Derek J. de S. Price (1922–83)

Historian of science and herald of scientometrics

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The 1960s, a golden age for academia in the USA, witnessed an unparallelled expansion of disciplines, among them the history of science and quantitative studies of science. A major pioneer in developing those fields was Derek Price, whose leadership of Yale University's newly created Department of History of Science and Medicine helped to bring national prominence to research about science and scientists. Price's legacy to the history of science, science policy, and scientometrics continues to be influential today. Three of Price's students recall the chemistry of the first years of the department, and reflect on their experiences with Price the scholar, teacher, and mentor.

Derek J. de Solla Price (1922–83) was known as an expert on the subject of scientific instruments, especially the astrolabe. One of his earliest discoveries, made in the library of Peterhouse, Cambridge, was of a description of an astrolabe in a document by Chaucer containing the only known autograph of the medieval author.[1] He also achieved fame for his detailed description and analysis of an ancient Greek calendar computer, recovered from the floor of the Aegean Sea, known as the antikythera mechanism[2] (*see* illustrations opposite).

Price was more widely appreciated for developing the 'science of science', now known in its quantitative aspects as scientometrics,[3] but also involving the new fields of bibliometrics and informetrics. His findings concerning the exponential growth of science, scientists, and scientific papers was but the beginning of a series of fundamental contributions to science indicators and the quantitative modelling of scientific activity, which included using citations to analyse and map scientific fields along with their structures and evolution over time, distributions and structures of authorships and theories of cumulative advantage, and informal collaboration in 'invisible colleges'.[4]

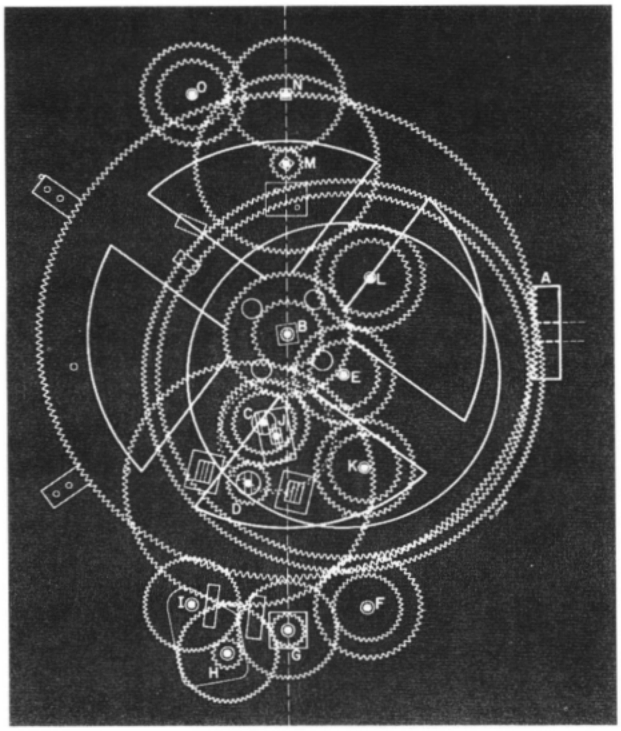
Life history and career

Price began his studies as a physicist, earning a PhD in 1946 from the University of London in experimental physics, for his study of the emissivity of hot metals. A Commonwealth Fund fellowship brought him to Princeton University that same year. However, jobs in physics were scarce, so he accepted a post teaching applied mathematics at the University of Malaya in Singapore for 3 years. One of his colleagues there was Professor C. N. Parkinson (formulator of Parkinson's Law), who taught maritime history. It was Parkinson who inspired a love for history in Price, which was to prove an enchantment for his whole life.

In Singapore, Price took the university's complete run of *Philosophical Transactions of The Royal Society* into his custody for safekeeping. He began to read through the volumes, becoming increasingly absorbed in the history of science. Stacking the volumes in chronological order as he read, he noticed that the piles increased exponentially with time, an observation that grounded his later extensive and creative studies in the science of science.[5] At the time, however, virtually no one took interest in his announcement of the exponential growth of science.[6] His interest in history of science thus whetted, Price pursued the subject at Cambridge University, from which in 1954 he received a second doctorate in history. For his dissertation, he had planned a survey of scientific instruments, but when he found the manuscript of Chaucer's on the equatorie, he narrowed his focus to that.

In 1957 he came to the USA as a consultant to the Smithsonian Institution's Museum of History and Technology (now called the Museum of American History); in 1959 he began a year as a visitor in the Department of History at Yale University. While at Yale, the Department of History of Science and Medicine was established, and Price found in it a permanent home.[7]

Fundamentally, it seems that Price was very Pythagorean. He was impressed with the extent to



General plan of antikythera mechanism: composite diagram showing all gearing; after Ref. 2

which numbers constituted reality – and relatively simple, harmonious patterns of numbers at that. Consequently, for him, scientific instruments (which serve as refinements and extensions of our gross and limited physical senses) were intermediaries between the world of numbers, which they provided through measurements, and the world of ideas and concepts, which we use those numbers to test. Instruments, quantification, science, and the science of science came together for him in the history of science. Indeed, he often remarked that this field embodied the academic trend for interdisciplinary studies: in itself, history of science was interdisciplinary. (Price was a member of the Editorial Board of *Interdisciplinary Science Reviews* from its inception in 1976 until his death.)

It is now more than a decade since Price died in London. Some of his earliest students, reminiscing about their mentor, decided to record their thoughts about the man who served as first Chairman of Yale's Department of History of Science and Medicine.

Eri Yagi returned to Tokyo in the mid 1960s and has spent her career at Toyo University; her publications include many that are in the quantitative tradition taught by Price.[8] Lawrence Badash, whose interests lie in the history of twentieth century physics and the role of scientists in the nuclear arms race, taught briefly at Yale and has been in California ever since. Donald Beaver, whose work follows most closely the interests established by Price, taught at the University of Missouri-Kansas City and then moved via Franklin and Marshall College to Williams College, where he has been since 1971.

