4. évfolyam · 10. szám · 1994. október

IMPAKT

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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Citation analysis confirms Australian science's declining influence

The quality of science in Australia relative to that elsewhere seems to have fallen markedly since the mid-1980s, according to a detailed analysis of the number and citation rate of papers produced by Australian and published last week.

Paul Bourke and Linda Butler, of the Performance Indicators Project at the Australian National University's Research School of Social Sciences, found that Australians still produce about the same proportion of the world's research papers — about two per cent — as in 1987.

But, building on a theme first raised last year by the US Institute for Scientific Information (ISI) in its publication *Science Watch* — and following similar work on British science carried out at the University of Sussex's Science Policy Research Unit — they also found that the citation rate had fallen by 25 per cent over the same period.

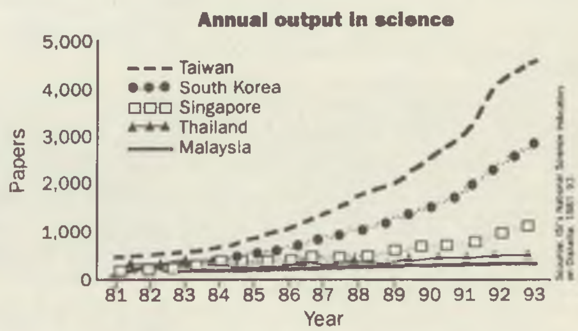
The breakdown of publication and citation rates disciplines in the analysis of ISI data reveals some large variations.

For example, Australia has managed to maintain its long-standing prominence in agriculture and earth sciences. Similarly, the citation rate remained reasonably stable in medical science, industrial biotechnology and food sciences.

But citation rates in the non-medical biological sciences, in physics and in chemistry have each fallen below the international levels they had reached in the mid-1980s. Even more worrying, citation rates dropped by 41 per cent in information, computing and communication technologies.

A comparison of the results with the performance of other countries in the ISI analysis showed that only one country (Sweden) has a similar pattern of producing as much research as a decade ago but being quoted less widely.

While Australia reflects on the declining impact of its science output, figures just released by the ISI (right) reveal that its Pacific Rim neighbours are enjoying a steady increase in the number of papers published and cited. Singapore in particular has seen a surge in papers published from 167 papers in 1982 to 1,220 in 1993.



Canada, Germany, Japan, the Netherlands, China and South Korea are each producing more research, and this is being cited more often. Researchers in France and Switzerland are contributing a stable proportion of papers, but they are earning more citations.

(Continue on next page)

Bourke points out that countries in which citation rates have been declining — namely Australia, Sweden and Britain — are also those in which a relatively high proportion of research is performed in universities. But he emphasizes that a lot of work is still needed before the reasons for the decline can be properly identified.

He speculates that possible reasons could include a general shift from basic to applied research — a major theme emphasized in government funding for science — the ageing of the Australian scientific community and a decaying research infrastructure.

Mark Lawson,
Nature, 370 (14 July, 1994) 86

NIH pondering further changes in grants approval process

Awards based on track record of researchers rather than proposals, grants of fixed size under consideration by Varmus

In the eight months that Harold E. Varmus has been director of the National Institutes of Health, his efforts to obtain more funding for research have been only minimally rewarding, he says. But his attempts to do better with the resources NIH does have are proving more fruitful.

For example, a trial of "triage" of grant applications has been successful enough that it soon will be expanded to cover all applications initiated by single investigators. To reduce the burden on reviewers, NIH study sections (peer review panels) are being instructed to concentrate on those grant proposals most likely to win funding after rejecting as many as half after preliminary review.

Another experiment to reduce paperwork for both applicants and NIH staff is getting under way. During a pilot test of the "just-in-time" system NIH will collect detailed information on grant budgets and researchers' backgrounds only for those proposals that have been approved for funding.

Varmus and top NIH officials also are mulling over more radical changes that go to the heart of how the world's premier biomedical research agency distributes some $8 billion a year in grant money. The goal behind these "reinvention" activities, as NIH calls them, is to ease the workload for applicants and reviewers while saving money for the direct support of research.

Scientists and research administrators got a chance to discuss the possible advantages, disadvantages, and impacts of such innovations earlier this month at NIH's Bethesda, Md., campus. No final decisions were made, but NIH will consider the reactions of the two dozen invited participants — which ranged from unqualified enthusiasm to undisguised discomfort — as it decides how to move forward.

Most controversial by far was a suggestion to shift the focus in funding grants from the particular research project proposed to the person being supported. Such "retrospective" review presumes that scientists who have demonstrated they can perform outstanding research will continue to do so.

With a few exceptions, NIH currently awards grants based on prospective review of an extensive research proposal. With retrospective review, only the significance of the proposed research would be evaluated, not a detailed account of the methods to be used.

The idea is championed by Nelson Y.S. Kiang, director of the Eaton-Peabody Laboratory at Massachusetts Eye & Ear Infirmary in Boston. "I want to get scientists back to doing science rather than writing applications," he told the group.

Retrospective review for established researchers would be welcomed by many young scientists who believe themselves to be at a disadvantage when competing with the stars in their fields, according to Howard Schachman, professor of biochemistry and molecular biology at the University of California, Berkeley. Schachman has been visiting universities to talk to faculty, administration, and graduate students in his role as ombudsman for NIH.

"Young people would love to see one [application system] for researches with established reputations and another based on proposals for individuals with no track record," he says. "They would like separate pools of money."

David Botstein, chairman of the genetics department at Stanford University school of medicine, also endorses the idea. "We should shake up the system so that study sections go back to looking for ideas rather than picking nits," he says.

NIH reinventing peer review process

Just-in-time applications. Detailed information on budgets and other administrative details will only be collected on proposals likely to be funded (pilot program under way).

Modular grants. Applicants would apply for set levels of support.

Retrospective review. Grants would be awarded based on accomplishments of person being supported rather than project proposed.

Triage of grant applications. Proposals deemed noncompetitive will not receive full peer review (experimental program being phased in).

