Department
of Hydrology
and Water
Resources

Chester C. Kisiel

Twelfth • Memorial • Lecture

Water Storage: Source of Inspiration and Desperation

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FOREWORD

The central issue around which the activity called "Water Resources" revolves is the discrepancy—real or perceived—between water available as a resource and its use for a set of purposes. Both availability and utilization have the same attributes: quantities at given point in time, location (in three dimensional space), quality (physical, chemical, microbiological), and the institutional-legal framework within which water resources are developed and subsequently utilized. One means by which part of this discrepancy is overcome, especially in its quantity-temporal dimension, is by storing water.

Storage of water was practiced by mankind since the pharaohs in ancient Egypt. The dam Sadd-el-Kafara south of Cairo was built early in the third millennium BCE. Water, however, does not need to be stored exclusively in surface reservoirs. Aquifers, if properly managed, can be effective storage components in a regional water resources system. With proper tillage practices, moisture can be stored in the root zone of soils in sufficient quantities for crop production.

The matter of water storage, especially in surface reservoirs, is intimately connected with flow phenomena. To explain flow and storage, one has to rely on assistance from the science of fluid mechanics. The study of moving fluids presents difficulties, because one has to take into account the shape of the vessel containing the fluid and type of the conduit through which it moves. The complexity of the streamflow increases when considering movement of water in nature, from precipitation on a watershed to the runoff measured at its lowest point. The shape of the watershed, its geology and soils, the vegetative cover and various human interventions contribute to situations when water storage is either a blessed inspiration for socio-economic development, or a source of desperation produced by insufficient information and erroneous analysis.

Nathan Buras

WATER STORAGE: SOURCE OF INSPIRATION AND DESPERATION

VÍT KLEMEŠ

Expecting the Unexpected

When contemplating the topic for this lecture, I had before me the image of Chester Kisiel as I remember him when we last met - it was in 1972 - and as I will always remember him: listening attentively (I was telling him about my investigations of the Hurst phenomenon, a hot topic in those days), with a hint of a smile in his face, a co-conspirator's twinkle in his eye and a notebook (or was it a card?) in his hand, on a lookout for some interesting detail, idea worth noting, some subtle twist that might arouse his curiosity. In short, his expression conveyed a guarded but eager expectation of some unexpected intellectual stimulus. When it came, Chester's response was, as always, the same: "Have you written it up? Please, send me a copy". At the time I could not yet oblige; and when, two years later, I was ready[1], Chester was no longer with us. It is from such and similar memories that the topic for this memorial lecture has crystallized.

What I propose to talk about is not so much water storage but rather the UNEXPECTED which plays such a crucial role in our lives, puts excitement and frustration into our work and is responsible for the tangled pattern of the process known as scientific progress. Water storage enters into the picture only by default: most of my work has revolved around water storage, so it should not be unexpected that the specifics I have chosen to illustrate the substance of the UNEXPECTED revolve around it as well. And, borrowing Nick Matalas' observation made in a similar context in the first lecture of this series, I hope that this "substance, more so than the specifics,... would have been of interest to Chester" and perhaps also to you, especially if you have been attracted by the inscrutable manifestations of CHANCE.

Mind you, when I say chance, I really do mean CHANCE, i.e. the UNEXPECTED: the unforseen good luck and bad luck; the most obvious things missed and the unintended discoveries made; misrepresentations that have survived a century; wheels, even broken wheels, reinvented ... What I don't mean is statistics and probability theory with which chance is most commonly associated. After all, they cover only the most trivial aspects of it, namely those that **can** be expected - just think how these disciplines cling on to concepts like expected value, limit theorems, asymptotic laws, probable errors, maximum likelihood, stationarity, normality, linearity, etc. and how they avoid like a plague anything unexpected, improbable, unlikely, unpredictable, nonnormal, nonlinear, and the like! It is true that, as Nick Matalas remembered here eleven years ago, Chester Kisiel was attracted by these disciplines; but the irony of their (unavoidable) preoccupation with the EXPECTED did not escape him - he mocked it in one of his papers[2] in a prayer of "the theoretical hydrologist":

Oh, Lord, please, keep the world linear and Gaussian!

Remembering the perspicacious twinkle in his eye, I suspect probability and statistics might not have satiated Chester for long and he might have moved on to some more subtle aspects of the UNEXPECTED. I saw a hint of this in his intent to write a paper with Allan Freeze on quotations from famous individuals, as Allan revealed in the sixth lecture of this series. After all, such quotations are notable for the very reason that they typically contain some unexpected observation or idea.

How the World was Saved from Klemeš Storage Models

With some reluctance, I must admit that most of the credit for this must go to the late Leonid Brezhnev. For prior to his 1968 "fraternal help" to Czechoslovakia, which landed me in Canada, I found it far easier and more enjoyable to make my own discoveries and write about them in Czech than to read, in various poorly mastered foreign languages, about discoveries of others. Brezhnev changed all that. At the University of Toronto, I was expected to teach students the standard methods - the Rippl Diagram, the Puls Routing Method, the Moran Storage Model, etc. - and I used the opportunity to go to the original sources to read and learn about them. And the more I read the more fascinated I became by the gems I discovered, and also exasperated when I saw how often they were lost or misrepresented. Only then I realized what a deplorable practice it was to cite original sources from second-hand accounts, how wide spread this "science by citation" was and how often this cavalier attitude misplaced credit for original contributions and denied it to those who deserved it. I also realized how close I myself had come to perpetrating this reprehensible routine and resolved to make it the first rule of my work to give proper credit wherever it was due (even if it should go to Leonid Brezhnev).

From here, it was just a small step to see that most of my own discoveries were either trivial or redundant (as were, by the way, many of those made by others) and that it might be more prudent to search for the original gems buried in the literature than, by indulging one's own ego, risk possible future embarrassments.

My very first two papers illustrate the danger of embarrassment by triviality of what looked like a bright idea. In the mid 1950s, a colleague of mine, Jaroslav Urban, proposed a new graphical method for flood routing by a storage reservoir in his doctoral thesis[3]. It impressed me very much and, as I was playing with the Urban Method, I got two bright ideas how to simplify it. Both were duly published[4] and I was especially proud of the second one which found its way into some handbooks and textbooks as the Klemeš Method. However, when I set out to write a computer program for it some ten years later, it became obvious that my bright ideas were rather trivial as were, in fact, the differences between most of the graphical techniques: they completely dissolved in any numerical algorithm. The reason is simple since the whole problem boils down to solving, for successive intervals, a linear water balance equation (inflow minus outflow equals change in reservoir storage) with a nonlinear flow-rating curve of reservoir outlets (usually the outflow can be expressed as a power function of storage) - a problem for which every self-respecting 2nd-year engineering student can now write a program during a lunch break. The graphical procedures find the solution by fixing the position of the straight line representing the water balance equation in a given interval such that the outflow also satisfies the rating curve. Since the slope of this straight line is given by the scales of the plot (Fig. 1, right side) all that is needed is to identify its one point - and the choice of this point is the only difference between most of the graphical methods. Fig. 1 shows the points employed by some of the more and less famous of them, namely those of Puls (which is practically identical to the Russian Potapov's and the Swedish Ekdahl's methods), Sorensen, Urban and Klemeš. When I realized this in the late sixties, I expected that somebody would surely burst this bubble soon but, as years passed and nothing happened, I eventually decided to do it myself[5].

