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I M P A K T

TÉNYEK A TUDOMÁNYOS ALAPKUTATÁSRÓL

Szilárd: Csak a tényeket írom le – nem azért, hogy bárki is elolvassa, csakis a Jóisten számára.
Bethe: Nem gondolod, hogy a Jóisten ismeri a tényeket?
Szilárd: Lehet, hogy ismeri, de a tényeknek nem *ezt* a változatát.

[*Leo Szilard, His version of the Facts.* *S.R. Weart & Gertrud Weiss Szilard* (Eds), MIT Press, Cambridge, MA, 1978, p.149.]

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Science and Society

The ways in which science and society are governed are quite different and the difference causes friction when scientific progress is of social concern. Science is dealing with the unexpected, the frontier, the search for a new path, not with the predictable, the established edifice, the walk down the well-paved road. In the aggregate, science is designed to make great progress on a wide front, but to predict which individual area will deliver a new discovery tomorrow is impossible. In general, this is well understood by both parties, science and society; but when science is asked to solve a problem its instinct is to start from fundamentals and proceed on its slow but inexorable timetable. When society – through its agent government – says "I need the answer now," the two systems have serious misunderstandings. Science, trying to be accommodating, frequently says, "I'll give you a progress report, but understand that we need more data to get a definitive answer." The "but" clause soon gets forgotten, so science gives an educated guess as to whether saccharin is carcinogenic, or dioxin is deadly poisonous, or the climate is warming, and later revises the first estimate, bewildering the public and making it distrustful of science. A report on cholesterol in the diet needs volunteers and those at high risk are the most likely to volunteer, but scientists know that preliminary reports for a high-risk group are helpful but should not be overgeneralized until a more normal group (and the more difficult to study) becomes the focus of study. The subject is too interesting to prevent premature publication and premature conclusions, but the new facts require revisions which lead the public to say "the scientists should make up their minds." At the frontier, scientists are individualists, not consensus groups, and science adds more facts and voices until a full understanding is approached asymptotically. The final value can be the truth at some level of detail but in some cases may simply reflect the exhaustion or exasperation of some of the participants.

In the coarse of a debate, not only do different scientists enter with different ideas, but new data are continuously uncovered. So science is not failing the public by changing its mind. Nor is it being irresponsible in volunteering a progress report. To refuse to give an educated guess to those who are paying the bill would be irresponsible unless the progress report is presented as though it were a final opinion. In a number of recent debates a premature release of a tentative conclusion became a congressional excuse for a final judgment, for example, in the case of the cancerogenicity of saccharine, despite the inconclusiveness of the data.

The great discoveries of science are the result of a range of discoveries in which an initial notion was suggested, but the final understanding required lots of work. The societal problems of climate change, public health, economic efficiency, and so forth are even more complicated than the related pure science problems, and it should be expected that they would be equally prone to revision and updating.

A good example of this revisionism is reflected in special series that ran in *The New York Times* the week of 21 March 1993, which reports that environmentalism is now showing a new trend toward cost-benefit analysis. The story gives an excellent account of excessive costs of some highly publicized risks and the past tendency of the

(Continued on next page)

Environmental Protection Agency to follow publicity rather than science in its approach to the environment. The change in sentiment is occurring because the evidence is accumulating that a "to hell with the cost" approach is impossibly expensive and the data on risks are now more definitive and less scary. Solid evidence can change minds, but getting the data requires time. Some scientists had explained that the early data were dubious, but they are ignored.

Scientists must assist in producing and explaining preliminary findings on scientific problems even if their instincts are to say "go away until I've solved the problem". And politicians must understand that progress reports should not be used as laws that are not allowed to modified. The alternatives are that government makes hasty decisions based on third-rate scientific advice and scientists refuse to give any opinions. Distrust between the partners arises when each forgets that the other is operating in an uncomfortable mode – scientists being focused to give premature conclusions, government being forced to delay decisions until evidence is acquired. This "odd couple" of science and government has produced an unparalleled standard of living for its people. It will produce even more if each partner seeks common ground and gives credit to its partner for willingness to compromise its normal operating procedures and to contribute toward a common goal.

Daniel E. Koshland, Jr.,
Science, 260:143, 9 April 1993.

It's a Small World After All

A személyi kapcsolatok és az azokból eredő – pozitív és negatív – befolyások döntő szerepe az emberi tevékenység minden területén közhelyszerű ismeret. Egyesek szerint ez alól a tudományos (kutatási) tevékenység sem kivétel. Azt, hogy ezeknek a körülhatárolásoknak milyen a mechanizmusa, ill. hogyan működnek, már sokkal kevesebben látják át. Ezt az u.n. "kicsi a világ" effektust járja körül az alábbiakban közölt esszé. (B.T.)

It has happened to must of us. You walk into an airport and suddenly you see an old friend or acquaintance. You say, "Isn't it a small world!" I met Harold Urey at a London airport this was about 15 years ago. A few years later, as I was about to climb the pyramids in Teotihuacan, Mexico, I saw my old friend Simon Newman of the United States Patent Office. It's not surprising, therefore, that someone wrote a song by that title. Walt Disney designed a delightful exhibit around this theme at Disneyland. As you ride the cable car through the tunnels, dolls of every nation sing "It's a Small World After All." I wonder if Disney knew that this "small world phenomenon" had been subjected to considerable scientific investigation.

There is a fair amount of literature derived directly from the term "small world phenomenon". Undoubtedly, statisticians indirectly considered one or more aspects of the problem long ago. Stanley Milgram of the City University of New York observes that the term was introduced in the social sciences by Ithiel de Solla Pool and Manfred Kochen while at MIT [1]. Belver Griffith of Drexel University states that Pool and Kochen's manuscript, first written in 1958 and only recently published in *Social Networks* [2], is considered the foundation on which small world studies are based [3]. The authors originally hesitated to publish their manuscript because "we raised so many questions that we did not know how to answer." [2]. But they hope that renewed interest in human network studies may answer their still unresolved questions.

About twelve years ago, in the first issue of *Psychology Today*, Milgram, while at Harvard University, described the small world problem this way. If you choose any two people at random, how many acquaintances must be linked together to complete a chain between them? X does not know Y but does know A, who knows B, who knows C, who knows D, who is Y's boss, spouse, professor or whatever [4]. The number of these links determined the smallness of the world in which we live. The fewer the links, the smaller the world. Of course, a definition of knowing or acquaintanceship is critical for precise studies. But, in fact, most researchers rely pretty much on the interpretation of participants in their studies.

