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STATISTICS AND PROBABILITY: WRONG REMEDIES FOR A CONFUSED HYDROLOGIC MODELLER

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INTRODUCTION

Confusion seems to be the main asset that one acquires from the run-of-the-mill graduate education in hydrology. One embarks on it equipped, in most cases, with an undergraduate degree in engineering (with a very few exceptions, one can't get an undergraduate degree in hydrology) and, after being taught (often by amateurs) rudiments of linear algebra, mathematical statistics, probability theory, systems theory and computer programming, is academically certified as an expert in hydrology - a geophysical science. The common result is a permanent or, at any rate, deeply planted inability to see the difference between technology, science and mathematics and mixing them in ways ranging from amusing to dangerous. To quote myself (Klemeš, 1988a),

"Suspended between a technology he does not practice and a science for which he has not been trained, his 'research' is naturally guided to performing elaborate pirouettes on the high wire of [mathematical] techniques connecting the two distant poles and holding him in place."

Having myself suffered such education and remembering the lessons it inadvertently taught me about its inherent dangers to both the practice and research in hydrology (not dissimilar to lessons which twenty years of life under a communist regime has taught me about dangers of social engineering), I have tried, alongside a few other crusaders (e.g., Philip, 1975, 1991, Nash et al., 1990; Bras and Eagleson, 1987), to help correct the situation by repeatedly exposing these dangers over the past twenty years (Klemeš, 1971, 1974, 1978, 1982, 1986, 1987a,b, 1988a,b, 1989, 1991, 1993). Although some recent developments indicate that not all the efforts were in vain, the old attitudes to hydrology are still holding fast in the academe and elsewhere and keep polluting hydrological waters.

It is, by the way, no coincidence that relatively few of the hydrologists of note were university professors but rather practicing professionals working for hydrological and water-resource services, agencies and research facilities (e.g., to name a few of the past generation, Hazen, Horton, Theis, Langbein, in the USA; Hurst in the UK; Sokolovskiy, Kritskiy and Menkel in Russia). In the academe, hydrology used to be (and, as a rule, still is) only an appendix to some other discipline and professors taught it as a sideline. Of late, this has been changing, though not always in desirable ways. While hydrology now may nominally be the main line of a professor, it need not be hydrology but merely "blackboard hydrology" (J.E. Nash, personal communication) what he is teaching. In view of the first paragraph, this of course should be no surprise.

The confusion is best evident in hydrologic modelling. In essence, most hydrologic models are developed as tools for aiding decisions in various water related technologies. However, since their creators consider themselves scientists, they often confuse these models with scientific models and expect of them new hydrological insights, forgetting that they had already prescribed what they hope to learn. The other confusion reflects the aforementioned way in which hydrologic scientists have been metamorphosed out of technologists. Since this has been achieved by teaching them mostly mathematical techniques, they tend to equate scientific substance with mathematical skill; hence a thicker layer of mathematical rigour and polish is thought to translate into a superior hydrologic validity of a model.

This is true in particular in the area of statistical hydrologic modelling where the fallacy is less easy to expose than it is in its deterministic counterpart. It is, for instance, relatively simple to find out how much better a flood forecast in a given river section is when, based on streamflows 50 km upstream, it is calculated by a complex or a simple hydraulic model of the river channel. On the other hand, it is virtually impossible to tell whether replacing biased parameter estimates with unbiased ones in a distribution model based on a 20-year record, improves the 100-year flood estimate or makes it worse. The point is that a 20-year long flood record does not contain the information necessary to make the judgement: Practically everything about the model is a guess and the whole modelling exercise is not science but merely a hydrologically motivated and statistically executed rationale masquerading as science on the pretext of the (spurious) mathematics of its formulation.

It is poor consolation that hydrology is not the only science where attempts are frequently made to create knowledge about nature by misguided manipulation of mathematical formulae. Professor George E.P. Box called such use of mathematics mathematistry in his R. A. Fisher Memorial Lecture of 1974 (written version: Box, 1976) where he lamented as follows: "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."

I would only add, and hope to be able to demonstrate in the following sections, that many of these procedures may have been quite appropriate for the purposes for which they originally had been developed but, to paraphrase Professor Box, overawed by what they do not understand, many investigators, and I sadly say, hydrologists among them, freely abuse them while being convinced that they are advancing science.

It is surprising how wide spread has been the following paradox: when an investigator is at the end of his wits in understanding the particular segment of reality on which he is an expert, he turns for help to formalisms of mathematics - an area most removed from reality and explicitly disclaiming any relation to it and, moreover, one in which he is not an expert!

It seems to me that statisticians, probability theorists and mathematicians have been much more aware of the futility of such approach than have been practitioners in the natural, engineering and social sciences. Let me quote some examples:

"Derivations based upon postulates which have no concrete physical interpretation are of little use to the scientist" (Fry, 1928).

"In economic and social sciences ... any statistical analysis must be closely knit with as full a theoretical specifications as possible" (Bartlet in 1953 as quoted in Bartlett, 1962).

"... unless the statistician has a well defined and realistic model of the actual process he is studying, his analysis is likely to be abortive" (Bartlett in 1954 as quoted in Bartlet, 1962).

"The ultimate object of analysis of a time series - as of statistical analysis as a whole - is to arrive at a deeper understanding of the causal mechanisms which generated it" (Kendall and Stuart, 1966).

"... the modern apparatus of the theory of small samples [as] a method for positive statistical inference ... does not inspire one with confidence, unless it is applied by a statistician by whom the main elements of the dynamics of the situation are either explicitly known or implicitly felt" (Norbert Wiener as quoted in Bartlett, 1962).

"... the form of the [flood peak] distribution is not known and any distribution used must be guessed ... since the part of the distribution we are interested in is well away from the part where observations provide some information ... [This presents a difficulty that] cannot be overcome by mathematical sleight of hand" (Moran, 1957).

"I am totally scornful of the idea that one can understand by pure thought, whatever that is, and to get some understanding of processes, you should open your mind to real situations, not the stupid hypothetical situations one finds in many papers" (Kempthorne, 1971).

I appreciate the opportunity to summarize my views on this and related issues as they pertain to hydrology here, at the Rothamsted Statistics Department, whose founder, the illustrious statistician Sir Ronald Fisher, felt very strongly about a solid scientific foundation of every statistical analysis and held in contempt *sterile mathematical sleight of hand.*

ANALYTICAL VERSUS SYNTHETIC MODELS

In my opinion (Klemeš, 1986), it is because of the fact that the basic training of hydrologists is in other disciplines for which hydrology is just one of many tools needed for their own purposes, that the vast majority of hydrologic models are, consciously or subconsciously, user-oriented. They are not meant to help hydrology but to make hydrology help someone else. Implicitly, and often explicitly, their makers take the attitude that the hydrology underlying the model is already known and, if it is not, too bad for hydrology because it should have been - apparently, somebody has not done his job and it is not their business to do it for him: their business is to build a model! All they can afford to do in such circumstances is to plug the hydrologic hole, come up with some quick fix and go ahead with the modelling - because the user is waiting!