I think Chester would have appreciated the following twist of the story which also illustrates embarrassment via redundancy. A colleague from Manchester Institute of Technology once sent me a reprint of a paper describing a new graphical reservoir routing method. He explained that his retired colleague had asked him for some simple routing procedure and he directed him to my method. The gentleman used it and, while working with it, got an ingenious idea how to improve it. The improved method was described in the enclosed reprint[6] and, as I was amused to see, it was identical to the Urban Method which I had

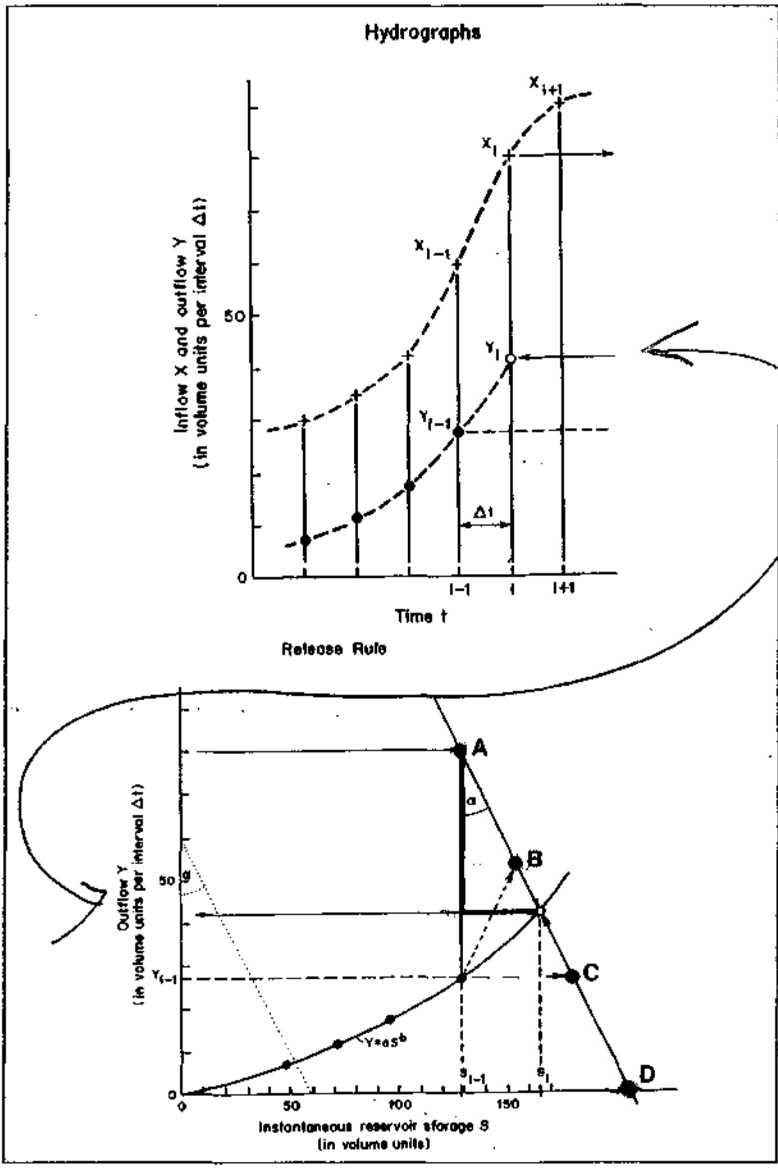

Figure 1. Common principle of reservoir routing methods: To find a position of the water-balance straight-line (whose angle α is given by the chosen volume scales) such that the difference between inflow and outflow in interval Δt is equal to the change in storage and the outflow obeys the release rule function.
One point is needed to fix this position. Examples of points employed: ***A*** - *Klemeš*, ***B*** - *Sorensen*, ***C*** - *Urban*, ***D*** - *Puls*[5].

been so proud to have simplified fifteen years before! When I revealed this to the author, he was amused as well and, in his reply, said it was not the first time that he reinvented the wheel (he did not realize it was a broken wheel in this case)- he had once derived a useful constant which he hoped would become known under his name, only to find out that it was already known in the fluid mechanics literature as the Froude Number.

In contrast, my next discovery was a breakthrough of great practical utility and deservedly is well known the world over. The only minor problem with it is that it is known as the Gould Storage Model - I barely finished the first draft of my paper when I found the whole thing neatly written up by Bernard Gould in an Australian engineering journal[7]. This shows that following the literature is an effective means for preventing embarrassment via redundancy. The unexpected twist came about fifteen years later when I gave a talk at the University of New South Wales. To honour Bernard, I talked about my applications of the Gould Model. After the lecture, Bernard told me in private that it was quite interesting but he would not recognize his model in what I was talking about and, as far as he was concerned, my talk was about a Klemeš Model. And so, for once, I may have missed a chance of not being redundant after all.

Fortunately, by that time my place in history had already been firmly secured by a discovery which earned me my doctorate. This discovery has proved to be completely immune to any danger of being found either trivial or redundant or, for that matter, of its scientific merit being questioned in any other way. It was a probabilistic method for the computation of the sub-annual component[8] to be added to over-year storage and it involved so many convolutions that there probably was, and ever will be, only one other person who has understood it. He included it in his book[9], now over quarter of a century old, but there is no doubt in my mind that neither of us remembers any more how the method really works and, being both retired, we are in little danger of being asked. The most unexpected aspect of this affair has been that this book, which cost less than one dollar when it was current, has recently appeared in an English translation[10] which sells for $165. This not only reflects favourably on the value of the Klemeš sub-annual storage model but further strengthens its immunity to potential criticism.

Could Wenzel Rippl Claim Damages from Harvard?

As far as I could find out, Wenzel Rippl owes his fame largely to the Harvard Water Program in whose publications his method was invariably used as the starting point for discussions of storage computations and a bench mark against which progress was measured. Older publications usually referred only to "mass curve" techniques but, starting with the profusion of references by Dorfman[11] to Rippl Method and Rippl Diagram in the 1962 classic volume by Maass et al.[12], these labels have become household words of the trade. But Rippl would have little reason to be grateful to the protagonists of the Harvard Water Program for his sudden fame.

Firstly, Dorfman's Rippl Diagram which found its way into most of the authoritative texts on the topic[13,14,15,16,17], was one that Wenzel Rippl had never used! While all these authorities represent it as a simple mass curve of reservoir inflow (and its tangents as mass curves of different rates of reservoir draft) as shown in Fig. 2a,b, the genuine Rippl's mass curve is an integral of the **differences** between the reservoir inflow and draft, i.e. a special case of the so called **residual mass curve** (Fig.2c).

Secondly, Thomas & Burden[18] set up Rippl's method as a straw man to be shot down because of its "defects" which, on closer examination, all come down to Rippl's failure to anticipate, in 1883, concepts advanced in the Harvard Water Program. His **method** of finding storage capacity from a time series of reservoir inflows was declared defective because he represented this time series by a historic streamflow record rather than synthetic flow sequences advocated by the authors. This alone makes one despair because the method has absolutely nothing to do with the nature of the time series to which it is applied! But what is even more unbelievable is the way his critics then set out to correct this "defect". They developed a **new procedure** for finding storage capacity for a given time series which they called the **sequent peak procedure** and which they proposed to be used "in tandem with synthetic hydrology". The point is that **the sequent peak procedure is identical to the original Rippl's method** as shown in Fig.2c! The only difference is that Rippl made the computations for his mass curve in a table by hand while his critics wrote a program to do the same by computer. In fairness to the innovators, one should not forget to mention two other improvements they introduced: they replaced Rippl's "crests and hollows" with "peaks and troughs" and changed his notation for the storage from J to S (as for their discovery that the computations must be run on two successive identical inflow series to get the correct value of storage capacity, this follows from the necessity to close the water balance over the computation cycle and has been common knowledge since the turn of the century[19]).

