DISCUSSION of "Editorial—Quantifying the impact of hydrological studies" *

Apocrypha, or "things that are hidden"—personal experience with "hidden" impacts over the past 50 years

V. KLEMEŠ
Water resources consultant, 3460 Fulton Road, Victoria, British Columbia V9C 3N2, Canada

The only matter of some substance where I differ from the 2007 Editorial by Koutsoyiannis & Kundzewicz (henceforth K&K) is their assessment of the increase of the rejection rate for submitted papers. If this increase has been the result of the "spectacular improvement" in *HSJ* indices, which in turn has attracted more submissions, then I do not regard it as "an unfortunate consequence" but as a useful supplementary index of *HSJ*'s improved quality; needless to say, its value should be regarded with caution, since, even in this age of severe information pollution, it is not necessarily true that the best journal would be one with a 100% rejection rate, notwithstanding the often intruding temptation to believe so.

I note with satisfaction that the "quality first" principle, which I had rather ineffectively promoted during my tenure as IAHS president 20 years ago, has finally prevailed. The then popular argument against it—that *HSJ* has to be "softer" on quality because one of its main objectives is to "support hydrology and hydrologists in less developed countries"—I have always regarded as fallacious and condescending. For, raising the level of these countries from their "less developed" state can hardly be achieved by offering them a "lesser quality" science. It is thus praiseworthy that the present *HSJ* editors attempt to meet the above objective by offering these countries high quality science on the one hand and, on the other, by engaging *HSJ*'s editorial board members and referees in helping their authors to improve the quality of their submissions.

The explicitly declared purpose of the Editorial is "to assess the current state in *HSJ*, in comparison to other hydrological journals". The specific object of this comparison is the journals' effectiveness in "dissemination and impact" of the published papers, as reflected in the numbers of their citations. K&K make it clear that such comparison does not do full justice to an assessment of the impact on "the state of hydrology", either of the particular papers or of the contributions of their authors and, in particular, of hydrologists who made contributions in other ways than by writing papers. They rightly point out that "the number of citations measures the popularity of an article" rather than its importance and impact with which it is only "positively correlated". However, neither the "popularity" of a published paper, nor its further "dissemination" and awareness of it, are fully reflected in its citations—and K&K mention many other factors partaking in the shaping of the state of hydrology which are virtually impossible to quantify.

To facilitate a better appreciation of such factors, my purpose here is to flesh out their general picture sketched in the Editorial by examples—all of them in the category of unquantifiable "anecdotal evidence"—drawn from my experience, over the past fifty years, as user of publications both in engineering practice and in research, as author, referee, editor, and in other capacities including chairmanship of a Publications Committee advising the management of Environment Canada on promotions of research scientists.

As a good example to start with can serve my "Hurst paper" (Klemeš, 1974), whose aim was to consider some possible physical causes of the Hurst phenomenon, as opposed to mathematical models that can generate it. As I am told (I myself stopped registering citations of my papers after

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retirement when I was no longer obliged to submit annual "Employee Appraisal" reports where they were one of the important indicators of a scientist's performance), the paper has registered over 100 citations, indicating a reasonable degree of its popularity (or at least awareness of it) and its possible impact. But the degree of correlation between these factors and the number of its citations may be insignificant.

For example, one Harvard professor who, as far as I know, had never cited the paper, had certainly increased the awareness of it. For, as he once told me, it was one of his favourites that he included in the set of "compulsory reading" for all his graduate students. Conversely, the relative "popularity" of the paper reflected in the number of its citations has had little impact on the *status quo* in hydrology, since the vast majority of "Hurst phenomenon research" is still focused on mathematical modelling of "long-memory" processes, rather than on the understanding of the physical causes of the phenomenon.

