

Current Comments[®]

How Sweet It Is— The ACS Patterson-Crane Award. Reflections on the Reward System of Science

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[Most people correctly think of ISI[®] as the publisher of Current Contents[®] (CC[®]) and Science Citation Index[®] (SCI[®]). However, they do not realize that we also produce a group of information services for organic chemists. In fact, the first of these services, Index Chemicus[®] (IC[®]) (now Current Abstracts of Chemistry and Index Chemicus[®] (CAC&IC[®])) was started in 1960. For more than 20 years, then, we have been in direct competition with Chemical Abstracts (CA).

That is one reason I was gratified to receive an award from the American Chemical Society (ACS), Dayton and Columbus Sections. Named for two influential editors of CA, Austin M. Patterson (editor from 1909 to 1914) and Evan J. Crane (editor from 1915 to 1958), the award is for contributions in the field of chemical literature, especially in the documentation of chemistry, chemical information storage and retrieval, and implementing and managing chemical information services. Actually, this was my second award from ACS; the first was the Herman Skolnik Award given to me in 1977 by the Division of Chemical Information.¹

In my acceptance speech for the Patterson-Crane Award, an adaptation of which follows, I speculated about the reasons I did not more actively pursue

research in chemical documentation after 1961—but instead focused on the scientometric applications of citation indexing.]

To be altogether candid with my friends in ACS and in the information profession, I am led to quote that genial polymath of American humor, Jackie Gleason: "How sweet it is!"

I am truly grateful that you have recognized my chemical information proclivities. Yet there is a bit of irony in this award. That irony has several facets. The first is that ISI did not make it "rich and big" in chemical information *per se*. ISI is known primarily because of CC and SCI. With all due respect to ISI's Bonnie Lawlor, vice president, and my other colleagues in ISI's Chemical Information Division, IC made us both loved and hated, but so far it has not been very profitable. For this reason, many people at ISI justifiably feel that it has been, primarily, a labor of love. It was not planned that way.

The *Index Chemicus Registry System*[®] will soon go online with the *Questel* system. We hope that this step will be rewarding financially as well as intellectually. But I believe that its greatest benefit to the world of chemistry will reside in its spurring CA to better things.

I said that IC, or CAC&IC as it is now known, is loved and hated. It is loved by

a relatively small group of devoted followers in the chemical and pharmaceutical industry and in academe. Most of them continue to use *CA* and other tools. So *CAC&IC* provides them with something special they are willing to pay for anyhow.

CAC&IC has been hated not only by some of our competitors, but even by some at ISI who felt that our energies should have been devoted to more profitable endeavors. Nevertheless, we engaged in a kind of holding action. We did pioneer in the use of Wiswesser Line Notation, which now makes it possible for us to go online in a significant fashion. But the use of Wiswesser Line Notation has not been widespread for various reasons.

About 20 years ago, I made the decision to pursue intensive research on citation indexing rather than chemical nomenclature systems. Some might say that this was done as a simple matter of realism. Could we really have matched the Goliath called *CA* in light of the great support it had received from the National Science Foundation (NSF) and elsewhere?

Still, there were other reasons beyond the economic. Before turning to these, I should report that in the early days of our chemical information system, I was so obsessed by the structural and linguistic approaches to chemical classification that I neglected the significant potential of citation indexing in organic chemistry *per se*. Oh yes, we did use *SCI* in unique ways, for example, to find applications or modifications of the Eschenmoser hydrolysis, but that was before we understood classification and mapping by co-citation analysis. Now my major preoccupation is with the use of these techniques, combined with minireviews, to create the encyclopedic *ISI Atlas of Science*[®]. So ISI is making a major transition from "mere" indexing and abstracting to *encyclopedism*. This is

comparable to changing *CA* into Beilstein.