Thirdly, the explicit purpose of Rippl's paper[20] was to challenge the then common practice of computing the storage capacity of a reservoir

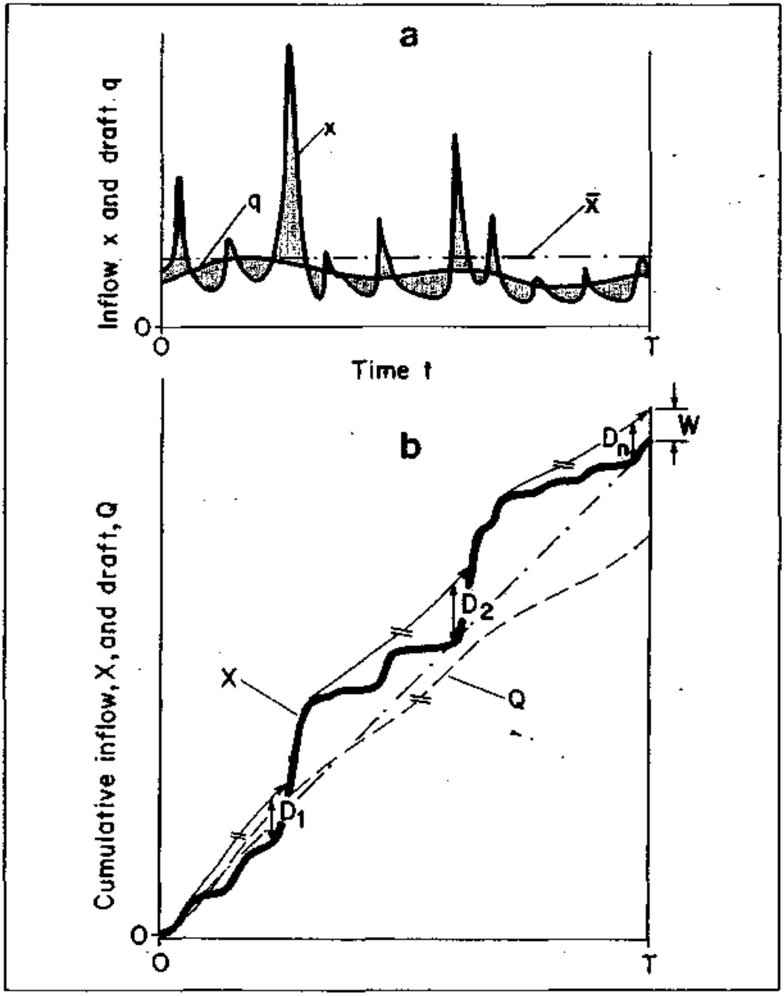

Figure 2. Definition sketch for various types of mass curves used for the determination of storage capacities (D_1, D_2, D_3) required in different periods to meet prescribed reservoir draft.

a - basic variables; **b** - common (absolute) mass curve; **c** - residual mass curve with respect to draft (Rippl's Method = sequent peak procedure); **d** - Hazen's procedure (reservoir behaviour diagram) is equivalent to Rippl's with deleted segments corresponding to periods of spillage (= periods when reservoir is full); **e** - common residual mass curve (computed with respect to mean inflow); its range R represents the storage capacity needed for "full regulation" (draft = mean inflow).

Note that for full regulation, Rippl's mass curve, Hazen's mass curve and the common residual mass curve are equivalent.

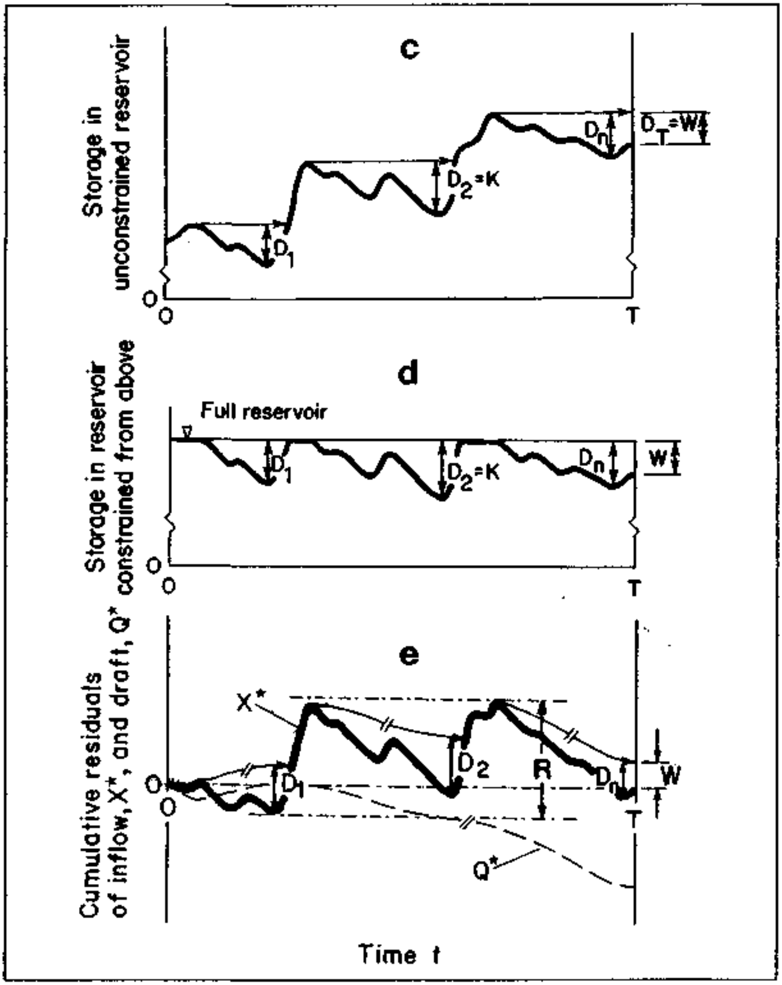

Figure 2—continued. Definition sketch for various types of mass curves used for the determination of storage capacities (D_t, D_2, D_3) required in different periods to meet prescribed reservoir draft.

a - basic variables; **b** - common (absolute) mass curve; **c** - residual mass curve with respect to draft (Rippl's Method = sequent peak procedure); **d** - Hazen's procedure (reservoir behaviour diagram) is equivalent to Rippl's with deleted segments corresponding to periods of spillage (= periods when reservoir is full); **e** - common residual mass curve (computed with respect to mean inflow); its range R represents the storage capacity needed for "full regulation" (draft = mean inflow).

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as the water deficit in the single driest year of record. His main point was to show that a few only moderately dry years may require a larger storage capacity if they occur one after another. He made this clear in the very first sentence of the first section following the paper's Introduction: "The purpose of the storage-reservoir is to equalise the fluctuations of supply and demand during an **indefinitely long period of time**" and reemphasized it again at the end of his paper: "**The limitation of the time considered to a year is erroneous in principle, because the year is in reference to the question to be solved an unessential condition**" (emphasis added). His method was designed specifically to facilitate the determination of the long-term, over-year, storage requirements; to demonstrate its ability to accomplish this, he used an example in which the "critical period" extended over two years.

Given all these efforts to make his point clear, I wonder how Rippl would feel if he had a chance to read the following comment made - what an irony! - in his defense by one of his admirers, the late Mike Fiering[21]: "**In fairness to Rippl**, it should be pointed out **his technique was intended to investigate within-year storage fluctuations rather than over-year requirements**"(emphasis added). I remember how Mike himself felt when I once brought this to his attention and I doubt Rippl could have felt much worse. "Sometimes you pay a price when you take a shortcut and rely on judgements of those you hold in great esteem" he commented with a sigh. He didn't have to tell me more; I knew he had been "present at the Creation" [of the Harvard Water Program], as he later put it in the second lecture of this series (in which, by the way, he proudly made the point that it was being given in the year marking the one hundredth anniversary of the publication of Rippl's paper), and I had a fair idea about who the senior Creators pronouncing definitive judgements on Rippl might have been.

The American Debt to Allen Hazen

I know of no paper dealing with water storage that would contain a greater number and variety of original ideas than does the classic "Hazen (1914)"[22]. And I know of no other author whose so many ground breaking concepts have been neglected for so long in his own country.

Hazen's central idea was to introduce an explicit quantitative measure of hydrological uncertainty into the so called storage-yield function of a reservoir designed to control low flows. With such a measure (alternatively expressed as reliability or risk of failure), the function has the form shown in Fig. 3. Hazen's aim was to construct a function of this kind that would have a **general validity**. To do this, the uncertainty measure was to be based on **general patterns** of streamflow fluctuations expressed in a probabilistic form. This idea has never taken hold in America and, after Sudler's lonely attempt[23], Hazen's hope that, after his "only one step in the development...further study... will ultimately lead to more certain and accurate knowledge of the whole subject" would have been in vain had the "whole subject" not been taken up abroad. It was in the USSR where it inspired very vigorous studies starting in the 1930s and, over the next about forty years, produced (there as well as in other European countries) results of lasting value which I attempted to summarize a dozen years ago[24].