On the other hand, the paper could have had an impact outside hydrology; and it was neither generated by its citations, nor is it likely to generate any. I learned about it from a short note that, out of the blue, once popped up in my e-mail:

"*As I had cause to reread your article today, I just had to write and tell you what a cracking read I believe it to be. As infinite memory processes are dragged into every new field with a dataset, history repeats itself: the engineers of the field begin to believe that their phenomena can only be explained through mystic long memory and everyone forgets about the physics. Every time I attend a talk which insists that long range dependence is the only phenomenological explanation to unusual statistics, I am very glad to have your article as a reference for the speaker ...*"

This note was sent by an Irish mathematician (to whose attention the paper had been brought years ago by his graduate supervisor) who later explained to me how, for example, my suggestion—that the Hurst effect could result even in a zero-memory process if its mean kept changing because of some irregularly occurring physical signals—can help in understanding the patterns in data sets transmitted in "internet traffic":

"*One such famous dataset was a digitised version of the Star-wars film, digitised by Bellcore. If one looks at the amount of data being sent, it appears (unsurprisingly) to have level-shifts from scene to scene. Within a space battle, a lot of information is transmitted, within a love scene, little is sent. Of course, as you rightly say in your article, from a single sample-path one cannot infer anything about what produced it without additional assumptions, but it seemed relevant to re-iterate your '74 remarks to this new audience.…Unfortunately the community of tele-traffic engineers are still more aware of the long range dependence work than the suggestion that the phenomenon can be explained by non-stationarity. This is why I had cause to read your paper again ...*"

Well, now at last I can better understand why my Hurst paper didn't have much impact in hydrology: it surely is easier to visualize physically caused "level-shifts" between space battles and love scenes than it is between runoff in different time periods.

Even more surprisingly, the paper may have had another unexpected, and certainly unintended, impact not reflected in its citations. One of my senior colleagues at the University of Toronto (U of T) was always puzzled when I mentioned "stochastic hydrology" since he had "never heard of such a thing". This was not surprising since, in the bygone days of 40 years ago, hydrology was treated as an appendage to fluid mechanics at the U of T and, as such, was taught by the Mechanical Engineering Department. And the expertise of this colleague was not even in fluid mechanics but in thermal power and heat engineering; moreover, the paper's impact was not in these areas either but was related to a course on "Technical writing" which this colleague was offering as a sideline. The paper appeared after I had left the university but, remembering my former colleague's amusement with my "stochastics", I sent him a reprint of it. Here is an excerpt from his response:

"*I am intrigued by 'The Hurst Phenomenon: A Puzzle?' At first glance, I thought , 'How on earth can Vit credit me with knowledge enough to read this', but at a further glance I perceived what I*

can only call the charm of beautiful, sophisticated (and simple) English exposition. I am very glad to have this paper. I will read it thoroughly (granted my limitations) and use it, I hope, effectively; I am tired of referring students to the British Medical Journal or the New England Medical Journal to see how writing can present, sustain and control technical evidence and argument. I now have a golden example in advanced engineering to show them".

It was beyond my wildest dreams that the paper, written in my Czecho-English idiom, could have a positive impact on English writing. By this note I was more surprised, flattered and proud (the more so when I later learned that my colleague had won a literary award for one of his short stories), than if the paper had converted Mandelbrot himself to my view (which of course didn't happen, though he cited it once and, if it caused him to hold any grudge against me, he didn't show it when we later met at my work place and at our home).

The preceding examples illustrate some "citation–independent" routes to both the awareness of a paper and its possible impact. But an even more unlikely case is when a paper generates an impact without necessarily generating any awareness of it. This may have happened to my "Dilettantism paper" (Klemeš, 1986), as suggested in a letter received from an eminent Irish professor:

"*I have just read, for the first time, your paper 'Dilettantism in Hydrology: Transition or Destiny?' It is certainly a remarkable document. My reaction to many passages was 'I would like to have said that!'... I shall try and change the emphasis in our hydrology courses in accordance with your thinking ...*"