I believe that the systematic identification of research fronts and the creation of appropriate minireviews in chemistry and other fields is the necessary solution to *information overload*. In fact, the better *CA* and ISI become at retrieving bibliographic information, the greater the need for the further condensation and synthesis of information. I hope you will agree, one day, that this application of information technology in providing value-added information will be as important as the invention of the abstracting service.

The point is especially relevant on this occasion. Patterson and Crane played an important part not only in the evolution of the abstracting and indexing service, but they also made special contributions to the development of nomenclatural science or what I prefer to call chemical linguistics. As long as science progresses, the natural and systematic languages of science will change. There will be a need for chemists who will devote themselves to these specialized languages. Just as reviewers make a special contribution to scientific progress, so do the science etymologists and others who specialize in one or more aspects of scientific classification. And when these informational activities are combined with laboratory or theoretical research, they will become all the more relevant and productive.

My *CC* essays have often referred to the work of the eminent sociologist Robert K. Merton, Columbia University,² generally considered the founder of the now mushrooming field of the sociology of science. Merton and others^{3,4} have described the "reward system of science" in detail. On an occasion such as this, it is appropriate to examine that reward system. Understanding the concept helps one to grasp the workings of

the democracy and meritocracy of science.

Awards, such as the Patterson-Crane Award, the Priestley Medal, and, at the apex, the Nobel prize, are *formal* expressions of the complex reward system of science. Often criticized, such formal awards are nevertheless gratefully received. Their prevalence tells us something significant about the sociology of the world of science and scholarship. Although I do not ordinarily make categorical assertions, I can say one thing categorical about the Nobel prize and the annual Academy Awards in Hollywood—Bob Hope will never get an Oscar and I will never get a Nobel prize. Thank heaven there is no centralized monopoly on awards in Stockholm, Hollywood, or elsewhere. So you can appreciate why I say so much about non-Nobel awards.⁵

Formal, explicit awards in science, such as the Lasker, Wolf, or Feltrinelli awards, are usually presented long after the recipients made their first important discoveries. While there are exceptions, most significant discoveries in science take a long time to mature and to be accepted as part of the common wisdom. Some few, like the Watson-Crick discovery of the double helix structure of DNA, take off like wildfire. However, when we studied the characteristics of Nobel prizes 20 years ago,⁶ it was clear that Nobelists were usually prolific and cited heavily over a long period of time.

Other rewards in science are accorded more rapidly but are far less dramatic and come in smaller doses. These are the implicit awards of scholarship. They too are part of the reward system of science. An *implicit* mini-award takes place whenever one scientist *explicitly* cites another. *Explicit* citation is the currency of science—each citation is a payment by one peer to another. If one believes in these generalizations it is not difficult to

imagine how one gets obsessed with studying ways to measure the cumulative impact of scientists or scholars by simply counting how often they were cited, that is, how much we collectively owe them for their contributions.

I did not discover the idea of counting citations to measure eminence. Over 50 years ago, citations were being counted as an indicator of scientific importance. For example, in the 1920s, P.L.K. Gross and E.M. Gross⁷ did their classic study of citations to rank scientific journals. Many others followed. But it wasn't until the inception of *SCI* that we could begin counting citations on a grand scale. All previous citation studies were done by scanning, or eyeballing, journal pages.

Why did I decide to spend so much energy on citation counting instead of pursuing more profitable avenues of research such as chemical indexing? Apart from the fact that we can't do everything, I suspect the choice was motivated by the usual desire of a scientist for recognition. Maybe mine was stronger than my need for money or power. Or perhaps I was convinced that the two could not be separated. In any case, one might also ask if it was the inevitable choice.

I came from a socio-cultural-economic family background that cultivated a deep sense of justice. Perhaps you might say I developed a supersensitivity to injustice, whether in the form of civil liberties or due recognition denied.

Through *CC* and continuous travel I met large numbers of scientists. In them I observed a constant theme. Many had done outstanding research. But a large number seemed never to have achieved much formal recognition. In the late 1950s, as the first *SCI* for 1961 emerged and our data base began to achieve critical mass, I realized that we could help identify many people who had never been formally recognized for their con-

tributions, even though they had been informally rewarded by frequent citation.