Presumably Milgram was one of the first people to systematically count the number of intermediates linking any two randomly chosen people [5]. Milgram selected three groups of "starters" The first group consisted of 100 Nebraskan stockholders. The second group consisted of 96 Nebraskans chosen at random. The third group consisted of 100 people living in the Boston area. The starters were all told about a "target" person, a stockbroker who lived near Boston, Massachusetts. Then they were given written instructions to send a document of some kind through the mail to someone, who was more likely to know the target or know someone else who would [6]. The starters were told something about the target person to help them decide what acquaintance to select. But only those acquaintances they knew on a first name basis were permitted. This is a narrow definition of "knowing".

While 296 starters were selected originally, only 217 (73%) cooperated. However, only 64 of these (29%) started chains that reached the target stockbroker. Of these, twenty-four were Nebraska stockholders, 22 Bostonians, and 18 Nebraskans chosen at random.

Milgram found that an average of five intermediaries were needed to link two people, that is a starter with a target! [7]. The documents reached the target through two major paths – occupational and residential. The former were generally the shorter paths. As they messages approached the target they often travelled along common pathways. Many messages reached one of three intermediaries who were probably "gatekeepers", people who have more than average contacts [8].

Milgram and his student Charles Korte, now at North Carolina State University, also experimented with 540 Los Angeles starters to learn if there were differences in chain-length due to social factors. All starters were white. There were nine white and nine black targets in New York City. Only 5.5 intermediaries were required to complete a chain between a starter and a white target, but 5.9 between a starter and a black target [1]. This might demonstrate that whites are slightly less familiar with black social structures, but Milgram asserts that the difference in chain lengths is not statistically significant.

Since only 29% of starting chains in the Nebraska-Boston study were completed, you might conclude that the number of intermediaries would be greater in a study having higher completion rates. John Hunter (Michigan State University) and R. Lance Shotland (Pennsylvania State University) point out that the probability of someone losing or discarding the relay document increases at every link in the chain [9]. Thus, if no documents are lost or discarded, chain lengths will be longer. Harrison White (Harvard University) designed a mathematical model to fit Milgram's Nebraska-Boston data and found that chain lengths increase from six to eight intermediaries when all chains are completed [10]. Stephen Feinberg and S. Keith Lee (University of Minnesota) confirm White's model with their own statistical analysis [11]. A.K.M. Stoneham (University of Cambridge) [12] and H.F. Andrews (University of Toronto) [13] use theoretical models to show how the size of a person's acquaintance network and his or her social class can lengthen or reduce a small world chain.

However, chain lengths in studies with high completion rates are not longer than Milgram's Nebraska-Boston chains having about five intermediaries. If anything, chain length is *not* significantly affected when the number of completed chains increases! Craig Lundberg (Oregon State University) directed two groups totalling 462 starters at targets working in the same Dallas business firms. With 263 completions (57%), the mean chain lengths for the two groups were 2.6 and 3.5 intermediaries [14]. Both chain lengths are significantly *shorter* than Milgram's Nebraska-Boston chains.

Shotland measured chain lengths between students, faculty, and administrators at Michigan State University. Fifty-five students and the same number of administrators and faculty acted as starters. Each starter was given six booklets to pass to two student targets, two faculty targets, and two administrator targets. Thus, a total of 990 chains were initiated and 60% reached their targets. The shortest chains extended from administrator targets and had a mean length of about one intermediary! The longest chains, from faculty starters to student targets, had a mean length of 5.55 intermediaries [5]. This is not significantly longer than Milgram's Nebraska-Boston chains, and it agrees exactly with his Los Angeles-New York chains with white targets (5.5 intermediaries).

Jean Guiot (Boston University) directed 52 French-Canadian starters from Montreal at a target person who also lived in Montreal. Forty-two chains (85%) reached the target, and the mean chain length was 4.7 intermediaries [15]. This is in close agreement with Milgram's Nebraska-Boston data. The mean length of chains originating with Boston starters was 4.4 intermediaries [7]!

Several researchers modified Milgram's small world method to examine other aspects of social networks. Peter Killworth (University of Cambridge) and H. Russell Bernard (West Virginia University) used a "reverse' small world method to measure how many acquaintances a typical person uses as first steps in a small world experiment. Instead of using one target and many starters, they presented a list of 1267 targets to each of 58 starters. For each of the targets the starters were asked to name an acquaintance who would act as the first link in a small world chain. They could choose to use the same acquaintance more than once. But a starter could choose a maximum of 1267 different acquaintances if no choices were repeated. The results show that many choices *are* repeated – the typical starter chose only about 210 different acquaintances [16].

Stephen Bochner (University of New South Wales, Australia), Eloise Buker and Beverly McLeod (Culture Learning Institute, Hawaii) examined friendship patterns between students living in an international dormitory [17]. In another study, Bochner modified Milgram's small world method to analyze acquaintance circles between people living in a high rise apartment building in Australia [18]. Bonnie Erickson and Paul Kringas (University of Toronto) determined how social distance between elected representatives in Ottawa and their constituents varies with the constituents' socio-economic status [19].

If you describe the small world problem to the average person, he or she may find it hard to believe that any two randomly chosen persons can be connected by only about five intermediaries. But then the average person doesn't have much insight or training in probability theory. Ask someone what the odds are of finding two people who have the same birthday at a gathering of 25 people. Most people find it hard to believe it is about even money.

Milgram says the small world problem is easier to understand when you "think of the two points [starters and targets] as being not five persons apart, but five 'circles of acquaintances' apart" – five 'structures' apart" [4]. Based on records kept by 27 persons of whom they came in contact with over 100 days. Ithiel Pool (MIT) and Manfred Kochen (University of Michigan) estimated that the average person's circle of acquaintances includes between 500 and 1500 people [2]. Assuming the mean number of person's acquaintances is 1000 we can predict the number links in a small world chain by asking what power of 1000 will cover the total population involved. In a population the size of the

US, it would take between two and three powers of 1000 to cover 220 million people. Thus, the mean length of a minimum chain between two random persons in the US would be *less* than two intermediaries.

