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UNNY SYMPOSIUM: WHY ARE YOU HERE?

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The purposes of conference attendance are discussed and the subject matter of this symposium is examined in its broadest scientific and engineering contexts. Examples are drawn from the published work of Denys Gamblin, Harold Thomas Jr., C. V. Theis, Morris Ettinger, Sir Peter Medawar, Arthur Morgan, Chick Sale, and Myron Fiering to place into perspective work in hydrology and environmental engineering. The scientific basis of those fields is explored with the purpose of focusing attention on the need for rigorous hypothesis testing. Statistical and stochastic methods for describing variability relevant to water resource and environmental engineering decision making are considered. The need for developing the scientific basis for the entire field is emphasized and examples are drawn from the field of environmental fluid mechanics to show the importance of coupling experimental work with theoretical developments.

PREFACE

This paper was prepared as a keynote dinner address for the symposium. Its purpose was to be thought provoking and to use both humor and quotes from a variety of sources to invite us to reexamine our work. I am a strong supporter of the use of crisp mathematical methods where they are appropriate. The larger purpose of this work is to assist us to place our work into the total context of science and engineering.

AN ANSWER TO A QUESTION?

The answer to the question posed in the title comes in several parts. First and foremost we are all here to honor the memory of our late friend and colleague, Professor Unny. We had planned this to be a celebratory gathering to wish Unny well on the occasion of his formal retirement. It was the privilege and good fortune of many of us to have known this warm, gentle, courteous, and generous man who gave enormously of himself in his efforts to improve the quality of the practice of our broad profession. Those with whom he worked closely know his generosity better than most. While we cannot thank him we must make sure that the proceedings of this symposium are a suitable memorial to his indefatigable spirit.

I first traveled to Waterloo, Canada, in 1978 to attend a symposium organized by Ed McBean, Keith Hipel, and Unny. When I read about that proposed meeting at the "University of Waterloo", it was with a little trepidation that I replied. I was not sure if there would be some replay of the 1815 "Peninsular War" where the Duke of Wellington defeated Napoleon near the town of Waterloo, Belgium. Consultation of relevant maps assuaged any fears that might have resulted from possible ignorance of geography. All of us now know that Waterloo, Canada, is home to fine colleagues who make all visitors welcome and comfortable.

There is a larger context for this meeting and scientific and professional meeting attendance in general. A meeting such as this provides opportunities for us to share our findings with others. We can sharpen our own ideas based upon what we hear and observe and through discussions with colleagues. The formal literature is remarkably linear and it is difficult to determine from it why something was done. A chat while consuming a suitable beverage provides the chance to fill in relevant details. Perhaps the most important reason for us to be here, apart from honoring Unny, is for us to renew old and establish new friendships.

We are all familiar with some less salutary aspects of meeting attendance. Travel requires the traveler to endure less than haute cuisine en route, and become tired of body and mind while traveling in seats and seating arrangements that clearly have been determined by some multi-criterion optimization scheme for a "standard" person of ergonometric form not found in any living person. This apparent optimization is for the express purpose of maximizing revenues, but not necessarily profits, of the transportation companies who have delivered us to this location. None of us benefits from having our biological clocks out of synchronization with the local chronometers. Such disorientation coupled with partaking of food and beverage accounts largely (I hope!) for the somnolent state that appears rapidly to be overtaking many in this audience. (I have a vague recollection of a less than remarkable -- it was memorable in the "fuzzy" sense -- wine supplied by Keith Hipel at our meeting here in 1981 having a similar effect on us on that occasion).

I doubt that anyone here had in mind coming for the express purpose of adding a "Least Publishable Unit" (LPU) to make a resume look a little better to unwise readers of such poor indicators of scientific and professional contributions. It is clear that the positive aspects of this symposium far outweigh any possible negative elements. We are all in debt to Keith and his colleagues for adding so much to our well being.

The results of this symposium will go beyond those of us who are the immediate participants. We hope that there will be a cadre of willing reviewers who will eagerly drop whatever else they are doing to respond immediately to requests by the organizing committee for review comments on each of the papers to ensure that the papers are improved where needed and can be made ready for formal publication. The set of papers will give rise to a multi-volume proceedings whose publication will create economic and entropic activity and will cause librarians world-wide (who are struggling valiantly to shelve ever growing collections) a little distress. With luck, readers, most likely largely in the form of enthusiastic graduate students, are awaiting the fruits of all our labors. Perish the thought of any of our contemporaries reading what we write! While these volumes will mark an important occasion they may become close to being an end marker of an era. We are approaching rapidly the era of electronic publishing and information delivery and access (Burges, 1990). Soon the most valuable coalition in research organizations will be those of research planners, technologically astute librarians, and the dynamic doers of research work, the latter being we students of all ages and levels of expertise and understanding.