Derek Price with a reconstruction of the antikythera mechanism

Creation of Yale University's Department of History of Science and Medicine

The first students: Badash

Badash first learned in the spring of 1960 that Yale planned to open a new Department of History of Science and Medicine. As a first year graduate student in physics, he was using an oscilloscope to decipher the circuitry inside what was literally a black box with several terminals jutting out from its sides. His laboratory partner[9] interrupted his concentration to tell him the news. This friend had been interested enough himself to discuss the matter with Price, who was heading the physical sciences side of the new programme.

The subject piqued Badash's own interest, too, for he had not realised that history of science was a recognised academic discipline. He did not know that one could get paid for doing it! In his *naïveté* he had assumed that the history of their subjects was written usually by retired physicians and scientists. However, Badash realised that he had a long standing, amateurish interest in the history of science, for as a senior physics major at Rensselaer Polytechnic Institute he had talked one of his professors[10] into giving him a grade for reading and discussing biographies of Galileo, Newton, Maxwell, and others.

Curious about this 'new' subject, Badash called on Price and learned, through a strong British accent laced with Americanisms, about the department's plans. Price's enthusiasm for history of science was contagious and his assurance that Badash's almost total lack of undergraduate history courses would not be a barrier for entry into the programme was most encouraging. None the less, Badash was conscious of receiving a 'hard sell', perhaps because he was one of the first potential applicants to the new department.

Price paraded before his eyes visions of studying Babylonian astronomy, the Scientific Revolution, the history of modern physics, and other exciting subjects. He also impressed upon Badash how much in demand historians of science were. Some Mid Western universities, he said, were bidding for newly minted PhDs by offering them lakeside cottages. Badash was not so gullible as to swallow that (when he graduated 4 years later there were good postdoctoral fellowships and jobs available, but no villas on the water); still, he was hooked on becoming a historian of science.

Eri Yagi

Eri Yagi was a fourth year graduate student in physics at the University of Tokyo when she learned, in a letter from Price, of the creation of the new department. Only a few years before, she had come across his pioneering paper of 1951 on the quantitative measure of the development of science, published in the *Archives Internationales d'Histoire des Sciences* (*see* Appendix, item 13). Yagi had been searching for a useful method to analyse the development of physics in Japan, something other than the traditional descriptive approach of the external history of science. To her, Price's statistical manner seemed well suited for analysis from a macroviewpoint.[11]

Yagi's interest in the history of science had developed from 'Bikini shock', the public reaction to a US hydrogen bomb test in the spring of 1954 that left Japanese fishermen injured by radioactive fallout. Then a fourth year undergraduate physics major at Ochanomizu University, she began to question her future as a molecular physicist. Looking for a path to a career of building appropriate scientific organisations for developing countries, including Japan, Yagi decided to study the history of physics, focusing on the external and institutional aspects. At that time there were but two graduate departments of physics in Japan where one could study the history of physics: under Professor Hidehiko Tamaki at the University of Tokyo and under Professor Takehiko Takabayashi at Nagoya University. No graduate school existed for the more general subject of history of science. She chose the University of Tokyo and entered in 1955.

There she began to apply Price's method to such Japanese data as the total number of doctors of science at Japanese universities (1898–1957) and the membership of the Physico-Mathematical Society of Japan (1877–1945). She found that Price's conclusion for the development of science in the West was confirmed in modern Japan, namely that nearly all curves showed exponential growth, with a doubling interval of 10–15 years. When Price received Yagi's letter containing these results, he invited her to work with him at Yale.

On his recommendation, Yagi was given a US Public Health Service training grant (1960–63), as well as a Fulbright travel grant. The terms of the latter grant, as well as rules of the University of Tokyo, required her to return to Japan in 1963, and she received her PhD in the Tokyo physics department 2 years later. Her dissertation consisted of two parts: statistical approaches to the history of science in Japan and the origin and development of Hantaro Nagaoka's 1903 atomic model.[12]

Beaver

Beaver came to Yale in September 1961, as a member of the department's second class. He had first heard of Derek Price the previous spring, during a first year of graduate study in physics at the University of Massachusetts when he became interested in studying the philosophy and history of physics. Early that year he drove to Yale to meet Price, soon to be named Avalon Professor of the History of Science, the first endowed professorship in the history of science in the USA. The two talked for about 2 hours, with Price, as usual, doing most of the talking once he had got started. During the conversation Beaver happened to mention having been impressed by the idea of the possibility of statistical laws of behaviour, as suggested by the laws of psychophysics in Asimov's 'Foundation' stories. Price, suddenly very excited and enthusiastic, got up from his chair and walked across the room; opening the doors to one of the cabinets supporting the bookcases lining the walls, he pulled out a looseleaf binder stuffed with graphs and charts. He showed them eagerly, pages and pages of them, again and again illustrating the phenomena of exponential like growth in a wide variety of human activities, not just those of science and technology. At the end of the conversation, Price said: 'It was good to have met you. I'm looking forward to seeing you in the fall.' And that was how Beaver entered the Yale programme, and set out on the scientometrical road away from the history and philosophy of physics.

Before the department opened in September 1960, Diana Long and Badash, among the first graduate students admitted, were asked to meet an official from the National Institutes of Health. Apparently they (and the programme) passed muster, for this body became a continuing source of support. This, however, highlighted a peculiarity of the department in having one foot in the Yale Medical School and the other foot across town in the College of Letters and Science.

The new department and its projects

The eminent physiologist and historian of medicine, John Fulton, had the vision and energy that moulded

Chronology of Derek Price[31]

1922	born in London (father was a tailor)
1938 (16)	laboratory assistant, South West Essex Technical College
1942 (20)	BSc in physics and mathematics, University of London
1946 (24)	PhD in metallurgical physics, University of London
1946–47 (25)	Commonwealth Fund fellow at Princeton, NJ, USA
1947 (25)	married Ellen Hjorth of Copenhagen
1947–50 (28)	lecturer in applied mathematics, University of Singapore (reading *Philosophical Transactions of The Royal Society* led to Price's Law: exponential growth of science)
1950 (28)	delivered paper on Price's Law at the International Congress for the History of Science
1951 (29)	published it in *Archives Internationales d'Histoire des Sciences*
1950–54 (32)	studied scientific instruments at Cambridge, UK
1954 (32)	PhD in history of science, University of Cambridge, UK
1957 (35)	came to USA as a consultant to the Smithsonian Institution, Washington, DC
1958 (36)	fellow at Princeton
1959 (37)	visiting professor at Yale
1960 (38)	'Heavenly clockwork: the great astronomical clocks in medieval China' (with Joseph Needham and Wang Ling)
1960 (38)	Avalon Professor of History of Science. Inaugurated and built up graduate studies in history of science and medicine at Yale (to 1978). Graduate students under Price: Lawrence Badash (1960–64), Eri Yagi (1960–63), Donald Beaver (1961–65)
1961 (39)	'Science since Babylon'
1962 (40)	Price–Yagi model for production of 'home grown' physicists
1963 (41)	'Little science, big science' (2nd edition in 1986)
1975 (53)	'Gears from the Greeks: the antikythera mechanism'
1981 (59)	J. D. Bernal Award from the Society for Social Studies of Science
1983 (61)	died of a heart attack in London

