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Little book, big book: before and after *Little science, big science:* a review article, Part I

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Since its publication in 1963, Derek Price's *Little science, big science (LSBS)* has achieved 'citation classic' status. Examination of the genesis of *LSBS* and the state of the discipline of the history of science in the UK and the USA in the late 1950s demonstrates that Price's ideas were formulated during a pivotal period in the development of socio-historical studies of science. Price's talent for innovation and synthesis at an unsettled but highly charged time, and his appreciation of the pioneering work in science studies of the crystallographer J.D. Bernal, are reflected in the uniquely profound and wide-ranging respects in which *LSBS* has contributed to the development of scientometric and sociological theory.

INTRODUCTION

In 1963, Columbia University Press issued a short book by Derek de Solla Price (1922–1983), a British historian of science recently appointed to a chair at Yale University, New Haven, CT. Entitled *Little science, big science* (*LSBS*) (Price, 1963), the book contained the text of four lectures that Price had delivered at Brookhaven National Laboratory, NY, over two weeks in June 1962, together with a short preface written in November of that year. Like Price's previous book, *Science since Babylon* (*SSB*) (Price, 1961), *LSBS* attracted several high-profile reviews that collectively supplied a mixture of praise and mild censure, but it is unlikely that any contemporary commentator could then have predicted the remarkable subsequent ascent of *LSBS* to the status of 'citation classic' (Price, 1983). Since the late 1960s, *LSBS* (reissued in a paperback edition in 1965 and in an expanded version[1] in 1986) has been assured of at least 30 citations annually, receiving more than 1,500 in the last 40 years.

How did Price's small book come to have such a large impact? In an attempt to provide a partial answer to this question, an examination is undertaken in the present, two-part essay of the position *LSBS* occupies in the history of what has come to be known as scientometrics. In the sections that follow in this Part I, accounts are given of the genesis of *LSBS*; of the state of the discipline of the history of science at the time of its publication; and of the general contributions of *LSBS* to the development of scientometric and sociological theory. In the following Part II, the results will be presented of a citation analysis, updating the authoritative survey carried out by Garfield (1985a, 1985b), and providing quantitative evidence of the sustained influence of Price's best-known work; and a review will be undertaken of the responses made by others to *LSBS* and to Price's later refinements of his own arguments. Particular attention will be paid in this latter section to the topic, central to *LSBS*, of the growth of scientific literature.

The two-part essay is intended as a contribution to the intellectual history of scientometrics. In its course, it will be argued (i) that *LSBS* emerged at a pivotal stage in the development of socio-historical studies of science; (ii) that it was Price's appreciation of the work of J.D. Bernal which fired his enthusiasm both for the study of social factors in science in general and for the application of quantitative methods in particular, and that consequently

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Bernal should be recognized as a major influence on the history of scientometrics; (iii) that the contribution of *LSBS*, as a seminal text in the sociology of science and science policy studies as well as scientometrics, was uniquely wide-ranging; and (iv) that the methods described and conclusions drawn in *LSBS* are of more than historical interest even for present-day 'cybermetricians'.

Firstly, however, it seems sensible to begin this explanation of the degree to which *LSBS* exercised its influence by exploring the historical context in which the work originated.

THE GENESIS OF *LSBS*

Derek John Price[2] was born in Essex, England, in 1922. As a child, as well as demonstrating considerable aptitude in mathematics and science, he 'delighted in playing with Meccano sets' (Beaver, 1985, p. 371) – meaning that the library and information sciences have more to thank the makers of that particular toy for than might immediately be realized.[3] Price graduated in 1942 with a BSc in Physics and Mathematics, and in 1946 with a PhD in Experimental Physics, both from the University of London. The primary focus of Price's interest during the war had been the optical characteristics of hot metals; Lefkowitz's bibliography (Bedini, 1984) lists a number of Price's publications on this topic from this early period. Following a year's stay in the USA, as a Commonwealth Fund Fellow in mathematical physics at Princeton University, NJ, Price took a three-year appointment to teach applied mathematics at Raffles College of the University of Malaya (now part of the University of Singapore).

Price's arrival in Malaya in 1948, at the age of 26, happened to coincide with his university's acquisition of a 270-year run of the *Philosophical Transactions of the Royal Society of London*, established in 1665 and commonly regarded as the world's first scientific journal. As the commonly repeated story goes (see, for example, Price, 1983, 1978), Price read through every volume. Not only did this adventure serve to kindle a new and lasting interest in the historical contexts of science, but the reader was struck also by what he identified as a particular pattern in the sizes of the piles of volumes when they were stacked in chronological order. Prompted to determine whether such a pattern was observable in other cases, Price collected data on the annual increase in the number of papers indexed by *Physics Abstracts* and – true to the hypothesis – found this increase to be exponential (i.e. dependent, to a constant extent, on the cumulative number of papers) rather than linear.