SOME FOOD FOR THOUGHT

By formal education, I am a civil engineer. Civil engineering to me has always been decision making under uncertainty. We have precious little data upon which to base our decisions. We have some fundamental understanding of physics, chemistry, and biology; and those knowledge bases are expanding rapidly. We look to the tools of formal mathematics and relevant offshoots to make numerative commentaries about the situations before us. Many situations defy enumeration because we have no data. Mental models of nature and potential designs, which capture far more than can be done by any computer, are eschewed by most quantitative reasoners. Perhaps that is why architects, geologists, geomorphologists, and good engineering foremen and forewomen are so effective at what they do. We still design, and design well, by using known principles to avoid approaches that we know will get us into trouble. It is the violation of the basics which gets us into trouble just as readily today as it did engineers and scientists a century ago (Kernot, 1965).

The broad fields of water resources and environmental engineering, and the related scientific disciplines, test our scientific and professional skills to the limit because we are largely in the position of trying to describe natural systems that have an overlay of human use and infrastructure. At best we have few observations, precious little information about the extrema, and are lacking understanding about many of the chemical and biological activities at relevant scales that are of importance to our basic scientific understanding and decision making activities. Whenever we can we need to make some relatively crude estimates of the uncertainties in our measurements and our predictions. The data and methods, by and large, support at best simple second moment ("first-order" uncertainty) approaches which lead to approximate statements of risk measures. I first came to appreciate the possibilities of this simple approach when I attended a lecture given by Allin Cornell (Cornell, 1972) at a meeting in Tucson, Arizona in December 1972 that was arranged by the late Chester Kisiel and David Dawdy. I thought that this approach is so obvious that we all ought to do it routinely and introduced it into my undergraduate classes. What I thought was so obvious has been eschewed by determinists (they appear to constitute a large fraction of "environmental engineering" university faculty members) who choose to teach as if the world was Laplacian. I plead guilty to making use of simple second moment schemes in elementary water quality modeling (Burges and Lettenmaier, 1975), estimating uncertainty in the areal extent of flood plain inundation (Burges, 1979a), and in propagation of parameter uncertainty in a complex conceptual rainfall-runoff model (Garen and Burges, 1981). A more complete approach was taken for water quality monitoring network design (Lettenmaier and Burges, 1977). Others have chosen to take the methods further but there are few situations where our knowledge and information bases can support such extensions. I have contributed also to discussions on rainfall-runoff models and their calibration (James and Burges, 1982b) and to aspects of synthetic streamflow generation (Lettenmaier and Burges, 1982).

SYMPOSIUM SUBJECT MATTER

This brings us to the subject matter of this symposium. When I was preparing this paper I had available a list of most of the paper titles. The areas of coverage are many and varied but involve principally topics that have been of concern for at least two centuries (Burges, 1979b) in the broad field of water resource engineering and water resource systems. The papers do not lend themselves readily to particular groupings. There are discussions of rainfall and evaporation time series and their representation with statistical measures and assumed stochastic structures. Floods and droughts remain topics of interest and the treatments are for both at-site and regional probabilistic summaries of the less common events. Various aspects of water quality, both surface and subsurface, and ways to substitute assumed structures for situations where we have few observations are given. Schemes for all kinds of data analysis are presented. How much can we say about "not very much"? As we become further removed from the collection of the fundamental data with which we work we lose much. I recommend to all that you reread the thoughtful remarks made by Luna Leopold about the preciousness of our data and how we describe them (Leopold, 1982)

No doubt much of the analysis was done for data extracted from "CD ROM" sources. My personal experience with these wonderful compact data storage devices is that they contain errors that can be found only by checking every number against the original source for transcription errors. We assume (erroneously) that those who recorded the numbers in the first place did so without error. Any record that is relatively complete or, worse yet, continuous, has had missing information filled in by methods that are not well documented (and most certainly not described on the CD ROM!), all in the interest of saving those of us who come along later from having to undertake such critical activities. Is the following a fair statement: I am sure that all data analyzed in the papers presented here have been checked personally by the users to verify that the numbers are correct? (I do not know of any multiple sensor data sets where all instruments worked flawlessly and concurrently). It is a fair bet, based on the paper titles, that at least one author at this symposium had to check the data that were used.

Ubiquitous questions of scale are addressed. These range from papers about global climate model predictions to descriptions of permeable media, the interactions of those media with the substances transported through them, and the transfer of matter through permeable media. There are papers concerned with reservoir operation; the tools from the field of control systems including Kalman Filters and various fuzzy logic approaches have been put to use. Operation of any reservoir system depends on good forecasts and several papers deal with this elusive subject area.

Are we creating or have we created a "Tower of Babel"?