Small world studies suggest that it is indeed a small world – that individuals are not nearly as isolated as many of us may think [20]. We are all intimately connected in a web of "invisible" acquaintances. In fact, a network of casual acquaintance ties reaches a larger number of people and covers a greater social distance than strong family or friendship ties [21]! Like "old boy networks", acquaintance networks make it easier for people to locate jobs [22], exercise political influence [23], and find available social services [24].

Derek de Solla Price observes that "old boy networks" in science lead to more informal relations between scientists. "In a small group, like high-energy physicists or Israeli scientists, personal linkages make it very difficult to exercise the norm of impersonality. You know the other people too well and have too many emotional links to them to be completely dispassionate about whether their paper should be published or whether their grant should be funded. When you start with what is already a small world and not the whole world, the small world phenomenon is responsible for the breakdown of impersonality in scientific groups [25].

Greater knowledge of the small world phenomenon among scientists might be useful in designing better communication systems. For example, Susan Crawford, director of the archive-library of the American Medical Association, interviewed 160 scientists involved in sleep research who informally contacted one another in the course of their studies. She found that 33 scientists were the focus of a great number of contacts. No scientist in the sleep research network was more than three persons removed from a "gatekeeper" scientist! In fact, information transferred to a gatekeeper scientist could be transmitted to 95% of the network scientists through only *one* intermediary or *less* [26]. Identification of similar gatekeepers in other scientific specialties could be a powerful tool in setting up lines of communications for rapid dissemination of current information.

I suppose it is not entirely surprising that one who studies citation networks or genealogical trees should be attracted to small world networks. Griffith's work on measuring informal communication in biomedical specialties is applicable [27]. Price's work on communication patterns in "invisible colleges" [28] is quite relevant, as is the more definitive work on Diane Crane [29].

Price sees a relationship between small world studies, ISI's data on clusters of scientific subfields, and his own work on the growth of science. "The size of the Griffith-Small clusters of subfields is about the same size as a person's network of personal acquaintances and the size of the Royal Society in the 17th century – an order of magnitude of 100 or so individuals. Since the days of the Royal Society, when you had one or two such groups of 100 in the world, even seven or ten years the number of groups has been doubling. As the number of scientists has grown, the number of groups and clusters or small worlds grow accordingly. The growth of science goes on through this growth of the number almost autonomous subfields that exist. This means that there is a very important constancy built into science" [25].

Based on personal experience, I'm sure that fewer than five intermediaries are required to connect any two scientists chosen at random. If you and I were to meet somewhere, there is a high probability that we would have a mutual first name acquaintance.

Although the world scientific community is spread throughout the globe, it is linked by common educational and professional/occupational contacts. If we include people we "know" through the literature then the chain is very short indeed. Failing anything else one can always talk about Linus Pauling, Harold Urey, Joel Hildebrand, or similar visible scientists. I've stopped counting the number of people I meet who took freshman chemistry with Joel Hildebrand. Professor Hildebrand has taught and lectured to over 100,000 freshman, graduate students, and scientists [30].

It is also probable that scientists meet more people professionally than the average individual. Science is indeed a very social business. For the elite there are academy memberships, international congresses, awards committees, and foreign scholar exchange programs. Every time a new contact is made the scientific world becomes smaller.

For the student just starting a scientific career it may not be very helpful to point out that he or she is linked to some other student in the world through a small group of well known scientists. But as I've said when discussing information encounter groups [31], it is not all that difficult to establish useful links in the communications system of science. Perhaps a greater awareness of the small world phenomenon will make more people aware that the democracy of science is reality, but only if you take advantage of the right opportunities.

The world of science, like the world at large, is vast. But we can identify, in science and in politics, "old boy networks" or whatever you may want to call them. Unlike politics, it is relatively easy to penetrate these scientific networks, if you have a legitimate basis for doing so.

The reason ISI is working so hard to produce maps of the small *and* large worlds of science because I believe the ISI *Atlas of Science* will aid scientists in identifying the appropriate intermediaries between them and whatever "target" they choose. Of course, there's a point at which the simile becomes far-fetched. But in the computer graphic system we are developing all you do is key in the scientist's name and almost immediately you see all the "starters" to whom this target is linked. Fifteen years ago, my brother Ralph established the graph theoretic dimensions of this problem at ISI [32]. While the computer graphic system is based on citation linkages it could easy be modified for related purposes. For example, by feeding in the names of all editorial board members one could quickly determine influence networks in the journal publishing business. Or one could use such methods to identify potential subscribers for new journals and magazines.

Garfield, E.
Current Comments, (43):299-304, October 22, 1979.

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Ha az olvasónak kétségei volnának, hogy valóban ilyen kicsi-e a világ, figyelmébe ajánljuk az alábbi részletet Karinthy Frigyesnek egy 1927-ben megjelent írásából. Aligha kétséges, hogy a prioritásra nemcsak az 1978-ban megjelent közlemény, de az 1958-as publikálatlan kézirat sem igen tarthat igényt. Hogy a gondolat hány láncszemen keresztül és hogyan jutott el a huszas évek pesti kávéházaiból az 50-es évek MIT-jére, az érdekes kutatási témául szolgálhat a szellemtörténet búvárai számára.

A kicsi világról

"... soha még ilyen *kicsike* nem volt a földgolyó, mint amilyenné mostanában lett – persze viszonylagosan. A szóbeli és fizikai közlekedés egyre gyorsuló irama összezsugorította a világot – elhiszem, hogy ez is volt már, az is volt már, mindenről volt már szó, de arról még nem volt szó soha, hogy amit gondolok, csinálok, amit akarok vagy szeretnék, arról – ha úgy tetszik neki vagy nekem – percek alatt értesül a föld egész lakossága – s ha személyesen akarok erről meggyőződni, napok alatt ott vagyok, hipp-hopp, ahol lenni akarok. Tündérország, ami a hétmérföldes csizmákat illeti, eljött e világra – némi csalódást csak annyiban hozott, hogy Tündérország sokkal kisebb országnak bizonyult, mint amilyen Valóság országa volt valaha. Chesterton azt írja valahol, nem érti, miért akarják a kozmoszt mindenáron valami igen nagy dolognak elképzeltetni a metafizikusok – őneki jobban tetszik egy icike-picike, apert, hercig, intim kis világmindenség gondolata. Nagyon jellemzőnek találom ezt az ötletet a közlekedés századában – jellemzőbb, mint amennyire elmés vagy igaz, s éppen ezért, mert a reakciós tudomány- és technikatagadó, anti-evolúcionista Chesterton volt vele kénytelen önkéntelenül elismerni, hogy az általa sokat emlegetett Tündérországot íme mégiscsak az a bizonyos "tudományos" fejlődés varázsolta elő. Hát persze, minden visszatér és megújul – de nem veszitek észre, hogy ennek a visszatérésnek és megújulásnak a tempója gyorsul, soha nem látott mértékben, térben és időben? Percek alatt kerüli meg gondolatom a glóbust a világtörténelem fázisait évek alatt daráljuk le, mint a megunt leckét –, ebből mégiscsak kijön valami, csak tudnám, mi? [...]

