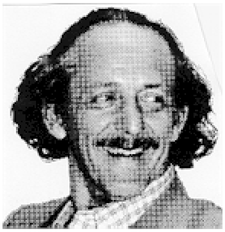

Current Comments

J.D. Bernal— The Sage of Cambridge. 4S Award Memorializes His Contributions to the Social Studies of Science

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Last September marked the tenth anniversary of John Desmond Bernal's death. But this great scientist is far from forgotten. In the past few years two biographies of Bernal[1,2] have appeared. These are in addition to numerous articles offering nostalgic reminiscences of him,[3,4] and new critiques of his work.[5] Additionally, last year the John Desmond Bernal Award was established by the Society for Social Studies of Science (4S) in collaboration with ISI®. It is in belated recognition of Bernal's contributions that I wish to offer this tribute. Actually, I find it difficult to believe that so much time has passed since I first publicly acknowledged his impact on my career.[6]

I first corresponded with Bernal in 1962, when *Science Citation Index*® (*SCI*®) was in its infancy. Although I feared that so great a man would be quite unapproachable, I nonetheless wrote to him to ask his opinion of the fledgling index. It turned out that he was most approachable, and, in fact, pleased to hear from me. He was interested in *SCI*, and we corresponded about it at length. Bernal initially thought the idea of such an index exciting and of potentially great value, but had reservations about the feasibility of the project. When we showed that the index was indeed possible, Bernal was delighted with it. In fact, he said of *SCI*: "I...think [it is] going to provide something really new and valuable in documentation material."[7] Later, in 1964, he agreed to serve on its editorial advisory board. About a year later he published a monumental review of *SCI* in *Science Progress*.[8] I could not have been more pleased with his endorsement. A copy of his review follows here as an appendix.

I had known of Bernal for a long time before this, however. As a teenager, one of my uncles gave me a copy of his book *The Social Function of Science*. I had just finished high school, and took it with me to the University of Colorado where it was the subject of much discussion—especially as it was also wartime. I felt the impact of Bernal again when I joined the Welch Library medical indexing project at Johns Hopkins University in 1951. Bernal had been a significant factor in organizing the Royal Society Empire Scientific Conference on Scientific Information in 1946.[9] The proceedings of this conference became a bible for me as a fledgling investigator. In particular, his idea of a centralized reprint center was in my thoughts when I first wrote about the as yet nonexistent *SCI* in *Science* in 1955.[10]

Many scientists and documentalists I met in those days knew and respected Bernal and also participated in that conference. They included Ralph Shaw[11] and Derek J. de Solla Price,[12] as well as G.M. Dyson, Mortimer Taube, B.C. Vickery, R. Fairthorne, J. Farradane, and D. Urquhart. James Murray Luck, the namesake of the National Academy of Sciences annual award for scientific reviewing, represented the academy at that conference.[13]

But Bernal was a man respected and revered even more in other fields of science. As a physicist, he made major contributions to the field of X-ray crystallography—the study of the structure of molecules through X rays—and to the knowledge of the atomic structure of compounds. As a historian of science, he wrote groundbreaking works emphasizing the social concepts of science. As a futurist, he often made correct predictions about the role of science in today's and tomorrow's society. As a social reformer, he was influential in creating and supporting world scientific organizations. People who knew him remark on his energy, his multiple interests, and his sharp mind. His influence in science was felt the world over. But since he had a prolonged illness before his death, the loss was perhaps not appreciated immediately by the entire scientific community. Bernal was always something of a hero figure to me, and I have always felt honored by my association with him. That is why it is all the more incredible that so much time has passed before my acknowledging this publicly.

Bernal was born in the market town of Nenagh, Ireland, on May 10, 1901. He was the oldest of the four children of Samuel Bernal, an Irish Catholic gentleman farmer, and Elizabeth Bernal, an American who was one of the first female graduates of Stanford University. As a young man, Samuel Bernal traveled extensively, and it was on an extended visit to the US that he first met Elizabeth. Upon their marriage, the Protestant Elizabeth converted to Catholicism. The conversion was wholehearted, and she raised her children in a strict Catholic atmosphere. This upbringing had a great effect on Bernal, as did the class-ridden Irish society that surrounded him. Both were to greatly influence his opinions and later life.[1] (p. 17-8)

Maurice Goldsmith, Science Policy Foundation, London, was a close friend of Bernal and one of his biographers. Goldsmith reports that Bernal's sharp scientific abilities and curiosity were displayed early. Goldsmith relates how, for example, using books and a lantern, the six-year-old Bernal performed a crude experiment in an effort to learn about X rays. He didn't learn much about X rays, but he did come close to setting the house on fire. At seven, Bernal tried to make hydrogen, having found the experiment discussed in a book. He managed, instead, to produce a magnificent explosion.[1] (p. 15-23) I should add that my friend Goldsmith serves on the editorial board of *Arts & Humanities Citation Index*™.