the new department from Yale's bureaucracy. His death in the summer of 1960 suddenly thrust the chairmanship upon Price's shoulders. Leonard Wilson taught the history of chemistry and the biological sciences; he is well remembered as a superb undergraduate lecturer and for the intricate, multicoloured anatomical pictures he drew on the blackboard. Decades later, Price commented that he had welcomed Wilson as someone intellectually and personally almost at an opposite pole, so that between the two of them students would experience a spectrum of styles and approaches to the history of science.

In the fall of 1961, Asger Aaboe came to preside genially over studies in Babylonian astronomy and mathematics, and the mathematics of later periods. Edwin Clark, another early addition to the faculty, specialised in history of medicine; on one occasion he provided a fascinating mock demonstration of how surgeons amputated a limb, using historical instruments from the Medical School's excellent collection. Lloyd Stevenson and Frederick L. Holmes were still later additions to the faculty; Stevenson chaired the department for several years.

The new department had an unusually strong international flavour. Price and Clark came from England. Aaboe was a Dane, Wilson and Stevenson Canadians, research associate Francisco Guerra a Spaniard, and visiting scholar Stefan Dedijer (a Yugoslav who once headed his country's Atomic Energy Commission) came from Sweden. Research associate Martin Levy, though an American, worked in the Middle East for many years.[13]

Even the secretaries came from abroad: Joy Day from England, Leonore Sorger from Peru, and long serving Christine Tattersall also from England.

Price's lecture classes and seminars

In the department's first year of 1960–61, the class consisted of Eri Yagi, Lawrence Badash, Diana Long, Patricia MacLachlan, David Musto, Herbert Winnick, and a few part time students. Price taught a full year survey on the history of the physical sciences, from antiquity to the twentieth century, and Wilson offered its counterpart in the biological and earth sciences. These were undergraduate courses, but the graduate student novices had to take them also. Early graduate seminars included those by Wilson on eighteenth century chemistry, Price on scientific instruments (in which the students made a variety of sundials and an astrolabe), Whitfield Bell[14] on science in colonial America, Nathan Reingold on science in nineteenth and twentieth century America, and Frederick Kilgour on the history of technology.[15] So called 'directed work' under one's supervisor was also offered as a graduate course.

In a graduate seminar on scientific institutions, Price discussed various societies, museums, periodicals, and laboratories. One day the director of Yale's Peabody Museum joined the seminar as a guest. It was warm, so the class sat on the grass in front of the Victorian house at 56 Hillhouse Avenue, where the Department of History of Science and Medicine was headquartered. Dillon Ripley was no doubt familiar with roughing it in the field, but the noted ornithologist, who soon was to be appointed Secretary of the Smithsonian Institution, looked just a bit uncomfortable in his impeccable three piece suit. Price, whose dress was usually more flamboyant, seemed comfortable in any pose or location.

Price was an impressive performer. He was observed to spend little more than 5 minutes jotting down notes on a small slip of paper and then to lecture to undergraduates for an hour without any other props. He held that anyone who really knew his subject needed no more reference material to deliver a good lecture. A few years later, however, Badash graded exams in that course and had occasion to sit through the lectures again. At that time he recognised that Price's lectures would have profited from a bit more attention. Price got the gist correct, of course, but some of his supporting facts did not correspond with what his graduate student had found in the texts. Yet that was Price's style: get the big picture correct and leave it to others to clean up the details.

Price's interest in quantitative science

Always interested in numbers, Price impressed upon his students the need to treat them sceptically. Were they honest observations or 'cooked' to conform to a theory or formula? Using both Babylonian astronomical tables (his students learned to decipher simple cuneiform numerical documents) and a Greek table of optical refraction credited to Ptolemy, Price took the differences between the entries, and then the second differences. Behold! The second differences were constant. Anyone who has ever taken data in an elementary science course knows that nature (or at least our recording of it) is never so constant. Not only were the tables cooked, but it was clear what 'theory' their authors followed.

In a graduate seminar on sociology and politics of recent science, Price suggested the use of a type of metric for the analysis of a group of scientific papers. The method itself had been developed for the study of group structure in sociology and psychology. To measure the 'connectivity' of scientific papers the value 1 was assigned if the later paper cited the earlier, and 0 if it did not. Using this technique, Price in 1965 published his pioneer work on the existence of the research front, 'Networks of scientific papers'.[16]

Price suggested to Beaver that he look at the bibliography of N-rays, so that perhaps he could make a study of that literature his seminar project. Yale's excellent library collections enabled Beaver to examine nearly every one of the several hundred papers in the bibliography, and to list for each paper the references it cited, both within the N-ray literature, and outside it. He finally prepared, on a large sheet of paper, a triangular matrix with ×s displaying which papers cited which, in chronological order. Beaver took the matrix and showed it to Price, who looked at it for a while, then held the paper up horizontally in front of his eyes, and squinted along the surface, down the diagonal of the matrix. Then he looked down the lines of ×s in the squares parallel to the diagonal. He looked, looked back again, and said: 'I can see the research front – you've got a picture of the cutting edge of research.' Price's next reaction was to decide how his idea might be measured or defended, and he set Beaver to work calculating the density of citations along the diagonals parallel to the main diagonal. The matrix, and some of the associated conclusions (especially those relating to the mopping up or summarising functions of review papers, and to 'classics') drawn from the study of N-rays, appeared in Price's classic 'Networks' paper. Of course there was much more to that paper, but Beaver always enjoyed the way Price could sew together bits and pieces of facts and observations into a splendid new fabric. (N-rays were, of course, spurious and never existed.)