Egyébként kedves játék alakult ki a vitából. Annak bizonyítására, hogy a földgolyó lakossága sokkal közelebb van egymáshoz, mindenféle tekintetben, mint ahogy valaha is volt, próbát ajánlott fel a társaság egyik tagja. Tessék egy akármilyen meghatározható egyént kijelölni a föld másfélmilliárd lakója közül, bármelyik pontján a földnek – ő fogadást ajánl, hogy *legföljebb öt* más egyénen keresztül, kik közül az egyik neki *személyes ismerőse*, kapcsolatot tud létesíteni az illetővel, csupa közvetlen – ismeretség – alapon, mint ahogy mondani szokták: "Kérlek, ismered X. Y.-t, szólt neki, hogy szóljon Z. V.-nek, aki neki ismerőse..." stb.

– Na, erre kíváncsi vagyok – mondta valaki –, hát kérem, mondjuk... mondjuk, Lagerlöff Zelma.

– Lagerlöff Zelma – mondta barátunk –, mi sem könnyebb ennél.

Két másodpercig gondolkodott csak, már kész is volt: – Hát kérem, Lagerlöff Zelma mint a Nobel-díj nyertese, nyilván személyesen ismeri Gusztáv svéd királyt, hiszen az adta át neki a díjat, az előírás szerint. Márpedig Gusztáv svéd király szenvedélyes teniszjátékos, részt vesz a nemzetközi nagyversenyeken is, játszott Kehrlinggel, akit kétségkívül kegyel, és jól ismer – Kehrlinget pedig én magam (barátunk szintén erős teniszjátékos) nagyon jól ismerem. Íme a lánc – csak két láncszem kellett hozzá a maximális öt pontból, ami természetes is, hiszen a világ nagyhírű és népszerű embereihez könnyebb kapcsolatot találni, mint a jelentéktelenekhez, lévén előbbieknek rengeteg ismerőse. Tessék nehezebb feladatot találni.

A nehezebb feladatot: egy szögecselő munkást a Ford-művek műhelyéből, ezek után magam vállaltam, és négy láncszemmel szerencsésen meg is oldattam. A munkás ismeri műhelyfőnökét, műhelyfőnöke magát Fordot, Ford jóban van a Hearst-lapok vezérigazgatójával, a Hearst-lapok vezérigazgatójával tavaly alaposan összeismerkedett Pásztor Árpád úr, aki nekem nemcsak ismerősöm, de tudtommal kitűnő barátom – csak egy szavamba kerül, hogy sürgönyözzön a vezérigazgatónak, hagy szóljon Fordnak, hogy Ford szóljon a műhelyfőnöknek, hogy az a szögecselő munkás sürgősen szögecseljen nekem össze egy autót, éppen szükségem lenne rá.

Így folyt a játék, és barátunknak igaza lett – soha nem kellett ötnél több láncszem ahhoz, hogy a földkerekség bármelyik lakosával, csupa személyes ismeretség révén, összeköttetésbe kerüljön a társaság bármelyik tagja. Mármost felteszem a kérdést – volt-e valaha kora a történelemnek, amikor ez lehetséges lett volna?..."

Karinthy Frigyes,
Pesti Napló, 1927 (részlet)

Correlates of Creativity

Striking the Mother Lode in Science. The Importance of Age, Place, and Time. PAULA E. STEPHAN and SHARON G. LEVIN. Oxford University Press, New York, 1992, xiv, 194 pp., illus. $29.95

The notion that creativity in science declines with advancing age is both familiar and controversial – not surprisingly, given its relevance for all of our professional lives. Less familiar are the scholarly origins of the "age decrement" concept. In the 1950s H.C. Lehman tried to establish the facts of the matter on the basis of lists of major breakthroughs or discoveries in science, which could then be associated with the ages of their principal authors. this led to generalization regarding the age at which such work was typically done and to the notion of a creativity peak: somewhere around 35, though varying somewhat from discipline to discipline. Both method and conclusions were criticized from the start, not least by Lehman's fellow psychologists. Subsequently sociologists of science, including Harriet Zuckerman and Robert K. Merton, pointed out that various factors intervene in this relationship, not least the typical changes in tasks that come with age and advancement. Only those scientists who are widely acknowledged to be highly productive are likely to continue to do research.

Stephan and Levin have taken these studies as their starting point and, on the basis of literature review and some original empirical work, have gone considerably further. Most intriguingly, they have taken a step back, in first asking how any relationship between age and scientific creativity or productivity might come about. What does a scientist need in order successfully to engage in research? How does his or her access to material resources (suitable employment, equipment, colleagues, and so on) change with age? How does motivation change as a scientist ages? These essential inputs are further broken down and analyzed. Drawing in part on the sociological literature, Stephan and Levin argue that a scientist's motivation can be understood as deriving from three distinct types of incentive: financial reward; the urge to solve puzzles; and the urge to secure the approval of one's peers. The authors' economic expertise is deployed intriguingly and originally in their attempt to compare the personal costs and benefits of engaging in research at different points in the career. A successful older scientist, for example, may have little to gain in publishing one more paper, and remaining in the laboratory may be associated with considerable opportunity costs (no time for that lucrative consultancy, for example). The overall argument, however, far transcends the purely economic.