In the USA, only Hazen's idea to use **synthetic streamflow series** (which to him was merely a means for achieving the end result) was brought to fruition by Fiering in his doctoral dissertation[25] done under Harold A. Thomas. The Thomas-Fiering Model has since become a "Ford's Model T" of stochastic hydrology.

In pursuing his central idea, Hazen resorted to several ad hoc clever tricks in his paper which were meant merely to ease the burden of computational and drafting work he had to go through. He probably did not attach much importance to them and would not have expected that each of them would be enough to assure him of a permanent place in history.

Thus, for example, to simplify the plotting of the many storage distribution functions he had to analyze, he invented the Normal probability paper which has become a basic tool of statistics, an invention for which he is seldom given credit.

To plot the data points on the graph in some systematic way (and, as he put it, "with sufficient accuracy with a 10-in. slide rule"), he computed their positions on the probability axis by the formula $P= (2m-1)/2n$ (where m and n are the rank and sample size, respectively) which is still known as the Hazen plotting position in hydrology.

He also must have been one of the first to question the universal validity of the contemporary doctrine (based on Galton's fits of the Normal distribution to genetics data and Edgewoth's observation of the central limit theorem in the 1880s) that distributions of empirical data approach the "normal law of error" as the sample size increases. Distribution functions of his long flow and storage series ($n = 300$ and 402, respectively) showed a pronounced skew which made him doubt the accepted dogma: "Much more numerous data ... would be required to settle finally whether the law of error ... is strictly applicable to long-term records." Had he foreseen what difficulties the departures from

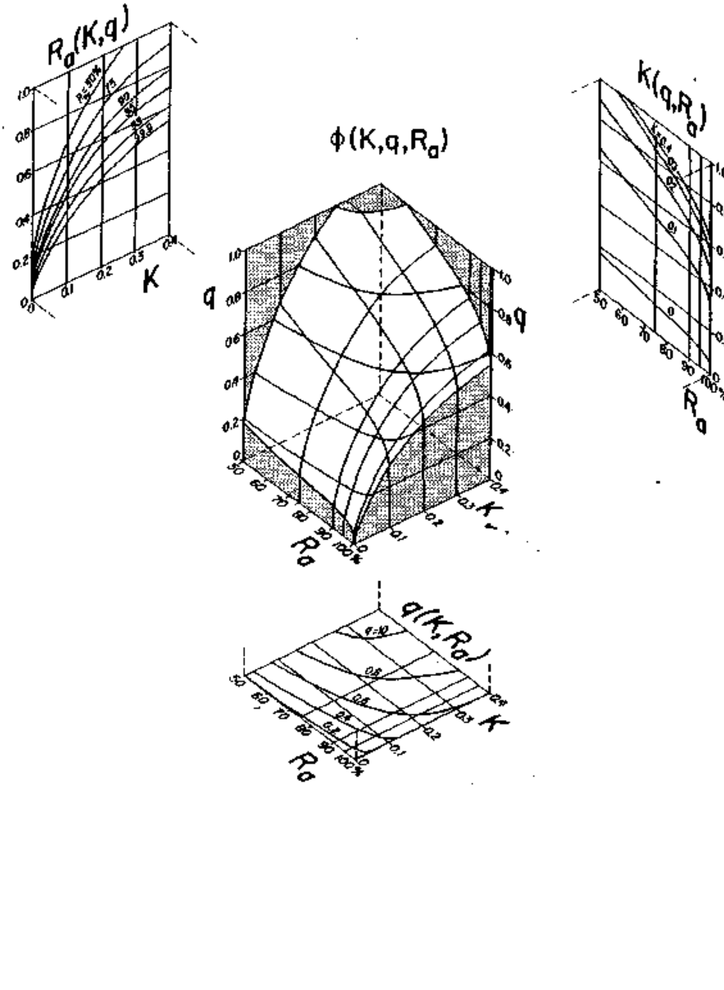

Figure 3. General reservoir storage-yield function $\phi(K,q,R_a)$. K - over-year storage (as fraction of mean annual runoff volume); q - draft (as fraction of mean flow); R_a - reliability (annual) = probability of non-failure year[24].

normality would cause to statistical and stochastic hydrologists, he might have turned a blind eye on his skew distribution curves and said the Chester Kisiel prayer himself.

When working manually with a 300-year long streamflow series as Hazen did, the application of Rippl's method becomes rather awkward. The reason is that, the draft usually being less than the mean inflow, the mass curve tends to run up and away from the drawing and, in addition, its irregular wavy shape makes it difficult to keep track of all the magnitudes of the individual storage values to be considered. Hazen solved this problem in an extremely simple but radical way: he simply dropped the segments of the curve corresponding to periods of spills (which are not needed anyway when one is only concerned with low flow regulation), thereby transforming it into a plot of the time series of reservoir storage fluctuations corresponding to the given draft. Such a plot has become known as **reservoir behaviour diagram.** It is shown in Fig. 2d where its difference from the Rippl scheme may seem minor. However, the enormity of the simplification it represents is obvious to anybody who has ever had to analyze (and understand!) in detail reservoir behaviour on a scale comparable to Hazen's. Moreover, Hazen's scheme substantially simplifies the computations and, unlike Rippl's procedure, is a genuine made-for-computer product. No wonder then that it has become the standard of the trade all over the world - except, it seems, the American academic circles where the original Rippl's method still reigns supreme disguised as the "sequent peak algorithm".

The irony of this situation can best be appreciated in the context of the now standard ritual, performed with a moving faithfulness in most American storage-related textbooks: first, Rippl's method is declared obsolete, then a paragraph of homage is paid to Hazen's genius and, finally, the sequent peak algorithm is presented in detail as the "modern technique" for solving storage reservoir problems.

Mike Fiering once told me that he had included Hazen's paper in the list of compulsory reading for all his graduate students. Alas, after becoming professors, his students seem to have abandoned this laudable practice of their professor. Recently, one of Mike's "grand graduate students", so to speak, (now a professor himself) sent me a draft of his paper in which, among other things, he advocated a new resiliency measure for reservoir performance - it was exactly the same measure Hazen used to characterize reservoir performance in his 1914 paper. The latter was not referenced and, it appeared, the author has never read it. However, since Klemeš was referenced several times for no good reason, I proposed a deal to the author offering him to trade two references to Klemeš for one to Hazen. That, so far, was all I could do for Allen Hazen in his native country.

Ups and Downs of the Residual Mass Curve

One discovery Allen Hazen did not make, but certainly would have liked to, was that of the "common" residual mass curve which integrates the deviations from the mean of a time series. That distinction went to Charles Sudler who probably didn't think much of his discovery himself. He used the curve to reduce the amount of drafting he had to do in his (otherwise rather abortive) attempt to advance Hazen's work on a general probabilistic storage-yield relationship (cf. 23).

This simple trick makes the residual mass curve a powerful and flexible tool. In storage analysis, it does away with the plotting of a separate mass curve for every different value of the draft. Instead, the same curve can be used for any draft because different drafts can be represented by tangents of different slopes (or curved shapes if they are non-constant) as shown in Fig. 2e; the added advantage is that the plot does not run up and away but unfolds neatly in the horizontal direction as do Hazen's "behaviour diagrams" which of course must be drawn separately for every different draft.

Hazen immediately saw the significance of Sudler's "incidental" innovation and commented[26]: "This paper contains a contribution of real importance ... The use of a mass curve, in which is shown only the accumulated surplus or deficiency as compared with the mean, permits a more convenient representation and accurate study of the data ... After having tried this method on an example the writer wonders that it was not done long ago."

This writer also wonders, namely why this technique, adopted the world over, has never been advocated in American textbooks which, as a rule, only casually mention the use of the basic mass curve (Fig. 2b), moreover, misrepresenting it as the "Rippl Diagram".