This user attitude tends to persist in their thinking even long after the modellers have considered themselves genuine hydrologists-scientists. After all, such a trait is by no means unique to hydrologists. It is well known, for example, that, after long exposure, hostages often adopt their kidnappers' attitudes. I was vividly reminded of this phenomenon during my recent visit in the Czech Republic when an old friend of mine told me: By far the greatest obstacle on our way to democracy is the fact that even the people who have always opposed the communist regime have assimilated its attitudes and ways of thinking.

Until about forty years ago, the situation was clear since the species of "hydrologic modeller" did not yet exist and, when some representation of hydrologic information was needed, convenient ways of doing it - models - were developed by those in need: engineers, water resource managers, foresters, planners, agronomists, etc.

The muddling of the waters started in the 1950s when, well before hydrology had a chance to establish itself as a scientific discipline in its own right (it still is far from it today!), the type of training of hydrologic specialists mentioned in the introductory paragraph was introduced. The muddle was made thicker by the fact that this happened simultaneously with the "discovery" of mathematical statistics and probability theory by the applied sciences on the one hand and with the advent of the computer and the "random number generator" on the other.

This historical collusion created, almost overnight, a new discipline - "stochastic hydrology" - by infusing old ingenious techniques of Allen Hazen and Charles Sudler (Klemeš, 1981) with "mathematical rigour" and computer efficiency. Only a few individuals clearly saw the danger and sounded early warnings. Among them was one of the originators of stochastic hydrology, the late Myron Fiering, who introduced the term operational hydrology 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 than ranked to aid and abet in the exercise of a decision" (Fiering, 1966).

Similarly devoid of hydrologic content has become "statistical hydrology", especially its all-important branch of distribution modelling and the jewel in its crown known as Flood Frequency Analysis (FFA). This I discussed in detail elsewhere (Klemeš, 1971, 1986, 1987b, 1988a, 1989, 1993) and here it suffices to say that their only connection with hydrology is that the numbers to which various distribution models are fitted have hydrological names. Their true nature is of no consequence because absolutely nothing of what is done with these numbers requires any hydrologic information, knowledge or experience. As I once put it (Klemeš, 1971), the data "... are treated as a collection of abstract numbers that could pertain to anything or to nothing at all".

It was Jim Wallis

By the way, ten years after I had made this assertion, an opportunity presented itself to test its validity, when a leading American FFA expert asked me for a set of flood peak discharge data to test his new regional FF model and explicitly requested that I give him no other information except the numbers. So I sent him a set of fabricated numbers from which his model duly produced the required regional flood distribution. Since I still have the computer printout with the results in my files, I can tell you in confidence that, for example, the ten-thousand-year flood for the region is 2.5181. While the units are not known (as requested, I supplied numbers only), the magnitude of the flood must be quite accurate because the parameters of the distribution model are given to 8 decimal places. To the credit of the investigator, I must say that he detected some peculiarities in my data, namely that some of the computed model parameters pointed to a small homogenous region, others to a large heterogenous region. I assured my friend he was right on both counts: the small region was my desk, the large region my imagination.

There is of course nothing wrong with employing various computational schemes for processing data for specific purposes, even if the schemes are in some sense obviously wrong, provided that the purpose is well served and the result is not unduly affected by the wrong aspect of the scheme. A typical case involves the use of the normal distribution on which Box (1976) commented as follows:"... the statistician knows, for example, that in nature there newer was a normal distribution, there never was a straight line, yet with normal and linear assumptions, known to be false, he can often derive results which match, to a useful approximation, those found in the real world".

This is often true in water management decisions where distributions of hydrologic variables like monthly flows, annual flows or extreme flows are routinely used as weighting functions for some associated benefits or costs. Given the fact that, on the one hand, economic criteria usually employ expected values and quadratic loss functions and, on the other hand, the basic rule of distribution fitting is to preserve the mean and the variance of the data, a satisfactory performance of the normal distribution (or other simple two-parameter models) is not surprising (Klemeš, 1977). An illustration of this was provided by Slack et al.(1975) who analyzed the effect on optimum design of the distribution model used for the representation of the distribution of floods. They concluded that "... the use of the normal distribution ... is generally better than either the Gumbel, lognormal, or Weibull distributions. Nothing is gained in terms of reducing expected opportunity design losses if the underlying distribution ... is identified over and above simply using the normal as the assumed distribution".

The important point is that, in such and similar cases, the purpose of the investigation was not to find a scientifically (i. e. hydrologically) correct type of the flood distribution itself but to use a simple approximate representation of its shape that would adequately serve some other purpose. The reason why I decided to play the aforementioned trick on my American friend was not to question the reasonableness and usefulness of his model for some decision-related application, *but because he presented it to me as a hydrological model capable of providing information about probabilities of real floods* even without any relevant hydrologic information. In other words, I just wanted to demonstrate that, as Myron Fiering might have put it, flood parameters generated by a distribution model unrelated to a specific hydrological situation are, by themselves, meaningless. But more on this later.

In short, the difference is one between an investigation where the objective is an insight into hydrology itself and an investigation where a description of hydrology is used only as a stepping stone for insight into some other problem. In the older hydrologic jargon, the former activity used to be labelled **pure** hydrology, while the hydrological component of the latter was, and still often is, referred to as **applied** hydrology (the connotation of the latter term being that, before hydrologic knowledge can be applied, it must be available). Transposing the difference into the sphere of modelling, one might perhaps refer to investigative and descriptive hydrologic models, or analytic and synthetic models, respectively.

It seems obvious that the two types of models require different approaches which cannot be freely interchanged. It is a prerequisite of effective modelling to be clear about which of the two objectives is being pursued. When I started on the crusade aimed at a clarification of this difference, I posed the problem 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." (At the time, I had no idea, that the same point was simultaneously being raised in regard to the practice of statistics by Oscar Kempthorne: "It is critical in *my opinion to make a differentiation between the acquisition of knowledge on the one hand and the making of decisions on the other hand*" [Kempthorne, 1971].)

I further pointed out that, "Logical as this concept seems to be, it is far from being implemented in hydrology in general and in statistical hydrology in particular".

This, to a large extent, is still true today, in part due to the fact that students of hydrology have been led to believe that applied hydrology becomes pure hydrology and, implicitly, a synthetic model becomes analytical, simply by infusion of more mathematics. This is as wrong as it can be; the difference lies elsewhere:

Analytical (investigative, scientific) model asks questions about nature, while synthetic model describes the obtained answers (or, in their absence, modeller's guesses) in a way useful to human endeavours. Analytical model is a component of the so-called scientific method, i. e. of the iterated loop between observation (data), hypothesis (theory, model), new observation, etc. Its intent is to have its result subjected to possible falsification by new data. Falsification is an aim and it is regarded as a success when it occurs because only then something new can be learned, a new, deeper question can be asked. With an analytical model, we are in the "knowledge business", to use Kempthorne's (1971) phrase.

To the contrary, synthetic model is not meant as an element of a learning loop but as a "final report" when all the testing has been done and the modelling result verified to agree with reality. At this stage, its falsification would not be a success but a disaster! One does not want to test the adequacy of a 10,000-year flood estimate by the collapse of a dam built to withstand such a flood! We are not in the knowledge business any more, we are out of school, in the real world, in the "survival business", and we want to be as sure as possible that our model can be relied upon.

