting systems responses from river basins by ever more sophisticated transformations, filtering, and "model identification" techniques, he would benefit more from trying to understand how relevant the notion of a river basin is in the first place, since there probably is an intrinsic difference between the behavior of, say, the Orinoco Basin and the basin of a Three Mile Creek near Moose Jaw, Saskatchewan. It also seems obvious that search for new measurement methods that would yield areal distributions, or at least reliable areal totals or averages, of hydrologic variables such as precipitation, evapotranspiration, and soil moisture would be a much better investment for hydrology than the continuous pursuit of a perfect massage that would squeeze the nonexistent information out of the few poor anaemic point measurements, since, notwithstanding his regrettable unfamiliarity with Thiessen polygons or kriging, even Lucretius Carus knew two thousand years ago that "nil posse creari de nilo." And it is highly likely that instead of mastering partial correlations, fractional noises, finite elements, or infinitely divisible sets, the hydrologist would more profitably spend his time by studying thermodynamics, geochemistry, soil physics, and plant physiology, because there is abundant evidence that "Research driven by a technique ... seems to be a poor bet, since almost invariably the technician's skill is a solution looking for a problem" [*Braben*, 1985].

However, most important of all is the realization that calling something hydrology does not necessarily make it hydrology. Without recognizing this, hydrology cannot be cured of its present dilettantism of which misconceptions are only the symptoms. Clarity in these matters will make the difference between institutionalizing dilettantism in hydrology and overcoming it.

Acknowledgments. While taking credit for the raised blood pressure, headaches, and queasiness that this paper may cause, I would like to share the blame for its appearance with F. I. Morton, I. Rodriguez-Iturbe, R. A. Freeze, V. Yevjevich, and N. Buras, who, in spite of knowing what it contained, were quite comfortable with the idea that it might be published. However, lion's share of this blame must go to S. J. Burges who invited me to prepare this paper.

REFERENCES

Aitchison, J., and I. R. Dunsmore, *Statistical Prediction Analysis*, Cambridge University Press, New York, 1975.

Box, G. E. P., Science and Statistics, *J. Am. Stat. Assoc.*, *71*(356), 791–799, 1976.

Braben, D. W., Innovation and academic research, *Nature*, *316*, 401–402, 1985.

DiFrancia, G. T., *The Investigation of the Physical World*, Cambridge University Press, New York, 1981.

Dumitrescu, S., and J. Němec, Hydrology—A look back and a look forward, in *Three Centuries of Scientific Hydrology*, pp. 16–22, Unesco-WMO-IAHS, Paris, 1974.

Feynman, R., *The Pleasure of Finding Things Out*, WGBH Educational Foundation, Boston, Mass., 1983.

Fiering, M. B., Synthetic hydrology, An assessment, in *Water Research*, edited by A. V. Kneese and S. C. Smith, pp. 331–341, John Hopkins Press, Baltimore, Maryland, 1966.

Frisch, P. R., *What Little I Remember*, Cambridge University Press, New York, 1979.

Hall, W. A., Biological hydrological systems, Paper presented at proceedings of the 3rd International Seminar for Hydrology, Professors, Agric. Exp. Stat. and NSF, Purdue Univ., West Lafayette, Ind., 1971.

Hašek, J., *The Good Soldier Švejk*, Penguin, Middlesex, England, 1974.

Hofstadter, D. R., *Gödel, Escher, Bach: An Eternal Golden Braid*, Basic Books, New York, 1979.

Klemeš, V., Negatively skewed distribution of runoff, Proceedings, Symposium of Wellington, N. Z., *Publ. 96*, pp. 219–236, IASH-Unesco, Paris, 1970.

Klemeš, V., The Hurst phenomenon—A Puzzle?, *Water Resour. Res.*, *10*(4), 675–688, 1974.

Klemeš, V., Storage mass-curve analysis in a systems-analytic perspective, *Water Resour. Res.*, *15*(2), 359–370, 1979.

Klemeš, V., Empirical and causal models in hydrology, in *Scientific Basis of Water Resource Management*, pp. 95–104, National Academy Press, Washington, D. C., 1982*a*.

Klemeš, V., The desirable degree of rigour in the testing of rainfall-runoff models, *Eos Trans. AGU*, *63*(45), 922, 1982*b*.

Klemeš, V., Conceptualization and scale in hydrology, *J. Hydrol.*, *65*, 1–23, 1983.

Klemeš, V., Operational testing of hydrologic simulation models, *Hydrol. Sci. J.*, *31*(1), 13–24, 1986*a*.

Klemeŝ, V., Hydrological and engineering relevance of flood frequency analysis, paper presented at the Symposium on Flood Frequency and Risk Analyses, Dep. of Civ. Eng., La. State Univ., Baton Rouge, 1986*b*.

Linsley, R. K., M. A. Kohler, and J. L. H. Paulhus, *Applied Hydrology*, McGraw-Hill, New York, 1949.

Linsley, R. K., M. A. Kohler, and J. L. H. Paulhus, *Hydrology for Engineers*, McGraw-Hill, New York, 1975.

Medawar, P. B., *Induction and Intuition in Scientific Thought*, American Philosophical Society, Philadelphia, Pa., 1969.

Needham, A. E., Growth (Biology), in *Encyclopaedic Dictionary of Physics*, edited by J. Thewlis, vol. 3, p. 544, Pergamon, New York, 1962.

Parkinson, C. N., *The Law and the Profits*, John Murray, London, 1960.

Slack, J. R., J. R. Wallis, and N. C. Matalas, On the value of information to flood frequency analysis, *Water Resour. Res.*, *11*(5), 629–647, 1975.

Sykes, J. B. (Ed.), *The Pocket Oxford Dictionary of Current English*, 6th ed., Oxford University Press, New York, 1978.

Thompson, D. W., *On Growth and Form*, Macmillan, New York, 1942.

Welles, J. F., The survival advantage of stupidity, *Speculations Sci. Technol.*, *7*(1), 17–21, 1984.

World Meteorological Organization, Intercomparison of conceptual

models used in operational hydrological forecasting, *Oper. Hydrol. Rep. 7*, World Meteorol. Org., Geneva, 1975.
World Meteorological Organization, Intercomparison of models of snowmelt runoff, *Oper. Hydrol. Rep. 23*, World Meteorol. Org., Geneva, 1985.
Yevjevich, V., Misconceptions in hydrology and their consequences, *Water Resour. Res., 4*(2), 225–232, 1968.

V. Klemeš, National Hydrology Research Institute, Environment Canada, Ottawa, Ontario, Canada K1A 0E7.

(Received March 4, 1985;
revised September 24, 1985;
accepted September 27, 1985.)