The diversity of topics indicates an active research community and this bodes well for the future. In our efforts to advance the state of our science and practice, have we started to develop a language that might be incomprehensible to intelligent people from other fields? It is with this thought that I share with you some observations made in England by Denys Gamblin in his book "Water on the Brain". He is making fun of and protesting the language used by younger "experts" who are explaining to seasoned managers how they should be doing their jobs. Do you find any common ground in the following?

"My head started to whirl and my heart sank to my boots when the opening speaker stated that 'Consummation of time in discussion is validated by the gain in commitment of the participants'. I have noted some of the jargon sprouted by different lecturers and I can but apologise if I have made some mistakes in writing it down as my brain was beginning to regard my ears with suspicion. Nevertheless I feel that errors could only improve nonsense such as this:
(a) The subjective spectrum may be eidetically retained by ingesting a wider awareness of the prescription of training needs analysis.

...

(d) The harmonization of process and flexibility is a polymorphous concept postulated as the nexus of managerial efficiency.

The speakers also had a predilection for enclosing words in boxes, drawing lines between them and describing the resulting maze as a 'communication reticulation'.
I cannot understand the above phrases and would welcome some lucid interpretation..." (Gamblin, 1979, Chapter 21)

The subject areas of the papers at this symposium include terms like "best fit" and various criteria are used for such purposes. There appears to be renewed interest in information theoretic measures of "entropy" as one way to try to fit some theoretical structure to data. It is regrettable that we have no universal way to determine how we might choose one fitting scheme over another. It seems to me that our best hopes rest with incorporating the best physics, chemistry, and biology we can into our descriptions of nature. I remind all of the sage observations of Harold Thomas Jr. in his acceptance remarks on the occasion of his receiving the Horton Medal from the American Geophysical Union . "...the fact that frequency distributions of hydrology typically exhibit long straggling tails impair the utility of conventional statistical methods for hydrological analysis. ...decades of effort using approaches such as the method of moments and of maximum likelihood applied to conventional distribution functions ... have not substantially improved our ability to formulate flood and drought frequencies much beyond the level achieved by Allen Hazen and others using simple graphical techniques in the early decades of this century" (Thomas, 1978). These remarks were made before the development of "L Moments", (see, e.g., Hosking and Wallis, 1993), but the nature of the problem he addressed remains the same.

I am sure that some of the papers presented will lead us in new and fruitful directions but I doubt that any of us could predict accurately at present which will be the most productive paths. Much of what we do and report will have limited utility because of the framework from which we start. The observations reported by Hamilton (1991) that 72% of the papers that are published in leading engineering journals remain uncited by other authors should be cause for concern for all of us. When is our writing "doing our learning in public"? I come from the old school that holds that it is preferable for us to do our learning privately whenever possible and display selections of our work when we have had time to think carefully about it. The reward schemes in North American universities and research organizations and the need for scientific research program officers to show "progress" from those they have funded are not conducive to scholarship. The pendulum must be due to swing the other way.

We should strive to write as clearly and economically as possible. Examples are provided in the writings of our late colleagues Walter Langbein (e.g. Langbein, 1979) and Mike Fiering. Nick Matalas reminded us of Mike's rare skill when he wrote Mike's obituary: "Fiering gained professional recognition early in his career, and it was sustained throughout. His standing in the scientific community came not so much from the number of his publications, numerous as they were, but from the richness of their content. He used mathematical expressions sparingly in his technical papers. They were there, but no more than were necessary. He had the rare gift of being able to say what he wanted to say in words with as much clarity as could be conveyed by mathematics. If words could do, words would be used. It was this ability that allowed the richness of his ideas to be so readily appreciated", Matalas (1993). Other excellent examples include the papers and books by Hunter Rouse; his "Hydraulicians are Human Too!" (Rouse, 1972) is a gem.

An incisive paper by C. V. Theis, "Aquifer and Models", (Theis, 1967) is inspirational. Consider the richness and power of the following commentary about transport in porous media. "...However, the simple and useful model for problems of well field development will mislead us if we apply it to problems of transport, in which we are concerned with the actual detailed movement of the water. ..."It would appear that the transport phenomena associated with movement of water through a heterogeneous aquifer probably differentially anisotropic at different levels is rather analogous to the stirring of sugar into a cup of tea. This is a process of breaking concentrated sugar solution into thin filaments so that diffusion into the remainder of the liquid is accelerated. Similarly the diverse elements of an aquifer would appear to break any volume containing a traceable component into filaments whereby the process of hydrodynamic dispersion and diffusion are accelerated. By such processes and by the improbability of having completely reversed elements of anisotropy in the path of any small volume of ground water, the hydraulic mixing is made irreversible." (Theis, 1967, p. 146). Does not this tell us a lot about the difference between laboratory and aquifer scale dispersion?