With this background, it is easy to find out whether a particular hydrologic modeller is a pure hydrologist (scientist) or an applied hydrologist (technologist) - just ask him what would please him more: if his model were falsified or verified?

It is now also easy to see why stochastic and statistical models are so popular with applied hydrologists - they are, as a rule, practically unfalsifiable. For when and how can anybody prove by observations additional to, say, a 30-year historic record employed, that an estimate of a 1,000-year flood, or a 3% probability of a reservoir running dry, was significantly wrong even if the reservoir ran dry in each of the very first five years after its completion and collapsed in a flood in the sixth year?

Does this then mean that the traditional statistical and stochastic hydrologic models are not scientific in the true (popperian) sense? I am convinced of it and have tried to convince fellow hydrologists for almost quarter of a century that such models are merely expedient rationales (Yevjevich, 1968) and don't have the carrying capacity for all the mathematical rigour with which they have been invested. The success of my efforts can be judged by the exponential growth in both the number and the rigour of unfalsifiable stochastic and statistical hydrologic models over the past two decades.

THE FALLACY OF "THEORETICAL MODELS" IN STATISTICAL AND STOCHASTIC HYDROLOGY

A good analogy for much research in statistical and stochastic hydrology is the anecdote about the drunk searching, at night, for his lost keys under a lamp post, not because he believes he had lost them there but because it is the only place where he can see anything. But there is a twist: The stochastic hydrologist finds the exploration of this well-lighted foreign place so fascinating that he soon forgets about his keys and his house and makes his camp right there, under the lamp post. The story about lost keys becomes just a habitual excuse to avoid looking silly, a façade of false dignity for the irrelevance of his research. It is this phenomenon that gives rise to mathematistry which, as Box (1976) pointed out, redefines a problem rather than solving it. Such redefinition may be "justified" by the most tenuous similarities and in the face of glaring dissimilarities between the two problems, as long as the new problem can be treated by a rigorous mathematical theory. Because, where there is a mathematical theory behind a solution, the solution is deemed scientific and therefore correct.

A strong incentive for backing a solution by a rigorous mathematical theory that admits no challenge and falsification comes from the explicitly or implicitly "applied" nature of most statistical and stochastic hydrologic models and from the tacit assumption that they provide a backbone for crucial decisions which the society cannot afford to let depend on fallible human judgement. We thus see that the tendency towards the replacement of scientific rigour with sterile mathematical rigour goes hand in hand with the tendency to replace (or confuse) analytical models with synthetic models.

An amusing consequence of this confusion is the following: While everybody knows that realistic estimation of probabilities of extreme hydrologic events, namely precipitation, flood flows, lake levels, snowpacks, etc., is not possible even in principle (see the quotation from Professor Moran in the Introduction), scientific papers, textbooks, handbooks and computer algorithms on how to do it rigorously by using "theoretical models" comprise the bulk of the literature on statistical hydrology.

The following examples illustrate this paradox.

A comprehensive review report on statistical models for flood frequency estimation (Cunnane, 1986) covers everything from Bayes theorem to effect of log transformations to a warning by one hydrologic modeller not to use historic information on rare floods because it "may cause a degradation rather than improvement in the estimates" (read: they may render the estimates based on "theoretical models" ridiculous). Among its 140 references, the report does not list the only one which treated the problem on the basis of its dynamics (Eagleson, 1972), nor does it list Moran (1957) who pointed out the fundamental weakness in the whole approach.

When discussing, in his comprehensive book on Frequency and Risk Analysis in Hydrology (Kite,1977), the problem how to assign a probability of exceedance (or an average return period) to, say, the annual maximum discharge in a 10 year observation record, the author gives an honest answer: "We do not know". But he ads: "And yet, for practical reasons, some probability of occurrence must be assigned to this flood". In the introduction, he indicates how this impossible task is to be accomplished: "An assumption must be made of a theoretical frequency distribution for the population of events and the statistical parameters of the distribution must be computed from the sample data". He says that the objective of his book is "to provide sufficient background information to enable an hydrologist to intelligently select a distribution to use in frequency analysis". Characteristically and inevitably, the book contains little hydrologically relevant information (it is difficult to come by) but it is a very good guide to the fitting of some simple distributions to small samples of numbers assumed to have been generated from them.

The rationale behind the theory of flood probability estimation can be summarized as follows: First and foremost, we (hydrologic modellers, applied hydrologists) must get the probabilities of extreme floods because the user (often our employer) needs them. Second, we know it can't be done because the available information is insufficient. Third, we know that a rigorous probabilistic theory exists for an entity called random variable which is defined in the way necessary to make the theory valid. Fourth, we know that real floods do not satisfy the assumptions underlying this theory. Fifth, we adopt this theory regardless because we must get the probabilities at any price (if we don't the user will get them from somebody else). Sixth, to compensate for the small inconsistency involved, we must construct the theoretical models with the highest mathematical rigour and polish their every detail to perfection.

When talking about statistical models, Professor Box (1976) observed, "It is inappropriate to be concerned about mice when there are tigers abroad". However, many statistical hydrologists (meteorologists, climatologists, ...) seem to subscribe to the following maxim: "It is easier to catch mice than tigers".

Two of the many tigers roaming in the dark and easily avoiding the light from the lamp post of flood frequency theory are exhibited in Fig. 1 (for more, see Klemeš, 1986, 1987b). While the numerical and geographical specifics of the example are fictitious, it illustrates two real-life situations. In case A, a substantial part of the basin of river R_1 is controlled by a lake which seldom overflows and the shaded basin area starts contributing to floods recorded in station S only when the lake is full. A similar situation occurs in the Santa Anna River basin in California with about one third of it controlled by Lake Elsinor and known to have contributed to down-stream floods in only a couple of times during the 80-or-so years of record. In case B, when the flood level along river R_1 rises above the elevation of the pass (shaded area), a part of the flood flow is diverted into river R_2. This is similar to the situation in the western border area between Canada and the USA where floods from the US Nooksack River occasionally overflow into the Canadian Sumas River (Klemeš, 1987b).

see photo on the cover of my book

see "regime 4" in enclosed chart.

Suppose that an existing 30-to-40-year record of annual flood peaks in station S is fitted by a "theoretical" distribution model F(Q) as shown and that the modeller has used all the mathematical sophistication available to fit the model. Also assume, as it is often the case, that he has used only the "numbers" and has no knowledge about (or interest in) the hydrologic or other conditions affecting the extreme floods in the basin. Now suppose that a large flood occurs with an estimated peak flow Q = 400 m^3/s. The model will assess it as a 10,000-year flood. However, it may well be only a 100-year flood if the actual conditions resemble case A since the lake may have been rendered ineffective and the proper distribution to use for that flood would be the "no-lake model" A(Q). Or it may well have been a million-year flood if, in addition to the lake, there also were a potential of flood overflow into river R_2 so that the proper model were B(Q). Moreover, given the notorious inaccuracy in the estimated flood peak discharges (they are practically never measured and, even if they were, their accuracy would not be better than about ± 25%), the actual uncertainty in the return period of the flood would be even higher, despite all the rigour invested into the "theoretical" model F(Q).