An important route to a paper's impact is via its citations in textbooks. By coincidence, the above paper may also have benefited in this way, as was suggested by a letter from an influential American professor who, curiously, used similar words to his Irish counterpart:

"*... in the course of my research* [for a new edition of his hydrology textbook] *I came upon your 'Dilettantism in hydrology' in WRR. All I can say is that I wish I had written it. In a word: Bravissimo!!! With your permission, I intend to quote several of your statements...*"

And even more important for a paper's impact is if a textbook not only cites it but adopts its methods, models or approaches, or endorses its findings. For me, particularly memorable examples of this were my very first two papers (Klemeš, 1960a,b) on graphical methods for flood transformation by a reservoir, both published in the Czechoslovak (henceforth abbreviated as CS) journal *Vodohospodársky časopis* or *VČ* (now also designated as *Journal of Hydrology and Hydromechanics* or *JHH*). The method proposed in the first paper was reproduced in a Polish textbook on hydrological forecasting already in 1962; and the method presented in the second—a simplification of the first—in an influential Slovak hydrology textbook a year later. Naturally, I was extremely flattered to see the two "Klemeš methods" in textbooks (one of them published abroad at that!). I then did not foresee that, 20 years later, it would fall to me (Klemeš, 1982) to "burst the bubble" and show that all similar graphical methods—on which many a reputation had been based—can be reduced to a few lines of computer code which an average third-year engineering student could now write during a coffee break. So I was greatly amused when, another 20 years down the road, I read in the *VČ/JHH* a *laudatio* for my 70th birthday that I was the author of the "famous" (in the English version), even "legendary" (in the Slovak version), Klemeš method.

This example also illustrates the point made by K&K that the impact of a paper (and the "fame" of its author) greatly depends on the kind of journal (national *versus* international) and on the language in which it is written. Should anybody doubt this, let him just ask any hydrologist who is a native English-speaker whether he has ever heard of the famous Klemeš method (to do justice to myself, I may note that, while not exactly famous, my second method is so embarrassingly simple that it is the only thing I have ever written that I can even now reproduce from memory). I thus emphatically endorse K&K's recommendation that papers submitted to *HSJ* should better be written in English.

And here I might add an additional recommendation of my own, namely that, as far as possible, they should be written in a grammatically correct, rather than politically correct, English.

This means, among others, that nouns of the common gender such as "anybody", "hydrologist", "researcher", etc., should be referred to by the masculine pronoun "he", rather than by "he/she". Even editors sometimes sin in this way which could cost the publication they edit a potentially interesting paper thus lowering its IF. This once almost happened to an editor of proceedings from an international conference, who sent me the proofs of my invited paper with corrections of my "he" into "he or she" when I was referring to "engineer", but not when I referred to "drunk" on the following pages. I advised the editor that I would withdraw my paper if he insisted on his "corrections".

K&K also note that one reason why the various impact indices usually do not reflect papers' citations in books is because the most influential data sets such as the ISI seldom include sources other than journals. However, even more important reasons may be that users of textbooks, handbooks and similar publications either do not feel the need to cite these at all, or they may cite only the book itself (rather than a paper cited in it), or mention only the names of authors referred to (e.g. "Nash model", "Gumbel distribution", etc.). And, above all: **most readers and users of books, journals and other publications do not themselves write books, papers and other publications**, thus having no opportunity to make citations (this may partly explain why almost 50% of the almost 40 million articles "tracked by ISI … have not received any citation at all"). They are students, employees of hydrological offices, designers, managers, and other practitioners. In such cases, the only trace of a possible impact is correspondence, remarks made in the course of a conversation, and similar "anecdotal evidence".