While there are many people who feel that scientists should await their rewards in heaven, I do not. And I felt that the informal payment by citation was inadequate unless it was used in a systematic way. A lot of people are passed over in the formal reward system of science. This notion is often expressed in the phrase about the French Academy with its number of 40 luminaries: "Who shall occupy the 41st chair?"⁸

You don't have to be a great thinker to realize that for every member of the French Academy there have been dozens of other qualified candidates, and that for every member of the US National Academy of Sciences or the Royal Society of London there are scientists of equal stature who are not yet members. Who shall occupy the 1,500th chair? Increasingly, and perhaps naively, I kept asking why is it that some scientists never receive the recognition due them?

We don't really know to what extent the charismatic or communicative abilities of certain scientists make them, or their discoveries, seem more important than others. Personalities vary. There will always be some deeply introverted people. Their accomplishments will not generally be as well known or recognized as those of their more extroverted counterparts. Many people are natural politicians or entrepreneurs. Others remain very private or aloof. Moreover, some will have connections and other advantages that other equally productive scientists lack.

SCI, and the citation analysis it made possible, became for me a vehicle to transform an informal system of recognition into an explicit reward system for science. Citation analysis could uncover many of the scientists who are deserving of all sorts of awards, that is, formal recognition. This is well illustrated by the

work of the John Scott Award Advisory Committee in Philadelphia. I have used citation data to support the candidacy of numerous scientists and inventors.⁹

I will not elaborate in detail on the use of citation analysis for the identification of significance in science. Using citation analysis for evaluating people is a very tricky business. It is fraught with opportunities for error. But if you use the data carefully, you can facilitate an intelligence gathering process that can allow for reasoned and thoughtful decisions. Citation analysis should not be a cop-out for laziness. For the time being, the *SCI* system is an incipient method for evaluating science and scientists. When it is properly refined it will one day become a standard indicator.

One of the significant problems with using citation frequency data to rank importance is the preoccupation with individual cases where there is no correlation with subjective measures of importance. Quite often one's most-cited paper is not considered, even by the author, to be his or her most important paper. H. Fraenkel-Conrat stated in a *Citation Classic* commentary on his most-cited paper that he could think of at least ten other papers that he considers more important.¹⁰ It should be noted, however, that these other papers are also highly cited.

Considering my own citation record, I am inclined to agree with him. What I perceive to be my most "important" or "original" paper is certainly not my most-cited one.

It is often forgotten that in citation analysis the absolute number of citations is not of decisive importance. Rather, as with IQ, it is the percentile that is more significant. Last year, for example, the *Journal of the American Chemical Society (JACS)* was cited over 100,000 times—it is in fact the most-cited journal in the world. Nevertheless, a paper published in *JACS* that has been cited 200 or

more times is a relatively rare and significant event. About 350 *JACS* papers have achieved this distinction. *JACS* has published between 30,000 and 40,000 papers in its long history. So less than one percent of these papers have been cited 200 or more times.

As in other fields of inquiry, certain anomalies can distort our perception of patterns. Just as the Lowry method is the most-cited paper ever published in the *Journal of Biological Chemistry*, the most-cited paper in the history of *JACS* is the 1934 paper by H. Lineweaver and D. Burk, "The determination of enzyme dissociation constants."¹¹ This paper has been cited over 6,250 times in the past 20 years alone. Yet it is surely not the most important paper ever published in *JACS*. Incidentally, it was clear from my correspondence with Lineweaver that he had never received adequate formal recognition.

We found early on that there were such anomalies when we used *SCI* to identify the most-cited papers of science. Since those early days we have been able to use such data, in large and small fields, to identify the *Citation Classics* of science. We have not been disappointed.