A most interesting development in the attitude to theoretical models for flood probabilities is indicated in a recent authoritative report on Estimating Probabilities of Extreme Floods produced by a high-level US committee (Com. on Techniques etc.,1988). After explaining the bureaucracy behind the committee's existence, the report continues with two theoretical chapters on Improving the Theoretical Basis and Flood-Based Statistical Techniques. The first of them starts with these words:

"Extreme or rare floods, with probabilities in the range of 10^{-3} to 10^{-7} (more or less) chance of occurrence per year, are of continuing interest to the hydrologic and engineering communities for purposes of design and planning. When compared to the very long return periods of interest, the historical record of such events is small; thus opportunities to test or compare estimated flood quantiles with experienced events almost never occur. Nevertheless, the need to design or plan for the occurrence of extreme floods is real. The committee believes that advances in the probabilistic modelling and statistical analysis of extreme events have been made and that these advances can be applied to improve extreme-event hydrologic analyses so that estimates of the probability of extreme hydrologic events will become possible".

Since the authors recognize that "estimating the probabilities of extreme floods will always require extrapolation well beyond the data set", they have "...identified three principles for improving extreme flood estimation,(1) 'substitution of space for time'; (2) introduction of more 'structure' into the models; and (3) focus on extremes or 'tails'..." In the next chapter, they define modelling thus: "Modelling here refers to distributional assumptions regarding the underlying random variables". With that background the authors get down to business: "Let Y denote the random variable representing the annual peak discharge ...", etc., etc. The reader learns many things about the small sample theory of random variables possessing a few simple distribution forms, about asymptotic unbiasedness, consistency, maximum likelihood estimation whose "asymptotic theory exists and enables one (under regularity conditions) to obtain an approximate standard error of an estimator or...confidence interval", etc., etc., but he learns absolutely nothing about the original problem, namely, how to estimate the probabilities of extreme floods.

However, the following three chapters on runoff modelling, data characteristics and research needs are written in a completely different spirit. In fact, they undermine the credibility of the expounded theories. For example, one reads that "It is particularly important to determine whether there exist physical processes which are critical to very large floods but which do not operate at lower discharges" - as shown in Fig. 1, a most important point, but nowhere reflected in the "theoretical" chapters. Or, "Streamflow and precipitation are complex multidimensional processes. They exhibit strong and complicated patterns of variability in both space and time" - true but disregarded in the underlying "theoretical" assumptions; or "Proper statistical treatment of historical and paleoflood data requires the use of auxiliary information in addition to the flood magnitudes"; or even "Research is needed on the mechanisms by which paleoflood and historical flood records are produced and preserved with an emphasis on constructing statistical models that reflect these mechanisms, as well as consideration of climatological changes that may have occurred between the historical event and the contemporary data record". All this is essentially equivalent to a warning that the theories presented in the first two chapters may well be useless. And, indeed, in the final short chapter on developmental issues and research needs we read: "There is little in this report that can be taken by the practitioner and applied without further development and or research."

In its very last paragraph, the report proclaims: "Many valuable lessons might be learned from study of this work". Indeed! Perhaps the most valuable lesson can be learned from the inconsistency described above. The reasons behind it are hinted at in the committee chairman's introduction: "The history of research and practice in flood probability estimation ... has been marked by sharp disagreements and well-defined schools of thought. All camps were represented on the committee..." Having the privilege to know most of its members, I can readily distinguish two camps, one making itself comfortable under the aforementioned lamp post, the other eager to get a good search light and set out after the tigers hiding in the dark.

TWO COROLLARIES

What's in a name!

The suggestive power of names is well known. Had, for instance, Hamburg rather than Berlin been a divided city, President Kennedy would, no doubt, have thought twice before proudly proclaiming "Ich bin ein Hamburger!"

Ten years ago, in an extremely stimulating televised interview (NOVA, 1983), the world renowned late American physicist, Richard Feynman, told a story how, as a boy, he learned about inertia from his father. When he once asked him why it was that the ball in his toy wagon rolled to the back of the wagon when he pulled the wagon forward and to its front when he stopped it, "... he says nobody knows. He said, 'The general principle is that things that are moving try to keep moving and things that are standing still tend to stand still unless you push on them hard.' And he says this tendency is called inertia, but nobody knows why it's true. Now that's a deep understanding. He doesn't give me a name. He knew the difference between **knowing the name of something and knowing something**, which I learned very early" (emphasis added). I believe that this early knowledge was one of the most important keys to Feynman's later accomplishments as a scientist and a teacher. I also believe that the lack of this knowledge is one of the main roots of confusions in statistical and stochastic hydrologic modelling. This is why I am taking the liberty of elaborating on the following point which may seem trivial.

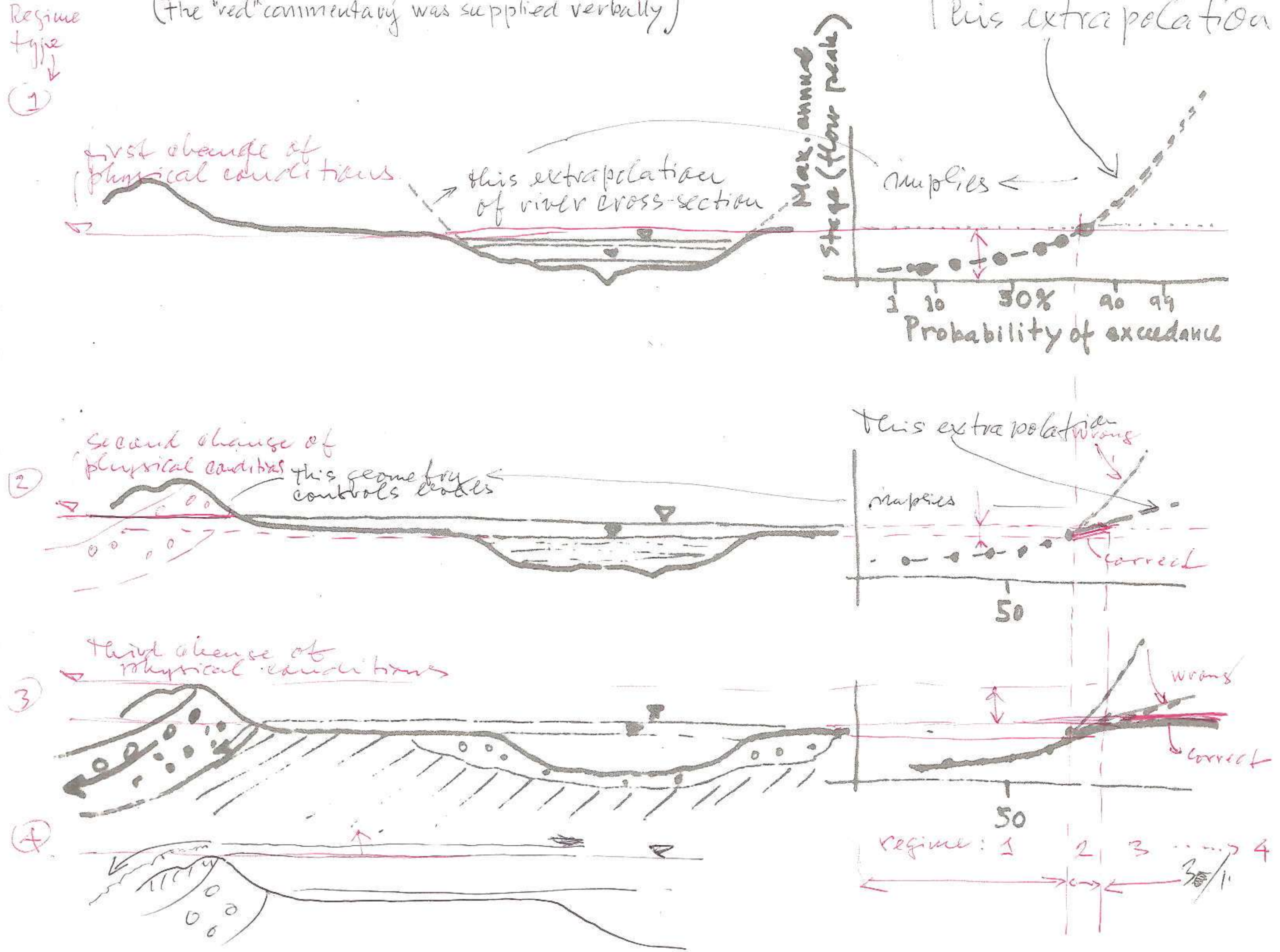
For example, if x is defined as a random variable with a prescribed form of probability distribution, then a mathematical function, p = f(x), describing this form, is routinely called "probability distribution f(x)" - it is just a convenient terminological shortcut for "mathematical expression describing the form of the probability distribution of x". The necessary condition for this to be true is that the entity p whose numerical value the expression f(x) specifies in terms of the value of x is a priori known to be a probability.