Returning to England with his new interests in 1950, Price began a second PhD, this time in the History of Science at Christ's College, Cambridge. Since his youth, Price had been fascinated by laboratory instrumentation, and he combined his technological and historical passions by focusing on the history of scientific instruments; it is for his work on the history and archaeology of geared astronomical clocks (Price, 1974, 1961, 1955; Needham et al., 1960) that he is now most widely remembered outside the field of scientometrics.

Price presented his initial findings on the exponential growth of literature to the 6th International Congress of the History of Science in Amsterdam, in August 1950 (Price, 1951a);[4] his paper was republished in the French journal, *Archives Internationales d'Histoires des Sciences* (Price, 1951b), and a follow-up piece covering similar material appeared five years later in the British popular-science journal, *Discovery* (Price, 1956; reprinted as Price, 1962). Price had remained in Cambridge for two years after completing his second doctorate in 1954; following a year's appointment at the Smithsonian Institution, Washington, DC, Price returned to Princeton in 1958, to begin a two-year fellowship at the Institute of Advanced Studies. In the October and November of 1959, at the invitation of Yale University's Department of History, Price delivered five public lectures at Sterling Memorial Library in New Haven, CT; the combined text of the lectures, including one entitled 'Diseases of science' that further expanded on the theme of exponential growth, was later published as *Science since Babylon* (Price, 1961).

In 1960, Price was appointed Professor of History of Science in the new Department of History of Science and Medicine established by John Fulton at Yale, becoming department chair the following year (and remaining there until his death in 1983). In June 1962, as the fourth annual holder of the George B. Pegram Lectureship at Brookhaven National Laboratory, NY, Price gave four lectures at that centre for atomic research, and these were similarly collected in the volume published the following year as *Little science, big science* (Price, 1963). Each of the Brookhaven lectures developed a different selection of the ideas discussed earlier in 'Diseases of science', and the trail can thus be traced back fairly directly to Price's first ruminations on this subject matter in the late 1940s.

'HISTORY OF SCIENCE' IN THE UK IN 1950

What was the state of the discipline of the history of science when Price entered his second doctoral programme in 1950? The question seems worth posing if we seek to evaluate the effect on his later work of the intellectual influences that were dominant in his newly chosen field.

Price's decision, taken in his late twenties, to transfer his affiliation from the field of physics to that of history of science, should not be construed as one that could have been made lightly or acted upon easily. At mid-century, the field was far from professionalized, either in the UK or in the USA. England had a single department of the history of science, established in the 1920s at the University of London by Charles Singer;[5] few teaching positions were to be found elsewhere. At Cambridge itself, the embryologist Joseph Needham[6] had spearheaded the formation, in 1936, of a committee charged with establishing a series of lectures in the history and philosophy of science; but, with the intercession of the war, it was not until 1948 that any consideration was given to creating a full programme in the subject (Hall, 1984). In 1950 – the year of Price's arrival, and two years after Herbert Butterfield's[7] delivery of the lectures that were published as *The origins of modern science* (Butterfield, 1949) – such a programme was offered to undergraduates for the first time, as an optional Part I of the university's natural sciences tripos.

Even at this date, and seemingly in the face of ideas developed by Butterfield himself in *The Whig interpretation of history* (Butterfield, 1931), the dominant intellectual figure for Cambridge historians of science remained George Sarton,[8] whose statement of 'the new humanism' (Sarton, 1931) had been published in the same year as Butterfield's work. Sarton insisted not only that study of the past is necessary for reaching an understanding of the present, but also that study of the history of science is necessary for reaching an understanding of the history of humanity – since, taking his cue from Auguste Comte, he equated science with 'positive' (i.e. objective) knowledge, and human progress with the ongoing acquisition of cumulative quantities of such knowledge. Sarton was keen to promote the ideas that the sciences and the humanities in fact form a unity, and that the history of science is the unique discipline that reflects this fact most clearly. Ultimately, Sarton's practical goal for the discipline was to bridge the gap between what C.P. Snow would come, rather belatedly, to call 'the two cultures' (Snow, 1959), and to allow members of both communities, scientists and humanists alike, to enhance their life and work.