During the past six years we have published 1,500 *Citation Classic* commentaries in *CC*.¹² The majority are written by eminent scientists who have been recognized by a variety of formal awards including the Nobel. But many are by scientists who have never been formally recognized. As we publish more of these *Citation Classics*, we are accumulating evidence to support Ortega's hypothesis—science progresses not only because of the paradigm changes introduced by great scientists. There are small breakthroughs that are not always perceived by historians of science because the microstructure of science is overlooked in the voluminous output of "Big Science." Those who make science

policy usually find it easier to deal with the elite, more visible scientists. They do not have adequate means for identifying much that is useful in science which goes unnoticed in the peer review groups that control funding. While it is often true that certain people are unjustly critical of the existing peer review system, there is also ample evidence it can be unfair.^{13,14} And this specific point gets me back to reasons for developing the branch of science policy studies called scientometrics.

Incidentally, discussions of citation analysis eventually lead to the problem of self-citation. I am probably one of the most self-cited people listed in *SCI*. The reason is simple. My audience in *CC* is constantly changing. I can't assume all readers have read my previous essays. In order to avoid excessive textual repetition I cite my own essays heavily. And that's exactly why there is also a considerable amount of self-citation in the scientific literature. But I like to believe that I too am an anomaly.

We have not yet completed a comprehensive analysis of self-citation among chemists or others. Self-citation between coauthors often exceeds 50 percent. First-author to first-author self-citation averages about 20 percent. Some self-citation is unavoidable or papers would be much longer. If you are among the few people who have been cited 10,000 times, like Carl Djerassi or H.C. Brown, then even 50 percent self-citation indicates prolific team output. When looking for their impact on others, it's the other half that counts.

I mentioned earlier that scientists often do not agree, either with peer judgments or citation analysis, on what their most original work is. When I asked Djerassi to name his best paper he would not do so.

Were I asked, I would say that my most "original" paper was the one which appeared in *Nature* in 1961,¹⁵ concern-

ing computer translation of chemical nomenclature. It occupied less than one page of print. That paper was based on my doctoral dissertation. However, I was not in a financial position to follow up on that research. Just as NSF had refused to give me grants to do citation research in 1953 because I was unaffiliated with any organization, they later refused support when I was affiliated with a for-profit organization.

It is widely believed that NSF funded ISI's original research on citation indexing, but in fact it was the National Institutes of Health (NIH) that originally financed research on the *Genetics Citation Index*. When the Fountain Committee forced NIH to change its regulations, a transfer of funds to NSF was arranged so that we could complete the project. I believe this was facilitated by people like Burt Adkinson, Ralph O'Dettle, and Sarah Rhodes. But in fact, NSF refused to publish the 1961 *SCI* we produced for the project. That forced me to make the most risky decision of my business career. We launched *SCI* and almost went bankrupt in the process. It should be said, however, that we are grateful for whatever assistance we received from government agencies directly or indirectly.

If my article on the mechanical translation of chemical nomenclature isn't my most-cited paper, what is? And why does my perception of its importance differ so much from that implied by its number of citations. My first paper in *Science* about citation indexes and others that followed have been much more widely quoted.¹⁶⁻¹⁸

Even though a few dedicated people have gone on to imaginative resolution of problems in dealing with automatic chemical structure techniques, my own work on the mechanical translation of nomenclature has long since been "oblit-

erated" or forgotten. It is cited only in historical reviews. In 1961, I made the decision to concentrate on research on citation indexes. Few then believed in the approach. Many told me I was crazy. These I do not name.

The field of citation analysis has grown enormously. Dozens of papers are published every month. Many of them rely on *Journal Citation Reports*® (*JCR*®),¹⁹ the last volume of *SCI* and *SSCI* for each year. The impact factors we calculate for over 5,000 journals are also used quite often. As in other methodology papers or data compilations, the authors may cite *JCR* but, by now, they often don't bother to cite the relevant papers. You don't cite *CA* just because you used it to search the literature.