Yagi also published a paper on an application of the metric. She looked at a series of reports written by a Japanese group of nuclear physicists that included Yoshio Nishina, Hideki Yukawa, and Shinichiro Tomonaga.[17]

A little before this fruitful seminar in early 1963, Price had met Eugene Garfield, who was developing his *Science Citation Index* (*SCI*). Price always looked on the *SCI* as a wonderful kind of scientific instrument for measuring what he called the

Derek Price, aged 40: drawing by D. Chittock, dated 2 May 1982

'epiphenomena' of science, and praised it as a boon to the academic world.

Science Citation Index

Garfield was necessarily more commercially inclined, but none the less very sensitive to the broader importance of the *SCI*. Price's use and promotion aided its growth in the early years, as did his suggestions for improving it. In return, he had access to unique data, which helped to lead him to a number of quantitative ways to understand and describe scientific activity. These included the intellectual ancestry of scientific papers as revealed by their citations, the halflife and impact factor of scientific papers and journals, and some preliminary work with co-citation analysis and mapping of scientific fields and subfields.

Price could be a spellbinder before an older audience as well as before his students. As a new professor, he delivered a series of public lectures on several of his special interests. These were published in 1961 as 'Science since Babylon'.[18] His exploration of the science of science formed the basis of one of these highly interesting presentations. From it, most of his graduate students learned for the first time about the growth rates of scientific personnel, periodicals, articles, funding, and other parameters. If it could be counted, Price inevitably sought to quantify its development over time. Also, for the first time, his audience heard the catchy phrase that 90% of all the scientists who ever lived were alive today (he asked where the several Newtons and Einsteins were) and saw his sigmoid curve. With doubling times usually of 10–15 years, many of the activities he counted would grow to enormous size. Indeed, Price predicted that if the growth of science were unrestrained, the entire US budget would soon be spent on science and every human on earth would be a scientist. Clearly, the curve had to bend over, into its familiar S shape; the thing that captured the attention of the fledgling coterie of science policy planners was the inflection point where negative curvature (a turn to a decline in the growth rate) would begin.

Inspired by Price's enthusiasm, Badash made a few simple counts, such as of articles in the early *Philosophical Transactions of The Royal Society* and US fellows of The Royal Society. His one serious endeavour in this field occurred at the end of the department's first year. The American Chemical Society (ACS) was much concerned with the increasing cost of its publications. Keyword guides to the literature and other aids to quick searching seemed not to reduce the volume of pages printed or result in other economies. Somehow ACS contacted Price, who agreed to study the problem. He saw as necessary an evaluation of just what the situation was, for the ACS had no record of the number of pages published each year or articles abstracted from 1907, when *Chemical Abstracts* (*CA*) first appeared.

Price had plans to spend the summer of 1961 in Europe; Badash had need of summer employment. After a trip to ACS headquarters in Washington to define the project, and with much advice from Price, Badash purchased a thick pad to cushion his elbow and spent endless weeks in the Yale library running his finger over pages of *CA*. He counted the number of abstracts on a page, obtaining good enough statistics to estimate the number of abstracts per year. He counted other things, such as the number of journals abstracted by *CA*, *CA* subscribers, *CA* staff as a function of number of abstracts, and so on. Then he plotted the various sets of data, looking for trends. To his dismay, most plots showed no pattern.

The one bright spot, however, was the number of authors per abstracted article. This showed, in a smooth curve, that early in the century most (~80%) chemical papers had a single author, as one would expect. The situation changed little by 1920, but it had begun to accelerate downward. In 1930 it remained above 70%, but by 1940 was ~65%, 1950 ~55%, and 1960 ~35%. Obviously, papers by two, three, and more authors increased in this period.[19] It was as nice a proof as one could wish of Price's claim that the nature of science was changing and that the condition we then already experienced should be called 'big science'. By 1955, team research had become the dominant mode of research production and team research was, of course, a characteristic of big science. Badash wrote up the report, Price modified it somewhat on his return to New Haven, and they sent it to the ACS. To their chagrin, the ACS was terribly disappointed. The society was really not interested in where they had been; they desperately wanted a solution to their astronomical publishing costs and thought that it could be provided by a couple of historians of science on the cheap.

Price as dissertation director

As a physics student, Badash's interest had been drawn mostly to radioactivity, atomic physics, and nuclear physics. While many areas of history of science appealed to him once he transferred to the new department, twentieth century physics remained in the forefront. Price had little experience with the history of modern physics, other than some cursory investigations of J. J. Thomson's career, so it was something of a pleasant surprise to Badash when his mentor suggested that his dissertation topic might be on the history of radioactivity. Badash remains unaware how or why Price obtained it, but the professor gave his student a roll of microfilm that contained the correspondence exchanged between Ernest Rutherford (at McGill, Manchester, and Cambridge) and Bertram Borden Boltwood, the USA's leading radiochemist, who spent his career at Yale. Price apparently recognised that these letters painted a most interesting picture of the scientific community during the century's first two decades, in addition to any technical commentary they contained. Indeed, Badash edited the letters for that very reason[20] and wrote his dissertation on the development of radioactivity, especially the US story.[21]

Badash recalls Price as a superb dissertation director; his style, however, might not work well for every student. Price started by requesting a written outline of the dissertation, chapter by chapter. Badash protested that he then knew the subject's history inadequately, had scarcely begun to locate manuscript resources, and had read hardly any of the published scientific papers. Price replied: 'Do it anyway.' Badash did it, and found it to be a very useful exercise, for it made him obtain a quick overview of the field and decide upon his research strategy. Of course, the final work departed from that first outline. That was not the point; Price wanted him to start thinking seriously, and wanted him to have a framework against which to test his ideas, even if the structure was necessarily flawed.[22]

As Badash wrote chapters of the dissertation, Price left him alone – which was precisely what his student wanted and needed. Badash required no external pressure to concentrate on the project and would have been uncomfortable with someone peering over his shoulder. However, when he had a draft, Price was immediately available to read it. Then they would meet to discuss it. In half an hour Price gave Badash enough ideas for a week's worth of digging in the library. That was Price's strength: his extremely fertile mind generated ideas by the bucketful. He was not especially interested in the details of the radioactive decay series, but he had a fine sense of what was important, what the issues were, and how something from a totally different field might be relevant to his student's work. Above all, Price taught his students that a historian must ask questions, preferably interesting ones.