It was the English engineer Harold Edwin Hurst who elevated the common residual mass curve to a position of prominence reaching far beyond the context of storage reservoir computations. When he used Sudler's technique in his studies of storage needed for full regulation (one where draft is equal to the mean inflow) of the Nile River by the large Aswan Dam[27], he didn't have the slightest idea that his humble engineering analysis would lead to one of the most unexpected discoveries about the statistical behaviour of long records of empirical phenomena, ranging from streamflow to tree rings, wheat prices, annual catches of Canadian lynx and beyond - the famous **Hurst**

Phenomenon. It was a pure case of serendipity and it is now so well known that there is no need to dwell on it here (cf.1).

What is not so much appreciated is that it established the common residual mass curve as one of the principal tools of time series analysis. And it was through the interest in this analysis that this curve has found its way back to America (though not to reservoir analysis where it came from). There was a time - and it was Chester's time here in Tucson - when it seemed that almost everybody, from the brightest minds at the IBM down to every other American graduate student in stochastic hydrology, was working on some properties of the residual mass curve. It has been one of the greatest inspirations to which water storage studies have ever led.

However, it may also easily turn into a source of desperation because of its simple "iron rule" which says that what goes up must come down again and vice versa. In other words, the plot of every common residual mass curve must come back to zero from which it has started. The curve has two features which conspire to make many an analyst see patterns that do not exist in nature and are pure chimeras created by the mathematics of the curve. One feature is that, unless a sample is extremely skew, about half of its deviations are above the mean and half are below; the other is that, as Feller[28] has shown, the sign of the first deviation tends to fix the shape of the curve for a long period. And so it happens that plots of common residual mass curves tend to exhibit up and down swings with typical lengths between 1/3 and 1/2 of the sample size. Innocent as this feature may seem, it has "proved" cycles of wide ranging periods in hydrological and climatic data, not to mention climatic changes!

Here is an example: Williams[29], after analyzing a number of precipitation and streamflow records, concluded:"If cumulative deviations from the mean are computed for hydrologic data, continuous periods of 10 yr to 35 yr or more will be revealed in which hydrologic records are consistently below or above their means" (a part of Williams' plots is shown in Fig. 4). Given the fact that most of his records were between 60 and 70 years long, it could have hardly been otherwise. Had he used 100 year long records, their residual mass curves would show cycles 20 to 50 years long. Conclusions similar to Williams' have been reached, on exactly the same grounds, by several Russian authors.

The point is this: the trends would be real if the storage where the deviations from the mean flows, etc., have been accumulated were real – that is, if the plots were "true Rippl diagrams" in the sense that the means represented real outflows from real reservoirs. As a matter of fact,

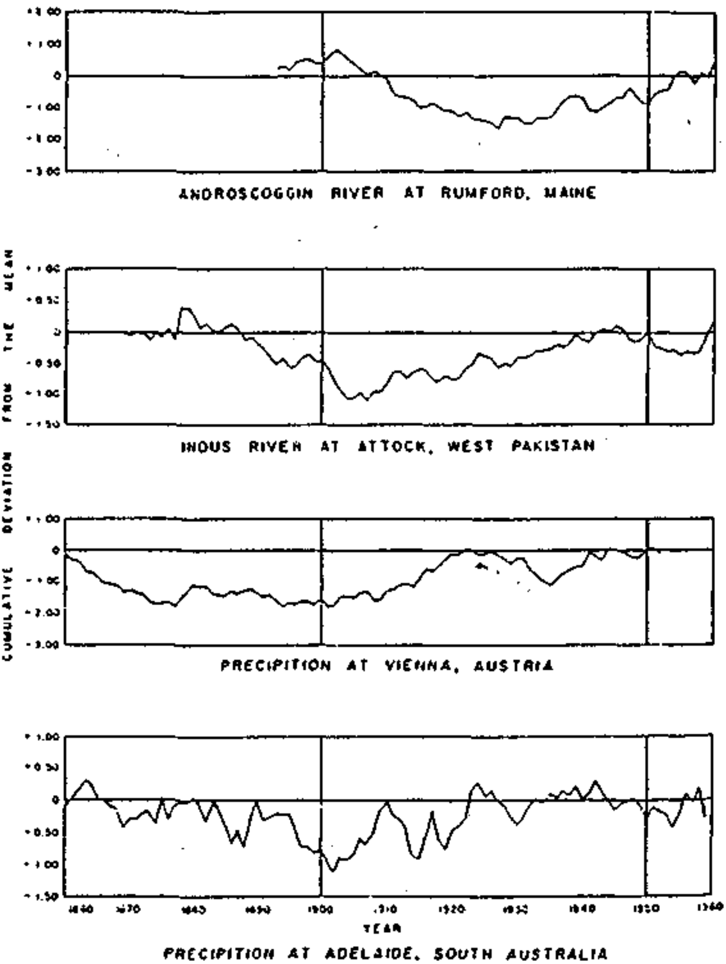

Figure 4. Some residual mass curves of hydrological records analyzed by Williams[29].

Nature does construct such true Rippl diagrams in the form of the historic records of fluctuations of glacier volumes, groudwater and lake levels! Unfortunately, or perhaps fortunately, **computer storage is not the same as water storage and can't cause climatic trends** as many people seem to believe. An example of the real thing and a computer chimera is shown in Fig. 5.

Even more misleading conclusions can easily be reached on the basis of residual mass curves of higher orders which, after about the 3rd

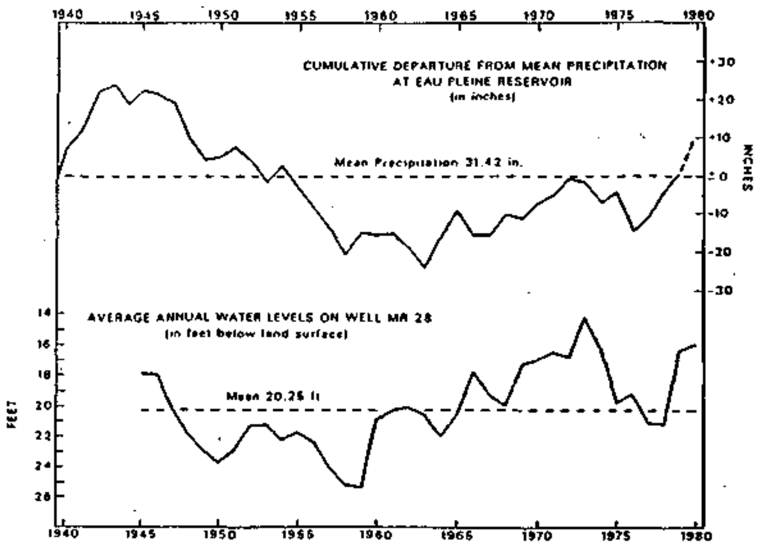

Figure 5. Observed annual groundwater levels and computed residual mass curve of precipitation at a nearby station.

order, converge to a pure sine wave with a period equal to the sample size, whatever the sample size may be and independently of the shape of the initial time series[30] (an example is shown in Fig.6). The most dangerous situation arises when a residual mass curve is computed from a historical record which itself already represents a "natural residual mass curve" of first or higher order. The result is then an almost perfect cycle extending over the whole historic record (cf. 30).

From Finite to Infinite Reservoirs

As every schoolboy knows, every water reservoir on earth is finite, from the smallest puddle, to ponds, lakes natural and man-made, to the world ocean. For dams, this is always certified in writing since every design specifies their maximum water level that must not be exceeded.

However, as Professor Moran explained in his classical 1959 monograph,

> "It being difficult to obtain explicit solutions for the finite dam, we attempt to simplify the problem. Since most of the difficulty arises from the boundary conditions..., it is natural to consider dams of infinite capacity and two cases now arise. We may consider what happens near the top of the dam when the *probability distribution* of [storage] *is very unlikely to take* values near zero. We may then regard the dam as infinitely deep ... Alternatively the conditions may be such that the probability of the dam ever being full is so small that we can take the dam as infinitely high and consider the probability distribution near the bottom."[31]

From that time on, many a mathematician writing on storage theory has started his paper with an apology to the engineer for the lack of realism in his forthcoming uninhibited musings about top-less or bottom-less reservoirs and assured him that the only reason for taking this liberty was mathematical convenience.