Opinions about Goldsmith's book were quite strong—some liked it, while others, like Dorothy Hodgkin, winner of the 1964 Nobel Prize for Chemistry, tried to discredit it.[14] Later, John Maddox, editor of *Nature*, commented on her review as follows:

> Polemic is...part (but ideally a small part) of book review columns. Professor Dorothy Hodgkin's review of Maurice Goldsmith's book on J.D. Bernal is more puzzling. Goldsmith set out to write a biography of one of his heroes, and was savaged for his pains by another hero-worshipper, his reviewer. No doubt the book is, as Professor Hodgkin says, "confused and inaccurate". It is also, however, a good book in the sense of being a good read and also (for a hero-worshipper) honest in that it deals with the warts as well as the achievements—sexual proclivities, ambivalence about Lysenko and all that. Maybe Goldsmith's book is "not the book that is needed about Bernal", but it is the only book we have.[15]

At age ten, Bernal began attending Stonyhurst College, a Catholic boarding school in England. After three years he transferred to a school that included more science in its curriculum. Following his graduation in 1919, he went to

Cambridge University.[1] (p. 23-6) The atmosphere at Cambridge at that time was electric: World War I had just ended, and many of the students were returning soldiers. Hodgkin, a close friend of Bernal, explains that the students "discussed endlessly all the problems of the day, science of course, in which there were many new developments, and economics, politics, and religion."[2] (p. 23) It was a revelation for Bernal, whose whole outlook, according to Hodgkin, was changed. He himself wrote of the time: " 'All Cambridge was a liberation, all the richness of thought was open to me.... In the whole field of thought I have no one supreme interest and am fascinated wherever I look.' "[2] (p. 23)

The experience was a turning point for Bernal in many ways. He discarded his religion, which had been a dominating force in his life until then, and he discovered and embraced Marxism, which would dominate the rest of his life. At Cambridge, he also gained a wife, Eileen, and the nickname that remained with him thereafter: Sage. Dora Grey, one of his friends, dubbed him that because he knew so much.[2] (p. 24) Apparently everyone else agreed, because the name stuck. I myself never knew this nickname until I began work on this essay.

In his final year at Cambridge, Bernal developed a passionate interest in the problem of the various possible symmetry arrangements of atoms in space. Despite the fact that, as a senior, he could ill afford the time to solve the problem, he worked diligently, devoting much of his time to it. The result was a lengthy paper, "On the analytic theory of point group systems,"[16] which won the Sudbury Hardyman prize of Cambridge in May 1923. The paper gained Bernal much favorable attention, although according to Hodgkin, "only one person, apart from Bernal himself, ever read the full version" of it.[2] (p. 26) But it was as a direct result of the paper that Bernal was invited to join Sir William Bragg at the Royal Institution following his graduation from Cambridge.

Bragg, one of the pioneers of X-ray crystallography and winner of the 1915 Nobel Prize for Physics, was then director of the Davy-Faraday Laboratory at the Royal Institution. Bernal was thrilled to be invited to work with him. The field of X-ray crystallography was relatively new at that time, and Bernal and the other researchers in the laboratory worked on a variety of problems. Bragg allowed his researchers a rather free rein, and Bernal set out to identify the structure of graphite. Previously published reports on graphite differed in their results and Bernal's task was to determine who—if anyone—was correct. He soon decided that the only way to proceed was to x-ray the graphite crystal as it was rotating. This process resulted in a series of photographs showing the diffraction of the X rays through the crystal. Properly analyzed, the structure of the crystal could be discerned. Hodgkin describes how Bernal first tried this method. He mounted "a crystal at the centre of a kitchen alarm clock with a piece of brass tube above it, within which was placed the film held in position by bicycle clips."[2] (p. 27) Despite the crudeness of the arrangement, it proved successful. Bernal then constructed a more suitable instrument with which to take the photographs. From the resulting photographs, he was able to successfully unravel the structure of graphite.[2] (p. 28) The previously reported structures, it turned out, had been both right and wrong—right in some aspects, and wrong in others. It was Bernal's first major success.[17] Since I don't yet have a citation index for 1925 to 1960, I can't tell you its citation impact during that period. However, for the years 1961-1981 *SCI* indicates 14 explicit citations to the paper—remarkable considering its age.