How shall we judge our work?

In 1971 a colleague introduced me to the stunningly sharp observations made by Morris Ettinger about how he saw the state of research in what was then known as sanitary engineering. (It is largely the same today but now goes under the label "environmental engineering"). The four page long paper entitled "How to Plan an Inconsequential Research Project" was published by the American Society of Civil Engineers (Ettinger, 1965). I suspect that Ettinger annoyed more than one reader but there must have been good reason for a relatively conservative organization to publish such a critical paper. I quote:
"..With the vast quantities of money and energy currently (1965) being spent on research, our society may be presented with another bumper crop of research. Such a possibility is not remote and its consequences are too serious to contemplate calmly." He lamented the lack of accomplishment in the field of sanitary engineering and listed eight research categories (in no particular order) that he felt described the state of what was being done. I list the first six here.
"1. The Platinum Bridge;
2. the "me Too";
3. the Frog Swallows the Ox;
4. the Mathematical Doodlement;
5. the Details of the Inconsequential;
6. the Ivory Tower Sewer -- (a) The One-dimensional Creek in the Thermostat ..."

Those of you who have not read this short paper might guess what he wrote about each. I will save you a little time and share his thoughts on topic 4.

"The 'Mathematical Doodlement' is a popular bit with your professor types seeking to establish a reputation. Our academic halls are filled with new guys who cannot understand it or measure it, but at least they can claim to formulate it. In the absence of data this cult has developed some profound nonsense. Even where there are 13,000 good observations available, the doodlers have a knack for diving into the pile and coming up with the 13 that fit or almost fit the doodle formulations and disregarding the rest. A wealth of dissonant observations of his own does not discourage a doodler. He develops his equation, presents his data, which do not support it, and then concludes that doodle equation #602 is a major advance leading to new insights. Another leading doodle device employs the business of 5 to 16 experimentally determined constants and parameters. If you introduce enough of these you just can't miss. Of course, many of the doodlers age and get off this kick, but a new wave of kids fills in behind them." (Ettinger, 1965). Do you find any of Ettinger's observations close to the mark?

WHAT IS THE STATE OF OUR SCIENCE AND PRACTICE?

I have been influenced over the years by many individuals. We are the products of many things and are what we are as the result of what we learn by example and by doing. I benefit enormously from long discussions about what we are doing and why we are doing it and what really needs to be done. Proximity of colleagues is important but I have been fortunate that some who are far removed from my normal place of work have found time for good long chats. It is extremely dangerous to name influential colleagues because of the great risk of sins of omission. (I hope those who I name will not be troubled by possible sins of commission.) When I choose arbitrarily from the broad fields of hydrology and water resources and put an approximate filter based on age of a decade or so older than me, the list of individuals who have inspired me and whom I admire professionally and personally includes: Roy Beard, Gedeon Dagan, David Dawdy, Jim Dooge, Pete Eagleson, Vit Klemes, Luna Leopold, Pete Loucks, Nick Matalas, Don Nielsen, John Philip, Dave Pilgrim, Harold Thomas Jr., Jim Wallis, and Reds Wolman. Three who are no longer with us whose wisdom I miss are Mike Fiering, Walter Langbein, and Ray Linsley. I have had the pleasure of discussions with all of these present and late leaders and have a sense of their many dimensions. They are all notable for the clarity of their thinking, writing, and perspective on our field. Their written work spans the range from brilliant intuition, to reporting and interpreting painstaking observations, to incisive application of mathematics and mechanics, to practical matters of engineering design and operation of facilities, and to significant contributions in the policy sciences related to water resources and environmental engineering. I regret that I never had an opportunity for a long chat with Unny because on the occasions we were together there were always many others seeking him out.

I am a hydrologist in no small part because of the influence of Rupert Vallentine, the head of my undergraduate department in Newcastle, Australia, who arranged with Ray Linsley for me to do my graduate studies at Stanford. Both Rupert and Ray came close to being Renaissance Men. Ray was the finest intuitive thinker I have ever met. He seemed to be about two decades ahead of anyone else in his vision of what was needed in the broadest context of where hydrology and environmental considerations fit in the scheme of things. Many think of him for his contributions to hydrology and water resources engineering and engineering economic planning, but he had a clear and early picture of the need for pollution abatement and so advised the US House of Representatives' Subcommittee on Science, Research, and Development in 1966.

I mentioned the above individuals so you will get a sense of the diversity of views and opinions that I have filtered in reaching some of my own views. I have been influenced profoundly by immediate family members. All of my graduate student colleagues over the years have challenged and sharpened my arguments and have been more than gracious in learning with me. I am sure that many would have been just as happy if I had not thrust upon them the most recent article that I had read that was of a philosophical bent. With that as a preamble, I have to admit to having been influenced in the last year or so by the writings of the late Sir Peter Medawar. I think he has articulated what science is all about as well as anyone that I have read. I want to share a few of his observations.