By the 1960s, beliefs of the kind promoted by Sarton about the nature of human progress would come to be characterized – in a revival of Butterfield's terminology – as 'Whig history'.[9] Just as Butterfield attacked the tendency of certain liberal constitutional historians of the nineteenth century to assess historical events on modern, anachronistic criteria, Sarton's critics would reject any account of scientific progress that assumes the truth of current scientific theory, and that this entails evaluating scientists' past work on the basis of the extent to which it has contributed to current theory – rather than allowing historians to come to an understanding of scientific change through the analysis of the social, economic, political or even psychological factors prevailing at the particular time and place. The latter proposal would come to be associated most commonly with Thomas Kuhn's (1962) interpretation of scientific revolution and earlier work in the history of science that was perceived to have assumed, implicitly or explicitly, a similarly externalist or subjectivist stance – such as that of Alexandre Koyré (1939), Robert Merton (1938), and J.D. Bernal[10] (1939) – would come to receive renewed attention. The Marxist orientation of some of this work would be noted in counterpoint to the Comtean origins of Sarton's positivism.

Price (1961, p. 126) recognized Sarton as a 'trail-blazer, far ahead of the spirit of his time', and (p. 13) as 'the man who did more than any other to found the history of science as a scholarly autonomy'. Other evidence suggests that it was Bernal, the crystallographer and lifelong Marxist, who in fact had greater influence than Price's Cambridge colleagues on the development of the latter's thought, both during his years as an apprentice historian and throughout his later career. After attending, in London in 1931, the Second International Congress on the History of Science where Boris Hessen delivered a controversial paper on the impact of socio-economic factors on Newton's scientific development (Hessen, 1931), Bernal turned to the history of science with enthusiasm, publishing a number of influential studies advancing his views on the mutual interaction of science and society, and on the consequent need for scientists to accept responsibility for the development of science policy and the promotion of social welfare (Bernal, 1954, 1953, 1939).

Garfield (1982) suggests that Price's acquaintanceship with Bernal may be dated from at least as early as 1946, when both men attended the Royal Society Empire Scientific Conference.[11] In 1964, 25 years after the publication of *The social function of science* (Bernal, 1939), Price contributed to a festschrift issued in that book's honour, alongside Bernal himself, Needham, Snow and others (Price, 1964). Here Price asserts (p. 202) that 'the first person to make any extensive inroads into the scientific analysis of science was J.D. Bernal', as he was the first to engage in 'the systematic collection of quantitative information ... to help us make precise a number of judgements that had previously been possible only as the result of qualitative opinionating' (pp. 204–205). Price (1983, p. 18) later wrote that his primary stimulations whilst writing *LSBS* were 'Robert Merton's writings in the sociology of science, ... Eugene Garfield's new work on citation indexing, and ... rereading Desmond Bernal's books which had prepared my mind for the initial sensitivity that led me to this field in the first place'. In 1981, on accepting the Bernal Award given to him by the Society for Social Studies of Science (4S) – the first winner of this award, established to honour Bernal's pioneering work in science studies – Price publicly acknowledged the inspiration of his friend, from whom he learned 'about scholarly style, good appetite and some sense of social and political responsibility' (Price cited in Garfield, 1982, p. 13).

Price and Bernal shared several easily discernible personal characteristics: both possessed seemingly inexhaustible reserves of energy; both were able, and always willing, to transfer to others their enthusiasm for their current topics of interest; both were adept at moving from one field to another, and to another, making important contributions in the natural sciences, the social sciences and the humanities along the way; both were happier dealing with the 'big picture'.[12] More specifically, many of the lines of thought developed by Bernal in his earlier books are echoed either in *SSB* – in which Price highlights, as does Bernal (1954) in *Science in history*, the impact of technological development on, and the role of artisans in, scientific discovery – or in *LSBS* – where Price argues for the participation of scientists in society's government on a basis similar to that on which Bernal urged scientists to take control of the process by which science determines social welfare.[13] As noted by Garfield (1979), *Science and industry in the nineteenth century* (Bernal, 1953) may have been the first work to present in diagrammatic form a citation network of the kind later examined by Price (1965) in 'Networks of scientific papers'; Bernal and Price both served on the editorial advisory board of Garfield's *Science Citation Index* from its initial appearance in 1963. The development of the 'science of science' in the 1960s on the model of the agenda established in *The social function of science* (Bernal, 1939) has been described as 'a sort of academic ''Bernalism without Bernal''' (Rose & Rose, 1999, p. 154). Bernal's influence on the progress made in information science (Muddiman, 2002) and in classification research (Justice, 2002) in the 1940s and 1950s is starting to receive the recognition it deserves, but the extent of the impact on scientometrics of his views has yet to be carefully evaluated. As Rose and Rose (1999, p. 154) put it, 'the philosophical origins of [Price's] brilliant analysis of the qualitative effect of the quantitative transition in the scale of science was never recognized in the US, where Price's work was acclaimed'.