The following example is instructive. It is an excerpt from a long letter from a gentleman working for the Transportation Department of the Alberta Government:

"*I would like to thank you for publishing the collection of papers in* Common Sense and Other Heresies [Klemeš, 2000] *... My current role is to set and maintain standards for bridge planning, essentially the design of bridge hydraulic openings ... With as many different ways of performing a frequency analysis as there are consultants doing it, I set out to learn a lot more about the method. I started with the books I learned from when I went through the university… Casting further afield I read Leopold, Thorne and Rosgen plus numerous papers of the mathematical kind. It was beginning to dawn on me that there were two main approaches to hydrology: mathematical and physical. When I read your book it all came clear and set in motion a minor revolution. ... Again, thank you for your life long battle. It appears you made little progress with the mathematical crowd, but in Alberta at least, you have made a huge impact on the people who actually do the design work. Let us hope the revolution spreads.*"

Another example of an uncited impact of one of my papers is an excerpt from the probably most touching letter related to my work that I have ever received. It was sent from "down under" by a student in the Department of Geology in the University of Canterbury. She wrote:

"*I have just finished reading your article 'A hydrological Perspective'* [Klemeš, 1988]. *Having completed an undergraduate degree on geology, a postgraduate diploma in engineering geology, and somehow floundered into a hydrogeological doctorate, ... I found myself not only incredibly ignorant and ill-prepared for it, but suffering a monumental identity crisis. What was this hydrology thing all about? ... Torn between geologists pushing technology because they could not cope with the science ... and engineers talking/using/breathing conceptual models without too much consideration of the field realities and whether the final numbers actually meant anything, it was like trying to find the cheese at the back of the refrigerator in a power cut at midnight! ... At last however some clarity, thanks to your article ... Thank you, I still haven't found the cheese but the light has just clicked on in the refrigerator.*"

While examples like these are not reflected in the various impact factors and indices, for me personally they mean more than all those factors and indices put together (sorry, K&K, no offence meant).

The last two related aspects that I would like to touch upon are *politics* and *ethics*.

Politics can have a profound effect not only on impact factors of journals, papers and authors, but on the state of hydrology in entire countries. In recent memory, this effect was widespread

during the Cold War era and its consequences are still being felt. The best example I know from personal experience is the case of the former Czechoslovakia. After World War II, the dominant figure in the country's hydrology (a professor, a member of the Academy of Sciences, the founder and first editor-in-chief of *VČ,* the founder and first director of a hydrology research institute—where, by the way, he offered me a position for which I have always been grateful) wrote an influential textbook adopted by all CS universities, in which he set the tone. In its introduction he declared that, while the pre-war hydrology in the country was influenced mainly by its developments in the Western countries, "*... after the Czech and Slovak hydrologists have gradually become familiar with the Soviet hydrology* ***due to the availability of the Soviet literature*** *after the liberation* [in 1945]*,* ***they abandoned the*** [Western] ***methods personifying stagnation and started adopting*** [Soviet] ***methods*** *which were more appropriate for the new planned economy*" (my emphasis). Exactly! But this was in a great measure because in the list of "available literature" the author gave not a single reference to American, British or French sources, while listing 27 Soviet books and papers. For the professor was also a high-ranking member of the Communist Party. Needless to say, the "nonavailability" of western literature was merely a smoke-screen for his political bias because, for example, his colleague professor's textbook on dams and reservoirs, published about the same time, listed dozens of American, British, French, Swedish and other Western sources, as did, for that matter, the earlier mentioned Polish textbook. And—bizarre as it may sound today—I have learned about the Unit Hydrograph, about Hazen's, Sudler's, Horton's, Varlet's work, about the books by Brater, Meyer, Linsley-Kohler-Paulhus and other westerners—guess where!—from the "available Soviet literature" listed in the textbook.