The mathematical expression f(x) has nothing intrinsically "probabilistic" about it and its probabilistic name is not derived from its mathematical form. The same expression may describe the form of a perfectly deterministic attribute of some entirely nonrandom variable denoted by x.

A few examples will illustrate how the lack of appreciation of such and similar differences, and the sheer power of names, affect statistical hydrology.

It is well known that the gamma distribution fits reasonably well the histograms of historic frequencies of annual flow totals of many rivers. For now, let us not dwell on the logical jump required to equate these frequencies with probabilities, and let us accept the common wisdom that annual flows of many rivers have gamma probability distribution. It is also well known in hydrology that, if we route a unit inflow through a cascade of linear reservoirs (release from each is a *linear function of its instantaneous water storage), the shape of the downstream hydrograph (so called Unit Hydrograph, i.e. the unit response function of the* cascade) also happens to have the form of gamma distribution in which case the latter has no probabilistic connotation. This mathematical similarity once ruined

(This is from my 30 year old lecture notes: the "red" commentary was supplied verbally)



a good part of an evening for me when a distinguished Australian hydrologist tried to persuade me that the Unit Hydrograph is a probability distribution because the gamma distribution is obviously a probability distribution and the Unit hydrograph is obviously a gamma distribution.

In probability theory, one often reads that something has one or other theoretical distribution, e.g., that the theoretical model of the sum of gamma-distributed random variables is again a gamma-distributed variable. The connotation of the term "theoretical" is that the form of the resultant distribution has been arrived at (from the given original propositions and descriptions) by applying the theory governing the operation in question, in this case the summation of random variables. However, the notion of "theoretical probability distribution model" has been transposed into hydrology to mean any mathematical function that has ever been used in probability theory or statistics to describe a probability distribution. Thus any such function, when fitted to a frequency distribution of floods (or of any hydrological variable), automatically assumes the status of its "theoretical probability distribution" although its selection may be completely arbitrary and have nothing whatsoever to do with any theory related to the process which generated the floods. Such fitted distribution model is then used for the estimation of the "theoretical" values of flood probabilities.

An example of this practice was cited in (Klemeš, 1988a); in that case, "theoretical" values of seasonal level fluctuations in an African equatorial lake, obtained by a fitted Pearson III distribution (which "...has proved to be the distribution of best fit") indicated that, in every year, seasonal flooding of a better part of Uganda by that lake has a small but real probability, while, in terms of the climatic, hydrologic and topographic realities, the "theoretical" values are pure fiction. *Because of their name, extreme value distributions still are among the favourite "theoretical" models for extreme hydrologic events. It was to no avail that the* illustrious late Australian statistician and probabilist, P.A.P. Moran exposed the fallacy already in the late 1950s when he wrote: "A great deal has been written on the subject, most of which is wrong in principle. ... Distributions which may be particularly useful in this connection are the type III and the Logarithmic Normal... These particular distributions are here chosen solely for practical reasons and there is no theoretical reason why they should fit observed series... It has been pretended that better results can be obtained by the use of Gumbel's 'extreme value' distribution. This is the asymptotic form of the distribution of the largest member of a sample from a given distribution. If we knew the latter exactly we could estimate the parameters of Gumbel's distribution. However, Gumbel's distribution depends solely on the form of the tail of f(x) which ... is usually outside the range of observations and can only be guessed at. No amount of mathematical presdigitation can remove this uncertainty. Another defect in this approach is the fact that Gumbel's extreme value distribution is attained very slowly - in the case of the normal distribution only for samples of size at least 10^{12}" (Moran, 1959).

Even the humble arithmetic mean is a frequent victim of the connotation of its name. In a process which does have a central tendency, the mean is its useful measure and can be used as a basis of other process characteristics, e.g., its "expected value", central moments, etc. The problem is that the mean can be easily computed regardless of the existence of any central tendency and it is often implied that a central tendency follows from the fact that the mean has been computed. If an analysis proceeds on this basis and a central tendency does in fact not exist, various unexpected things can happen as I once had an opportunity to demonstrate in connection with the "Hurst phenomenon" (Klemeš, 1974).

The most blatant abusers of the arithmetic mean are of course climatologists and meteorologists who dare to call the often unrealistic meteorological conditions described by long-term means "the normals" and the normal actual conditions "anomalies". Economists and politicians are not much better but potentially far more dangerous by implying that below-average conditions are intrinsically undesirable, a sort of social evil to be eradicated. Below-average harvests have already been accepted as legitimate reasons for subsidies and, given the unmitigated power of the media to create illusions of reality out of ad-nauseam repeated words, "normal" weather conditions and above-average income may soon be added to the list of basic human rights.

What's not in a name!

Another problem which mathematical modellers often don't appreciate is the following: A model, and a mathematical operation in general, even when it is *properly named and valid in the sense that it models what it is supposed to, does not tell the modeller the physical limits of its validity, nor does it tell him* whether a physical mechanism it implies does in fact exist.

This was Yevjevich

*) Suppose that one has a theoretically (i.e., based on a physical theory of flood formation) reasonable model for flood probabilities on the Manitou River on the Manitoulin Island in Lake Huron. Suppose, further, that the model was fitted to a 50-year historic flood record and, after additional 50 years of observations, it is found to agree perfectly with the whole century-long evidence. Can the model be used to make a realistic estimate of a million-year flood peak flow on the river, I once asked an esteemed American stochastic hydrologist. "Of course it can" was his reply. I disagreed on the following grounds: It may well be that under the conditions necessary to produce such a flood, the whole Manitou River, and even the Manitoulin Island itself, would disappear so that no such peak flow could ever arise there (Klemeš, 1989).