But the idea met with strong resistance from others at the meeting. "It's a terrible idea that's going to foster the worst sort of old-boy, old-fogy network," says Sharon Boehm Murphy, professor of pediatrics at Northwestern University in Evanston. "The emphasis on track record could work to the disadvantage of women who may have taken time out for [raising] a family."

Others point out that preparing a detailed research proposal can be a valuable intellectual exercise. "We are talking about people asking for as much as $1 million from the federal government," says Elvera Ehrenfeld, dean of the school of biological sciences at the University of California, Irvine. "It's not such a terrible thing to ask... what they are going to do for the next few years."

Wendy Baldwin, deputy director of NIH's Office for Extramural Research reminded the group that the idea of retrospective review was only up for discussion. "The trick is to use the appealing features. It's not all or nothing."

Retrospective review might be appropriate when a researcher is renewing a grant, many scientists agreed. The researcher's accomplishments over the last grant period would be reviewed, not his or her entire career.

But even under those limited circumstances, retrospective review could have drawbacks, says Terry A. Krulwich, dean of the graduate school of biological sciences at Mt. Sinai School of Medicine in New York City. "We might trade off the work of the applicants for the workload of the study section," she says. "We'd have to look more carefully at the experiments in the [researcher's] publications."

Less controversial was a proposal to experiment with modular or "chunk" grants. NIH would establish a limited set of award sizes — say small, medium, or large grants of $100,000, $150,000, or $200,000 — and researchers would apply for a particular category of grant. Applicants would be spared the tedium of preparing detailed budgets and reviewers could focus on the science.

The idea is particularly attractive to David E. Boettiger, professor of microbiology at the University of Pennsylvania's school of medicine. He fears that small laboratories that tend to focus on riskier research are losing to larger labs in the competition for scarce funds.

"Chunk grants would let large projects compete with large [projects], and small with small," he says.

Other scientists appreciate that modular grants would reduce the time spent on preparing budgets that often are unrealistic anyway. "The budgets we are finally awarded bear no relationship to what we ask for," notes Ira S. Mellman, professor of cell biology at Yale University and chairman of an NIH study section. "And what we spend the money on bears no relationship to what we wrote down."

Varmus is concerned about an assumption underlying modular grants that research institutions would share the cost of the research. "Do we move away from the long-standing commitment to pay the full cost of research toward the concept of grants-in-aid?" he asks. "What would be the effect on indirect costs?"

The National Heart, Lung & Blood Institute is testing modular grants. Its experience will be carefully evaluated before further applying the concept at NIH, Baldwin says.

Pamela Zurer, C&EN, (July 25, 1994) 20

Chemical research fraud uncovered in Germany

Chemical research published four months ago in prominent international journal — prompting the journal's editor to comment "If it's true, it's spectacular" — has been found to be based on fraud by a graduate student.

In February, a group of German chemists published a paper in *Angewandte Chemie* describing extraordinary enhancement of enantioselectivity by use of a magnetic field [*Angew. Chem. Int. Ed. Engl.*, 33, 454 (1994)]. The research team was led by organic chemistry professor Eberhard Breitmaier of the University of Bonn and included Bonn graduate students Guido Zadel and Catja Eisenbraun, and chemist Gerd-Joachim Wolff of Bruker Analytical Measurement Technology in Rheinstetten.

Breitmaier and coworkers reported that addition of alkyl Grignard reagents to aromatic aldehydes in magnetic fields of 12 tesla produced asymmetric alkylarylcarbinols in enantiomeric excesses as high as 98% (C&EN, Feb. 28, page 36). Put into perspective, magnetic fields of that strength would cause protons to resonate at 50 MHz. The group also reported that reduction of alkyl ketones with lithium aluminium hydride in such magnetic fields gave carbinols in enantiomeric excesses as high as 68%.

The affair began to unravel almost at once as chemists in a number of labs around the world rushed to repeat the work but could not reproduce the results. Breitmaier says he then discovered that Zadel had fabricated the research.

For example, one of the claimed results was reduction of propiophenone to 1-phenyl-1-propanol in 55% enantiomeric excess. As Breitmaier explains in a letter to *Angewandte*, Zadel had spiked the solution of propiophenone starting material with (+)-1-phenyl-1-propanol. When other scientists in Breitmaier's research group repeated the work with solutions prepared by Zadel, their findings confirmed Zadel's results.

However, the deception was revealed when these scientists analyzed Zadel's leftover solutions by gas chromatography and polarimetry. Breitmaier says that Zadel then acknowledged carrying out fraudulent manipulations.

Many scientists may be reassured by the elapse of only four months from publication to exposure. That's the conclusion, for instance, of one of the chemists who tried unsuccessfully to reproduce the results — organic chemistry professor Nicholas J. Turro of Columbia University. Turro calls exposure of the hoax "an example of good science. The system purified itself. And it took a short time to resolve."

Angewandte editor Peter Gölitz tells C&EN that "If the results had been right, they would have been of the utmost importance for questions of the origin of life and the synthesis of chiral drugs." To keep damage to the scientific community to a minimum, he will publish a letter of retraction by Breitmaier, Eisenbraun, and Wolff in a June issue of the English edition and in a July issue of the German edition.

S. Stinson, C&EN, 7 (June 27, 1994)

The peer review system

Increasingly, the peer review process in science has been a target for criticism. (Much of this criticism has been summarized in the recent book, *Peerless Review*, by D.E. Chubin and E.J. Jackett, State University of New York Press, Albany, 1990.) It has been attacked as being biased, unreliable and harmful toward innovative science. Nearly every practising scientist has probably had a private complaint about the system, occasionally feeling that a manuscript or proposal has been unfairly treated by the anonymous reviewers. On the other hand, most scientists are staunch defenders of the peer review system, arguing that only qualified specialists can properly judge cutting-edge research. The discussion, however, tends to be rather emotional, often relying on fragmentary anecdotal evidence for support of the various positions.