THE HISTORY AND SOCIOLOGY OF SCIENCE IN THE USA IN 1956

Two years after completing his second PhD and on the lookout for permanent employment in his new field, Price left Cambridge for the USA, moving first to the Smithsonian Institution, Washington, DC, then to the Institute of Advanced Studies at Princeton University, NJ, and finally (in 1959) to Yale University, New Haven, CT, where he stayed for the remainder of his career.

In most respects, history of science in the USA was at this point no more highly professionalized than Price had found it to be in the UK. Until his death in 1956, Sarton had been 'plowing a lonely furrow' (Toulmin, 1977, p. 149) at Harvard University since the 1920s, and keeping afloat his journal, *Isis*, for whose financial benefit the History of Science Society had been established in 1924. Harvard's president, James Bryant Conant, finally promoted Sarton to tenured Professor of History of Science in 1940;[14] after the war had ended, and while public awareness of the role of science in the allied victory was relatively high, Conant himself delivered four lectures that were published as *On understanding science* (Conant, 1947), in which he urged that the historical approach be used in teaching science to non-scientists. Conant invited Kuhn, then a young graduate student in theoretical physics, to be his teaching assistant; two years after completing his PhD in 1949, Kuhn found himself teaching Harvard's first course in history of science, at a time when only six other institutions in the USA offered similar courses (Kuhn, 1984).

The year following the publication of Bernard Barber's *Science and the social order* (Barber, 1952), the Harvard physicist Philipp Frank gave a presentation at the National Science Foundation (NSF) extolling the practical usefulness to policy-makers of knowledge of the history and philosophy of science, and of understanding science as a social institution. The NSF began to consider which social sciences should come under its expanded remit, and funded the American Philosophical Society to hold a conference in 1955 on the history, philosophy and sociology of science: speakers included the sociologists Merton, Barber and Talcott Parsons. In 1957, a year after Kuhn had left Harvard to set up a programme in history of science at Berkeley, the NSF set up a Social Science Research Program (SSRP) that included a division for the History and Philosophy of Science; in a striking example of the phenomenon of exponential growth of the kind documented by Price, the SSRP's budget increased by a factor of 20 over the next eight years (Rossiter, 1984).

The late 1950s was a time of tremendous expansion in all sectors of higher education in the USA, and the history of science was just one of the many disciplines to benefit. Price's arrival coincided with the period in which its professionalization began in earnest; John Fulton's establishment at Yale of a new Department of History of Science and Medicine in 1961, with Price as its chair, was one of a series of initiatives whose inspiration could be traced back to the post-war concern of Conant and others that graduate scientists be capable of teaching science to the layperson, given the major importance which science was now perceived by the public to have assumed in daily life. In effect, Price owed his own livelihood to the very transition from little science to big science that formed the central topic of his book.

Meanwhile, the sociology of science of the 1950s was a young discipline associated in most minds with the work of Sarton's former student, Robert Merton, at

Columbia.[15] The field was dominated, as sociology in general in the US had been for some time, by Parsons' functionalism, the aim of which was to explain how society keeps itself from falling apart. Merton's (1942) paper, 'Science and technology in a democratic order', had identified a set of social norms that, he supposed, guided scientists' actions, thus ensuring the continued progress of science – science being just one of the institutions that, as functionalists tended to assume, worked fairly well in a modern democratic society. But by the time Price wrote *LSBS*, the concerns of sociologists of science had begun to change. Merton had already signalled an emerging interest in scientific status, stratification and reward with a paper on priority claims in scientific discovery (Merton, 1957); Warren Hagstrom's (1965) book on individualism in the scientific community was soon to be published; and researchers had begun to question the force of socially accepted norms guiding scientists' behaviour for the common good. Following Kuhn, historians of science, too, began to join sociologists in socially oriented studies, examining the influence, over time, of institutional and structural as well as psychological and epistemological factors on the practice and direction of science (Brush, 1974).