Stemming from his work with graphite was another, even larger, success. Bernal had found, while working with graphite, that the process of indexing the photographs taken of the crystal was very tedious. He developed a chart to simplify the process of classifying the reflections, which was published in a 1927 paper.[18] The paper, which describes the theory of X-ray diffraction, is considered one of his most important by many. From 1961 to 1981 it was cited at least 30 times. Hodgkin believes that it "is as useful reading today for many beginners in crystallography as it was in [1927]."[2] (p. 28) At about the same time, Bernal developed the "universal X-ray photogoniometer," an instrument designed to make single crystal rotation photography easier.

In 1927, Bernal left the Royal Institution to accept a lectureship at Cambridge. He stayed there for ten years, during which time he began to attract his own group of followers. Among them were Hodgkin, Max Perutz,[19] who won the Nobel Prize for Chemistry in 1962, and Isadore Fankuchen, who, with Bernal, did major work on the structure of viruses. At Cambridge, Bernal continued his important work on biological molecules. One of his first achievements there was in the study of sterols.[20,21] In 1932, while studying these compounds, he demonstrated that the formula then considered correct for one of them was actually incorrect. Later he did work on the structure of proteins, and discovered that by taking X-ray photographs of wet crystals he could produce detailed reflections of the structure of these previously elusive molecules. He also did work on the structure of viruses; in particular, he defined the structure of the tobacco mosaic virus.[22] This work is often considered the foundation of modern molecular biology.[23]

Bernal became professor of physics at Birkbeck College, University of London, in 1937, a position that he held for the rest of his career. At Birkbeck he did extensive molecular work, first on viruses, and also continued his earlier work on the poorly understood structure of water and other liquids.[2] (p. 50) His scientific research was briefly interrupted, however, by the outbreak of World War II. Although Bernal was an outspoken Marxist, and was a member of the Communist Party for a few years in the 1920s, his reputation as a scientist was already great. Sir John Anderson, then the Tory British Minister of Home Security, was convinced that Bernal's scientific abilities were essential to the war effort, and Bernal joined the research and development department of the Ministry of Home Security for the duration of the war. He was influential in several applications of science to the war effort.

One of Bernal's first tasks was to study, together with Solly Zuckerman (now Lord), the effects of bombing. By analyzing the relationship between the number of bombers used and the population of the target area, he was able to predict—quite accurately—the number of casualties and the amount of destruction. This work enabled the British to determine how much bombing, and what kind, was needed to destroy any particular objective. According to Goldsmith, this work "transformed the study of bombing and its effects from rumour and guess work to a policy based on thoroughly scientific and practical principles."[1] (p. 93)

Another major project on which Bernal worked involved the D-day landing in Normandy. Relying on childhood memories, photographs, and reports published in the scientific journal *Proceedings of the Linnaean Society of Caen*, the scientists working on the project, including Bernal, were able to accurately assess the conditions of the beaches—such as beach gradients, composition of the beaches, underwater obstacles, and tides. Earl Mountbatten, under whom they worked, reported that this knowledge "made significant contributions to techniques for dealing with under-water obstacles and defences on the beaches."[2] (p. 55) Later, Bernal was involved in HABBAKUK, a project to construct large, unsinkable aircraft carriers made of wood pulp and ice. As always, those who came in contact with Bernal, including Mountbatten and Winston Churchill, were impressed with his tirelessness, his generosity, and his incredible analytic mind.[2] (p. 58)

Following the war, Bernal resumed his research efforts at Birkbeck College. He continued work on the structure of water, and by 1962, "had succeeded in presenting an approach which linked a way of treating liquids with that of crystalline solids. He was able, also, to provide a material representation in space of the instantaneous positions of the molecules in a liquid."[3] Hodgkin notes that since then, with the growing use of computers, many of Bernal's observations have been verified.[2] (p. 52)

Yet despite all the important scientific work for which he is directly credited, and although he became the UK's first professor of crystallography, Bernal is perhaps even better known for the work that he inspired. In a recent article, Perutz explains how he operated: "Bernal would set his student a problem, help him or her to find a solution, and then insist that the paper be published in the student's name only, brushing aside any thanks for his generosity. What held him was the grand sweep of an idea of a problem, but he was impatient with detail."[4]

Goldsmith, borrowing from C.P. Snow and Bragg, expands still further on Bernal's gift for inspiration:

> His strength lay in causing other minds to light up. Most of the leading names in Britain working on molecular biology and the analysis of protein crystals were either Bernal's associates or students. Wherever he went he left behind intellectual "fall out", providing more than sufficient for a lifetime of scientific work. If one traced back almost any fruitful line of crystallographic work it would be found that Bernal assisted at its conception, but, significantly, left the child to be brought up by foster-parents.[3]

But Bernal's efforts were not restricted to lab-oriented pure scientific research. He was also deeply fascinated by the history of science, an interest stimulated at the 1931 International Congress on the History of Science. At that meeting, a group of Soviet scholars had expounded their view of science as a social phenomenon. In contrast, the then current Western European view held that science was pure knowledge, and as such, unrelated to social and economic conditions. This new idea sparked a great deal of interest in Britain. Bernal was particularly enthusiastic, and he assigned himself the task of creating a social evaluation of science. He published his first book on the topic, *The Social Function of Science*,[24] in 1939. Many have considered it a "pathbreaking publication,"[25] and "a prophetic indictment of the non-use or mis-use of scientific resources in capitalist societies."[23] As mentioned earlier, it influenced my early career and undoubtedly dozens of others. MIT Press recognized its classic status by issuing a new edition in 1967.[26]

In 1964, 25 years after its original publication, a *festschrift* was published in honor of this book. Called *The Science of Science*,[27] the work was edited by Goldsmith and Alan Mackay, Birkbeck College, University of London, and incorporated essays by many well-known scientists, including Snow, P.M.S. Blackett, Gerard Piel, C.F. Powell, Herbert Coblans, Peter Kapitsa, Alexander King, Joseph Needham, J.B.S. Haldane, N.W. Pirie, R.L.M. Synge, M. Korach, and Price. Bernal himself wrote the final essay in the volume, in which he commented on the changes in science since he first wrote *The Social Function of Science*. He wrote: "I would now conclude that to a very large extent the book has fulfilled its original object: to make people aware of the new function that science was acquiring then and would increasingly acquire in the future, in determining the conditions of human life and—as it is now tragically revealed—of the very existence of humanity."[28] An American version of the book, entitled *Society and Science*, was published simultaneously.[29]

In 1954, he expanded on the themes of his first book in a two-volume work, *Science in History*.[30] In the first two parts of this work, *The Emergence of Science* and *The Scientific and Industrial Revolutions*, Bernal detailed the history of science from Stone Age culture through the Industrial Revolution. According to Loren R. Graham, Columbia University, these sections were a major contribution in that they illustrated "the role of artisans and technology in the development of science and emphasized the impact of economic differences in society upon science."[25] The final two sections, *The Natural Sciences in Our Times* and *The Social Sciences: Conclusion*, deal with the current state of the natural and social sciences. Graham believes that it is these parts that most distinguish Bernal's work from that of his contemporaries. According to Graham, "Bernal is interested in where science is taking man and how man can control the process; the past to him is a means of understanding the present and predicting the future.... Bernal has produced an eminently successful and valuable study."[25]

The impact of Bernal's work is partially demonstrated by a check of *SCI* for 1961-1980, and *Social Sciences Citation Index*® (*SSCI*®) for 1966-1980, which shows there were about 3,350 citations to Bernal's work. *SCI* accounted for 3,150 citations, *SSCI* for about 200. The citations cover a wide range of Bernal's work, of course—both his work in molecular structures and the social sciences. Nonetheless, the number of citations his work has received is respectable by any standard. A list of his books appears in Figure 1.

When he wasn't experimenting or encouraging others in their work, Bernal was busy pursuing another of his lifelong beliefs—that scientists everywhere must work together in promoting science in humanity's best interest. He believed strongly that science did not operate in a vacuum, that it was instead both a direct result of, and an influence upon, society. As a result, he felt that scientists were responsible for pursuing science with the welfare of society always in mind. He likewise believed that scientists should have a direct role in managing society. An ardent Marxist, Bernal argued that the communist state was the only state in which the ideal collaboration of science and society could be effected. He believed, says Perutz, that

Figure 1: The books of J.D. Bernal.

The world, the flesh and the devil: an inquiry into the future of the three enemies of the rational soul. London: Cape, (1929) 1970. 76 p.
The social function of science. Cambridge, MA: MIT Press, (1939) 1967. 482 p.
The freedom of necessity. London: Routledge & Kegan Paul, 1949. 437 p.
Marx and science. London: Lawrence & Wishart, 1952. 56 p.
The physical basis of life. London: Routledge & Kegan Paul, 1952. 80 p.
Science and industry in the nineteenth century. Bloomington: Indiana University Press, (1953) 1970. 230 p.
Science in history. Cambridge, MA: MIT Press, (1954) 1969. 4 vols.
World without war. London: Routledge & Kegan Paul, (1958) 1961. 308 p.
The origin of life. London: Weidenfeld & Nicolson, 1967. 345 p.
The extension of man—a history of physics before 1900. Cambridge, MA: MIT Press, 1972. 317 p.