Badash's dissertation on radioactivity even allowed him to return, in a small way, to the quantitative study of science. He counted the number of papers published by Americans early in this century, and found that the graph showed inflection points that corresponded well with conclusions about the development of this science that he had already discussed.[23]

60 years of Japanese science analysed

Yagi carried her quantitative studies to a higher level. In the course of directed work with her mentor, the Price–Yagi growth model of Japanese physics emerged in 1962. Yagi had already shown that the Price exponential theory could be confirmed in Japan for the growth of modern physics, but how this growth was caused became the next question to be answered. From biographical data of physicists who were active in Japan between 1870 and 1930, Price and Yagi were able to distinguish three groups. Group I consisted of foreign teachers and the first batch of foreign trained Japanese physicists; group II comprised the Japanese students of group I; whereas group III embraced the Japanese students of Japanese teachers. The number of the last group, termed 'home grown physicists', showed normal exponential growth.[24]

Price's idea of grouping the data was based on an analogy with seismic waves. He explained to Yagi that the first shock wave was group I, and its secondary waves were the other groups. His analogy may have had its origin in some of their earlier discussions in which Yagi explained to him that foreign teachers had engaged in studies of such local phenomena as earthquakes and trained their Japanese students in seismology. Price had a great talent for combining such seemingly unrelated historical facts and presenting his ideas in a new and fascinating fashion.

Vital statistics of journals

Beaver, too, examined birth, growth, and death rates, beginning a study of the quantitative history of scientific and technical journals in the USA; this became a summer project in 1963. In it, Beaver found several new relationships describing the vital statistics and age structure of scientific journals, and developed a descriptive mathematical model for them. On average, in each year for 180 years, the number of American journals had grown by 7%, a rate which resulted from births of 16%, and deaths of 9%, striking extant journals at random.[25] The study involved coding and keypunching over 1000 cards of data, and persuading the computer centre to sort the data in various ways – something easily accomplished on a personal computer with the database software of today, but far more difficult in 1963. Beaver even attended classes in Fortran, and designed a pilot program for the sorts, which the computer centre then took over, refined, and executed. Besides the exponential growth and decay of the journals, his data showed that the number of journals aged n or more years was proportional to n^{-1}, and that half of the past's scientific literature could be found in a few core journals, equal in number to the square root of the total number of journals.

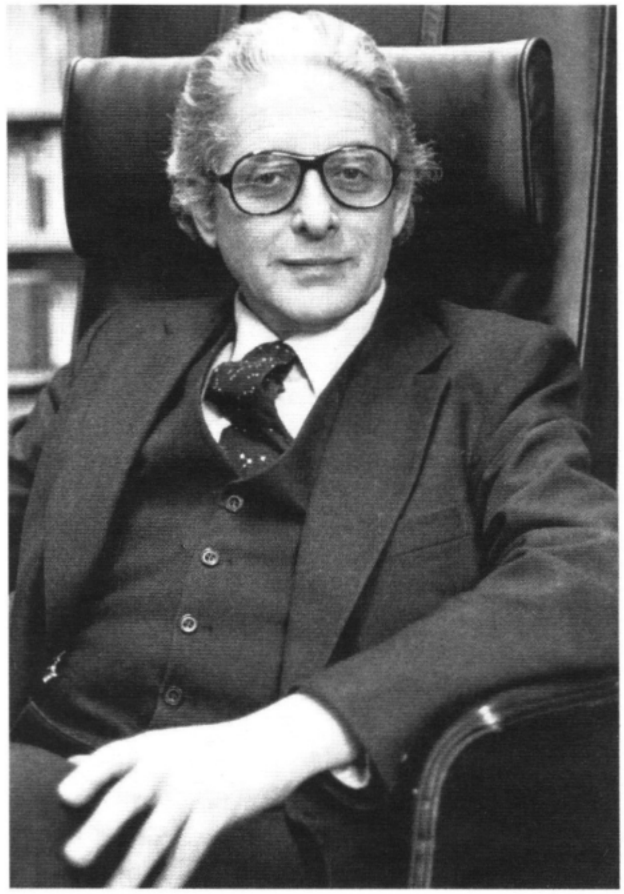
For the next 3 years, Beaver worked as Price's research assistant, and they managed to produce two major pieces of research, involving collaboration in an 'invisible college', and the mapping of scientific fields. Working on the invisible college was one of the high points of Beaver's work with his mentor, who rarely formally published with others, having had only a handful of other collaborators. In that sense, Price was very much the individualist in research.

Maps of research groups

The dynamics of that research were relatively simple, in comparison with the mapping work they did together. Price came to Beaver one day saying that he had a great idea. Price had learned about an experiment involving scientific literature being carried out by the National Institutes of Health (NIH), which had established what it called information exchange groups, or IEGs. The idea was to identify a research area for which NIH would act as a central communications coordinator and supplier: members of the IEG would submit 'papers' based on recently concluded research, before publication or even submission to scholarly journals, and NIH would distribute copies of the papers to all the members of the IEG. Price thought that it might be worthwhile to choose one of the seven subjects being supported, and study the research practices of that group, as revealed through the preliminary accounts of its members' research. So he signed up for 'Oxidative phosphorylation and terminal electron transport'. Price was proud to say he did not understand the terminology or the subject, and thus he could be 'objective' in analysing its literature. Beaver only learned the significance of the subject in the late 1970s.

As one of the group, Price received copies of all the 'papers' produced by the IEG members, and Beaver encoded data about each one on an index card, in order, by title, authors, and cited papers, differentiating the latter between references to IEG produced items, and items from outside the IEG. Then Price and Beaver got to work sorting the cards in different ways, and counting up authors, co-authors, and groups, looking at their evolution over time, and calculating productivities in various ways.