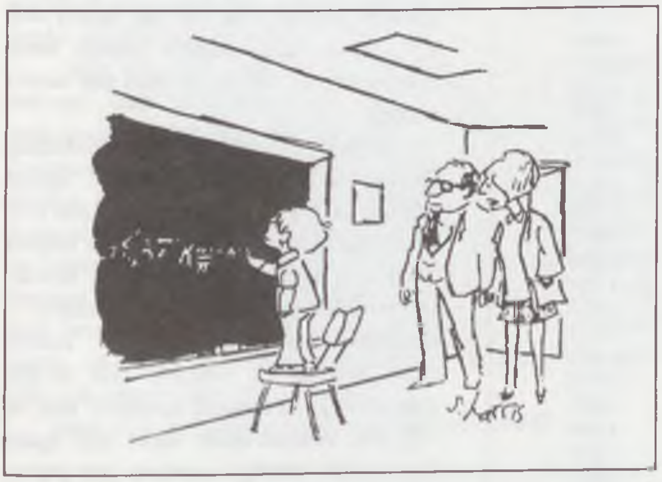
To be young and talented is not enough, for opportunities are unequally distributed through time and space. Best of all is to be young and talented *and* doing one's Ph.D. in a laboratory and a field in which a major new approach, theoretical breakthrough, or research technology is emerging. The fortunate few will acquire skills appropriate to the new approach before they have become widespread. Luckier still if they happen to do so at a time in which academic jobs are readily available. The model that Stephan and Levin try to articulate is a complex one. Being young in the 1960s, say, when universities were expanding, was a different matter from being young in the 1980s. Generational effects, so understood, intervene in the relationship between age and achievement. There are disciplinary effects, too, since the possibility of mastering a new approach (plate tectonics, chaos theory) or research technology (lasers, gene splicing) before it has become widespread depends upon the state of the science in question at the time. All this is summarized by saying that what matters is to be in the Right Place at the Right Time ("RPRT").

In the penultimate chapter of the book, the attempt is made empirically to investigate, separately, the effects of "pure aging" (by which is meant age corrected for motivation, resources, and so on) and generation ("vintage") on scientific creativity and productivity. This was done by linking up data drawn from the National Research Council's Survey of Doctorate Recipients with data drawn from the Science Citation Index. The method allows the authors to look independently at the publication patterns of individuals over time, from the year in which they obtained they doctorates, and also to compare "vintages" with one another. Three areas of physics and three areas of earth science were chosen. "Vintages" were then defined in relation to the periods, in each specialty, at which major conceptual or methodological changes took place. The conclusion, roughly speaking, is that age matters, although not very much. Scientists publish somewhat less as they age, and they are less likely to do pathbreaking work. These effects are more noticeable in the physical sciences than in other disciplines. The significance of "vintage" proved elusive: in any event it was not the case that more recent vintages were always more productive.

For any sociologist of science this work has to be seen as a major advance on most studies of scientific productivity. Stephan and Levin have recognized that scientific research is a collective activity (so access to colleagues is an important input to anyone's work) and that its practice is specialty-dependent. But I wonder if they have fully appreciated the implications of accepting these central sociological tenets. The ultimate objective of the book is to draw conclusions regarding the effectiveness with which the U.S. scientific community carries out research. Since research in the physical, biological, and earth sciences is nowadays principally carried out *by* (not just *in*) groups, was it proper to approach the matter through the examination of individual aging? Would the productive research group not have been the wiser starting point? Thanks to careful sociological work (by Shinn and others) we know that such a group displays complex divisions of labor. Drawing out the full implications of research data, in some disciplines, may require the distinctive contribution of a laboratory's most senior members. Given expected disciplinary variation, how much sense does it make to generalize about the scientific community as a whole? Though no statistician, I am also a little worried at the use of data regarding individual scientists to draw conclusion regarding the scientific community.

Just because of the authors' concern with diminished (relative to what?) national scientific productivity the book ends disappointingly. Though I agree with all they have to say here about the effects of excessive competitiveness and pressures to secure grants and to publish, I still regret the superficial way in which these effects are introduced in the final chapter. The final polemic does detract from the scientific quality of an interesting book.

Stuart Blume,
Science, 259:107-108, 1 January 1993



"He's a prodigy, all right, but all his work deals with subjects such as the elasticity of bubble gum and the molecular content of hot dogs."

"This is even better than computer camp."

Strongest Exports of U.S. Researchers: Papers in Physical, Computer Sciences

When the world's researchers sit down to settle their intellectual debts, many send their payments to the United States. Their "checks" take the form of citations, overt references in an author's writings that serve both to acknowledge debts and to cancel them.

Each year the Institute for Scientific Information follows this "money trail" worldwide. In 1991, it recorded more than 12.2 million individual citation payments. As the figures presented show (see above), the current trade balance in research is decidedly in favor of the United States.

When one considers articles of every type from every field, U.S. papers are currently taking in 52% more citations per paper than the world average. Of course, the "trade surplus" for the United States varies by field, and there are even a few fields in which America has been running up a deficit lately.

Table 1.
Citation Impact of U.S. papers vs. All Papers

Rank	Fields	U.S.Papers 1987-91	U.S. Impact	World Impact	U.S.: World
1	Physics	66,353	5.64	3.37	+64.7%
2	Chemistry	56,968	4.02	2.47	+62.8%
3	Geosciences	19,897	3.53	2.32	+52.2%
4	Computer Sciences	14,444	1.59	1.05	+51.4%
5	Materials Science	13,019	1.40	0.94	+48.9%
6	Biology	229,228	6.71	4.82	+39.2%
7	Astronomy	12,319	6.05	4.44	+36.3%
8	Engineering	32,175	1.42	1.06	+34.0%
9	Agriculture & Environmental Sciences	31,425	1.99	1.58	+25.9%
10	Mathematics	18,229	1.34	1.07	+25.2%
11	Clinical Medicine	150,455	3.72	3.15	+18.1%
12	Plant & Animal Sciences	55.098	2.03	1.86	+9.1%