"science and Marxism would bring prosperity and peace to all mankind."[4] The horrors of the Irish society in which he grew up no doubt had exerted much influence on his opinions in this matter. One of the more unfortunate aspects of this belief, however, was Bernal's defense of the Russian geneticist T.D. Lysenko, who tried to refute and discredit the Mendelian theory of genetics. Ironically, according to Goldsmith, at the same time he was defending Lysenko, Bernal's own laboratory and students were doing the work that ultimately discredited Lysenko's ideas. Undoubtedly, Bernal's reputation was seriously damaged by this affair.[1] (p. 190-7)

In pursuit of his internationalist beliefs, Bernal was also instrumental in the founding of the World Federation of Scientific Workers in 1946. According to Hodgkin, the organization was "designed to bring together scientific workers from all over the world, 'to promote understanding and co-operative action between the member organizations...to work for the fullest utilization of science in promoting peace and the welfare of mankind.' "[2] (p. 68) Bernal is credited with first conceiving of the organization, and he was largely responsible for drafting its constitution. Members included the British Association of Scientific Workers, the French Association des Travailleurs Scientifiques, and the Indian Association of Scientific Workers.

Bernal died on September 15, 1971. He was 70 years old. He was survived by his wife, four children, and seven grandchildren. The last ten years of his life were difficult—he suffered from several strokes and was often unable to work. Yet he maintained his intense interest in all things scientific up to the end. Of this time Hodgkin writes:

> In his long illness one would often find him very wretched, suffering from his lack of power any longer to change the state of the world, which he saw as increasingly perilous and miserable. Too much that he had hoped for had not happened. And yet he could still be delighted by nature and by scientific discoveries—the structure of insulin, the exploration of space. One of my last happy memories of him is seeing him examining a little sample Professor Tolansky brought to him, of minute silica spheres from the surface of the moon.[2] (p. 71-2)

Since I was regularly visiting London in the late-1960s, I was fortunate enough to meet with Bernal on several occasions.

Throughout his life, Bernal received many honors in recognition of his work. Although his communist sympathies may have occasionally hindered full recognition of his scientific accomplishments, it is probably more relevant to say that his participation in social and political activities prevented him from achieving more as a scientist. He was certainly not ignored in his lifetime. He was elected a Fellow of the Royal Society in 1937, and was an honorary member of a number of foreign scientific societies, primarily in Eastern Europe, including the USSR, Hungary, Poland, Romania, Czechoslovakia, and the German Democratic Republic. In 1945 he was awarded the highest honor of the Royal Society, the Royal Medal. He served as president of the International Union of Crystallography from 1963 to 1966. Although he did much work of Nobel quality, he never won the prize himself. That was a fact that surprised many.[3] According to J.G. Crowther, a British science writer, "Bernal's demonstration that the accepted formula for the steroids was wrong—which led to the correct path to the synthesis of sex-hormones—should have been sufficient in itself to earn him a share in a Nobel Prize."[31] However, Wolfie Traub, one of Bernal's students now at the Weizmann Institute of Science, Israel, writes:

> Nobel Prizes are generally given for important scientific work based on far-sighted scientific vision and carefully planned, sustained and orches-

trated teamwork. Bernal had little patience for detail and careful planning. He never organised a scientific team, though he inspired many protégés and he certainly had brilliant scientific vision and insight. As a young man of 30 he used quite simple X-ray measurements to show that the sterol structure, derived by the Nobel Prize-winners Wieland and Windaus, was wrong. It took another half dozen years before Bernal's protégés Crowfoot (later Hodgkin) and Carlisle determined the correct structure. He showed that proteins in crystals must have well defined structures and how it was possible to obtain X-ray data defining these structures. It took Perutz and Kendrew...a quarter of a century to actually determine the first protein structures by analysing such data. In 1938 Bernal and Fankuchen showed that even viruses, complete living individuals, have ordered structures in crystals. Many fine scientists have continued this work and the first detailed structures of viruses have only appeared in the last two or three years.