In discussing "Experiment and Discovery", (Medawar, 1979, Chapter 9), Medawar states " Experiments are of four kinds; in the original Baconian sense, an experiment is a contrived, as opposed to a natural, experience or happening -- is the consequence of 'trying things out' or even of merely messing about". ... "*Aristotelian Experiments* ... This experiment, too, was contrived -- to demonstrate the truth of a preconceived idea or to act out some calculated pedagogic plot" ... "*Galilean Experiments* ... A Galilean is a *critical* experiment -- one that discriminates between possibilities and, in doing so, either gives us confidence in the view we are taking or makes us think it in need of correction. ... experiments are very often designed not in such a way as to *prove* anything to be true -- a hopeless endeavor -- but rather to refute a 'null hypothesis' ... The fourth category he described as "Kantian Experiments" which are thought experiments.

"... The forms of experimentation characteristic of the natural sciences are Baconian or Galilean; upon these, it may be said, all natural science rests. ... The effect of Galilean experimentation is to preserve us from the philosophical indignity of persisting unnecessarily in error ... . Any experienced scientist knows in his heart what a good experiment is: it is not just ingenious or well executed in point of technique; it is something rather sharp; a hypothesis does well to have stood up to it. Thus the merit of an experiment lies principally in its design and in the critical spirit in which it is carried out."

What forms of science provide the basis for hydrology and environmental engineering? While some would have us differentiate between what is science and what is engineering, I choose not to distinguish one from the other. It seems to me that much of the scientific foundation for hydrology and environmental engineering might best be described as Baconian while we need more Galilean science to make progress. If we consider environmental engineering to be involved largely with engineering decision making under uncertainty, then elementary stochastic methods help quantify our ignorance but may offer little insight to improvements we might make in interpreting and understanding the natural and human influenced environments.

Much of the science of both fields fits into the "what if" category and does not lend itself to rigorous hypothesis testing. Sufficient measurements are taken for decision making but too few measurements are taken for discriminating between likely possible causative processes. Much more work needs to be done where well posed hypotheses can be tested in a Galilean sense. Many of the questions we ask and try to answer are truly "trans-scientific", a term coined by Alvin Weinberg (1972) and introduced to the hydrologic community by John Philip (1975). Trans-scientific refers to "questions which can be asked of science and yet cannot be answered by science". To check this for yourself consult a representative issue of any of our major journals and decide how many of the papers have postulated hypotheses or tested them. Many of the approaches proffered would require measurements or descriptions that may be extremely difficult to make. The limited validation is often of the form of comparison of some flux which is the result of unidentifiable paths. Many combinations of paths and flux rates could generate approximately the signal which causes the calibrated model or system description to be accepted as "about right". The curse comes of course when we need to extrapolate. Many see hope in finding appropriate scaling laws. We all know that the central limit theorem bails us out as we combine enough random variables but that is not of much solace.

Environmental systems and engineering

I have long held that an effective way to proceed in all aspects of engineering is to follow a broad systematic approach. The earliest articulation of the systems method that I have found was that defined by Arthur Morgan around 1910 (Burges, 1979b, Morgan, 1971). Morgan referred to his approach as inclusive engineering analysis, which he defined as "The principle in essence is that, to whatever extent that the importance of the work justifies, every possibility for solution of the problem, whether promising or not, should be explored, with effort to become aware of unrealized and unexpected ways of approach; and that each possibility be explored to the point where, in comparison with other methods of solution, it either is proved to be inferior or finally emerges as the best possible solution" Morgan (1971, p24). The work of the Morgan Engineering Company in the Miami Valley, Ohio is testimony to science and environmental engineering at its best. (There are two generations of water and environmental engineers in North America who do not know about the pioneering flood damage mitigation works in the Miami Valley that have been in place for more than seventy years. Study these works; there is much to be learned from them). While the work is best viewed for its visionary application of science and engineering in the widest context to solving a disastrous flooding problem, it is characterized also by attention to details. To me there are two main issues in the best applications of environmental engineering: exploration of a broad range of alternatives that might meet the real objectives, and the three d's, details, details, and details.

One of the most enlightening statements of the "systems approach" that I have read (it has extreme relevance to the topics of this conference) was provided by American humorist Chick Sale (Sale, 1929). His statements were made by his character, Lem Putt, who was "champion privy builder of Sangamon County". Lem was advising a potential customer, Elmer Ridgway, about crucial considerations in the construction of a privy (Sale, 1929, p16). (For those whose experience includes only the benefits of functioning indoor plumbing the significance of Lem's attention to detail might not be apparent at first). Lem advises Elmer:

"Now, about the diggin' of her. You can't be too careful about that," I sez; "dig her deep and dig her wide. It's a mighty sight better to have a little privy over a big hole than a big privy over a little hole. Another thing; when you dig her deep you've got her dug; and you ain't got that disconcertin' thought stealin' over you that sooner or later you'll have to dig again."