Earlier this year, I was confronted with a similar problem in a much more concrete setting. In order to reach high safety standards for its dams, a hydro-power corporation in Canada computes the so-called Probable Maximum Flood (PMF) which they are supposed to withstand, as a flood produced by the combination of a 1,000-year snowpack and a 1,000-year air temperature sequence; both these values are obtained by extrapolation of simple distribution models (e. g., cube-root normal for the snowpack) fitted to short historic records (20 to 50 years). Among my reservations to this approach were the following: First, in northern Canada, when the snowpack reaches certain thickness, it may not melt during one summer season and may start accumulating, perhaps even initiating glaciation of the area. As a result, the tail of the distribution may be much different from that of the distribution fitting the few independent historic annual accumulations, and it may be necessary to invoke the storage theory to guess its likely form. Second, should such a situation arise, it may conceivably lead to one of two diametrally different outcomes, (1) an almost continuous availability of snow (+ ice) layer identified now as having only a 10^{-3} probability and thus perhaps a 100-fold increase of the probability of the computed PMF and a proportional decrease of dam safety, (2) more likely, the situation might be a consequence of a climatic cooling, in which case not only the probability of the temperature sequence now used may be practically zero but the river may cease to exist because of glaciation thus making any dam on it 100% safe from flood damage.

However, *even such simple mathematical constructs like the arithmetic mean may imply unrealistic physics and invalidate modelling results. The physical* equivalent of replacing in a model the actual daily precipitation with, say, its monthly mean would be first to store the total monthly precipitation volume somewhere (no computer will ever be able to do that) and, at the end of the month, go back in time and start continuous spraying at the mean rate from day one. This is actually "going on" in most General Circulation Models which produce no flash floods but abound in drizzle (especially in the dry summers on Canadian prairies, I suppose). Needless to say that many hydrologic modellers eagerly use outputs of these models for predicting effects of climate changes on probabilities of floods and other hydrologic phenomena.

Running means, customarily plotted in the middle of the averaging windows, clairvoyantly anticipate future trends of physical processes long before Mother Nature has any idea of them. For example, 5-year running means of precipitation in Fig. 2 (together with similar plots for temperature and discharge not shown here) have been found to provide an "indication of underlying sustained variations in climatic conditions"; they also show that "precipitation appears to have increased marginally before suffering a decline" (Collins, 1984). As can be seen, this underlying increase coincides with the actual (presumably anomalous?) drop in 1968. However, this could be readily remedied by taking, for instance, 10-year running means which I have plotted as a dotted line. Their other advantage would be that the phase of the "sustained underlying variations" could be made better to coincide with the actual ones and that the precipitation increase at the end of the period (caused by a sustained underlying variation?) could be anticipated almost three years before the trend reveals itself in the 5-year means and, remarkably, while the actual precipitation is still drastically (but anomalously?) declining.

scaling 2

*) For another example see enclosed chart

= fluctuations on different time series

The last example that I will present here concerns the running integral of deviations from sample mean ("residual mass curve"), a favourite tool for analysis of hydrological and climatic records and time series in general. Indeed, many "significant" periodicities and climate changes have been discovered with its help. Thus, Williams (1961) reached the following conclusion by analyzing records between 60 and 70 years long: "If cumulative deviations from the mean are computed for hydrologic data, continuous periods of 10 years to 35 years or more will be revealed in which hydrologic records are consistently below or above their means". One problem is that, as Feller (1966) showed, such cumulative series exhibit long swings even when the underlying process is completely random and the length of the swings grows with the length of the series. A related problem is that residual mass curves of higher orders of virtually any series converge to a single sine wave extending over the whole sample, whatever its size may be (Klemeš and Klemeš, 1988). This convergence, caused by amplification of the first Fourier frequency by successive integrations, is very rapid and robust, and is virtually completed in the fourth order (Fig. 3).

However, the point that I want to emphasize in the present context is that there is a considerable difference in who is doing the integrating, the analyst on his computer or Mother Nature in the field. Suppose that precipitation forms a stationary random series x with a mean $\bar{x}$. The cumulative series $y_t = \int_0^t (x - \bar{x})\,dt$ may well exhibit suggestive quasi-cyclic patterns but they have no hydrological or other physical significance since the storing of the differences from the mean has been done only symbolically, with numbers in a computer or on paper as shown in Fig. 3.

Now suppose that the same precipitation falls onto a large lake into which it is the only water input and that the only output is evaporation which is relatively constant over time and approximately equal to $\bar{x}$. Then the series of water level fluctuations in the lake will have essentially the same shape as the above series y_t but now the series is a record of a real physical process and its longer and shorter epochs of higher and lower levels will represent epochs with relatively uniform, but from epoch to epoch genuinely different, environment (or "climate") for the fauna and flora within different elevation bands of the lake shore. But it is important to appreciate that such a real "micro climate" cyclicity (inferred, say, from lake varves) may have no macro climatic significance. Hence, to use such varve record as proxy data for precipitation may be misleading.

One can, of course, easily compute a second order residual mass curve of the x series as $z_t = \int_0^t (y - \bar{y})\,dt$ which will have an even more pronounced sinusoidal shape but will have no physical significance. On the other hand, nature may produce a series with practically the same shape by, say, allowing for a small fraction of the lake output to take place as seepage proportional to the water stage, accumulate it in another lake downstream (or in an aquifer) which again may have a constant output equal to the mean input. Conceivably, processes of still higher cumulative orders may well arise which will exhibit real sinusoidal periods over some time interval N during which the output of a particular system is approximately equal to the mean (over N) of the input - and all this may be embedded in a perfectly random controlling environment as in Fig. 3.

As a result, it is not possible to judge the significance of cyclic patterns in a computed residual mass curve of a natural process without knowing the process' physical structure - to what extent it itself is a cumulative process and, if so, of what order. For example, a hydrologist may refrain from using a residual mass curve of lake levels because of their cumulative nature. On the other hand, he seldom hesitates to use it for streamflow records which are regarded as simple stochastic series. However, streamflow may well be a cumulative process due to the aforementioned mechanisms (St. Lawrence River, draining the Great Lakes, is a good example) and then the significance of the mass curve cyclicity may be grossly over rated. Even more important in the present era obsessed with the detection of climatic changes, is the possibility that some paleohydrologic and paleoclimatic reconstructions and modelling based on proxy data may be flawed.

HYDROLOGIC MODELLING AS "KNOWLEDGE BUSINESS"

Hydrology and statistics were not among the sciences that motivated me when I was a student. I wanted to be an engineer and design dams. There was no contradiction because no statistics course was required in my engineering curriculum and hydrology was a marginal affair from which I remember three things: the Thiessen Polygon, the Rational Formula and condoms; the latter, stretched over a hollow metal cylinder and weighted down in the middle, were used by a graduate student to model the shape of the drawdown cone around a well (he soon had to switch to electrical analogy because his professor found it difficult to get research money for condoms).

On the job, I soon realized that the most important thing in the design of a dam is to correctly specify the storage capacity of the reservoir: that determines the size of the dam which controls its cost. With great enthusiasm, I plunged into the theory of storage only to find out that, unfortunately, its methods rested on probability theory and its inputs on statistical hydrology. The latter was always considered known ("Given a gamma-distributed random input ...", etc., etc.). However, since it was not known to me, I set out to find out.