Guardians of Science by H.-D. Daniel represents one of several attempts to systematically and objectively study the peer review process. Daniel was given access to the manuscript files of *Angewandte Chemie* for the year 1984. He investigated the files for all communications received that year, 449 papers. By choosing this early year for study, he was able to examine the fate of papers that were rejected by the journal by looking to see whether they had eventually been published and whether they had received a favourable reception, at least as measured by the Science Citation Index, and compare it to the reception of those papers accepted and published.

What emerges is a detailed picture of the review process at *Angewandte Chemie*, a protocol that is fairly typical of chemistry journals that we are familiar with. For those unfamiliar with the detailed workings of a scientific journal, this book provides invaluable details concerning what kinds of manuscripts are received, who submits them, who reviews them and their ultimate fate. He even includes synopses of referee reports as examples of the reviews that are submitted.

The book reports a variety of statistical analyses that attempt to answer the questions that have been raised about the peer review system, particularly the issues of reliability and bias. Unfortunately, these results are difficult to interpret. For example, does a higher acceptance rate for papers submitted by German professors indicate bias toward senior scientists or does it indicate that senior scientists on average do high quality research and write good papers? Citation counts give some indication of the reception of an article but, for most papers, may not be a clear indication either of quality or impact. To be sure, Dr. Daniel is careful not to over-interpret the data. He presents it clearly and allows the reader to draw the ultimate conclusion.

On the whole, the book is tersely but clearly written. There were a few places where more detail would have been useful to these reviewers. In Chapter 8 a fuller discussion and justification of the chance corrected statistics used to measure reviewer agreement would have made that section of the book more intelligible to the non-specialist. While the description of the review process at *Angewandte Chemie* is fairly complete, we had two questions that were never explicitly answered. First, does the journal only accept manuscripts in German? If so, many of the statistics concerning the nationality of authors and reviewers are hardly surprising. Second, is it the editorial philosophy of *Angewandte Chemie* to select one 'specialist' reviewer and one 'generalist' reviewer? At various points in the book, Dr. Daniel suggests that this practice explains why review comments often do not overlap, but he never tells us explicitly whether the journal he has studied chooses its reviewers in this way.

We suspect that most experienced scientists will not find the results of Dr. Daniel's study to be surprising. His data indicate that the peer review process at *Angewandte Chemie* is working fairly well. Some mistakes are certainly made, but the decisions, on the whole, seem to be thoughtfully and carefully made. Those readers wanting a closer look at the peer review process at work will find this an interesting but rather dry work. Those readers looking for ammunition for further attacks on the peer review system will be disappointed.

J. Kovac and G. Guichion
Trends in Analytical Chemistry, 99 (9)(1994) X XI

Are science parks virtually dead?

Successful trials of a communications system devised at the University of Leeds could signal the end of the fashionable science parks in which academic researchers work side by side with high-tech businesses. What the Leeds researchers call a Virtual Science Park will allow researchers at the university to keep in constant contact with colleagues in industry while travelling no further than the nearest computer.

By using the Internet to link groups of researchers, they can work together no matter where they are located. The £2 million project is being led by Peter Drew of the School of Computer Sciences at Leeds, together with BT, IBM and the Regional Technology Network in Barnsley.

The software designed by Drew and his team includes a program that will automatically search out a colleague and set up a video conference call if he or she is signed on to their computer. Other programs will make it easy to set up a web of information, so that university researchers and their counterparts in industry can share documents via the computer network, and even collaborate on projects from separate offices.

Kurt Kleiner, New Scientist, 19 (23 July 1994)

Peer Review Reforms Get Good Review

One of the biggest experiments going on right now at the National Institutes of Health (NIH) doesn't involve rats, mice, cell cultures, or viruses. Instead, the research subjects are biomedical scientists, and the research focuses on how they wriggle through a maze of reviews each year to obtain $8 billion in federal funds. The experiment is designed to see whether NIH's peer review system — which sorts these 38,000 grant seekers into winners and losers — can be made simpler, fairer, and more efficient.

NIH began testing new approaches to peer review shortly after Harold Varmus became NIH director in 1993, in response to suggestions that the venerable system is in need of a tuneup. Last week, Varmus and his deputies met with scientists from around the country at a "round table" to discuss how the experiments are going. Varmus came away so encouraged by the response, he says, that he wants to start implementing some reforms and expand the testing of others.

In a telephone interview, Varmus said he and his assistant director for extramural research, Wendy Baldwin, want to make wider use of the "triage" approach to sorting grant applications, tested this year by 12 review panels. This technique is designed to eliminate 30% to 50% of the submissions off the top as "noncompetitive" before they're sent to a panel for full review. Varmus adds, however, that "we may change the terminology," because noncompetitive is "a pretty rough term" to use in rejecting first-time applicants.

Baldwin said NIH also intends to implement a "just-in-time" rule for providing data, so that only those who make it through the first cut would be required to submit detailed budgetary and administrative data. And to make it easier to submit such data, NIH plans to increase the use of electronic networks, giving researchers a personal identification number (PIN) so that they can access government computers to send or retrieve information. NIH managers also aim to broaden the scope of some peer review groups (study sections) and test a system of "chunk grants," allowing applicants to apply for small but fixed amounts of cash and thereby minimize the need for detailed budget estimates. Finally, Varmus wants to find new ways of rewarding innovative ideas. He says "a lot of people are concerned that study sections have become too conservative," nitpicking at flaws rather than concentrating on scientific merit. There ought to be a way of giving an advantage to risktaking applicants, he says.

Most of these ideas are now being tested on a small scale, and most received warm support from the several dozen attendees at the round table. One idea, however, sank like a lead weight: a suggestion that NIH switch from evaluating grant proposals prospectively to a retrospective evaluation of the applicant's previous research. The goal of such an experiment, advocated by Nelson Kiang, director of the Eaton-Peabody Laboratory for eye and ear research in Boston, would be to drastically simplify the review process.

Kiang said that anyone seeking a grant should be asked to provide detailed information about previous accomplishments, but only a brief sketch of the research for which they seek funding. Postdocs, for example, could be reviewed on the basis of their theses. He notes that the current system requires pages of detailed descriptions of future work, along with precise data on staff and equipment costs in each future phase of study. Such details, David Botstein of Stanford University said, are mere "bureaucratic fantasies," created to satisfy the review process but rarely followed. But when an NIH staffer presented this idea to the round table, several speakers — particularly women and others who spoke for minority or young scientists — objected that retrospective review would favor the "old boys" who are already well established.