Price's Sartonian belief in the reality of scientific progress accorded easily with the premises of functionalist sociology. As he puts it in *SSB* (Price, 1961, p. 136), science is '... this cumulative activity that sets our culture apart from all that has come before'; moreover (p. 93), 'there is in the field of science a cumulative accretion of contributions that resembles a pile of bricks'. But Price's concept of science was that of the sociologist: he treated it as an institution created by and consisting of human activity, and in this sense his 'science of science' should be viewed as a social rather than a natural science.[16] Moreover, Price's appreciation of Bernal's work gave him a keen awareness of the social context of scientific activity that would serve him well at a time of unique apprehension about the use, and potential abuse, of scientists' power.[17]

THE CONTRIBUTIONS OF *LSBS*

As a historian (or 'humanist'; Price, 1959[18]) of science, Price's contributions to the field were numerous and distinctive; one might choose to highlight his accounts, summarized in *SSB* (Price, 1961), of the origins in a 'Graeco-Babylonian melting pot' (p. 19) of the 'peculiarly scientific' basis of modern western civilization; of the development of sophisticated geared astronomical calculators in ancient Greece and medieval China; and of the previously under-appreciated role of artisans and instrument-makers in driving scientific progress in early modern Europe and North America. Price's personal conviction (p. ix) was that historians of science 'must soon take their place at the forefront of scholarship to help preserve and advance all that we hold dear in civilization', and he hoped (p. 128) that his 'humanities of science' would do for the scientific world just what economics does for the whole of business and commerce'. However, the particular strand of his work that received its fullest expression in *LSBS* may be characterized as an effort to popularize a *quantitative* approach to the history and sociology of science that he called the 'science of science', and which later became known as scientometrics.[19] Price has thus been celebrated both for his humanistic treatment of scientific matters, and for his scientific approach to humanistic concerns.

Price's contribution to scientometrics was partly methodological, by virtue of his illustration both of the kinds of technique that he (and others) had developed and of the kinds of problem area in which such methods had been applied, and partly empirical, by virtue of the results that had been obtained by engaging in the quantitative approach.

The arguments contained in *LSBS* may be summarized as follows:

1. *A science of science is possible.*
Price begins (in the preface, pp. vii–viii[20]) by making the assumption that science can itself be studied 'scientifically' – that is, by careful measurement, description and classification of regularities in the occurrence of certain real-world events, such as the engagement of a person in a scientific task, their appointment to a scientific job, their publication of a scientific paper, their collaboration with another scientist or their usage of another's work.
2. *The growth of science has been exponential.*
In the first lecture proper ('Prologue to a science of science'), Price supplies empirical data in support of his observation that the growth in size of various scientific populations (scientific workers, scientific publications, dollars spent on scientific work, etc.) has been exponential (i.e. that the growth has occurred at a constant rate over a long period of time). The growth rate is such that the size of science has doubled every 10 or 15 years since the seventeenth century.
3. *The growth of science may be modelled by a logistic curve.*
He goes on to make a prediction that, at some point in the near future, it will become clear that the period of exponential growth so far observed is but a prelude to a period of linear growth at a gradually declining rate, and eventually to a period of growth at a rapidly declining rate, in a pattern characteristic of the logistic model, in which growth is described by an S-shaped curve converging toward an upper limit or point of 'saturation'. The prediction is based on a deduction (revisited in greater detail in the fourth lecture) that exponential growth in science cannot continue forever, given the upper limit imposed by the size (and

growth rate) of the human population and culture of which it forms a part. As the number of scientists relative to the number of non-scientists increases, for instance, more than 'la crème de la crème' is skimmed from the tip of the tail of the Gaussian distribution of scientific talent in the general population, and already we begin, Price says (p. 103), to 'scrap[e] the bottom of the barrel'.[21] Nevertheless, Price allows that the onset of linear growth need not foreshadow the 'senility' or 'death' of science, and identifies several possible alternative scenarios, including the supposedly plausible model of 'escalation', when a new logistic curve rises 'phoenixlike on the ashes of the old' (p. 25) with renewed exponential growth, as a result of some form of radical reorganization.