As an engineer, I have always (that is, since I first learned about these intriguing concepts while translating Moran's book into Czech) felt uneasy about such statements. I had a feeling that their apologetic tone was just a disguised discrimination, a subtle way of depriving the engineer of an equal right to share in this enviable source of inspiration. This inequity and injustice of being excluded from the privileged circles weighted heavily on me and I could not get the thing off my mind. But once, in a flash of insight, I realized that, in fact, **all real-life reservoirs, including river basins, were top-less!** Naturally, I was delighted by this discovery which assured the historically underprivi-

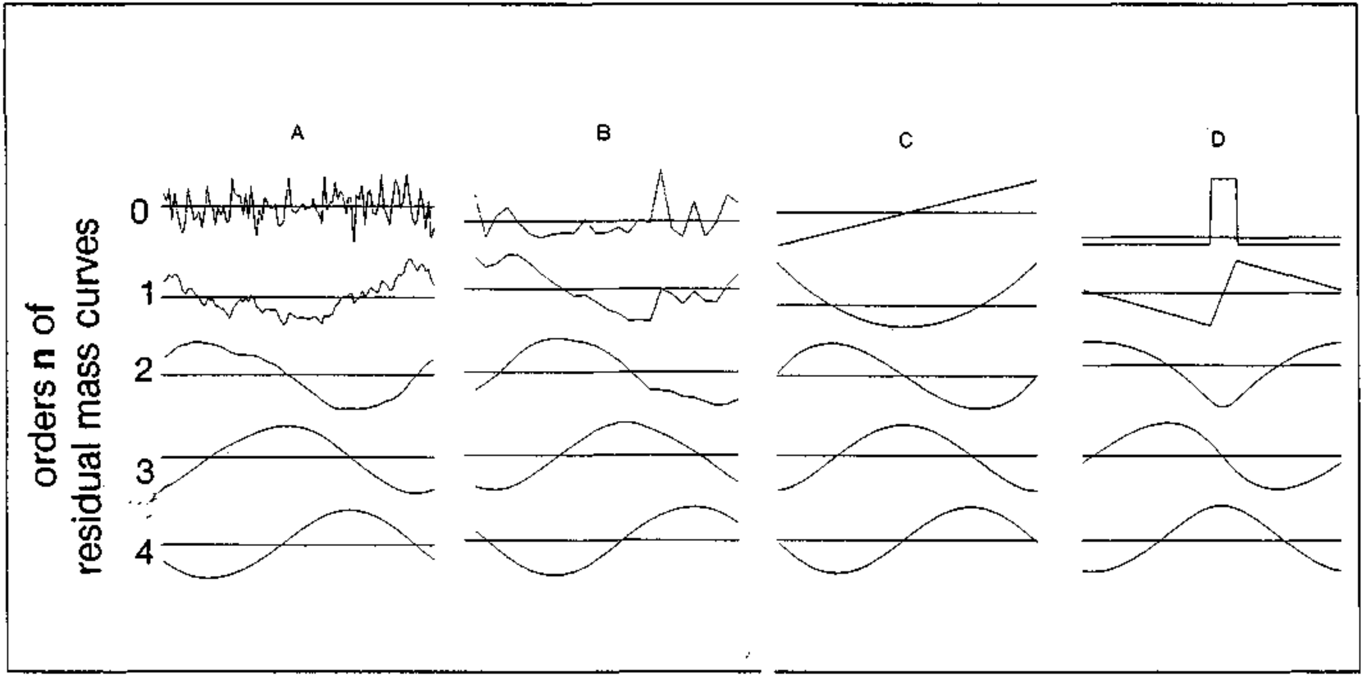

Figure 6. Examples of common residual mass curves of higher orders (order 0 represents the original time series). **A** - annual flows of the Rhine River in Basel, Switzerland (one hundred years, 1851-1950); **B** - random series generated from a lognormal distribution (sample size n = 25); **C** - linear trend line; **D** - rectangular puls[30].

leged engineers of equal opportunity and gained them access to the refined intellectual pleasures of the scientific elite. I raised the issue with Professor Moran at the earliest opportunity and assured him that there was no need for apologies: because precipitation distributions are always fitted with models that have infinite upper tails, no reservoir, be it a puddle or an ocean, can be prevented from exceeding its "maximum level" and thus from being effectively top-less. While admitting he has never thought about it in that way, Professor Moran, a kind man that he was, had no objections, especially when I pointed out that Noah's experience proved my point.

I consider the above finding my most important and lasting discovery and never since I made it ceased I to be fascinated by top-less reservoirs, to the point that I even bought one when the first opportunity presented itself. It was a beautiful small private lake near Ottawa, the Canadian capital, and it served as a source of inspiration to the whole family and many friends for a number of years.

An afterthought: Could it be that the reluctance of American professors to adopt Hazen's behaviour diagram stems from the fact that it so openly and clearly shows his reservoirs to be bottom-less? Or has their infatuation with the sequent peak algorithm something to do with the fact that it implies both bottom-less and top-less reservoir but makes neither feature too conspicuous?

Some Improbable Developments in the Probabilistic Theory of Reservoir Storage

It is paradoxical that it should be a probabilistic theory that is so richly endowed with improbable developments. It is true that, because of language barriers, eagerness to make one's own discoveries accompanied with reluctance to "waste time by reading all the obsolete old stuff" (as one young professor recently confided to me), some seemingly improbable occurrences should in fact be expected. Yet, the probabilistic theory of storage seems to be endowed with them over and above its rightful share.

Thus, for example, the equivalent of the famous 1954 Moran storage model for uncorrelated annual inflows was published by an obscure Russian engineer named Savarenskiy in 1940[32]. Ironically, not only was Savarenskiy never given credit for it in the West (it appears that the first English-language reference to him appeared in my paper[33] given at the same symposium where Chester Kisiel said the theoretical hydrologist's prayer), but had continuing difficulty getting it even in the USSR where his model was routinely attributed to Kritskiy &

Menkel, despite their repeated disclaimers. Another instance is the model for correlated annual inflows which was published in the same year, 1963, by Lloyd[34] in England and Kaczmarek[35] in Poland.

While all coincidences look unexpected, some are more unexpected than others. In this regard, the 1940 Kritskiy & Menkel[36] model for correlated seasons stands out in a class by itself (by the way, it was published in the same issue of the same journal as was the Savarenskiy model). When I once mentioned this model to Professor Moran, his face lit up with amused disbelief. He pulled out a paper from his shelf, leafed through it and made me read the following statement from it: "The above method neglects the possibility of correlation between flows in successive months. To take account of such correlation would be vastly more difficult." Then, with an obvious delight, he showed me the author and year of publication: Moran, 1955[37]. What apparently amused him most was the fact that Kritskiy and Menkel were just young engineers with no formal training in statistics and probability when they made their startling discovery, 15 years before he - a professor of probability and statistics - declared it vastly difficult.

However, as I subsequently found out, an even more unexpected aspect of their discovery was that Kritskiy and Menkel themselves apparently did not fully appreciate its extent. It took them nineteen more years to develop a rather complicated model for serially correlated (annual) inflows and they did not realize that a simple and elegant solution to the problem was implicit in their 1940 seasonal model: it just required to make the seasonal flow distributions and the season-to-season correlations identical and identify the "season" with the whole year. All that was needed to reformulate the theory was to drop the subscripts identifying the different seasons! Who knows whether the authors have ever realized this. When I did, Professor Kritskiy was already in his eighties and, though we still corresponded, I refrained from asking him.

This episode, together with my sincere interest in unhindered and rapid progress in storage reservoir research, has moved me to propose, at this point, the following extension of Chester Kisiel's prayer:

... and deliver us from all sources older than five years, including our own papers.