Readers may be interested to hear about a recent use of *JCR*. It was used in an appraisal of the journals published by the Royal Society of Chemistry. This appeared in their journal *Chemistry in Britain*.²⁰ I was somewhat amused by the fact that their journals have suffered because of the mischievous way in which they were named. The author of the article took ISI to task for failing to unify the citation counts for their journals. He expected us to account for all the abominable names the chemical society has chosen to give their journals past and present. There must be a special place in purgatory for people who give journals names such as *Journal of the Chemical Society—Perkin Transactions Part II—Physical Organic Chemistry*. Whatever happened to simple titles like *UK Journal of Chemistry*? To me that would be an acceptable journal title. The author of that paper in *Chemistry in Britain* is right in recommending that some soul-searching is in order. Perhaps the British may learn that a rose by any other name does *not* smell as sweet. Let me turn to a decidedly sweeter subject—Evan Crane.

By a strange coincidence, just before Gerard Platau, the award committee chairman, telephoned me, many of my old files were being destroyed. Among them was my correspondence with Crane. I first met Crane at the Johns Hopkins University indexing project in Baltimore, Maryland. Losing those files is unfortunate because we corresponded about many matters of possibly current interest. Among them was a research position for me at *CA*, which never materialized.

Who knows what might have happened had I come to Columbus back then. I like to think that there would now be a *Chemistry Citation Index*. This would be part of the Chemical Abstracts Service. I suggested this many years ago. Charles Bernier, former editor of *CA*, was interested in the idea and mentioned it in *Chemical & Engineering News* as a potential *CA* improvement. Had the idea materialized then, most chemists today would use citation indexes routinely.

But perhaps it is not too late. If there is a viable basis for such a venture, then ISI will gladly discuss this with *CA*, just as we are discussing other discipline-oriented indexes with other professional societies. In spite of the existence of the huge multidisciplinary *SCI*, there ought to be small discipline-oriented citation indexes available in chemistry, physics, psychology, mathematics, earth sciences, etc. One might think that online services would make this unnecessary, but I believe printed indexes, along with printed journals, will be with us for many more years. Just as *CA* found it had to create numerous sections, it is not surprising that *SCI* may have to do the same. Indeed, in our new online system there will be such segmentation, so that users can "limit" searches by classes of subject matter.

I also believe all editors ought to have access to a cumulated citation index of their own journal. In addition to the usual retrieval uses, this would help them to evaluate their judgments in selecting papers. At the International Conference of Scientific Editors in Philadelphia we discussed an article-by-article journal citation index. We have developed a report which tells the editor the fate of each article published in the journal for each year. This involves the use of an enormous integrated *SCI* data base covering 20 years of data.

Back to the early days—Crane convinced me to become a volunteer abstractor for *CA* so I could learn something about the process. This had a great impact on me. I studied the indexing procedures of *CA* in great detail. I received from Bernier the complete indexing records for hundreds of articles and compared their treatment to that by *Biological Abstracts* and the old *Current List of Medical Literature*, now called *Index Medicus*. Those studies led to my proposal for a "Unified index to science," presented at the International Conference on Scientific Information in 1958.²¹ This idea has for all intents and purposes been displaced first by *SCI* and by other online equivalents now being developed.

Much has changed in 30 years. Looking back it seems like a short time. In conclusion, though, I like to think that this award right here in *CA*-land expresses the way in which the competitive spirit in this pluralistic country ultimately works to the advantage of all concerned. I am touched that you could put aside the rivalries. Of course these rivalries will still continue in one form or another. Even Muhammad Ali and Joe Frazier can be friends.

I have not yet had a chance to work out the details. But I want you to know

that this award money will be used to promote the field of chemical information retrieval in one way or another.²² We need to find better ways to help people when they are younger. At the turn-

ing point of a career it is not just the money that helps you make a decision but also the realization that someone really cares.

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