4. *The saturation point of science will be reached by all nations roughly simultaneously*.
   Price makes the additional observation that the rate of growth of science in some of the countries that have come later to the pursuit of modern scientific progress – Japan, China and India, for example – has been higher than it has in Europe and North America. On this basis, Price predicts that science will soon emerge as a kind of handicap race, with competing nations reaching the finishing line (or saturation point) at roughly the same time, no matter what time they originally began (p. 102).
5. *Author productivity data demonstrates that 'good' science does not grow as fast as science in general*.
   In the second lecture, 'Galton revisited', Price presents data to support his observation that 'good' science has not been growing as fast as science in general, and formalizes this observation in terms of what has come to be known as his 'square-root law', which states that half of the scientific literature is produced by a number of authors equal to the square root of the total number of scientists (i.e. equal to that total number taken to the power of 0.5). Thus, the number of 'good' scientists (where a 'good' scientist is considered to be one whose publication productivity is above a certain threshold) is at any given time roughly equal to the square root of the total number; and as this total number increases, the square root of that number increases at a lower rate.[22] Price maintains that he is able to derive his square-root law from the 'inverse-square law' of productivity proposed by Lotka (1926) and others on the basis of their own empirical observations of regularities in the distribution of scientific publications among authors.
6. *Citations may be used to evaluate the quality of a scientist's work*.
   Price goes on (in the third lecture, 'Invisible colleges and the affluent scientific commuter') to hypothesize that similar distributions might be observed of citations among cited authors, and of requested articles among journal sources. He suggests that 'amount of use' (i.e. the number of times a paper is cited or requested) could provide a more valid measure of the scientific importance of a scientist's work than 'amount of productivity' (i.e. the number of papers published).
7. *Citation and/or usage statistics may be used to measure the obsolescence of a field*.
   Price suggests that the median age of the references cited in a given source article (or journal issue, journal volume or set of journals representing the production of a particular field in a given year) may be used as an indicator of the rate at which the source article (or journal or field) is ageing. Without formally noting the precise nature of the distinction between the two methods, Price refers also to the *cited* half-life of a journal, which may be calculated by counting and dating the citations it receives, or the occasions on which it is circulated in a library. Price finds that the decline in usage of the average scientific paper has been exponential (i.e. that the rate of decay has been constant over a long period) and surmises (p. 81) that, since the rate of growth in the number of scientific papers has been similarly exponential, 'any paper once it is published will have a constant chance of being used at all subsequent dates'.
8. *Invisible colleges will emerge as a solution to the current information-retrieval problem*.
   Also in the third lecture, Price considers the information problem of science – the difficulty that the scientist faces in identifying relevant literature, given the sheer size of the universal collection of scientific publications – and predicts the renewed ascendance of 'invisible colleges', made up of relatively small numbers of top-level scientists in highly specialized fields, meeting at conferences, exchanging preprints, and generally engaging in 'high-grade scientific commuting' (p. 85). Price points out that the response of science at the last time it faced such an information crisis, in the mid-eighteenth century, was a similarly radical innovation in the form of the abstracting journal; this time the communication crisis will be solved, Price suggests, 'by reducing a large group to a small select one of the maximum size that can be handled by interpersonal relationships' (p. 85).
9. *Scientists should seek political responsibility*.
   In the final lecture, 'Political strategy for big scientists', Price turns his attention to the political implications of his observation that the cost of science is increasing at an even faster rate than its personnel or product. As an explanation, Price points to the willingness of employers to pay larger and larger sums for 'good' scientists as the number of scientists relative to the number of non-scientists increases, and as the proverbial barrel-bottom of scientific talent begins to be scraped. As a recommendation, Price suggests that, since 'the scientist now holds the purse-strings of the entire

state' (p. 111), he should actively seek to acquire political power.

10. *Big science is qualitatively different from little science.* As a whole, the book may be considered as a description of the transition from the 'little science' practised in Europe for 300 years to the 'big science' of the 1950s onwards.[23] Price is clear that the two modes are distinctive not purely or even primarily in terms of their size or quantity, but in terms of certain significant differences in quality. Unique characteristics of 'big science' include its declining growth rate, converging toward saturation; its dominance by invisible colleges; and its potential for driving wholesale and far-reaching social and political change. In his concluding paragraph (p. 115), Price summarizes (i) his observations: '... that a whole series of annoyances and difficulties in scientific manpower and its literature are part of a single process in which at last we find a change in the state of science the like of which we have not seen for 300 years ...'; (ii) his predictions: 'The new state of scientific maturity that will burst upon us within the next few years can make or break our civilization, mature us or destroy us ...'; and (iii) his recommendations: '... we must look for considerable assumption of power by responsible scientists ... knowing better how to set their house in order than any other men at any other time'.