Varmus noted that NIH already uses retrospective review in some ways — openly in judging the work of intramural staffers and implicitly in awarding extramural grants. "It would be naïve," he told *Science*, "to think that when we review applicants we are just looking at the proposal." Reviewers also take into account an individual's experience, track record, and his or her sources of funding. Varmus said he recognizes that "people are concerned about squeezing the new blood out of the system." However, it might be possible to use retrospective review more often for scientists seeking grant renewals.

While this idea got a mixed response, the related proposal for "chunk grants," put forward by David Boettiger of the University of Pennsylvania, got a warmer reception. "I was a little surprised by the enthusiasm" for the concept, says Varmus, who likes it himself. The idea is to set aside a pool of money for research projects costing, say, $50,000 to $200,000 a year, and to award a specified number of small, fixed-price grants each year. The goal would be to have applicants and reviewers spend less time on budgets and focus almost exclusively on science. Varmus says it "is definitely going to warrant more attention" and will be tested first by the National Heart, Lung and Blood Institute.

Varmus predicts there will be "more pilot studies" and "more discussions" before NIH endorses any of these concepts for use across the board. Some people, he adds, "have criticized me for paying attention to peer review as though I'm considering it a substitute for getting more money," but, he argues, this is not the case. Varmus says he is "just facing reality" in recognizing that NIH isn't likely to get a big budget increase. Meanwhile, he does want to "instill confidence" in the system and persuade researchers that "we're doing things as fairly as we can."

Eliot Marshall,
Science 265 (22 July, 1994) 467

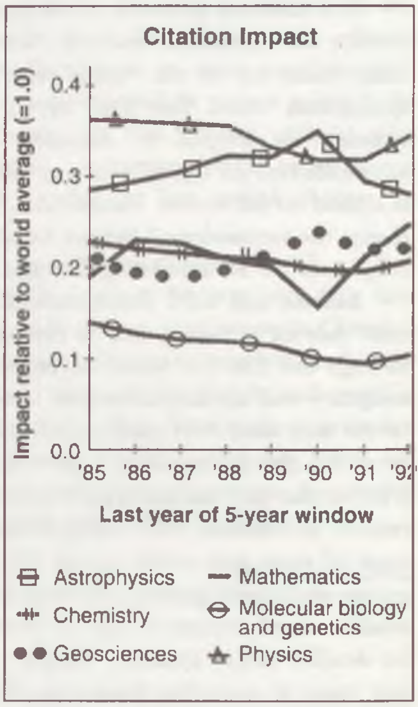
Russian science seen from the West

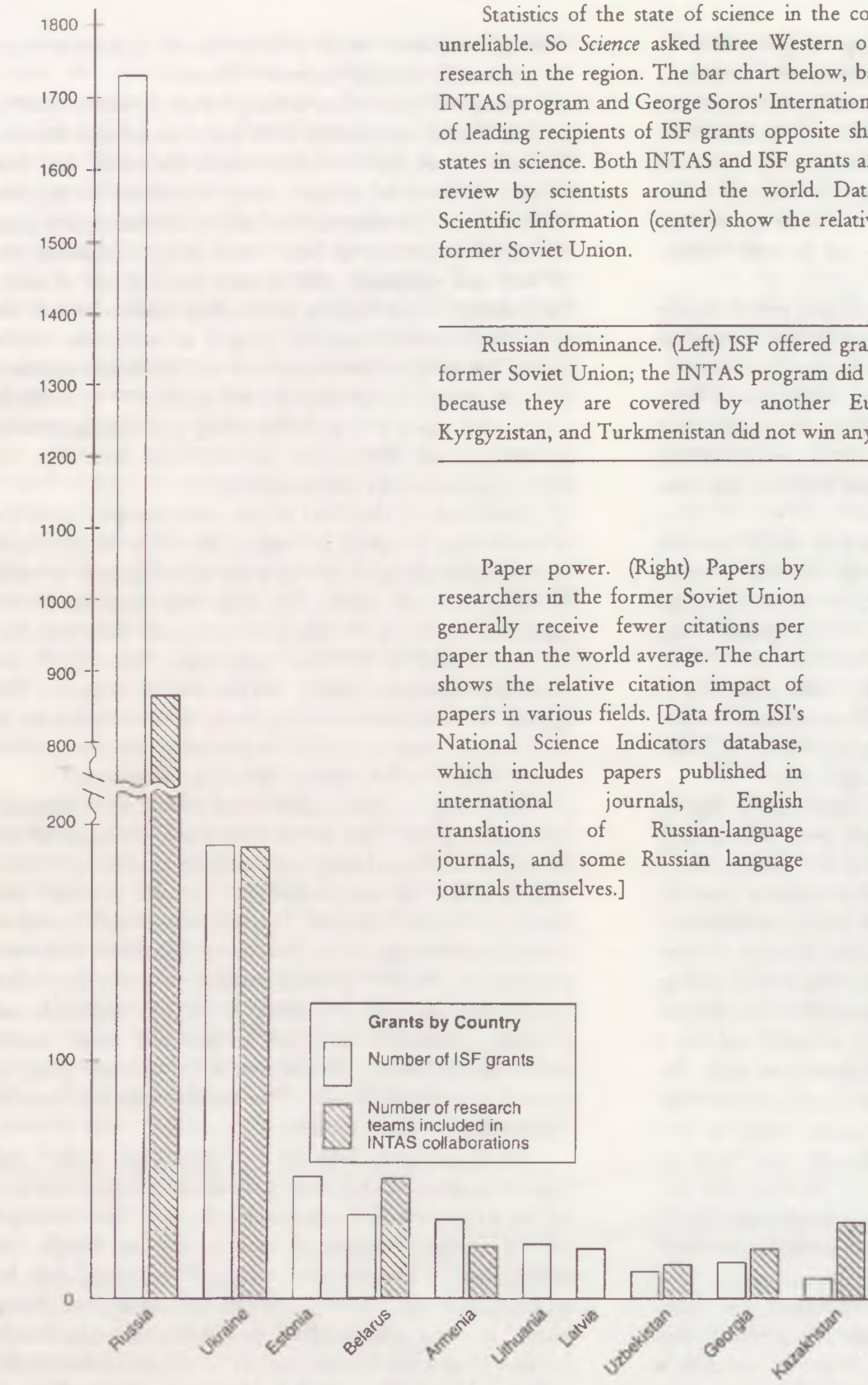
Statistics of the state of science in the countries of the former Soviet Union are unreliable. So *Science* asked three Western organizations for soundings on the state research in the region. The bar chart below, based on data from the European Union's INTAS program and George Soros' International Science Foundation (ISF) and the table of leading recipients of ISF grants opposite show how Russia dominates the ex-Soviet states in science. Both INTAS and ISF grants are awarded on the basis of extensive peer review by scientists around the world. Data from Philadelphia-based Institute for Scientific Information (center) show the relative strengths of selected fields within the former Soviet Union.