This experience may have been one of the reasons why I attach much importance for "dissemination and impact" to textbooks and other books. The other reason I explained in the first paragraph of my Preface to a recently published selection from my papers (Klemeš, 2000): "*...to be well rounded, a book would have to encompass a much broader area than is the patchy field of my first-hand expertise...*". How strongly I feel about hydrology textbooks can be glimpsed from the following example. About a quarter of a century ago, one prospective Canadian author applied to the Inland Waters Directorate of Environment Canada for a grant of $10 000 for the publication of a hydrology textbook he intended to publish. His application landed on my desk with an attached slip asking for "recommendation of action". After looking at the book's outline overflowing with Bayesian analyses, cross-entropy, Wakeby distributions and other latest "advances in hydrology", I scribbled on the slip: "Offer him $20 000 if he drops the idea". A corollary: a renowned Austrian professor once told me he was writing a hydrology textbook. Knowing his work reasonably well, I was looking forward to it and, when we met about five years later, I asked him when his textbook would appear. "I am still working on it" was his reply. Another three or four years went by before we met again and I still had not seen his book, so I asked him what happened. "I dropped the idea. In the end I realized I didn't know enough hydrology…" and, after a pause, "I mean enough to write a textbook".

As for the effect of politics on individual authors, publication and citations of their papers, I don't have to search far from the above "available literature" example. After my emigration to Canada following the 1968 Soviet-led invasion, I dropped out of my former director's favour and the Klemeš method, occupying three pages in his textbook, apparently ceased—together with the eight papers I had by then published during his tenure as editor-in-chief of *VČ*—to be "appropriate for the planned economy". For none of these appeared in the 1972 special issue of *VČ* commemorating the 20th anniversary of my former work place which, supposedly, cited all papers published by the institute's scientists during its 20-year existence. Not surprisingly, vanishingly few Czech and Slovak authors dared to cite my papers during the period from the 1968 invasion to the 1989 "velvet revolution"". Curiously, my papers were quite often cited by Soviet authors, sometimes in considerable detail; after all, "*Quod licet Iovi, non licet bovi"*.

The effect of politics on publications was similar. To test it, I adopted Noah's method of sounding the waters and the habitability of the Earth after the Flood. But, instead of sending ravens and doves from an ark, I was sending (via my director's official submissions to make them less

objectionable) an occasional paper or discussion to *VČ* from Canada. And back they were coming rejected, with explanations of the editor-in-chief that they could not be accepted, "not because of their scientific content but because of the person of the author". I eventually abandoned this method when the *VČ* assistant editor, my good old friend, used the opportunity of his visit abroad to call me, imploring me to stop sending my "probes" because he had to draft the embarrassing rejection letters for the chief editor.

Such practices were not limited to Czechoslovakia, an were even more common in non-technical fields. An amusing example from the former East Germany (DDR) comes to mind: after one dissident writer had been banished, harassed and jailed for years, the International PEN Club appealed to the DDR government to stop his persecution, pointing out the writer's literary qualities and renown. The government's reply was, in a nutshell: "What renowned writer?? He didn't publish a single book in ten years!"

On the other hand, sometimes the political impact can work in reverse, so to speak: rather than politics having impact on an author, an author can have an impact on politics, even without writing a book or paper or otherwise intending it. I had two such experiences during the tenure of my IAHS presidency.

One was connected with the 1988 international symposium on mountain hydrology convened by IAHS (and co-sponsored by UNESCO and WMO) in the Slovak High-Tatra mountains. I was determined to open it, but, as a person sentenced *in absentia* to two years in prison and confiscation of all property for living in Canada without permission of the CS government, I was initially denied a visa, a standard policy in such cases. But, after our notification of the CS national representative of IAHS to the effect that denial of my entry into the country may jeopardize our sponsorship (on our instigation, similar notifications were sent by UNESCO and WMO), at the last minute I was issued a "special visa", the CS ambassador stressing to the Canadian Department of Foreign Affairs how difficult it was because I had to be "decriminalised for two weeks" by six CS ministries.