The Enigma of Negatively Skew Runoff

Prayers notwithstanding, one must face the facts of life. One of them is that the probabilistic distribution of annual runoff is skew. When I started working with storage reservoirs in the late 1950s, Hazen's doubts on this point had long been put to rest and a positive skew of annual runoff was taken for granted. The only unresolved problem seemed to be the mathematical form of the distribution. The favourite of the time and place was Pearson III with a zero lower bound, i.e. the 2-parameter Gamma. Its coefficient of skewness is double of the coefficient of variation, $C_s = 2\ C_v$. In the naivety of a young engineer, I thought there was some profound hydrological reason behind all this and, though I didn't know what it was, I was sure the authors of the textbooks advocating this preference knew. I was quite astonished when I later found they not only didn't know but mostly didn't even care(!) and were satisfied that the fit was good. This finding was more than a disappointment. It caused the first crack in my hitherto firm belief in the scientific nature of statistical hydrology, a crack that soon developed into a chasm dwarfing the Grand Canyon. But it inspired me to look for the hydrological basis of statistical properties of hydrological phenomena myself and the skew of annual runoff served as a good introduction into this fascinating area.

An incentive to start working on the problem presented itself with the appearance of Yevjevich's classic work[38] containing records of annual flows from 140 rivers around the world. However, when I plotted the C_s vs. C_v relationship for these data (Fig.7), my attention and curiosity were diverted to the unexpected fact that the skew coefficients were spilling over to negative values! By that time, thanks to Leonid Brezhnev, I already had access to the cream of western hydrologists and lost no opportunity to sound them out about the reason **why** that should be so. The results were quite devastating. In the best case, they had no idea, in the worst, they didn't even get my point. Thus, for example, the highest priest of contemporary American hydrology told me that I could "flip around" a positively skew distribution and get a good fit to a negatively skew one; his Australian counterpart suggested that, if I raised the C_s to the second power, I would "get rid" of my problem! I did not know how to make them see that I didn't want to fit anything, that I didn't want to get rid of the negative skew - that I just wanted to know what its cause might be.

One who got the point immediately was Chester Kisiel. When I discussed the problem with him in the summer of 1969, I already had some hints. I noticed, for instance, that the negative skew tended to be coupled with positive serial correlation in the flow series. That pointed to storage and, indeed, the rivers concerned had either large lakes, aquifers or glaciers in their basins. Chester listened attentively, scribbled in his notebook and then asked me to send him a copy of whatever

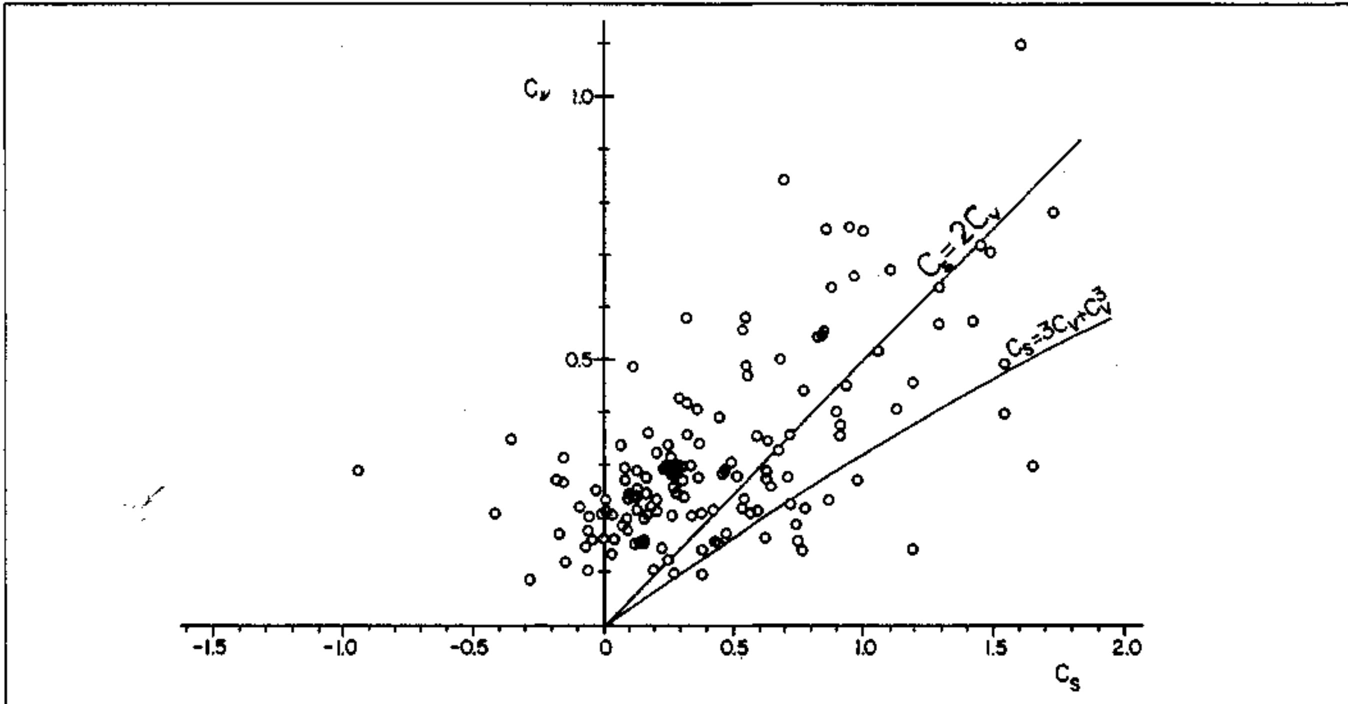

Figure 7. Relationship between the coefficients of variation (C_v) and skew (C_s) for historic records of annual flows for 140 rivers. The two lines show the functional relations for the Gamma-2 and the lognormal distributions.

I might write about it. I thought it was just a polite gesture and soon forgot about it. But, the next April I got a short note from Chester; it read: "Dear Vit: At last summer's meeting in Logan, Utah, we discussed your ideas on physical interpretation of skew coefficients. I wonder now if you ever had occasion to write this into a suitable format for discussion." So I send him what I had and, two months later, I read in his next letter: "Incidentally, I found your paper on skew coefficients quite interesting." I thought it was just a polite way of acknowledging the receipt of my paper. But Chester apparently meant it and kept thinking about the problem for, a year later, he returned to it again in one of his letters: "Your New Zealand paper[39] is of considerable value and interest to me. We need many more efforts along these lines."

Only then it dawned upon me where Chester's interest and persistence were coming from: It was from the deliberations that had led him to propose his prayer at the 1967 Fort Collins symposium. For, in his paper (cf. 2), the skew of output from hydrological systems was one of the problems he was puzzling about. He noted earlier results showing that "a single nonlinear reservoir, $S=KQ^n$, transforms a normal input to a non-normal skewed output with reduced variance ... in contrast to a linear storage system which reduces both variance and skewness of its inflow probability distribution" (Chester's emphasis). However, those results implied that zero variance and skew were the lower limits for hydrological variables. Chester summarized the situation in a cryptic comment: "Hydrologic systems are, in general, nonlinear in their transformation process. Very few theoretical results are available to predict output statistics." So, in retrospect, it was quite natural that Chester's interest was aroused by a physical mechanism that could lead to a negative skew of distributions of outputs from hydrological systems.