Alas, there was little to be found out! There was virtually no "accumulated knowledge" about statistical and stochastic behaviour of hydrological phenomena, no theory. There was only accumulated knowledge about techniques: how to calculate the moments, the correlation coefficient, fit a "probability" distribution, etc.

Now I know that this was not unique to statistical hydrology. I was struck when, about ten years after it was published, I came across Oscar Kempthorne's paper on "Probability, Statistics and the Knowledge Business" (Kempthorne, 1971) where he claimed that such attitudes were in fact imported into the sciences by statisticians, that this was how many statisticians saw their own work! To quote: "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".

Wherever the responsibility may have been, this certainly was the situation in statistical hydrology in the early 1960s when I turned to it to find answers to questions like why should streamflows follow one or other distribution, be random or serially correlated, etc. There were no answers. Worse, there were not even such questions! The Big Names, whose books I was reading and some of whom I later had a chance to consult in person, often didn't even seem to understand the problem I was raising. For example, when I once asked the then dean of American hydrology what could be the reason that the distributions of some annual streamflow series were negatively skewed, his answer was: "When it is so, you may be able to fit it well with some positively skewed model if you flip it over". Later, when I proposed an answer to my question at a symposium (Klemeš, 1970), a leading Australian hydrologist commented: "If you took the square of the skew coefficient you would get rid of your problem". They didn't seem to understand that my problem was not how to fit something or how to get rid of something that doesn't fit, but to get a scientific insight into something. This was Ven Te Chow

In short, statistical hydrology as a knowledge business was not yet defined. From those days, I remember only two instances fitting into this category. One was Kalinin's (1962) attempt to trace the tendency to gamma distribution in annual runoff to the binomial-like alternation of wet and dry periods within the year; the other was Yevjevich's (1963) model explaining the origin of serial correlation in time series of annual runoff. I tried to formulate the concept of statistical hydrology as a knowledge business in the late 1970s when I called it physically based stochastic hydrologic analysis (Klemeš, 1978); by that time I could already cite about two dozen references dealing with its different specific aspects.

Nowadays, the problem is no more the recognition that hydrology, including its statistical and stochastic aspects, is a physical science rather than a mere input to water management decisions (e.g.,Com. on Opportunities, etc., 1991). The problem is the continuing obfuscation of the difference and evasion of doing hydrologic science by citing difficulties in securing the funding (no, condoms don't appear in hydrology research proposals any more; on reflection, nowadays this could be helpful for the same reasons it wasn't forty years ago). The tendencies to "push the hydrology ball to the end of the forward moving wagon of science" reflect the inertia of the historic professional and educational structures within which hydrology as a whole has been developing and within which it still operates. It still is typical that the academic home of hydrology is in Engineering rather than Science departments of universities and the bulk of hydrological work is carried out by organizations whose chief purpose (or "mandate") is some kind of Resource Management.

For example, in the Canadian Federal Government, the National Hydrology Research Institute, which now operates within the Conservation and Protection (formerly Environmental Management) Service of the Environment Department, gradually evolved from a water resource engineering unit attached, in sequential order over the past forty years or so, to the following Departments (R.H. Clark, personal communication): Resources and Development; Northern Affairs and National Resources; Mines and Technical Surveys; Energy, Mines and Resources; Environment and Fisheries; Environment. The word "hydrology" first appeared in its designation in 1962 when a Hydrology Branch was formed by my friend and former colleague, R. H. Clark. Ironically, after his departure, and several years after the word "research" had been proudly added to its name, the role of the National Hydrology Research Institute was officially defined in 1986 as developing, applying and advising on the technology required by clients managing Canadian waters. And, as late as two years ago, an official report of the Canadian Associate Committee on Hydrology, entitled Canadian Hydrological Science (Canad. ACH, 1991), contains these three laconic sentences buried *in its thirteen pages of heroic prose on scientific challenges, deepening of knowledge about basic processes, favourable political and social climate, etc.*: "Hydrological research expertise is by and large focused on applied, not fundamental science"; "The research priorities of most Canadian hydrological establishments address technology"; and "Should funding become available, scientists currently conducting research in applied hydrology, water management and allied sciences would likely be enticed to shift into basic research".

So it goes (as the narrator in Kurt Vonnegut's [1969]novel Slaughterhouse-Five comments when something sad but apparently inevitable happens).

CONCLUSIONS

In a nutshell, my thesis has been that, if a hydrologic modeller is confused about the difference between mathematics, physical science and decisions, statistics and probability theory won't make his models better and, quite possibly, can make them worse, even if the techniques he may be employing are formally correct and rigorous. The obvious remedy is first to clear up the confusions some of which were discussed in the preceding sections and only then turn to the two disciplines. The essentials of the cleanup process may be summarized thus:

To learn the difference (1) between something and the name of something, in particular between a physical process and its mathematical model, (2) between science and technology, in particular between hydrology and water management decisions, (3) between the aims of analytical (investigative) and synthetic (descriptive) models, (4) between a scientific statement (falsifiable theory) and a rationale (unfalsifiable theory).

To appreciate that (1) what is a scientific statement in one context may be only a rationale in another, (2) mathematical formulation does not transform a rationale into a scientific statement, (3) rationale is useless as a tool for advancing knowledge, (4) rationale may be useful, even inevitable, in a decision context, (5) a mathematical formula is not responsible for the validity of its application.

It would also be helpful if prospective hydrologic modellers were taught hydrology, as well as statistics and probability theory, by professors who had already mastered what was said in the two preceding paragraphs.

Acknowledgement. This essay is dedicated to the memory of the late Professor P.A.P. Moran, a wise statistician, kind man and a sadly missed friend.

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Figure Captions

Fig. 1. Example of a possible influence on the flood-distribution tail of different physical conditions not effective during smaller floods on which records in station S are available: A - small floods generated by the shaded part of the basin are detained in the lake; large floods fill it up and render it ineffective. B - after flood level exceeds the elevation of the pass (shaded area), part of flood is diverted into river R_2.

Fig. 2. Annual precipitation totals at Sion, Switzerland, and their 5- and 10-year running means (annual data and 5-year means after Collins, 1984).

Fig. 3. Residual mass curves of higher orders for random series of different lengths generated from a log-normal distribution (Klemeš and Klemeš, 1988).