He continued to advise his client about the need to do things properly (Sale, 1929, p. 24):

"There's a lot of fine points to puttin' up a first-class privy that the average man don't think about. It's no job for an amachoor, take my word on it. There's a whole lot more to it than you can see by just takin' a few squints at your nabor's. Why one of the worst tragedies around here in years was because old man Clark's boys thought they knowed something about this kind of work, and they didn't. ... From all outside appearances it was a regulation job, but not being experienced along this line, they didn't anchor her."

"Hallowe'en night come along, darker than pitch. Old man Clark was out there. Some of them devilsih nabor boys was out for no good, and they upset 'er with the old man in it. ... the old man had gotten so confused that he started to crawl out through the hole, yellin' for help all the time. (His) boys recognized his voice and come runnin', but just as they got there he lost his holt and fell. After that they just called -- didn't go near him. So you see what a tragedy that was; and they tell me that he has been practically ostercized from society ever since."

Do you see any parallels between Lem Putt and the activities of those involved with planning, designing, and operating water resource systems? This leads me to the wisdom of the late Mike Fiering. In one particularly incisive paper in 1976, Mike wrote about the entire enterprise of the "systems approach" and how we might be missing the needs of society. This message is important for all of us whether we be involved in development of water resource projects, operation of existing facilities, or planning research experiments.

"The early sparring and jockeying is of critical importance to the future of the proposed project because it usually is here that the ultimate decision-maker is identified. This is critical because of the importance of the objective function in system planning. The engineering literature is replete with mathematical models, optimization techniques, Bayesian analyses, exotic formulations for synthetic flows, and all manner of computer studies. We seek optimal plans, optimal operating policies, optimal estimates of parameters, optimal anything. We are swept up in a litany of automatic computation, sensitivity analyses and model making. It has become a new religion.

But what is generally overlooked is that none of these models can meaningfully be used unless the individuals impacted by them and by the systems which they represent can agree on the objective function. In the early days ... it was convenient and prudent to hold to the naive assumption that men of good will ... could mutually agree on an objective function whose maximization (or minimization, or whatever) would obviously be 'in everybody's best interest'. In retrospect, perhaps it was unwise to take this simple-minded approach because we soon became enamored of the technical issues without much attention to the fact that they are useful only insofar as they apply to social and economic situations in which the objectives are mutually agreeable". (Fiering, 1976)

I hope that the progression from the wisdom of Arthur Morgan and the practicality of Lem Putt to the warnings that Mike Fiering wrote almost twenty years ago does not indicate a trend. To Mike's cautions and observations we can add that there are now additional criteria and even more has been added to the literature that addresses the "nice problems" that enable the writers to demonstrate their mathematical proficiency but have little to do with the "wicked problems" of the world around us.

A new paradigm of environmental systems engineering is unfolding. The older paradigm is the one that served Mike Fiering, Pete Loucks, Warren Hall, and others well in the early days of the application of operations research techniques to environmental systems problems. Mike articulated beautifully the limitation of the approach. Pete Loucks pioneered display of systems information that would be useful to decision makers and planners. The new paradigm is less bound by mathematical abstraction and is more in tune with needs of people, ecosystems, how we represent system structures, and the direct linkage of computers (with software and hardware designed to allow for human frailty) into system operation. Two examples follow. A futuristic scenario for the application of user-friendly computers to hydro system operation has been proposed by Jery Stedinger and Chuck Howard (1993). Their approach embraces the best elements of all previous approaches. In the second example, Sheer (1991) stresses the difficulty and importance of defining objectives, and the value of developing joint operating policies for various facilities and the associated coordination between groups of users of water. I think Dan Sheer understood the message that Mike Fiering gave in 1976 and recognizes the extreme difficulty in developing management objectives, an area where critical work needs to be done.

The best modern work is a rediscovery of and implementation of elements of the work of people such as Arthur Morgan. The Morgan's, Howard's, and Sheer's, practitioners of our profession, give us much to cheer about; they have never been constrained by the shackles of the mathematical formulations of nice problems. They are attracted to wicked problems and offer creative total system solutions. Many research questions have been and will be generated from application of their approaches to environmental systems problems.

The bulk of all problems worthy of our attention are "wicked", a term coined by Jon Liebman in the 1970's. All of us should strive to do other than add small increments with equations that were developed for parts of larger problems. Little is to be gained by following an inductive approach that only can interpolate within what is known. It will do nothing to enhance our broad understanding of environmental systems, hydrology, and the practice of environmental engineering. Medawar (1982) gives strong arguments against the inductive approach. What is always needed is lucid writing about the state of the world and the small part we play in it, and brilliant experiments and interpretations of them.