> Bernal's interests moved from one great scientific problem to another, from science to politics to philosophy to history to sociology. He was always writing, travelling, lecturing, advising, posing problems and making provocative suggestions. He didn't have the patience to earn a Nobel Prize, but those of his protégés who did...would be the first to acknowledge his greatness.[32]

Traub's sentiments are echoed by another of Bernal's colleagues, Mackay. During a recent visit to ISI, Mackay noted that Bernal's contributions to science were diffuse, and "diffuse contributions do not receive the Nobel Prize."[33]

Of course, Bernal's fame went beyond the scientific community. For example, he was mentioned in Lillian Hellman's 1973 story "Julia." In the piece, which details the history of Hellman's friendship with her childhood schoolmate Julia, Bernal is described as one of the few people the radical Julia ever spoke of with respect.[34]

In 1981, to honor Bernal and his pioneering work in the social study of science, the 4S established, together with ISI, the John Desmond Bernal Award. The 4S, which was founded in 1975, is an international society dedicated to promoting "research, learning and education in the social studies of science."[35] According to 4S President Arnold Thackray, University of Pennsylvania, the organization will present the John Desmond Bernal Award annually, in order to "recognize outstanding scholarly achievement in the social studies of science, without restriction of field or nationality."[36] The award is the society's highest honor, and is meant to honor a scholar's achievement over a period of time, rather than a single work. ISI provides financial support for the $750 award and subsidizes administrative costs.

Last year, the prize committee established by 4S voted unanimously to present the first award to Derek J. de Solla Price. Although I had nothing to do with this decision, I was nevertheless delighted with it. Price not only serves on the editorial board of *SCI*, he also continues to make significant contributions to its continuing development.[12] Incidentally, de Solla and Bernal are both Sephardic names.

The 4S honored Price for his many contributions to the social studies of science. A pioneering scholar, Price has published numerous works over a period of years that have had a profound impact in this field.[36] Among them are the well-known books, *Science since Babylon*[37] and *Little Science, Big Science*,[38] in which Price explores the history of science, and develops the idea of the exponential growth of science. *Little Science, Big Science* has become a classic, having been cited by hundreds of authors in the past few decades. Price has also been influential in developing several branches of the study of science, particularly the field of scientometrics.[36] The John Desmond Bernal Award honoring these achievements was presented to Price at the 4S annual meeting last November. In his acceptance speech, Price acknowledged the impact Bernal had on his career, noting that his work in the social theory of science was partly inspired by Bernal. He added: "I am doubly honored by this award commemorating a person for whom I had much love, and from whom I learned a little about scholarly style, good appetite and some sense of social and political responsibility."[39]

Bernal was for me, as for many others, a great and inspiring man. He anticipated the modern revolution in science communication when he wrote about the need for a more effective form of scientific communication in *The Social Function of Science*. In that book, he already saw that there was a need for a reference work that would give scientists access to a large body of scientific literature—both past and present. His own comments to me personally when I was struggling to create and refine *SCI* were important, as was his help in publicizing its significance for the sociology of science as well as for information retrieval. Others found all the reasons that it couldn't work.

Incidentally, in an appendix to *The Social Function of Science*, Watson Davis, Science Service, argued for the creation of what could be called a predecessor to ISI, an organization he called the "Scientific Information Institute." This organization would be concerned with the "utilization and development of methods of publishing, duplicating, indexing, selecting and distributing scientific information and bibliography that are novel in their application to this problem."[26] (p. 449)

It was a desire to publicly recognize his help that compelled me to honor Bernal in my own small way in 1975. In that year I dedicated the first published large-scale statistical analysis of journals, the *SCI Journal Citation Reports®*, to Bernal. I think that what I said then still sums up my feelings about him: "Dedicated to the memory of the late John Desmond Bernal, whose insight into the societal origins and impact of science inspired an interest that became a career."[40]

Though he died ten years ago, Bernal's achievements are as meaningful today as ever before. Bernal's understanding of science especially transcended his own time. In fact, he was way ahead of his time. The final paragraphs from his book *The Social Function of Science* illustrate the particular genius of Bernal well, and I am pleased to be able to quote them here:

> In science men have learned consciously to subordinate themselves to a common purpose without losing the individuality of their achievements. Each one knows that his work depends on that of his predecessors and colleagues, and that it can only reach its fruition through the work of his successors. In science men collaborate not because they are forced to by superior authority or because they blindly follow some chosen leader, but because they realize that only in this willing collaboration can each man find his goal. Not orders, but advice, determines action. Each man knows that only by advice, honestly and disinterestedly given, can his work succeed, because such advice expresses as near as may be the inexorable logic of the material world, stubborn fact. Facts cannot be forced to our desires, and freedom comes by admitting this necessity and not by pretending to ignore it.
>
> These are things that have been learned painfully and incompletely in the pursuit of science. Only in the wider tasks of humanity will their use be found.[26] (p. 415-6)