Source: ISI's Science Indicators Database, 1987-91

Table 2.
Citation Impact of U.S. Papers vs. All Papers, 1987-91

Field/Subfields	U.S.Papers 1987-91	U.S. Impact	World Impact	U.S.:World Percent
Physics	66,353	5.64	3.37	+67.4
General	29,330	6.97	4.15	+68.0
Applied	30,422	5.07	3.07	+65.1
Optics/Acoustics	6,601	2.35	1.89	+24.3
Chemistry	56,968	4.02	2.47	+62.8
General	6,614	4.61	2.37	+94.5
Physical	22,377	4.96	3.02	+64.2
Chemical Engineering	5,827	1.64	1.00	+64.0
Analytical/Inorganic	10,471	3.41	2.28	+49.6
Organic	11,679	3.61	2.66	+35.7
Geosciences	19,897	3.53	2.32	+52.2
Geology	17,627	3.95	2.74	+44.2
Petrology/Mining	2,270	.26	.27	-3.7
Computer Sciences	14,444	1.59	1.05	+51.4
Materials Science	13,019	1.40	.94	+48.9
Metallurgy	2,433	1.18	.55	+114.5
General	10,586	1.45	1.12	+29.5
Biology	229,228	6.71	4.82	+39.2
Microbiology & Cell Biology	21,752	10.65	6.12	+74.0
Experimental biology	50,272	5.82	4.11	+41.6
General	4,365	3.68	2.62	+40.5
Biochemistry & Biophysics	42,622	7.80	5.83	+33.8
Life-Sciences Chemistry	8,537	5.58	4.27	+30.7
Molecular Biology & Genetics	14,972	10.03	7.75	+29.4
Pharmacology	23,245	3.68	2.85	+29.1
Biotechnology	1,805	2.72	2.13	+27.7
Immunology	16,032	8.07	6.48	+24.5
Neurosciences	33,399	5.35	4.61	+16.1
Physiology	12,227	5.59	4.54	+23.1
Astronomy	12,319	6.05	4.44	+36.3
Engineering	32,175	1.42	1.06	+34.0
Mechanical	8,305	1.44	.90	+60.0
Electrical	9,246	1.94	1.29	+50.4
Aerospace	2,995	.64	.45	+42.2
Environmental/Civil	9,310	1.18	1.05	+12.4
Nuclear	2,319	1.21	1.14	+6.1

To obtain the best assessment of various fields and subfields of U.S. research, *Science Watch* examined articles (original reports or discovery accounts only, but not reviews, letters, editorials, etc.) that were published and cited between 1987-91 and that carried at least one U.S. author address. The citation-per-paper averages for U.S. papers of different types were then compared to the corresponding world averages and the percentage difference calculated. For example, physics papers published by U.S. researchers from 1987 to 1991 attained a citation impact score of 5.64 during this same five-year period, whereas the average for the world was 3.37. Thus, U.S. physics papers were cited some 67.4% more than the world average.

Indeed, physics (including theoretical and applied physics, optics and acoustics) is the field which the U.S. holds the largest lead in citation impact compared with the world as a whole. Other areas of the physical sciences – chemistry and geosciences – follow close behind at +62.8% and 52.2%, respectively. Computer sciences, too, at +51.4%, earned more than half again the world's average citations per paper, and this realm rounds out the top third of the table above.

(Continued on next page)

Table 2.
Citation Impact of U.S. Papers vs. All Papers, 1987-91
(Continued from page 8)

Field/Subfields	U.S.Papers 1987-91	U.S. Impact	World Impact	U.S.:World Percent
Agriculture & Environmental Sci.	31,425	1.99	1.58	+25.9
Food Sciences	5,844	1.97	1.33	+48.1
Agronomy	6,369	1.09	.76	+43.4
Ecology	17,011	2.30	1.94	+18.6
Agricultural Chemistry	2,201	2.31	2.18	+6.0
Mathematics	18,229	1.34	1.07	+25.2
Clinical Medicine	150,455	3.72	3.15	+18.1
General	55,262	5.17	3.89	+32.9
Cardiology	8,704	4.59	3.46	+32.7
Neurology	4,520	3.91	3.00	+30.3
Hematology	1,924	6.89	5.31	29.8
Surgery	5,525	1.50	1.17	+28.2
Urology	4,627	2.56	2.13	+20.2
Otolaryngology	7,326	1.61	1.34	+20.1
Dermatology	2,918	2.49	2.10	+18.6
Reproductive Medicine	4,932	2.56	2.27	+12.8
Oncology	3,692	3.84	3.44	+11.6
Radiology	8,793	3.21	2.90	+10.7
Anesthesiology	2,821	2.56	2.33	+9.9
Psychiatry	4,420	3.56	3.29	+8.2
Social Medicine	3,359	2.77	2.56	+8.2
Gastroenterology	5,918	4.16	3.90	+6.7
Dentistry	5,059	1.47	1.40	+5.0
Medical Technology	5,475	3.84	3.78	+1.6
Orthopedics	9,412	1.15	1.33	-13.5
Plant & Animal Sciences	55,098	2.03	1.86	+9.1
Aquatic Sciences	7,726	2.76	2.17	+27.2
Veterinary Medicine	10,322	1.27	1.02	+24.5
Zoology	12,934	2.44	2.17	+12.4
Entomology	8,082	1.40	1.26	+11.1
Botany	16,034	2.15	2.12	+1.4

Source: ISI's Science Indicators Database, 1987-91.

The middle group includes fields in which the U.S. tallied between one-third and one-half more citations per paper than the world. This foursome is diverse in both subject matter and size. Materials science, at fifth with a relative citation impact score of +48.9%, is tiny compared with biology, sixth at 39.2% and with more than 17 times the number of papers as materials science. Astronomy and engineering come seventh and eight, at +36.3% and +34.0%, respectively.

The last four in the table are agriculture and environmental sciences, mathematics, clinical medicine, and plant and animal sciences, all of which were cited up to third more than the world average.

In terms of world share of papers published within the group of elite, international journals that ISI indexes, the ranking for the United States is a bit different (see table). In some high-impact areas such as computer sciences and geosciences, the United States holds a substantial share of the papers. On the other hand, in physics, chemistry, and material science, the U.S. share is relatively small although the impact of U.S. papers is high. As is turned out, there was no correlation between impact and world share.

The large table above provides a disaggregated view of the citation data for each of the 12 major fields. As always, taking the average of many members in a set can sometimes mask extremes contained with the set. For example, biology as field turns in a moderately strong performance, collecting 39.2% more citations than the world average. But U.S. papers in the subfield of microbiology and cell biology do almost twice as well (+74.0%). Likewise, plant and animal sciences, the field in which the United States holds the slimmest lead relative to the rest of the world (+9.1%), includes aquatic sciences and veterinary medicine, which attracted 27.2% and 24.5% more citations per paper than the world average, respectively.