One of the most rewarding features of that work came when they realised that they could check Price's



Derek Price at Yale University

hypothesis that collaboration represented an ad hoc response to a shortage of scientific manpower, a means of 'squeezing out' a fraction of a paper's worth of product from those who only had a fraction of a paper's worth of research in them. (Such essentialist speech may seem strange in the 1990s, but it made perfect sense in the mid 1960s.) Price and Beaver had noticed that the IEG group structure consisted of small clusters of authors centred around a core continuant, and that a large number of authors' names appeared only once, in co-authorship with a number of others. It looked as if the research group structure were a core–transient one. They decided to credit co-authorships by two different methods, in what they called full (each co-author received one paper's credit) and fractional (each of the n co-authors of a paper received one-nth of a paper's credit) authorships, in order to see what differences in productivity they might reveal. It turned out that the distribution of papers among authors followed the same relationship in either case, down to authors who produced only one-quarter of a paper. Price was delighted with the result!

Invisible colleges

The consequence of a lot of grinding work turned out to be 'Collaboration in an invisible college', which became a pioneering work in the study of scientific collaboration.[26] A gratifying qualitative confirmation of a quantitatively derived conclusion gave the research a special character. Price and Beaver had noticed two very large collaborative groups forming over time, groups which rarely referred to each other's papers. Following his earlier intuition about the appearance of abstracting journals (when there were about 300 journals, there needed to be some new and more efficient way of organising or indicating the information therein), Price hypothesised that this was a sign of a breach or division in the field, and that it would soon be breaking into two subfields.

In fact, this new IEG example pretty much confirmed him in the notion that no subfield survives a size of about more than 300–500 people – that typically being enough to found a new journal and create a new society. Any larger, and the group begins to split. A year or two later Price told Beaver with great satisfaction that he had found out at a conference that his prediction was correct. The two groups were indeed formed of disciples of two strong but warring personalities, with very different ideas of how further research in oxidative phosphorylation was to be carried out.

Price's 'ideas of the moment'

The mapping research was probably the most exciting and demanding work Beaver did with Price. It certainly beat writing a thesis under his direction. Price had data. He had masses of data. He had sociological data on the class status of fathers and sons for different countries, such as Great Britain and Denmark; he had economic data on imports and exports between the industrialised Western countries; and he had even managed to obtain from the Southern New England Telephone Company aggregate data on the numbers of calls from one to another of all the different Connecticut exchanges. He also had some very interesting aggregate data on the citations of the journals of the American Psychological Association.

But what they did with that data! That is the closest Beaver ever came to seeing Price think; and Price did a lot of it. At the end of an afternoon, Beaver would go off to calculate, seemingly endlessly, masses of numbers fitted into matrices and vectors (and in 1965 Beaver had no calculator). At 2 or 3am, he would either finish or quit, and try to get some rest in anticipation of what the next day would bring. Almost invariably, Price would start out by saying, 'Hello, what've you got for me?' and then proceed virtually to ignore what Beaver would hand to him. His next words would be something to the effect that, 'I had another idea last night, and I think it's the way to go. What we've been doing just isn't going to work, it isn't quite right.'

With those words threatening to render effervescent or irrelevant Beaver's previous evening's work, off the two went discussing his new idea. Beaver had a vested interest in pointing out the idea's shortcomings, but simultaneously could not but be charmed by the energy, enthusiasm, and persuasiveness with which Price always presented his 'idea of the moment'. Usually Beaver lost, but at least had the pleasure of anticipating how the numbers would bear out Price's new ideas. Would there be a pattern or not? Would the expected results fall out or not? That anticipation enabled him to get through the hours of painstaking number crunching that followed; and sometimes, just sometimes, he would see suggestive patterns or ideas himself, something to offer up in the next afternoon's discussion.

Simple representation of data

Early on, Price had some ideas for producing a simplified representation of the sociological data, and they worked rather well. From the output, one could immediately see differences in class stratification over time. For example, in Great Britain, everyone was coming together into one great middle; in Denmark, on average, everybody tended to move up, while maintaining the number of different classes. Beaver does not recall now how the bottom class kept regenerating itself as a higher and higher floor, or how the top class kept adding penthouses, but the recollection is very clear of a move upward as a whole, compared with a levelling as a whole.

One afternoon, Price and Beaver had a long discussion of why that model could not simply and easily be extended to the journal data for psychology. At that time Beaver had taken the previous day's suggestions and calculated for hours, and come in with results that pleased no one. There was something wrong with the model. In the middle of the discussion, Beaver remarked that he did not really think that journals' 'sizes' were simply measures of the numbers of articles they had published. For example, he said, some journals were more important than others, and hence were 'bigger', even though the number of articles they contained might be fewer than those for other journals. That the same journal might be perceived by others as having a different size immediately struck Price's fancy, and then and there he proceeded to modify the model.

The next day he had yet to have another inspiration, and so Beaver was able to report that the modification had significantly improved their representations of the data. Beaver made one other major contribution to the project, pretty much offhandedly, when Price and he were discussing a problem with the functions Price had developed for doing the mapping. The details and context are virtually forgotten – they had perhaps been taking counts of citations and adding them together, but Beaver recalls saying: 'Why don't you multiply them, as in independent probabilities?'

That did it. It produced the breakthrough that allowed them to make maps of all their data, and to represent the data sets in the most economical way, as the product of a row vector, a column vector, and a doubly stochastic matrix. Price, the former physicist, did not miss the analogy to a set of eigenvectors with a matrix of transition probabilities. Thus represented, the sociological, economic, and telephone maps turned out to make the meaning of the data easier to grasp and understand than did then current conventional methods. For example, they produced a map of the import–export data that put the USA, Great Britain, Canada, and Ireland together as a cluster, with the distances between the countries just as in a geographical representation; then there were the Benelux nations, the Scandinavian bloc, and so forth, all as if the economic mapping had simply redone the geographical globe, in a slightly contorted but recognisable way.

The journal citation maps turned out to be equally fascinating, and, except for one initial problem, quite representative of disciplinary structure and affinities in psychology. The initial problem was that a couple of journals turned out to be equidistant from all the others. After a day, or maybe two, Price came in and said: 'I've got it. They're review journals. Because they review all the fields of psychology, they come out blanketing the subfields, and they really *are* equidistant from all the other journals.' Sure enough, the interpretation removed the difficulty – but also reinforced their feeling about the power of their modelling.