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Review of the *Science Citation Index*

When I first heard about the *Science Citation Index* from Dr Garfield himself over 2 years ago, I could not quite imagine what it would be like: now I have been studying it for some time and can see something of what Dr Garfield means in his idea of its being a new dimension in indexing. Indeed, I was proposing to do an article on it for *Science Progress*, but the volumes were of such weight and density of information that they could not be treated in a hurry. I resolved to give it a test in some detail and then to make a quantitative assessment. Meanwhile *Science Progress* changed Editors and there was no time left to produce this article in advance of the review already prepared by Professor Ziman and published in the previous issue.

The value of the *Science Citation Index* was immediately apparent to me because I had tried to do the same thing in reverse order in writing about various aspects of the history of science. The publications of science effectively form a network of mutual reference which can be traced out from any particular point from which one chooses to start. It is a graph in the mathematical sense. The *Citation Index* is constructed so as to produce an almost infinite number of such graphs. Its value as an index is another matter altogether. The real question is, what is such an index needed for? If it is simply to find out where a certain paper is, the existing one-way indices contained in the Abstracts should be sufficient. However, an index has more functions than this: it should give the relationships of any given paper to all other papers that are appearing, not necessarily on the same subject or in the same detailed field but anywhere in science, and this function the *Science Citation Index* admirably fulfils.

Nevertheless, the criticism as to coverage which has been made by Professor Ziman, is, I think, quite a valid if a temporary one. I was also immediately struck on looking at the index for 1961, with the peculiar and restricted list of its source journals, though there was a notable improvement in 1964. Remember, the Index contains two kinds of lists of references, those of the source papers and those of the papers quoted. The latter is universal. Every paper cited in a source paper is referred to whatever the journal in which it occurs and this covers a large amount if not all of available scientific literature. I say if not all, because there are certainly a great number of scientific journals that have not been quoted yet. They formed the tail of the journal distribution already noted in Dr Urquhart's classical study. They are journals which are appearing at irregular intervals from obscure places. The first list of source journals is, I think, a subject of considerable criticism of the index. Dr Garfield points out that the source material is drawn from a limited number of journals which contain the greater bulk of the publications in the different fields but Professor Ziman counters this by pointing out that it does not cover many of the chief 'quality' journals.

Now, I have attempted—a long time ago, it is true, at the time of the Royal Society Information Conference (1948)—to find out from a random sample of working scientists what papers are actually consulted and what journals they occur in. This enabled me to carry out what I thought was a useful function by sorting out the journals in a particular field—in this case physics, the same as Professor Ziman's source—and noting that they fell into three fairly definite categories: the most cited and the most read papers, which, in my opinion, should be in every physics laboratory library, the less often cited ones, which it seemed to me sufficient that they should be in the library of the university or large scale research institute and, finally, the group containing far the largest number of journals but not the largest number of papers, which would be sufficiently accessible if they were found in some national science library.

This is not the classification which is used in the *Science Citation Index* and its limited nature gives rise to the justified criticisms made by Professor Ziman; for instance, to leave out the *Philosophical Magazine* is, as he points out, indefensible. But, as Garfield answers, it is a logical mathematical process. However, the question is not in the logic but in the object of the reader's enquiry; if it is to get at the most commonly needed paper, it may well succeed but if the object is to get a

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quality view of science, the *Citation Index* has started in the wrong place. Dr De Solla Price, in *Big Science Little Science* [*sic*] discusses the importance of the set of modern 'invisible colleges' or coteries of those who contribute most to the advance of science. Many of their members do not publish, as such, at all but simply communicate privately among themselves. The next set, which contains the same people and others as well, are those who publish only in the quality journals. I remember very distinctly at a meeting in New York, an author saying that he was not surprised that his paper had not been read because it appeared in an obscure journal, *The Proceedings of the Royal Society (B)*.

All this criticism, however, seems to me to be misplaced. We do not want to know, necessarily, what papers appear in the select quality journals: we can find that out most easily by looking at the journals themselves. What we want to see are the kind of papers that appear in the other journals and still have interest, and for these the *Science Citation Index* does provide. The fact that a reference occurs several times in the *Science Citation Index* indicates that it has been found to be of interest to many actually carrying on scientific research. I say found to be, because it does not follow that, if there is a reference, the paper referred to has actually been read by the author of the paper referring to it. He may want merely to guard himself against the accusation of having ignored so-and-so's work.