Scientific Basis of our work

Some have argued that we can do much these days with our existing understanding and the associated mathematical quantification of our knowledge and information. This may be appealing to those who are concerned about possible harm from ultraviolet radiation so they have ample reason to do their work in offices and make use of sophisticated computers. I cannot help but wonder if such folks have followed the debate on the possible harmful effects on humans of electromagnetic fields. At any rate the beauty of theory and the mathematics and computational devices that are needed to solve the resulting models does much to keep many talented people occupied relatively harmlessly. Of course major problems await attention. Without skilled observation much of what is attempted fits either the Aristotelian or Kantian approaches to science. Werner Heisenberg summed up this situation succinctly: "The transition from the 'possible' to the actual takes place during the act of measurement". Nowhere is this illustrated better than in three examples given by Gerhard Jirka.

In the present era when some eminent hydraulicians have commented that much of what needs to be done can be done numerically, it is comforting that scientific progress is still being made by those who conduct careful and thoughtful experiments in conjunction with development of theory. Jirka (1992) reported, in a paper based on his 1989 Ippen Lecture to the International Association for Hydraulic Research, on experimental findings that elucidated three important phenomena in environmental fluid mechanics. The first was concerned with salt water intrusion control in strongly stratified estuaries. There was no way possible for someone starting with physical descriptions of fluid movement alone to elucidate the controlling phenomenon. The second example involved gas transfer at the air-water interface. Adequate equations for the relevant phenomena do not yet exist. His third example was for coherent structures in turbulent shallow water flows. Here the phenomena of interest involve large scale periodic motions. Understanding such motions will be crucial for progress in a broad range of environmental fluid mechanics applications. The processes are not yet sufficiently well understood for numerical modeling representation.

All manner of Kantian experiments could be performed but none could describe adequately the difference between dispersion at the laboratory and macro-scale dispersion at field scales. The late C. V. Theis commented on the puzzle of macro dispersion in ground water flow in an interview with John Bredehoeft in 1985. The puzzle, taken up by C. V. Theis (Theis, 1967), Harold Thomas Jr. at Harvard, and David Todd at Berkeley and their students in the early 1950's, has occupied some of the best minds in the world for at least the last two decades. Stochastic theories of various complexity have been developed to help explain field observations. At age eighty-five, C. V. Theis reminded John Bredehoeft of his (Theis, 1967) observation that the field scale phenomenon might be explainable by refraction as flow passes through different conducting regions which clearly span a range of distance scales. The scale issue of dispersion is not limited to flow in porous media. John List and colleagues (List, et al, 1990) describe the influence of spatial scale on dispersion in the coastal waters of Southern California. C. V. Theis's observations and the findings of John List and colleagues should be heeded by those planning measurement programs from which they will infer dispersive properties.

STOCHASTIC AND STATISTICAL METHODS -- FUTURE DIRECTIONS

I have commented elsewhere (Burges, 1986, James and Burges, 1982a) about some future directions in the science and practice of what we do. With hindsight, I realize now that I wrote little about what stochastic and statistical methods offer. Stochastic methods are enormously useful and powerful for interpolating within our data but cannot be substituted for needed experiments or measurements. I have used simple stochastic methods to gain some approximate "what if" sense of what might be expected in future inflow scenarios to reservoirs. I have used statistical and stochastic methods to get a sense of uncertainty in flood magnitudes and spatial drought frequency. The period between about 1965 and 1985 saw the major developments in stochastic methods that help us in engineering decision making, data collection network design, and quantification of aspects of uncertainty. Stochastic and statistical methods offer few ways to add to our knowledge although they help us extract information from data. As useful as stochastic methods have been for me, I have placed new emphasis on understanding hydrological processes so that I will have a better understanding of extremes of floods and droughts, flood plain inundation, the hydrological consequences of land-use change, and many other problems.

Stochastic methods have many uses but largely in an interpolative sense or for helping describe phenomena that would be difficult to characterize otherwise. Stochastic methods ought to have something to add in work related to the influence of climatic variability on ecosystems and societies. Before we launch too far in application of such methods, we need to note the sage observations of thoughtful individuals like Gilbert White (1991) about the relative importance of various aspects of any changes in variability or climatic norms. Helpful guidance is provided in the writings of Klemes (1990) and Dooge (1992). Vit Klemes reminds us that much of what has been contributed to the water resources management aspect of the climate variability discussion fits squarely in the "what if" category and is not helpful for decision making. Jim Dooge has us revisit the work of Robert Horton and suggests the use of some form of larger scale calculus rather than the smaller scale Newtonian "fluxions" that are the basis of much of what is written.