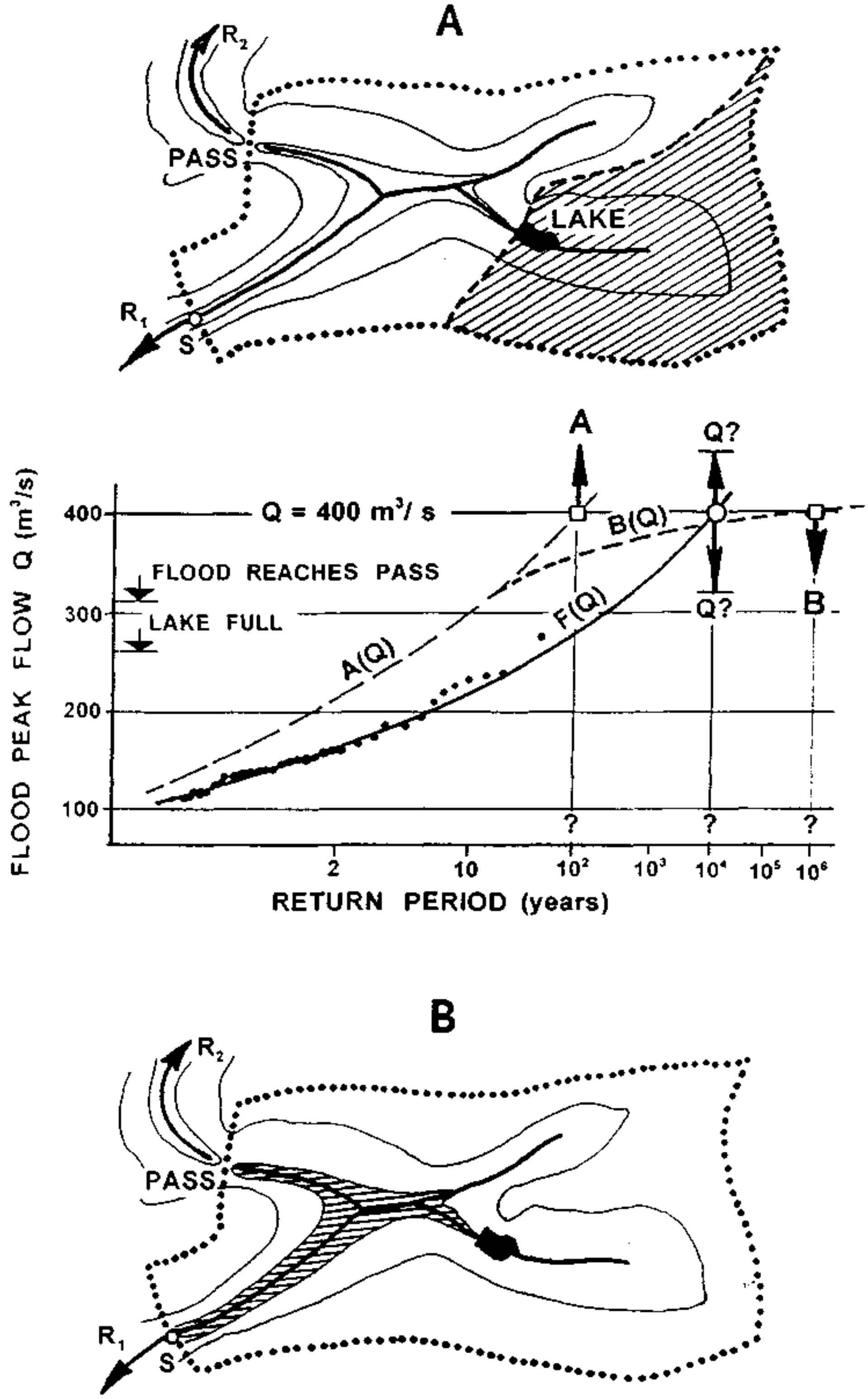
A
R_2
PASS
LAKE
R_1
S
A
Q?
Q = 400 m³/ s
B(Q)
FLOOD REACHES PASS
LAKE FULL
F(Q)
A(Q)
Q?
B
400
300
200
100
?
?
?
2
10
10^2
10^3
10^4
10^5
10^6
FLOOD PEAK FLOW Q (m³/s)
RETURN PERIOD (years)
B
R_2
PASS
R_1
S

FIG. 1

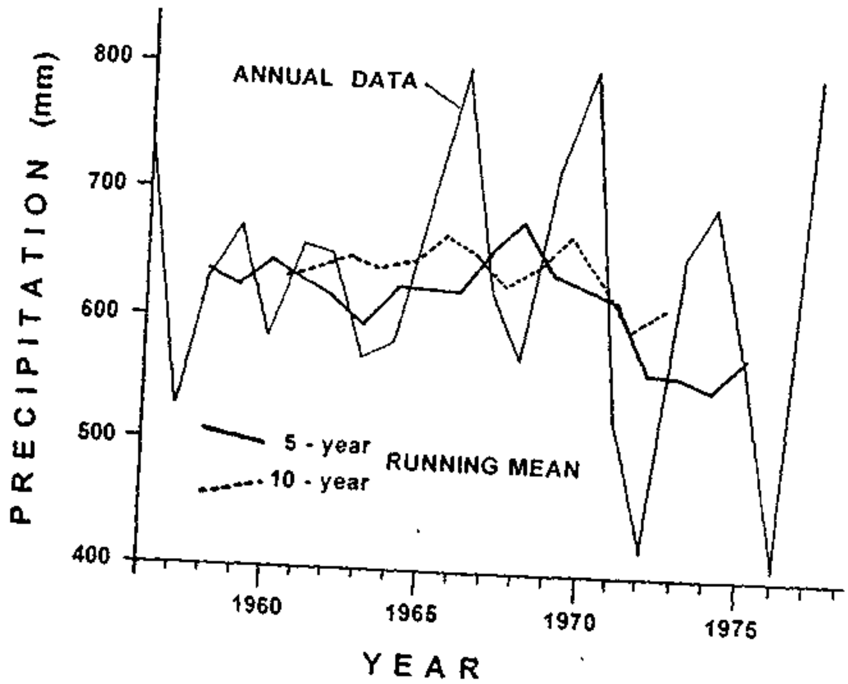
800
700
600
500
400
PRECIPITATION (mm)
ANNUAL DATA
5 - year
10 - year
RUNNING MEAN
1960
1965
1970
1975
YEAR

FIG. 2

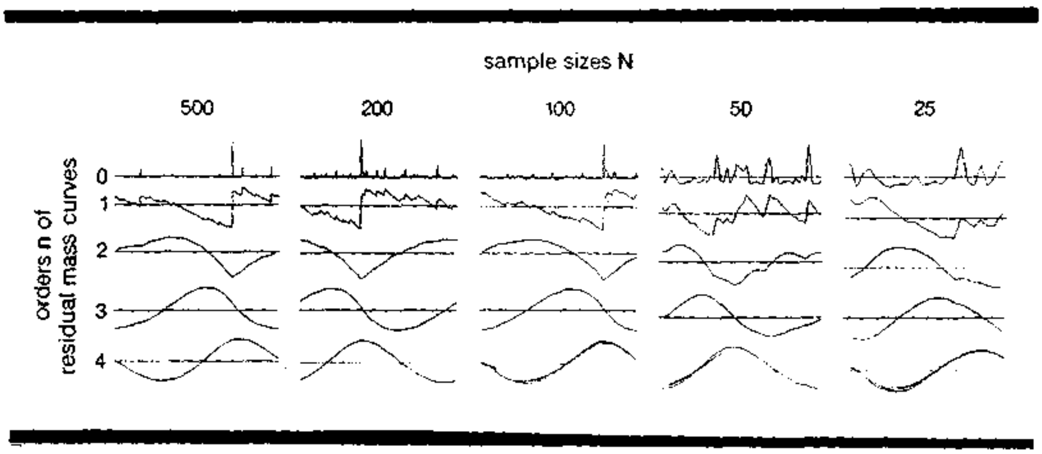
sample sizes N
500
200
100
50
25
orders n of residual mass curves
0
1
2
3
4

FIG. 3