Only two U.S. subfields underperformed the world average. One is petrology/mining (-3.7%). The other is orthopedic medicine, the poorest performer at -13.5% compared to the world. Not too long ago, a U.S. orthopedist, writing in a leading medical journal, noted a "decline of original contribution in clinical research and development by orthopedic surgeons in the United States" (*New England Journal of Medicine*, 323(9):608-9, 30 August 1990). "We have become very dependent on our foreign colleagues for major advances in clinical sciences that translate into effective patient care," he observed.

Does U.S. orthopedic medicine need mending?

Science Watch, 3 (September 1992) 1-2

U.S. Share of World's Papers, 1987-91,
in ISI's Science Indicators Database

Rank	Field	Percent
1	Astronomy	46.8
2	Computer Sciences	40.9
3	Mathematics	39.5
4	Geosciences	37.0
5	Biology	35.4
6	Plant & Animal Sciences	31.4
7	Engineering	30.6
8	Agriculture & Environmental Sciences	30.4
9	Physics	26.8
10	Clinical Medicine	26.2
11	Chemistry	20.1
12	Materials Science	19.1

The U.S. National Labs: Does Their Research Measure Up?

For some time now, the national laboratories of the U.S. department of Energy (DOE) have been the subject of increasing scrutiny. Policy makers are openly questioning the necessity of funding the weapons labs – Sandia, Lawrence Livermore, and Los Alamos – at the same levels as during 1980s when the threat from the Soviet Union was considerably greater than it is today. Those wrestling with the ever expanding federal budget deficit are wondering how much of the $6 billion currently spent each year on the national labs might be saved. And some politicians, worried about the nation's economic competitiveness, are asking whether the DOE labs can shift their missions toward civilian research and work more closely with industry – in fact, in some cases to become contract research shops for industry.

There is little question that changes are coming for the national labs, but when, how much, and what type of changes are yet to be determined. Thus, it seems an appropriate moment for *Science Watch* to examine how scientists themselves regard the research conducted by the DOE labs. The method used here is citation analysis, which reflects the influence that research at a given facility has had on others in the scientific community

Science Watch surveyed the scientific papers from eight large DOE labs that were published in journals indexed by ISI from 1981 to 1992. The papers of each were then divided into subfields based on the journals in which they appeared and the journal-subfield classification scheme employed in ISI's *Current Contents*. The labs were then ranked according to their mean citations per paper record in 1981-92 (papers published during 1981-92 and cited over the same period) and in the most recent five year period, 1988-92 (papers published during 1988-92 and cited during the same period). To be ranked in a subfield, a lab had to have produced at least 100 papers in a given period; an exception was made for

General Physics

1988-92 (U.S. average = 5.80)			1981-92 (U.S. average = 11.94)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Brookhaven	9.44	759	Brookhaven	17.37	1,951
Argonne	7.51	767	Berkeley	16.42	2,410
Berkeley	7.43	1,015	Argonne	13.59	1,728
Ames	7.36	229	Sandia	12.73	529
Livermore	5.95	977	Los Alamos	12.47	3,947
Oak Ridge	5.81	868	Oak Ridge	11.78	1,976
Los Alamos	5.60	1,680	Livermore	11.69	1,936
Sandia	5.28	221	Ames	9.78	550

Applied Physics/Condensed Matter Physics

1988-92 (U.S. average = 4.31)			1981-92 (U.S. average = 7.83)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Argonne	6.26	1,474	Argonne	9.09	2,998
Brookhaven	6.23	1,006	Berkeley	8.94	2,871
Ames	5.85	597	Brookhaven	8.80	2,220
Sandia	4.99	1,434	Sandia	8.55	3,017
Berkeley	4.91	1,372	Ames	8.54	1,205
Los Alamos	4.79	1,813	Oak Ridge	6.84	3,156
Livermore	3.71	1,108	Los Alamos	6.60	3,579
Oak Ridge	3.70	1,448	Livermore	5.66	2,145

Physical Chemistry/Chemical Physics

1988-92 U.S. average = 4.60)			1981-92 (U.S. average = 9.44)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Sandia	6.63	380	Berkeley	15.58	1,655
Berkeley	6.57	637	Los Alamos	12.75	965
Argonne	6.36	549	Sandia	12.09	941
Ames	5.26	242	Livermore	10.82	329
Los Alamos	4.76	338	Ames	10.46	512
Livermore	4.61	152	Argonne	10.42	1,331
Brookhaven	4.60	323	Brookhaven	9.58	796
Oak Ridge	3.51	301	Oak Ridge	8.93	782

Analytical, Inorganic & Nuclear Chemistry

1988-92 (U.S. average = 3.90)			1981-92 (U.S. average = 8.60)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Ames	5.62	246	Ames	11.42	563
Oak Ridge	5.41	328	Argonne	9.50	346
Argonne	4.76	164	Berkeley	9.06	256
Brookhaven	4.10	105	Brookhaven	8.26	307
Berkeley	3.58	90	Oak Ridge	7.09	659
Los Alamos	2.26	155	Los Alamos	5.81	353

Materials Science

1988-92 (U.S. average = 2.20)			1981-92 (U.S. average = 3.85)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Berkeley	3.69	352	Berkeley	5.80	842
Oak Ridge	3.47	399	Oak Ridge	5.08	789
Sandia	3.16	350	Sandia	4.81	1,013
Los Alamos	2.99	218	Ames	4.27	211
Ames	2.99	116	Los Alamos	3.85	470
Argonne	2.72	247	Livermore	3.56	404
Livermore	2.56	186	Brookhaven	3.40	401
Brookhaven	1.94	158	Argonne	3.36	647

Lawrence Berkeley Laboratory in analytical, inorganic & nuclear chemistry in 1988-92, when it produced "only" 90 papers.

For each subfield and for each period surveyed the average citation impact scores for all U.S. papers are also indicated, at the top of each ranking.

The results show that the research impact of these large DOE labs, as measured by citations per paper, generally exceeds the U.S. average in 1988-92 than they did in 1981-92.