My other brush with politics resulted from a remark in my "From the President" column in the *IAHS Newsletter* (Klemeš, 1989) that "… I hold South African hydrologists no more responsible for their government's racial policies than I hold Chinese hydrologists responsible for the Beijing massacre". I made this remark in response to my Minister's denial of permission to my accepting an invitation of the South African National Committee for IAHS to deliver a keynote address at the 1989 South African National Hydrology Symposium. For—as I argued to my superiors—there had been no objections to my official visit to China (this paragraph may be of particular interest to *HSJ*'s editors since the incident made life difficult for the editor of its sister publication). As it happened, a politically astute colleague of the editor made him change my "Beijing massacre" into "policies of the Chinese government" which change could not be consulted with me due to my absence from home. The editor reached me after my return, reporting that the *Newsletter* was ready to go, with "just a minor change in the wording in your column". When I learned what the "minor change" was, I told him that if "Beijing massacre" is not restored, he would have to print my resignation letter in the next issue. And so the poor editor shredded the several hundred copies of the *Newsletter* and had a new batch printed and distributed. We still remember that "bad day" when we occasionally meet. But, as I later learned, the half-sentence in my column (which certainly has not registered any citation) considerably boosted my standing with both the South African and the Chinese hydrologists.

Turning now to ethical aspects of papers and their citations, I regard as crucial the fairness in giving credit where credit is due. I disapprove of the practice common, for example, in Canada and the USA, to include among a paper's authors the names of professors, office chiefs, and other persons who did not contribute to its scientific content and provided only financial or logistical help; the proper—and obvious—place for such acknowledgement is the Acknowledgements section. I also disapprove of the common (in my days, anyway) European university practice, where a professor gave only a one-line acknowledgement for "help" to his assistants and graduate students, who often were genuine coauthors of his books—and sometimes even that was missing,

as once happened to me: instead, I received a copy of the book with a dedication "To dear comrade Klemeš with thanks for help".

The above principle also holds for citations. I disapprove of the rather common practice where a junior author cites papers of his graduate supervisors, former professors and mentors, just as a gesture of courtesy and respect. Here I can give a rather amusing example: a junior American author with whom I had become acquainted used to send me drafts of his papers for comments. In one of them he proposed what he thought was a new metric for assessing the hydrological effectiveness of a reservoir. In the paper, he cited several of my papers not directly related to the matter, but did not cite a paper by Hazen in which he introduced this measure already in 1914. So I proposed to the author a deal: to exchange two citations of Klemeš for one of Hazen. Of course, the lack of a citation may not be intentional but due to a limited time horizon of author's literature search, language barrier and other reasons already mentioned. It is unethical only if it is deliberate with the intent to claim the author's complete originality (the absence of references in Einstein's celebrated 1905 paper on special relativity has been suspected of this by some historians of science who have pointed out that the paper owed much to the Michelson-Morley experiment, and the work of Lorentz, Poincaré and others).

A rather crafty infringement on ethics I once encountered in an author's acknowledgement. He gave a talk at our institute on the effect of climate change on runoff, in which he presented more than 200 different "scenarios" constructed by combining all incremental changes in all forcing functions, regardless of whether a particular combination made physical sense. In my office after the lecture, he asked my advice for the best place to publish his findings. I pointed to my waste basket and changed the topic. To my surprise, I later saw his "findings" published in a paper, with an acknowledgement of my "valuable advice". I have reasons to believe that the acknowledgement should have hinted that I had refereed, and approved of, the paper.

In closing, I would like to congratulate Drs Kundzewicz and Koutsoyiannis for their thorough and thoughtful Editorial and hope that my comments and observations may help appreciate the difficulty of the task they chose to tackle, and somewhat mitigate the "tyranny of impact factors" they have pointed out. Their conclusion that "The number of citations to published articles provides a satisfactory (but not ideal) basis to characterize their dissemination and impact" should be understood in the same spirit as Sir Winston Churchill's comment on democracy that he made in a speech to the House of Commons in November 1947: "No one pretends that democracy is perfect or all-wise. Indeed, it has been said that democracy is the worst form of Government except all those other forms that have been tried from time to time".

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