Science, 264 (27 May, 1994) 1260-1261

Russian dominance. (Left) ISF offered grants to researchers in all countries of the former Soviet Union; the INTAS program did not include Estonia, Latvia, or Lithuania because they are covered by another European Union program. Tadjikistan, Kyrgyzistan, and Turkmenistan did not win any ISF long-term research grants.

Paper power. (Right) Papers by researchers in the former Soviet Union generally receive fewer citations per paper than the world average. The chart shows the relative citation impact of papers in various fields. [Data from ISI's National Science Indicators database, which includes papers published in international journals, English translations of Russian-language journals, and some Russian language journals themselves.]





Power centers. (On the next page.) The International Science Foundation received more than 9000 requests for its first round of long-term research grants, which was reviewed by some 50,000 scientists. A total of 2611 grants was awarded, with an average value of $15,000 over 18 months. Institutions awarded at least 10 grants are shown in the table to the right.

ISF's Top Awardees

Institution	City	Country	No. of Grants
Moscow State Univ., Chem. Dept.	Moscow	Russia	66
Ioffe Physico-Technical Inst.	St. Petersburg	Russia	44
Semenov Inst. of Chemical Physics	Moscow	Russia	41
Lebedev Inst. of Physics	Moscow	Russia	40
Institute of General Physics	Moscow	Russia	36
Moscow State Univ., Physics Dept.	Moscow	Russia	33
Institute of Organoelement Compounds	Moscow	Russia	31
Institute of Applied Physics	Nizhnii Novgorod	Russia	28
Moscow State Univ., Inst. Physico-Chem. Biol.	Moscow	Russia	27
Zelinskii Institute of Organic Chem.	Moscow	Russia	27
Institute of Catalysis	Novosibirsk	Russia	25
Inst. of Radioengineering & Electronics	Moscow	Russia	25
Shirshov Institute of Oceanology	Moscow	Russia	24
Russian Res. Ctr. — Kurchatov Inst.	Moscow	Russia	23
Institute of Space Research	Moscow	Russia	23
Inst. of Low-Temp. Physics and Eng.	Kharkov	Ukraine	20
Inst. of Evol. Animal Morphol. & Ecol.	Moscow	Russia	20
Moscow State Univ., Biol. Faculty	Moscow	Russia	20
Joint Institute for Nuclear Research	Dubna	Russia	19
Inst. of Theoret. & Exptl. Physics	Moscow	Russia	19
Institute of Chem. Physics	Chernogolovka	Russia	19
Inst. of Geochem. and Analytic. Chem.	Moscow	Russia	18
Steklov Institute of Mathematics	Moscow	Russia	18
Institute of Solide-State Phys.	Chernogolovka	Russia	17
Institute for Cardiol. Research	Moscow	Russia	17
Engelhardt Institute of Mol. Biol.	Moscow	Russia	16
Institute of Physics of the Earth	Moscow	Russia	16
Institute of Cytology and Genetics	Novosibirsk	Russia	16
Institute of Protein Research	Pushchino	Russia	15
Institute of Nuclear Physics	Novosibirsk	Russia	15
Institute of Nuclear Safety	Novosibirsk	Russia	15
Institute of Metal Physics	Ekaterinburg	Russia	15
Moscow State Univ., Mechanics & Math. Dept.	Moscow	Russia	13
Institute of Crystallography	Moscow	Russia	13
Institute of Physical Chemistry	Moscow	Russia	13
Center for Cancer Research	Moscow	Russia	12
Shemyakin Inst. of Bioorganic Chem.	Moscow	Russia	12
Institute of Cytology	St. Petersburg	Russia	12
St. Petersburg Univ., Physics Dept.	St. Petersburg	Russia	12
Institute of Gen. and Inorganic Chem.	Moscow	Russia	12
Inst. of Chem. Kinetics & Combustion	Novosibirsk	Russia	11
Institute of Physics	Kiev	Ukraine	11
Institute of Geology	Moscow	Russia	10
Landau Inst. of Theoretical Physics	Moscow	Russia	10
Nuclear Physics Institute	St. Petersburg	Russia	10
Inst. of Exptl. and Clinical Medicine	Riga	Latvia	10
Institute of Mathematics	Novosibirsk	Russia	10
Institute of Geol. of Ore Deposits, Petrography, Mineralogy & Geochem.	Moscow	Russia	10
Institute of Spectroscopy	Troitsk	Russia	10

The Midas touch

An elite corps of researchers funded by a single institute is stealing the limelight in biomedicine. Phyllida Brown asks if there are lessons to be learnt from this success.

When a rich and eccentric businessman set up an institution for biomedical research as a tax dodge in 1953, nobody could have predicted the impact it would have on science forty years on. Today, the Howard Hughes Medical Institute, the richest private philanthropic body in the US, is making its presence felt in a big way. Its small band of hand-picked scientists appears to have struck gold.