From the journals which are most often quoted in the citations, Dr Garfield has successively added to his list of reference journals. Thus, by a process of successive approximation, it should be possible to build up the most intrinsically self-checking lists of journals by iteration and, in the end, arrive at a set which would satisfy even Professor Ziman. This process, however, is bound to be slow and may fatally compromise the *Citation Index* before it is completed. It might have been better to start from a list compiled on different principles, one, at least, including the quality journals of science, a list by no means impossible to compile and to check by the same process of successive approximation. Such a list, moreover, need not necessarily be much greater than the present one and, therefore, the danger that Professor Ziman foresees of an impossible increase in price may not occur.

The justification of the *Citation Index* should, however, not be allowed to stand or fall by the completeness of its source list. Its essential value is, as claimed to be, that it is a new dimension in indices which should enable the poly-dimensional graph on the progress of science to be mapped out for the first time. Such a graph is a necessary stage in drawing up or planning any strategy for scientific research as a whole. I have, indeed, attempted to put forward such graphs in relation to key discoveries in science in the past, notably for the explanation of optical rotation by Pasteur in 1848 and for the discovery of X-ray diffraction in 1912 by von Laue. Now the index itself is being used for another graph for the key problem of the genetic code.

All great discoveries in science have an inter-disciplinary aspect and, indeed, it is this lack of distinction between the disciplines that is one of the chief virtues of *the citation* index. It is already lending itself to further studies in the science of science and will do so even more in the future.

Nor is the use of the *Index* to the individual scientist to be despised. It can be far more useful than for the purposes of self-satisfaction or disappointment. Beyond his immediate friends and colleagues, the working scientist does not really know—or did not really know before—what happened to his work, who read it and what they did with it: now he has the chance to do so. I had, like many other users of the *Index*, the reasoned curiosity to look at the list of references to my own papers. I say reasoned, because I have been in science publication for over 40 years and this should give the measure of the duration of the citability of the scientific paper. I had to do this for myself because I was the only person who knew what was in the papers. To do it for anyone else in detail would prove a Herculean task. What I found, to my surprise, was the large amount of coverage there was: about 40% of everything I had published since 1924 was referred to at least once in the *Index* and many papers were referred to several times. This would seem to indicate that the coverage of the *Citation Index* is not as bad as it has been painted. However, this may be because my readers cover nearly all the disciplines of science: if I had kept to one narrow field I might have missed the citations because of the lack of coverage of particular journals.

By and large, the choice of papers quoted corresponded to my own idea of their importance. The citations showed clearly, also, the erosion of scientific literature by mere time.

For instance, papers before 1930 were only cited three times but the matters they dealt with are already well-established in the text books. The citations have a definitely bimodal distribution; in common with most of those from Britain or European countries there is a large gap due to the war. There could be no quotation, of course, from classified sources.

How far such a check furnishes proof of the usefulness of the index depends on one's judgment as to the values of different types of scientific information. It is also based on economic judgment. Now that the *Index* is no longer subsidized, it stands or falls by its value to its clients. If these are satisfied that they pick up through the *Index* most of the valuable advances in materials and methods that they need, they will continue to buy it and more will join them. On the other hand, to extend its coverage in the way indicated by Professor Ziman to satisfy a relatively small though very important group of advanced scientists, might very well price it beyond the pockets of the institutions where these scientists work. It seems it has done so already in Britain where the price appears astronomical, although I am told that it is by no means so in the United States where, I suspect, the institutions which purchase the *Index* also have full stocks of the conventional abstract journals.

The result may well be that, like too many other aspects of science, the scientists outside the United States will be at a permanent disadvantage by not possessing this valuable instrument for understanding and picturing the whole processes of science. There would be a good case for keeping it in at least one or two libraries in Britain so that it could be consulted when necessary.

It may even be possible that the dilemma posed by Professor Ziman between the cost and coverage can be met by a more judicious first source list. The enormous labour of making and keeping up to date the *Citation Index* even with the use of computers—without which the task would have been quite impossible—is in itself an index of the incredible confusion that has accompanied the publication explosion in the last two decades. In a rational publications system, the making of the index would itself be an automatic part of the publication process. Citation entries would have been made automatically on publication and properly centrally stored, thus enabling a much better citation index to be had far more speedily at a much lower cost.

Such a change is, indeed, long overdue and its wider effects will be felt when it becomes a necessity to devise some economic scheme to replace the incredible waste of brains and time that exists in the present scientific publication systems. Dr Garfield is to be congratulated that he has not waited for this but has plunged into the mess of present-day publication and tried to extract from it in a logical way some indication of its interrelationships.

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