Growth of seminal success

Since those early days, Henry Small of the Institute of Scientific Information (ISI) and many others have greatly refined those procedures, and the mathematical creation of clusters and structures and maps from citations, co-authorships, co-words, and textual analysis has become tremendously sophisticated and powerful. At that time, however, Price and Beaver were technologically quite backward. Although they might have used the computer, Price did not know how, and was not inclined to learn; what little Fortran Beaver had picked up was not going to be sufficient. Price was proud of his ability to compute quickly, using small, simple models to support or reject a hypothesis, without recourse to lots of fancy power. When it came to the numbers he almost always turned out to be right. That practice of his may be why he had such sympathy for the scientometricians of the former Eastern Bloc, because they, too, had to use their intuition and imagination in the absence of electronic wizardry.

During the 3 years that Beaver worked with Price, he also worked on and finished his doctoral thesis, a statistical–historical study of the American scientific community between 1800 and 1860.[27] Beaver used data from his research on scientific journals to generate a list of the most productive American scientists, 138 individuals who produced more than 50% of the science of those years. Using that list as an 'objective' characterisation of the elite, he followed up its generation with a prosopographical study of the scientists' biographical and professional characteristics. Coincidentally, Clark Elliott and George Daniels were pursuing very similar lines, marking a significant historiographical transition from central concern for the well known heroes of science, to consideration of scientists of lesser rank and renown, together with their sociocultural milieu.[28]

Training students for the profession

Price encouraged his students to become 'socialised' to the profession early, by attending meetings and presenting papers, submitting articles for publication, and trying for research grants. On one occasion Badash submitted a grant proposal to the National Science Foundation. Because most of the department's funds came through the Medical School, this proposal was sent there for processing. To his dismay, the Dean's office refused to forward it, saying that the odds were strongly against a graduate student receiving such a grant, and that they did not want to lower Yale's 'batting average'. Badash happened to know a high level administrator on the College side who was not so caught up in the institution's image; he extracted the proposal from the Medical School and sent it to the foundation. In fact Badash did not receive the grant, but he did learn a lot about how to write a proposal.

With Price's endorsement, Yagi gave a paper on her work at the 1960 meeting of the American Association for the Advancement of Science, and was a commentator at the 10th International Congress for the History of Science, held 2 years later in Ithaca, NY and Philadelphia, PA. Badash, Diana Long, and David Musto also attended the congress, and Badash gave papers at annual History of Science Society meetings in Bloomington, IN and Montreal. Beaver first tried his wings in spring 1966 at the annual meeting of the Midwest Junto for the History of Science, held in the Linda Hall Library in Kansas City, MO. Graduate students also were expected to present talks at the department's colloquium series.

Price often tried to steer jobs toward his students. The advertising agency for Laboratory for Electronics (LFE) was located in New Haven, CT and needed someone to help to write copy for LFE's highly specialised products. Badash was sent to apply, and when he detected a mixture of kilocycles and megacycles in a piece of draft copy that the agency had composed, they concluded that he knew enough about science and hired him. Another, more interesting, job Badash had was locating photographs to be used in the Time-Life Books volume on 'Matter'. He insisted that he receive a copy of each picture the editors gathered, and thus acquired a handsome start of a collection centred on atomic and nuclear physics. Badash even became perhaps the world's only manufacturer of replicas of ancient Greek sundials. Following some directed work on astronomy in antiquity, he constructed moulds of wood, glass, and sheet metal into which he poured plaster of Paris to form both conical and spherical scaphe models. With Price's help, he sold a set to the Adler Planetarium in Chicago, IL, site of the Western Hemisphere's best collection of astronomical instruments; the Burndy Library in Norwalk, CT also received a set, as did the Yale Collection of Scientific Instruments (of which Price was curator).

One spring day in 1966 Price dropped a pile of computer paper on Beaver's desk and asked him what he might make of it. The printout was a preliminary list of the candidates for the forthcoming 'Dictionary of scientific biography', and gave names, dates, and the proposed length of the article for each scientist. Always delighted to evaluate data, Beaver spent a week making quantitative analyses and found a number of lacunae as well as biases in the planned coverage. Shortly afterward, Price and Beaver met in New York with Editor Charles Gillispie and with Robert Merton and Harriet Zuckerman. As a result, the dictionary rethought its coverage, and there was some suggestion of possible employment for Beaver in the fall. He, however, decided to take the regular university position for which he had just been interviewed at the Junto meeting, a position at the University of Missouri at Kansas City.[29]

Beaver also benefited from a more conventional academic opening. Through Price, he obtained a temporary post teaching a history of astronomy survey at Brooklyn Polytechnic Institute, NY when Professor Edward Rosen was unable to deliver the already scheduled course in the spring semester of 1966. In all these efforts to hearten his students to become professionals in the larger arenas of history of science, Price was never dogmatic or strongly opinionated. A gentle comment here or a word of encouragement there; mostly he inspired by his own activities.

Price's personality

It did not take long for Price's students to become aware that he was a person who generated strong feelings in others. People either liked him or disliked him: unfortunately, too often the latter. Thanks to such animosity, graduate students in Yale's Department of History were actively discouraged by their professors from taking courses in History of Science and Medicine. (Conversely, History of Science and Medicine students were never encouraged to enlarge their history backgrounds in what has long been regarded as the USA's premier history department.) Partly, it was Price's outward arrogance that put strangers off; this characteristic may have been the result of some immaturity in his makeup. To his students, he was often overcritical. Backhanded compliments were too frequent: 'It's quite good, but not *very* good.' This made Price something of an 'antirole model' for his students, who in later years endeavoured to be more understanding and supportive of their own students. Yet, he was certainly not without positive traits, and his role model behaviour in socialising students to the profession has been mentioned above. Beyond this, he was exuberant, accessible, and frequently supportive. Most importantly, Price radiated such enthusiasm for his field that his students knew that they had made the correct choice.

Around 1962, the well known philosopher of science Norwood Russell Hanson came from Indiana University to join Yale's Department of Philosophy. Hanson had studied and worked in England, and he and Price were old acquaintances, possibly even competitors for the same job. One day Hanson walked into Price's office and, in the British accent he affected, said, 'Oh, hello there, Derek', to which Price, in the American accent he affected, said, 'Hiya, Russ'. The reversal was amusing to observers, but the humour was lost on the participants. Despite their overt friendliness, there was an awkwardness between these two professors. Hanson avoided even attending History of Science colloquia, where his presence would have been worthwhile, at least to the graduate students; he did not want anyone to think that he was trying to insinuate himself into the new department. Price, for his part, seems not to have invited Hanson to any History of Science functions, apparently guarding his own turf jealously.