A study from the Institute for Scientific Information in Philadelphia shows that researchers whose salaries are paid by the HHMI were responsible for almost a quarter of the 200 most-cited biomedical papers published in scientific journals last year (see Graph). At the time, the HHMI employed just 222 investigators. This is an impressive performance set against that of the tens of thousands of other biomedical researchers in the US and worldwide.

Citation analysis measures the number of times other scientists refer to a paper in their work. And although scientists debate the value of this method as a measure of a paper's quality, most accept it as a good indicator of the paper's impact on its field.

The implications of the HHMI's high score are arousing widespread interest among the bodies that fund biomedical science. Some are asking if the NHMI has a formula that should be copied. The giant biomedical foundations, such as the Wellcome Trust in Britain and the Rockefeller Foundation in the US, and government agencies such as the US National Institutes of Health and Britain's Medical Research Council, are watching with interest at a time when many are considering radical new strategies for monitoring the outcome of their investments.

Over the past few years, Hughes investigators have been responsible for a number of key findings, including the discovery of the gene for cystic fibrosis, an ingenious test to show which TB bacteria are sensitive to drugs, and the genetic basis of fragile X syndrome.

But there is no magic in the "Hughes formula". Its shrewdest move, say researchers and policy analysts, has been to pick people who are already established and successful. No one should then be surprised if the *crème de la crème* continues to do well, whoever pays them.

Purnell Choppin, the institute's president and a virologist formerly based at Rockefeller University, New York, thinks the winning formula is a combination of things. "We try to pick the right people, provide them with the right kind of support, review them very carefully and allow them to proceed. And we do it with the minimum of paperwork." Most Hughes researchers say what they like most about working for the institute are its appreciation of the importance of pure science, and the freedom to pursue risky or innovative ideas or even change direction.

The HHMI sees itself as an "institute without walls". Scientists scattered across the US receive a salary from the institute, but remain at their own institution. In return, investigators are required to spend 75 per cent of their time on research, and are offered contracts lasting three, five or seven years. Crucially, the institute believes in supporting people, not projects.

But this is no meal ticket. The clear expectation is that Hughes scientists will deliver consistent, high-calibre results. They are kept under scrutiny by a strict and regular process of peer review. And every year some 12 to 14 per cent of investigators' contracts are not renewed.

Not everyone is impressed. Critics say the institute's policy of picking established high-fliers is safe, unadventurous and undemocratic. No one applies for a Hughes post — instead, they are nominated by their institutions and their work is scrutinised by two outside panels and six in-house scientists, who decide whom to select. Stanley Katz, president of the American Council of Learned Societies in New York, dismisses this method as "the old boys' taste test".

Choppin disagrees. The idea that only established high-fliers are selected is out of date. "There is an attempt to identify the very best people, but this doesn't mean only senior people. We have in recent years had an emphasis on appointing people in the early stages of their careers," he says.

Scientists and policy makers in other research funding bodies do not dispute Choppin's assessment of the ingredients for success-picking the right people, supporting them and reviewing them carefully. But opinions vary widely on the right balance between the three. Controversy focuses on the last two ingredients — the type of financial support a scientist receives, and the way work is evaluated.

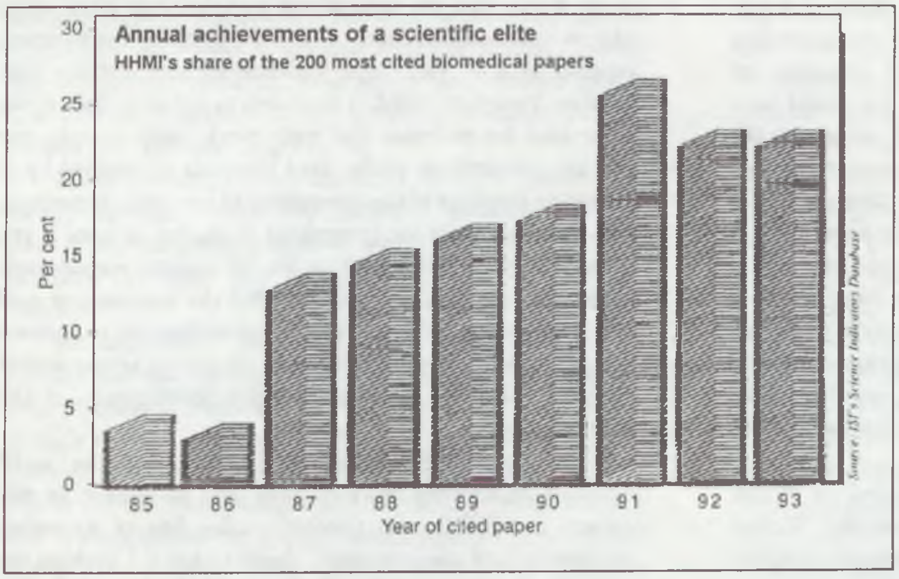
Different organisations use a wide range of funding approaches, from short-term project grants, such as those offered by government agencies, to fellowships of up to 10 years offered by the Royal Society. The Wellcome Trust stresses that it supports careers not projects, while the Imperial Cancer Research Fund in Britain and the Max Planck Society in Germany are similar to Hughes in that they pay scientists salaries. But beyond the traditional complaint that short-term funding ties scientists' hands, no one knows which system works best for which people.

For evaluation, most organisations use some form of peer review. But, surprisingly, science-funding bodies have never developed quantitative measures for monitoring their researchers' performance. Quantitative measures could supplement peer review and help funding bodies to disentangle the relative importance of each of Choppin's ingredients for success. For example, the approach might help to show whether short-term or long-term funding produces more influential results.

The idea of using quantitative measures for assessing scientific performance horrifies some researchers — and some funding bodies. But science is an open-ended enterprise and given that funds are limited, most accept that achievements must be assessed in some quantitative way.

One method for gaining a rough quantitative estimate of how money translates into knowledge is to track the funding bodies acknowledged in published papers. In the case of biomedicine, this source of information has gone virtually untapped.