Spatial and temporal statistical and stochastic methods have been put to good use in the broad fields of water resources and environmental engineering. We have already a few well known acronyms, ARMA, DARMA, PARMA, and so on. (The list appears to have been developed by an alumnus of the U.S. National Aeronautical and Space Administration!) Most environmental decision making problems involve multi-site (spatial and temporal) situations. Efforts that yield improved multi-site schemes will be more useful than those expended on univariate schemes where the gains will be marginal. Extensions from the univariate to the multivariate domain involve much more than changing notation from scalars to vectors and matrices. Development of a practical multisite ARMA synthetic flow model was far from routine (Stedinger et al, 1985).

The problems of concern in surface water hydrology are driven largely by an annual climatic cycle. Variations about that cycle of practical importance are aperiodic so any schemes that impose periodic structure to time series to facilitate their parameterization are unlikely to be useful; the stochasticity of greatest interest is masked by the forced structure of the model. All of the time series model structures are regressions of one form or another and depend on strengths of correlations for their utility. They are best for describing near average conditions (the nice problems). They are least suitable for providing risk measures for the extremes which are the "wicked problems" that cause greatest disruptions to society. We need physically supportable marginal distributions and sequential conditional probabilities of levels far removed from the average process level. I can see no way that massaging statistical and stochastic methods will yield answers to these issues, which remain largely trans-scientific.

There remains a need for ways to disaggregate from larger time and space scales to smaller time and space scales. This problem is faced by those working to couple catchment scale hydrology with the hydrology represented in large-scale general circulation models as well as those who wish to disaggregate total storm rainfall depth to temporal patterns for use in small-scale catchment hydrology. The stochastic disaggregation methods we have were developed for situations where process dynamics were relatively unimportant (streamflow volumes). They satisfy mathematical expectation of the relevant random variables and preserve a few central moments. What is needed is some way to provide patterns that are much more in accord with nature where dynamics are important. Disaggregation of precipitation volumes remains a thorny problem. In recent work, I could not find any representative patterns for disaggregating total large-storm rainfall volume that would yield realistic temporal patterns. Unny contributed to this broad class of problems by developing pattern recognition algorithms for streamflow volumes (Panu and Unny, 1980). I fear, however, that the successes that were achieved for streamflow volumes will not be repeated for precipitation patterns.

CLOSING THOUGHTS

We gathered here to honor Unny and to be challenged and refreshed. Addresses such as this are given usually by someone elderly, or by someone elderly and distinguished. I was not sure when Keith Hipel and his colleagues asked me to be with you this evening if they were commenting delicately upon my advancing years or were establishing a new criterion for banquet speakers. I chose to assume the latter and in that light have a few closing observations.

Our work must build upon a solid scientific foundation. Much of that basis was provided for people of my generation by previous scholars but hydrologic laws remain elusive (Dooge, 1986). During my twenty-three years as a member of the research community, I have observed many exciting developments. Some of the best have been the beautiful experiments performed at the Borden site in Ontario, Canada, and the interpretations of those data sets. Our data are precious and they are all we have! Multiple challenges have been posed by contamination of soil and ground water as a result of too little attention by many for the well-being of future generations. Our knowledge is still being developed to approach such problems. Stochastic treatments have yielded helpful ways to interpret the sparse data from the field for such problems but much needs to be done. I am at best a lay reader in this broad field but it seems to me that models and methods for progress will need to build on the type of work defined initially by Allan Freeze, Gedeon Dagan, and Lynn Gelhar. Brilliant experiments and observations are needed in the laboratory to help formulate relevant process descriptions. I do not have a full knowledge of the work in this field but it seems that much is to be achieved by following up on the visualization approaches used by John Wilson and colleagues (Wilson et al, 1990, Wan and Wilson, 1994). I am but rephrasing and repeating the visionary comments of Theis (1967): the efforts of many will be needed to test developments stemming from laboratory investigations at field sites.

I have given less emphasis to subject areas that I know best but remain fully aware that much needs to be done in the entire spectrum of the fields of surface water hydrology, hydro-climatology, landscape evolution, and in water resources and environmental engineering. Pete Eagleson chaired a committee of the US National Research Council (1991) which produced a report "Opportunities in the Hydrologic Sciences" which contains a flavor of the types of puzzles and problems that need our attention. The dominant message in that work and in the report of an earlier committee chaired by Mike Fiering (National Research Council, 1982) is the need to continue developing the science of our field. There is much to do. Those committees have served the role, in part, of world class composers. As individual policy, physical, and biological scientists and engineers, we are the musicians who will remain little other than limited skilled technicians until we learn to perform harmoniously as an orchestra. We cannot afford to wait for the appearance of a world-class conductor to lead us, whomever she may be.

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