SUMMARY

In beginning to explain, in the first part of this essay, the extent to which Price's work has had impact, I have explored the historical context in which *LSBS* originated. I have noted, *inter alia*, the timing of the book's emergence at a pivotal stage in the development of socio-historical studies of science, and the extent of Bernal's influence on Price and thus on the history of scientometrics. I have also summarized the uniquely wide-ranging arguments that the work contains. But I have not yet examined the processes by which *LSBS* has come to be regarded as a seminal text in the sociology of science and in science policy studies as well as in scientometrics. In Part II, I shall attempt to locate the place of *LSBS* when viewed from an alternative perspective – that of the several communities of scholarly practice that Price's work may be said to have subsequently inspired. I shall argue that the methods described and conclusions drawn in *LSBS* remain of far more than historical interest, and that its continued status as 'citation classic' is assured.

NOTES

1. Issued three years after Price's death, *Little science, big science ... and beyond* (Price, 1986) added nine of his previously published papers, ranging from 1965 to 1984, plus a foreword by Robert K. Merton and Eugene Garfield, Price's 1983 note on his 'citation classic', and an afterword by Garfield.
2. Price began using the name Derek de Solla Price in the 1950s, in honour of his mother Fanny de Solla. Biographical detail is provided by Herner (1990) and in obituaries by Beaver (1985), Bedini (1984), Crawford (1984), Garfield (1984), Griffith (1984), Kochen (1984) and Mackay (1984). Bedini (1984) includes a comprehensive bibliography of 14 books, and 240 published and 14 unpublished papers, prepared by Lefkowitz (pp. 101–115). Price's personal papers are archived at the Centre de Recherche en Histoire des Sciences et des Techniques, Paris.
3. The celebrated Indian library scientist, Shiyali Ramamrita Ranganathan (1872–1972), is said to have been inspired to create his faceted Colon scheme for bibliographic classification by his sighting of a Meccano set in Selfridge's department store in London in 1924.
4. In Price's own words, 'It passed totally unnoticed, and was very ill-received when I entered Cambridge ... It went over like a lead balloon ...' (Price, 1983, p. 18).
5. Price dedicated *SSB* to Singer and to John Fulton, at whose invitation Price came to Yale.
6. Joseph Needham (1900–1995), a devout Christian and loyal Marxist, made his Cambridge career in biochemistry before developing a lasting interest in classical Chinese science, encouraged by the arrival at his laboratory of three Chinese researchers in 1936. From 1942–1946, he served as director of the Sino-British Science Co-operation Office in China, where he met the historian Wang Ling with whom he and Price collaborated in their study of medieval Chinese clocks. Needham played a central role in the creation of UNESCO, and published the first volume of his monumental *Science and civilisation in China* in 1954, a work that was later criticized both for its 'diffusionist' stance (neglecting the possibility of independent invention), and for its 'Whiggish' tendencies (evaluating Chinese sciences from a modern European perspective). The former criticism might justifiably be levelled at Price's treatment of mechanical discoveries in *SSB*.
7. Herbert Butterfield (1900–1979) was appointed to the Chair of Modern History at Cambridge in 1944. On its publication, his *The origins of modern science* 'appeared a courageous and groundbreaking tour de force' (Toulmin, 1977, p. 148), and came to exert a profound, international influence.