But, in Britain, a new initiative is set to change this. The Wellcome Trust, whose research budget of around £160 million, close to the $268 million (£175 million) spent by the HHMI, is setting up a new tool called ROD, the research outcomes database. It will allow funding bodies to see what their grant holders or employees have produced, and it will allow policy makers to see which bodies are supporting particular fields of science.



ROD will record the funding bodies acknowledged on all biomedical papers published by researchers from Britain and Ireland. It will distinguish different types of financial support, such as project grants and grants to investigators. Many other organisations, including the MRC and the Cancer Research Campaign, are participating in the database, which is expected to be running by the end of the year.

Joe Anderson, who runs the unit at the Wellcome Trust that is developing the database, stresses that ROD is not some crude measuring tool to assess the output of individuals. The idea, he says, is to assemble the quantitative information then explore with working scientists and others how to use it. "Once we get the papers, the funding agencies and charities can add them up, weigh them, and do citation analyses for particular [fields]," he says. "But I suspect they will make a more important use of them, which is to read them."

Provocative

In other words, quality is best judged by intelligent review. Back at the Hughes institute, nobody doubts the quality of the work. And the evidence that there is such a pronounced elite in biomedicine raises a provocative question. If so few scientists can produce such a large proportion of influential results, why bother to fund the rest? Put crudely, if 222 scientists can produce a quarter of the key biomedical papers, why not pick the best 888 scientists and throw the world's research money at them?

Most scientists — including Choppin — dismiss this idea immediately, saying it reflects a naive misunderstanding of the way research works. Alec Jeffreys at the University of Leicester, who developed DNA profiling and holds one of the HHMI's international research scholarships, thinks it is equivalent to saying that because the US does most of the world's science, other countries should stop trying.

"Science is a horse race," he says. "There is only one winner, and sometimes the winners can't be spotted. What the Howard Hughes is doing is picking all the winners. But you need the racetrack to get the winners. Without the rest, nobody gets onto the track in the first place." Funding bodies have to take the risk of supporting those who have not yet found their form. Some would say that Jeffreys himself is an example of the scientist who came to success, not from a well-known centre of excellence, but from a more unexpected background.

There are also huge differences between the policies of different research organisations. While the HHMI can choose its investigators, government bodies such as the MRC are obliged to spread their money over areas of research that meet national needs. Michael Kemp, research policy development manager at the MRC, says the council funds research that is essential to public health but not necessarily comparable with the glamorous kind of neuroscience that attracts peak citation rates. Clinical trials, for example, are important but rarely attract high citation rates.

Choppin admits that these demands spread government agencies' money more thinly. He also stresses the need for young scientists to be supported from their earliest years if science is to prosper. For this reason, the institute spends money on science education in schools and this year it recruited a batch of younger scientists as investigators. If the institute maintains its high quality and citation rates with these younger unknowns, observers will know they must take the HHMI's formula seriously.

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Scientists' Competitive Behavior Is Not Peculiar to Our Competitive Age

James Watson fluttered the dovecotes of academia, to say nothing of the wider reading public, by telling us of having joined with Francis Crick in an enthusiastic toast "to the Pauling failure.... Though the odds still appeared against us, Linus had not yet won his Nobel." It would, seem that Watson had violated the mores that govern contest behavior in science and the public disclosure of that behavior.

Yet, how mild and restrained is this episode by comparison with judgments on contemporaries set out in public by great scientists of the heroic past. Although historical facts to the contrary are abundantly available, there emerges a new mythology that treats competitive behavior of scientists as peculiar to our own competitive age.

This introduces an instructive paradox. These, indeed, are changing times in the ethos of science. But Watson's brash memoir does not testify to a breakdown of once-prevailing norms that call for discreet and soft-spoken comment on scientific contemporaries. A memoir such as his would have been regarded as a benign model of disciplined restraint by the turbulent scientific community of the 17th century. That it should have created the stir it did testifies that, with the centuries-long institutionalization of science, the austere mores governing the public demeanor of scientists and the public evaluation of contemporaries have become more exacting rather than less. As a result, Watson's little book, so restrained in substance and so mild in tone by comparison with the caustic and sometimes venomous language of, say, Galileo or Newton, violates the sentiments of the many oriented to these more exacting mores.

All of this brings us to the question touched off by the responses of many scientists and laymen to the Watson memoir. We are perhaps ready to see now that those responses relate to the long-standing denial that through the centuries scientists, and often the greatest among them, have been concerned with achieving and safeguarding their priority. The question is, of course: What leads to this uneasiness about acknowledging the drive for priority in science? Why the curious notion that a thirst for significant originality and for having that originality accredited by competent colleagues is depraved — somewhat like a thirst for, say, bourbon and 7-Up? Or, in Freud's self deprecatory words, that it is an "unworthy and puerile" motive for doing science?

Ambivalence Toward Acclaim

In one aspect, the embarrassed attitude of a Darwin or Freud toward his own interest in priority is based upon the implicit assumption that behavior is actuated by a single motive, which can then be appraised as good or bad, as noble or ignoble. It is assumed that the truly dedicated scientist must be moved only by the concern with advancing knowledge. As a result, deep interest in having one's priority recognized is seen as marring nobility of purpose as a scientist.

There is, moreover a germ of physiological truth in the suspicion enveloping the drive for recognition in science. Any extrinsic reward — fame, money, position — is morally ambiguous and potentially subversive of culturally esteemed values. For as rewards are meted out, they can displace the original motive: Concern with recognition can displace concern with advancing knowledge. An excess of incentives can produce distracting conflict.

In another aspect, the ambivalence toward priority means that scientists reflect in themselves the ambivalence built into the social institution of science itself.

That ambivalence also derives from the mistaken belief that concern with priority must express naked self-interest, that it is altogether self serving. On the surface, the hunger for recognition appears as mere personal vanity, generated from within and craving satisfaction from without. But when we reach deeper into the institutional complex that gives added edge to that hunger, it turns out to be anything but personal, repeated as it is with slight variation by one scientist after another. Vanity, so called, is then seen as the outer face of the inner need for assurance that one's work really matters, that one has measured up to the hard standards maintained by at least some members of the community of scientists. Sometimes, of course, the desire for recognition is stepped up until it gets out of hand. It becomes a driving lust for acclaim; megalomania replaces the comfort of reassurance. But the extreme case need not be taken for the modal one. In providing apt recognition for accomplishment, the institution of science serves several functions, both for scientists and for maintenance of the institution itself.