The last development Chester was to see along these lines appeared in a paper[40] which Professor Moran asked me to write for a symposium here in Tucson in September 1971, of which he was co-chairman. My paper showed clearly (and to my delight) that top-less nonlinear reservoirs could do the trick. I then pursued the idea further and Chester would have appreciated Fig. 8 in which I summarized my explorations some ten years later[41]. It shows that even a very positively skew input like the lognormal can be transformed into a negatively skew output by a suitably nonlinear reservoir, namely one whose outflow is proportional to storage raised to a power $b \ll 1$. But this is only a demonstration obtained by stochastic simulation - a general mathematical formulation proved to be beyond my reach and, as far as I know, Chester's cryptic comment that "very few theoretical results are avail-

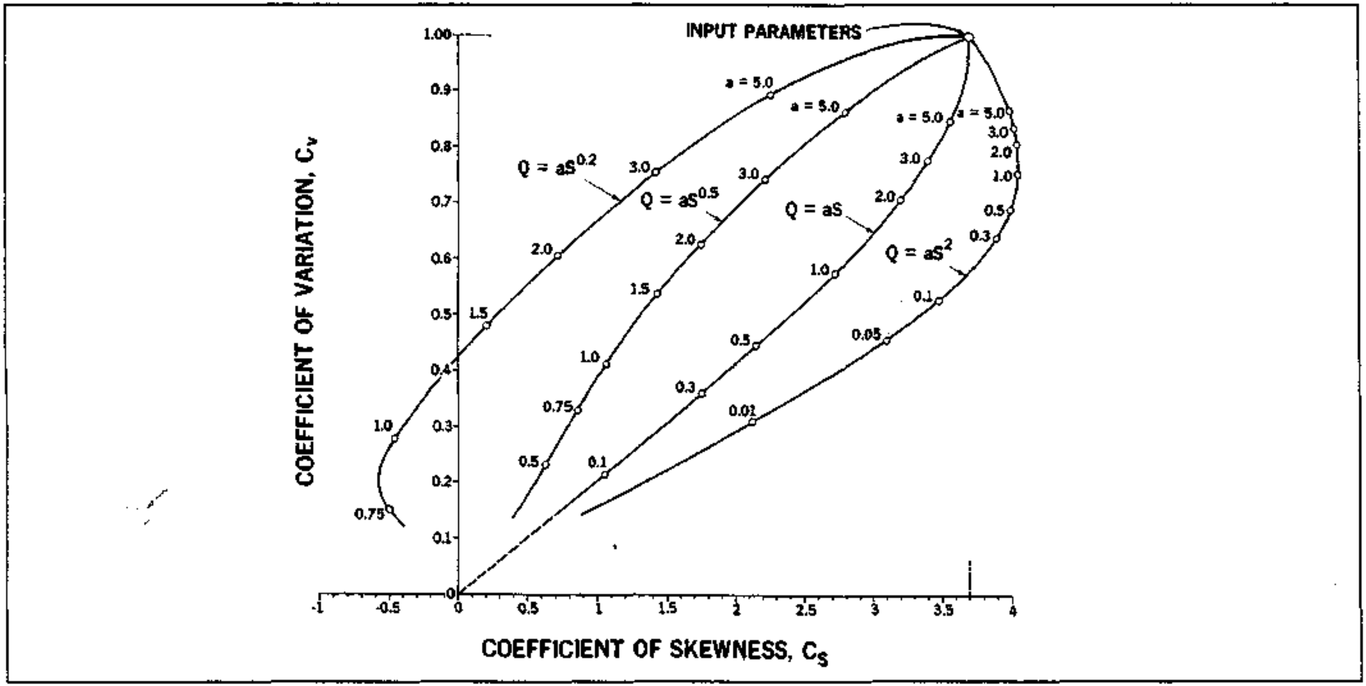

Figure 8. Relationships between the coefficients of variation (C_v) and skew (C_s) for output from one nonlinear reservoir fed with a random series generated from a lognormal distribution with C_v=1. The reservoir is defined by its release function **$Q = aS^b$**, where **Q** is outflow rate, **S** is storage and **a,b** are constants. Each curve corresponds to a reservoir with a given value of **b** and a range of values **a**[41].

able to predict output statistics" (from nonlinear hydrologic systems) is still valid today, a quarter of a century later.

It has occurred to me that, already at the time of his request for the Tucson paper, Professor Moran must have known that my efforts (to get theoretical results for my top-less nonlinear reservoir) were doomed to failure but, a kind man that he was, he probably didn't want to discourage me by telling me so. For, in a paper on the future of stochastic modelling which he published shortly after[42], he wrote: "... it is clear that nonlinearity is an all pervading problem and here we are confronted, if not with a brick wall, at any rate with a hill of rapidly increasing slope." If it was a hill for him, no wonder it proved to be a Matterhorn for me.

In retrospect, I am glad I did not realize the futility of my "efforts along these lines" when I first set out to find out how things work in stochastic hydrology, which journey eventually led me to the high country of nonlinearity. If I did, I might have been fitting straight or "flipped around" Gamma-2 and other distributions to this very day and, who knows, may even have joined the distinguished gallery of experts who write authoritative treatises on the most rigorous scientific ways of doing it (currently, it is the method of linear moments, no doubt). But one thing is sure: I would have missed a lot of excitement (even frustrations can be exciting if they are of the right kind); a wealth of unforgettable intellectual exchanges with extraordinary people like Chester Kisiel, Pat Moran and Mike Fiering, to name just some of those whom I met on this journey and who have since passed away; and invaluable insights into how things work - and why they sometimes don't.

Let me close with a quotation which Chester would have certainly included in his intended paper, had he had an opportunity to write it. Its author is the late American physicist, Nobel prize winner, Richard Feynman[43]:

"The thing that doesn't fit is the thing that's the most interesting, the part that doesn't go according to what you expected."

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BIOGRAPHY

VÍT KLEMEŠ

Vít Klemeš received a Civil Engineering degree and a Ph.D. in Hydrology and Water Resources in what used to be Czechoslovakia: the engineering degree from the Technical University in Brno (the Czech Republic) and the doctoral degree from the Technical University in Bratislava (Slovakia). For almost a decade, he worked in planning, design, construction and maintenance of dams and water resources systems with Czech and Slovak government agencies. Following this period, he joined the Hydrology and Hydraulics Institute of the Slovak Academy of Sciences where he did research in storage reservoir theory.

In 1966, Vít Klemeš came to Canada where he initially taught at the University of Toronto. During the period of 1972-1989, he did research in hydrology (primarily stochastic aspects of hydrological phenomena) and water resources systems at the National Hydrological Research Institute of the Canadian Federal Department of the Environment in Ottawa and Saskatoon; during the last ten years of the period, Vít Klemeš was the Senior Scientist.

While working for the Canadian Department of Environment, Vít Klemeš travelled broadly, giving lectures on all the five continents. He was a visiting professor at the California Institute of Technology, the Swiss Institute of Technology in Zurich (ETH), and at the Monash University of Australia. From 1987 to 1991, he was President of the International Association of Hydrological Sciences.

Since 1989, Vít Klemeš has lived in Victoria, British Columbia, Canada.

BIOGRAPHY

CHESTER C. KISIEL

JULY 9, 1929 - NOVEMBER 5, 1973

Chester Kisiel was born in Harrison township, the eldest of six children. He came from an immigrant background, from the steel mills of Pittsburgh. Chester had to work from the time he was fourteen years old to help support himself and to help pay his educational expenses, and he never stopped working until his untimely death on a handball court in Tucson.

At the time of his death, Chester Kisiel was a Professor at The University of Arizona, holding appointments in both the Department of Hydrology and Water Resources and the Department of Systems and Industrial Engineering. He came to Arizona in 1966 from the University of Pittsburgh, where he taught in the Civil Engineering Department from 1954 to 1965. Professor Kisiel was educated in Civil and in Sanitary Engineering at Pennsylvania State University and at the University of Pittsburgh. He received the degree of Doctor of Science from the latter institution in 1963. He was in the U.S. Air Force, 1951-53, serving in Japan and Korea.

There was a theme to Professor Kisiel's professional work — it was his continuing effort to bring mathematical and modern engineering methods to bear on problems in hydrology. And he was not content to deal with existing problem statements. In many cases he refined and reformulated the problem itself, or he identified problems before many of his colleagues were aware of their existence. The fruits of his efforts are evidenced in his many publications, in several international symposia in which he played a leading organizational and scientific role, and perhaps most important of all, in the stimulation and guidance he gave to his colleagues and students. By these efforts he established an international reputation and a deep personal esteem on the part of those with whom he collaborated.

Chester Kisiel had the gift of self-examination, which is another way of saying that he had the gift of honesty. He tried to be honest with himself and honest with others. He could forgive many things but not something, that in his view, was a dishonest piece of work.

Professor Kisiel brought to bear prodigious gifts in pursuing his goals. He had the gift of hard work and uncompromising standards. He was a hard task master, but he never demanded more of others than he was willing and able to do himself. He had the gift of sound instinct, both with regard to technical matters and in the assessment of the strengths of his colleagues. He had the gift of stimulating and working with others across many disciplines.