8. George Alfred Léon Sarton (1884–1956) obtained a doctorate from the University of Ghent, Belgium, in Celestial Mechanics in 1911 and founded the journal *Isis* a year later as the organ of a new synthetic discipline that was to combine historical, philosophical, and sociological approaches to the study of science. Sarton edited *Isis* for 40 years; Poincaré and Durkheim were among its first board members. Sarton emigrated to the US in 1915 and eventually found work at Harvard, embarking on a prodigious programme of writing; the first volume of his *Introduction to the history of science* was published in 1927. In 1952, Pfizer donated funds to the History of Science Society (which Sarton had founded in 1924) to establish a George Sarton Medal for outstanding contributions to the history of science; Sarton was the medal's first winner.
9. Around the same time, and against Butterfield, however, some historiographers were attempting to restore credibility to the 'Whiggish' idea of progress as the meaning of history; see, for example, Carr (1961).
10. John Desmond Bernal (1901–1971) was born in Ireland and raised in a strict Catholic home. After spending his undergraduate years (1918–1921) at Cambridge, where he abandoned his religion and embraced Marxism, Bernal accepted an offer from the x-ray crystallographer William H. Bragg to work at the Royal Institution. (It may be of passing interest to note that, while completing his second doctorate, Price was later to help Bragg's son and fellow Nobel prize-winner, W. Lawrence Bragg, reorganize the archive and museum of the Cavendish Laboratory in Cambridge.) Bernal moved back to Cambridge in 1927 for a newly created lectureship in structural crystallography, before returning to London ten years later as Professor of Physics at Birkbeck College, where he remained for the rest of his career. Working for the British government during the Second World War, Bernal participated in the planning of the Normandy landings; in 1946, he co-founded the World Federation of Scientific Workers.
11. Unfortunately, from the printed record, it is not easy to determine the accuracy of this memory of Garfield's. There were in fact two major conferences held under the auspices of the Royal Society around this time: the Empire Scientific Conference of June/July 1946, and the Scientific Information Conference of June/July 1948 (Vickery, 1998). The reports of both conferences were issued in 1948 (Royal Society, 1948a, 1948b). Misleadingly, Garfield cites the former, and mentions the participation of information scientists Fairthorne, Farradane, Shaw, Taube, Vickery, and Urquhart as well as Bernal and Price. Yet, of these names, only Bernal's may be found in the report of the 1946 conference (Royal Society, 1948a), as the contributor of a brief comment (pp. 698–699) on 'The form and distribution of scientific papers'. A list of 114 delegates appears in this report (pp. 9–10), but the 200 additional participants (p. 12) are not listed; the few names that would be recognizable to today's information scientist include that of Bradford, who contributed a paper (pp. 729–748) on 'Complete documentation' to the same session on 'Scientific information services' in which Bernal was involved. Another paper in this session, a 'Review of information services' by R.S. Hutton (pp. 686–698), discusses the growth of scientific publications, citing Hulme, Bradford and Bernal himself. Turning to the report of the 1948 conference (Royal Society, 1948b), we find that all the people named by Garfield, with the exception of Price, are listed among the 230 delegates (pp. 215–222). Bernal's role in the planning and running of this latter conference was extensive: he served on its Organizing Committee under the joint chairmanship of Egerton and Salisbury, chaired the steering committee for the section on 'Publication and distribution of papers reporting original work' (p. 223), contributed three papers (including his controversial but prescient proposal for all scientific periodicals to be replaced by a system of distribution of single papers by a centralized agency, pp. 253–258), and was responsible for the recommendation (p. 190) to establish a committee on classification that eventually resulted in the formation of the Classification Research Group in 1952. A total of 500 people participated in this conference (p. 45); was Price among them, on leave from Malaya? The printed record does not show.
12. Compare, for instance, Perutz's assertion that 'What held [Bernal] was the grand sweep of an idea of a problem, but he was impatient with detail' (Perutz cited in Garfield, 1982, p. 9), with Beaver on Price: 'His drive to compete in many different fields was reflected in his tactic of sensing a promising new area, getting in early and mapping its broad outlines, and leaving to others the legacy of filling in the details' (Beaver, 1985, p. 374).
13. Notwithstanding his leanings to the political left, Price was no Marxist however, and he stopped well short of advocating, as Bernal did, the communist state as the ideal site for the integration of science and society.
14. Prior to then, Sarton had been retained on a yearly contract as a lecturer.
15. The sociology of science may be distinguished from a socially oriented history of science like Bernal's not only by its emphasis on the more recent past, but by its self-identification with the social sciences rather than the humanities, and by its aim to produce statistical generalizations rather than an understanding of particular contexts (Beaver, 1978).

16. It may be argued, however, that Price was not always to appreciate the implications of holding such a view, and was instead prone, for instance, to overstate the power of the inductive method in his new science.
17. As Toulmin (1977, p. 153) notes, 'It is probably no accident that this new interest [of historians in socially oriented studies] became active just at a time when there began to be widespread disquiet about the current institutions of American science ...'.
18. Referring to his coinage of the term 'humanities of science', Price regretted that '[u]nfortunately the term Scientology has already been bespoken by the sect of Mr. Ron Hubbard ...' (Price, 1961, p. 130).
19. If scientometrics is considered a 'second-order' field – just as the histories and sociologies of science (in particular) and of scholarship, knowledge production and knowledge use (in general) are sometimes similarly regarded – then it may be helpful to view the history of scientometrics (to which the current essay is a minor contribution) as a third-order field.
20. Price asks (p. vii), 'Why should we not turn the tools of science on science itself? Why not measure and generalize, make hypotheses, and derive conclusions?'
21. For an intriguing demonstration of the significance of the 'topnormal' distribution in science, see Seglen (1992).
22. Price calls this 'a powerful law of diminishing returns' in *SSB* (Price, 1961, p. 120).
23. Price notes that the term 'big science' was coined by Weinberg (1961, p. 164). Galison and Hevly (1992) collect a set of papers that revisit the origins and characteristics of this notion.

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