When Beaver laboured mightily to produce the first few pages of his dissertation, Price looked at the introduction, which tried to summarise as well as provide context and justification for some quantitative strategies. Drop those pages, Price advised; it was a waste of time to repeat something he himself had already said – and besides, said it better!

Price's unnecessarily critical character gradually moderated as he grew older. Yagi recognised this tendency in his review of 'Science and society in modern Japan', of which she was a co-editor. Price gave a positive evaluation of the book together with an unexpectedly generous comment: Yagi 'emerges as perhaps a key figure amongst the younger professional historians of science'.[30]

Another example of 'un-Pricean' behaviour was the case of the undergraduate woman who came to consult him in the late 1970s. Sensing that she was a bit nervous and unsure of herself, Price assumed a most avuncular role, quickly set her at ease, and chatted for 2 hours about her plans and hopes at Yale. Such Price 'epiphenomena' did much to soften his image in later years.

Conclusion

Despite the critical attitudes Price brought to his mentoring relationships, his students profited greatly from his high expectations and standards, whether they continued with him or not. At the beginning of his US career, Price was full of energy and ideas, which he offered and defended proudly, brashly, and combatively, a maverick who delighted in turning conventional ideas upside down and inside out. His dynamic enthusiasm made Yale's department an exciting and productive new star in the history of science, quickly rivalling its already established peers at Cornell, Harvard, Princeton, and Wisconsin. In at the virtual beginning of science policy, science indicators, scientometrics, the sociology of science, and the relations between science and technology, Price's teaching, lecturing, and writing greatly stimulated public and scholarly interest in those fields to which he had passionately devoted himself. Perhaps the 1960s were unique in providing an opportunity for individuals to have so much influence. Perhaps that is why no one has since come along who can so invigorate and stir up a field as could Derek Price.

Acknowledgements

The following is a short history of the present paper and demonstrates the international support from various people and organisations, for which the authors are very grateful.

In November 1992, Eri Yagi participated in an exchange programme at the University of Strasbourg, a programme established between the University of Strasbourg and Toyo University. She was invited by Professor Baudouin Jurdants to deliver a lecture (in English) on Derek Price. Eri Yagi recognised the French interest in the early years of scientometrics at Yale. Three letters were written by her to inform those interested: Alan Mackay, FRS, Professor Emeritus in the Department of Crystallography, Birkbeck College, University of London; Lawrence Badash; and Donald Beaver. Based on information from Alan Mackay, Dr Anthony Michaelis, the Editor of *ISR* and the oldest friend of Derek Price, asked the authors to write a paper for his journal.

From March to July 1993 Lawrence Badash visited Japan on the exchange programme between California and Meiji Gakuin universities. Badash and Yagi had several discussions in Tokyo. He wrote the first draft in Japan on 25 June 1993.

Donald Beaver, at the invitation of Professor Hildrun Kretschmer, delivered a paper on Derek Price at a plenary session of the 4th International Conference on Bibliometrics, Informations and Scientometrics in Berlin on 12 September 1993. Some of the present material is taken from that lecture.

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9. The partner was Robert H. Kargon, who has been professor of history of science at Johns Hopkins University for many years. He chose not to enter Yale's new department, but waited a year to see how Badash liked the subject – and then went to Cornell University to study under Henry Guerlac.
10. This was Heinrich Medicus, who, in retirement, became very active in the American Physical Society's Division of History of Physics, where some three decades after Badash's graduation they again worked together.
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13. Two other very helpful supporters were Elizabeth H. Thompson and Madeline E. Stanton at the Medical-Historical Library.
14. Bell was Associate Editor of the Franklin papers being published by Yale, and an expert in colonial American science. He later became Librarian of the American Philosophical Society and then its Executive Officer.
15. Reingold was a visitor that year from the Library of Congress and, like Bell, was 'dragooned' into rounding out the faculty of the new department. He returned to Washington, DC, where he became Editor-in-Chief of the Joseph Henry papers, and later a senior scholar in the Smithsonian's Museum of American History. Kilgour, Head of the Yale Medical School Library,

was one of the pioneers in the early development of computerised library records and interconnections; deeply involved in what has come to be the Online Computer Library Center, he subsequently became Librarian at Ohio State University.

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The manuscript was received 1 September 1994.

Bibliographic appendix

The following is a bibliography of the writings of the late Derek de Solla Price, formerly Avalon Professor of the History of Science, Yale University, prepared by Ann Leskowitz of History of Science, Yale University, PO Box 2036, Yale Station, New Haven, CT 06520, USA.

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The library and papers of the late Professor Price have been deposited at La Villette, Musée National des Sciences, des Techniques, et des Industries at 211 avenue Jean Jaurès, 75019 Paris, France. The Adler Planetarium in Chicago has also established a collection of Price's writings and publications.

This bibliography has previously been published by Silvio A. Bedini in Annali dell'Istituto e Museo di Storia della Scienza (1984, **9**, *(1), 95–115), but may not be widely known.*

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Donald de B. Beaver is Professor of History of Science at Williams College and Chairman of the Program on Science and Technology Studies. He received an AB in physics from Harvard and was a schoolteacher for 2 years before beginning postgraduate study. After receiving his PhD in 1966, he taught history of science at the University of Missouri-Kansas City, and at Franklin and Marshall College. In 1971 he came to Williams to participate in its History of Ideas Program, and to establish a curriculum in history of science. Since then he has been a visiting fellow at Yale, and a Sloan New Liberal Arts scholar at MIT. His research interests fall mostly in scientometrics and the social history of science. Helpful comments on his three fundamental papers in *Scientometrics* on the history and sociology of co-authorship of scientific papers, written with Richard Rosen, were provided by Derek Price. Professor Beaver is currently at work on a preliminary sketch of 200 years of science at Williams, and on a biographical treatment of Sarah Wallis Bowdich Lee (1791–1856), an accomplished British natural historian, author, and illustrator, the first European woman systematically to collect plants in tropical West Africa.