The community of science provides for the social validation of scientific work through peer assessment. In this respect, it simplifies that famous opening line of Aristotle's *Metaphysics*: "All men by nature desire to know." Perhaps but men of science, by culture, desire to know that what they know is really so.

The organization of science operates as a system of institutionalized vigilance, involving competitive co-operation. It affords both commitment and reward for finding where others have erred or have stopped before tracking down the implications of their results or have passed over in their work what is there to be seen by the fresh eye of another. In such a system, scientists are at the ready to pick apart and appraise each new claim to knowledge. Only after the originality and consequence of their work have been attested by significant others can scientists feel reasonably confident about it. Deeply felt praise for work well done, moreover, exalts donor and recipient alike; it joins them both in symbolizing the common enterprise. That, in part, expresses the character of competitive cooperation in science.

Reassurance By Recognition

The function of *reassurance by* recognition has a dependable basis in the social aspects of knowledge. Few scientists have great certainty about the worth of their work. Even that psychological stalwart, T.H. Huxley, seemingly the acme of selfconfidence, tells in his diary what it meant to him to be elected to the Royal Society at the age of 26, by far the youngest in his cohort. It provided him, above all, with much needed reassurance that he was on the right track; in his own language, "acknowledgment of the value of what" he had done. And since, like the rest of us, Huxley was occasionally inclined to doubt his own capacities and to think himself a fool, he concluded that "the only use of honors is as an antidote to such fits of 'the blue devils:' "

The drive for priority is in part an effort to reassure oneself of a capacity for original thought. Thus, rather than being mutually exclusive, as the new mythology of science would have it, joy in discovery and the quest for recognition by scientific peers are stamped out of the same psychological coin. In their conjoint ways, they both express a basic commitment to the value of advancing knowledge.

But authentic reassurance can be provided only by the scientists whose judgement one in turn respects. As we sociologists like to put it, we each have our reference groups and individuals, whose opinions of our performance matter. Our peers and superiors in the hierarchy of accomplishment become the significant judges for us.

Darwin writing Huxley about the *Origin of Species* "with awful misgivings" thought that "perhaps I had done, and I then fixed in my mind three judges, on whose decision I determined mentally to abide. The judges were Lyell, Hooker, and yourself."

In this, Darwin was replicating the behavior of many another scientist, both before and after him. The astronomer John Flamsteed, before his vendetta with Newton, wrote that "I study not for present applause. Mr. Newton's approbation is more to me than the cry of all the ignorant in the world." In almost the same language, Erwin Schrödinger writes Einstein that "your approval and [Max] Planck's mean more to me than that of half the world." And a Leo Szilard or a Max Delbrück, widely known as exceedingly toughminded and demanding judges who, all uncompromising, will not relax their standards of judgment even to provide momentary comfort to their associates, are reference figures whose plaudits for work accomplished have a multiplier effect, influencing in turn the judgments of many other scientists.

Protecting Others' Priority

Other strategic facts show the inadequacy of treating an interest in recognition of scientific work as merely an expression of egotism. Very often, the discoverers themselves take no part in arguing their claims to the priority or significance of their contributions. Instead, their friends or other more detached scientists see the assignment of priority as a moral issue not to be scanted.

For them the assigning of all credit due is a functional requirement for the institution of science itself. After all, to protect the priority of another is only to act in accord with the norm, which has been gathering force since the time of Francis Bacon, that requires scientists to acknowledge their indebtedness to the antecedent work of others. As Peter Kapitza says of his master, "If anybody in publishing his work forgot to mention that the given idea was not his own, [Ernest] Rutherford immediately objected. He saw to it in every possible way that... true priority be maintained." Or, to take perhaps the most momentous instance in our day, there is Niels Bohr, agitated by the thought that Lise Meitner and Otto Hahn — and, for that matter, Otto Frisch and Fritz Strassmann — might have their priority in the splitting of the atom lost to view in the avalanche of publicity given the Columbia University experiments, going to immense pains to set the record straight (just as he was later to devote himself to the task of getting governments, and physicists too, to consider the human consequences of nuclear weapons).

Erwin Chargaff is correct, I believe, in suggesting that the Watson memoir "may contribute to the much-needed demythologizing of modern science." But as I have tried to suggest, to put the accent on "modern science" is only to displace the old myth with a new variant. In noting this, I am scarcely alone. Some practicing scientists, both before and after *The Double Helix*, have put aside the myth that competition for originality in science is alien to joy in discovery and that the drive for recognition should occasion self-contempt. Hans Selye asks his peers: "Why is everybody so anxious to deny that he works for recognition?... All the scientists I know sufficiently well to judge (and I include myself in this group) are extremely anxious to have their work recognized and approved by others. Is it not below the dignity of an objective scientific mind to permit such a distortion of his true motives? Besides, what is there to be ashamed of?" And, as though he were responding to this rhetorical question, P.B. Medawar goes on to argue: "In my opinion the idea that a scientist ought to be indifferent to matters of priority is simply humbug. Scientists are entitled to be proud of their accomplishments, and what accomplishments can they call 'theirs' except the things they have done or thought of first?

"People who criticize scientists for wanting to enjoy the satisfaction of intellectual ownership are confusing possessiveness with pride of possession."

Robert K. Merton,
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Kezdet és vég — a világmindenség

A fizika legnagyobb érdeklődést vonzó területein kevés a változás: az 1993-as év végének három éllovasa meg tudta őrizni a helyét. A magas hőmérsékletű szupravezetés, valamint a világító szilicium tulajdonságai állnak elsősorban a figyelem középpontjában, ezt a 4., 5. és 7. helyen álló nagy idézettségű munkák is bizonyítják.

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Készült az Argumentum Könyv- és Folyóiratkiadó Kft. nyomdájában

Felelős kiadó: az MTAK főigazgatója