Different labs clearly have different areas of strength and weakness. As for strengths, Brookhaven ranked first in general physics; Argonne topped the list in applied physics; Ames placed first in analytical, inorganic & nuclear chemistry; Berkeley bested all others in material science; and Sandia took top honors in nuclear engineering, for both periods.

As for weaknesses, Oak Ridge was last in physical chemistry and in biochemistry/biophysics for both periods, and it fell from sixth to last in applied physics, comparing 1981-92 to 1988-92; Brookhaven ranked at the bottom or near to it in physical chemistry and material science during both periods; Livermore placed last or next to last in applied physics; and Los Alamos was last in analytical, inorganic & nuclear chemistry during both periods.

Nuclear Engineering

1988-92 (U.S. average = 1.85)			1981-92 (U.S. average = 2.81)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Sandia	3.64	332	Sandia	5.27	889
Berkeley	2.63	351	Berkeley	4.15	865
Brookhaven	2.61	324	Brookhaven	3.33	1,021
Argonne	1.96	451	Oak Ridge	3.17	1,830
Los Alamos	1.93	546	Argonne	3.09	1,645
Livermore	1.83	321	Los Alamos	2.73	1,581
Oak Ridge	1.62	582	Livermore	2.50	797

Earth Sciences

1988-92 (U.S. average = 3.62)			1981-92 (U.S. average = 7.93)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Sandia	4.79	101	Berkeley	9.36	291
Livermore	4.30	214	Los Alamos	9.28	676
Berkeley	4.30	128	Livermore	6.64	396
Los Alamos	3.91	307	Sandia	6.17	224

Experimental Biology and Medicine

1988-92 (U.S. average = 3.38)			1981-92 (U.S. average = 7.22)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Berkeley	4.78	200	Oak Ridge	7.42	706
Livermore	4.36	100	Brookhaven	7.39	404
Los Alamos	3.70	136	Berkeley	7.29	506
Argonne	3.68	130	Livermore	7.18	254
Oak Ridge	2.96	176	Argonne	6.75	386
Brookhaven	2.92	111	Los Alamos	5.45	355

Biochemistry and Biophysics

1988-92 (U.S. average = 6.83)			1981-92 (U.S. average = 13.94)		
Laboratory	**Cites/Paper**	**No. papers**	**Laboratory**	**Cites/Paper**	**No. papers**
Los Alamos	6.91	129	Berkeley	14.75	483
Berkeley	5.60	220	Brookhaven	13.17	423
Brookhaven	5.32	140	Los Alamos	13.09	303
Oak Ridge	4.44	157	Oak Ridge	11.21	359

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The significance of PhDs

Átalakulóban van tudományos minősítési rendszerünk, gőzerővel folynak a doktori akreditálási eljárások. E tevékenységi kavalkádban talán hasznosnak bizonyulhat néhány – e kérdéssel kapcsolatos – nyugat-európai szempont ismertetése.

There is no doubt about it, the PhD is a prestigious qualification. This is plain from the fact that, despite the widely acknowledged subsistence level of grants for postgraduate students, every year several hundred young scientists embark on the long journey to doctoral status. Indeed, there will be final-year undergraduates even now contemplating this as the next step in their career. The aim will be to learn the art of research as the pupil of an established practitioner.

When it works, this is marvellous: three years of scholarly discovery and productivity culminating in the acquisition of rank in one's chosen profession. The title "Dr" is there to use in all sorts of circumstances in nonprofessional life, with all the status that it brings. Though the system is not perfect, it has much to commend it and I, for one, am glad I went through it.

Of course, the PhD has not always been around. It began in Germany in the early part of the 19th century. In those days, it was awarded for not very well specified reasons. The famous German chemist, Justus Liebig, for example, actually bought his PhD, though in view of his later achievements perhaps we should not dwell on that. Many scientists certainly acquired a PhD without having done much original research. For example, the English chemist Edward Frankland obtained his from Marburg in 1849 in the strength of less than a year's work.

The PhD came to Britain soon after the First World War. It was one of a number of changes to British scientific training and research which stemmed from the realisation that the war had been a close run thing. One reason more or less unanimously agreed for the near defeat of the Allies was the overwhelming technological superiority of the Germans. And that superiority was founded on a superior education system for their scientists and engineers.

There was, of course, resistance by some British academics to the new qualification. They called it contemptuously "that German degree". Oxford (where else?) went its own way and styled its doctorate "DPhil", though it was no different from PhD awarded by other universities, requiring the preparation of a thesis based on between two and three years of original research. The Oxford DPhil is no longer unique, since the University of Sussex and the University of York helped themselves to that designation in the 1960s.

In the early days, the PhD viva was a public examination, as it still is in some Continental universities. Don't think the public was not interested, either, if by "public" you include other research students studying for the doctorate. Quite an audience would turn up to watch the performance. In universities that have a strong emphasis on research, PhD examinations could prove very popular. By comparison with this, the agonising face-to-face examination that constitutes the modern PhD viva seems quite civilised. At least it has the merit of privacy.

Despite the status of the PhD, there is actually some doubt about what it really says about its owners. What is PhD-standard research? A research student, anxious for reassurance, asked me this soon after I had been awarded mine. "Is this PhD standard?" he said as he showed me a computer program for modelling hazard contours around chemical plants. As a newly qualified organometallic chemist, I had absolutely no idea. In the end, I came to the conclusion that the criterion for a PhD was probably a three-year project that led to about four or five original papers in the scientific literature.

However this begs all sorts of questions. Which journals are real literature? How original is "original"? How long should the papers be? And if publications is the criterion, why write a thesis at all? My mind began to reel, and I was glad to get away from academic life and leave such unanswerable questions behind.

But these questions have not gone away. This is because I have discovered that my own PhD was only the first of a number of similar milestones which have marked out my career. With time, I acquired my first subordinate with a PhD. Later, I was flattered to be asked to help supervise a PhD student of my own, albeit distantly as industrial supervisor to a project within the Science and Engineering Research Council's scheme of cooperative awards in science and engineering – the so called CASE awards. Then, the ultimate proof of my reputation as a scientist: I was asked to be examiner in PhD viva.

The more I think about it, the more I think a career in science can be summed up in a paraphrase of T.S. Eliot's Prufrock: "I have measured out my life in PhDs."

J. Nicholson, New Scientist, 49-50, 15 May 1993

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