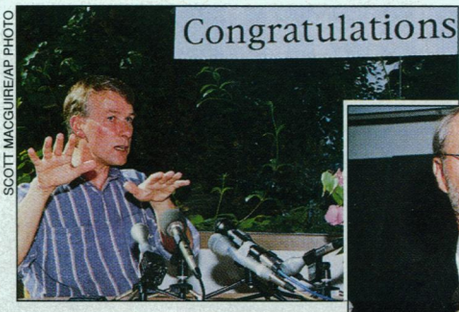
The Culture of Credit

Scientific ideals call for collaboration and sharing. But in today's competitive scientific enterprise, a tremendous premium is placed on individual credit, setting the stage for conflict

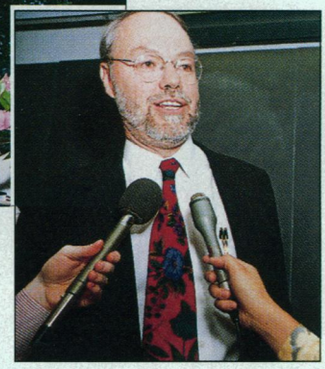
In science, smooth collaboration is the ideal. All scientific work depends on data, interpretations, and materials supplied by others. And all researchers know, whether they've been taught in a class or simply assimilated the relevant principles from those around them, that a scientist is supposed to respect the collaborative nature of the process: Credit is to be shared appropriately; the findings of others—even from competing labs—are to be cited; students are to be treated generously; materials and data are to be shared freely. Somewhere, somehow, every scientist learns those largely unwritten rules.

Then there's reality. In science, as in so many other professions, the coin of the realm is not collaborative generosity but credit—credit for individuals. One reason is that scientists need acknowledgment for the endless hours in the lab and for their own creativity. "Credit is a bottomless pit—there's never enough for most people," says Harvard University geneticist Philip Leder.

But ego isn't the only reason credit is crucial in science. At a time when budgets are tightening like vises and the number of bright competitors seems to grow exponentially, credit for discoveries can make the difference between treading water and sinking in a scientific career. With credit from one's peers comes access to all-important grant funding, easier publication in leading journals, and a steady supply of the grad students and postdocs who make the lab run. As James Darnell Jr. of Rockefeller University puts it, credit is "coming to matter more and more as we get deeper into this insanely competitive phase of science."



SCOTT MACGUIRE/AP PHOTO



JULIA MALAKIE/AP PHOTO

The final cut. The 1993 Nobel Prize awarded to Richard Roberts *(above)* and Phillip Sharp *(right)* for the discovery of gene splicing reopened old wounds among some of their former collaborators.

Inevitably this drive for credit can clash with the ethic of collaboration. During the past 3 months, *Science* has investigated how the culture of credit operates in the collaborative scientific enterprise. Our investigation shows that the system often functions smoothly. And our investigation has identified researchers who are regarded by their peers as paragons of collaboration. Yet our probe has also turned up cases in which the drive for individual credit—stoked by prize committees and the media—overshadows the collaborative nature of the scientific process, distorting the record and leaving researchers feeling bruised.

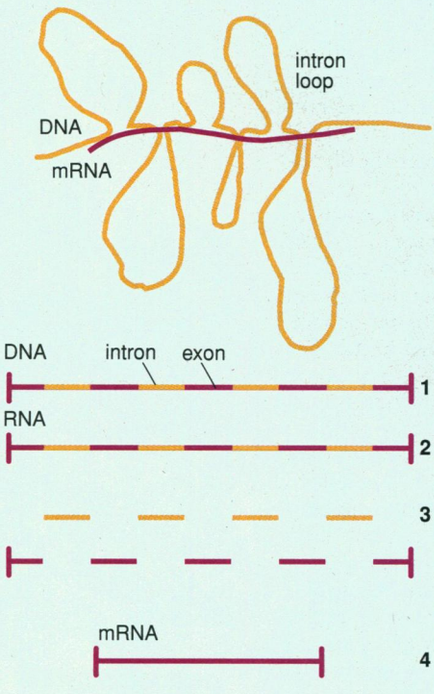
Four's a crowd

One situation that left many researchers feeling shortchanged was the allocation of credit for the key discovery of gene splicing. The allocation of credit culminated in the awarding of the 1993 Nobel Prize in physiology or medicine, which went to Richard Roberts, formerly of New York's Cold Spring Harbor Laboratory (CSHL), and Phillip Sharp of the Massachusetts Institute of Technology (MIT). But the Nobel Prize for Roberts and Sharp was only the final act in a 16-year drama in which credit was narrowed down from a sprawling enterprise involving more than a dozen researchers to just two individuals.

"Ninety percent of the people at Cold Spring Harbor didn't get the credit they deserved," says John Hassell, who was part of the CSHL group. Daniel Klessig, another key contributor at CSHL, adds that "a lot of the wounds have healed with time, but, with a lot of people, the scabs were pulled back off" by the Nobel Prize. Part of the responsibility for that, of course, must be laid at the door of the Nobel Prize rules, which allow no more than three winners. Yet the winnowing of credit for this dramatic discovery had begun long before the Nobel Prize.

In 1976, Klessig, then a graduate student under CSHL Director James Watson, was working with Roberts and postdoc Richard Gelinas on a perplexing problem. At the time, the consensus was that genetic information is continuous: The uninterrupted strip of DNA in a gene is transcribed into messenger RNA (mRNA), which is used to make proteins. According to that view, all mRNAs should be different, because they come from different places in the genome and code for unique proteins.

But Roberts, Klessig, and Gelinas had some puzzling evidence suggesting that different mRNAs from a virus called adenovirus all began with the same sequence of 11 nucleotides. When they mixed the mRNAs with adenovirus DNA, they got an even bigger shock. When mRNAs and DNA are mixed, the mRNAs "hybridize," or bind, to their points of origin on the DNA; the adenovirus mRNAs all did just that—except for the 11-nucleotide portion, or 11-mer, which didn't bind to the corresponding DNA.



SOURCE: ALBERTS *ET AL. MOLECULAR BIOLOGY OF THE CELL* (GARLAND PUBLISHING INC., ED. 3, 1993)

Cut and paste. A gene is a segment of DNA. Many genes contain regions called exons, which carry the code for proteins, interspersed with introns, which do not carry genetic code (1). After a gene is "transcribed" into RNA (2), the exons are snipped out by enzymes (3). The exons are stitched together by other enzymes to yield a messenger RNA (mRNA), which is used to make a protein (4). Among the evidence for gene splicing are images in which loops of DNA fail to bind to an mRNA *(above)*.

Indiana: Wrong Answers—But No Right Ones

For 2 years, cell biologist Alvin Tesler helped run a course on research ethics for second-year graduate students in biology at the Chicago campus of Northwestern University that Tesler admits was lackluster. Researchers and administrators delivered lectures on the usual topics—authorship, intellectual property, conflict of interest, and the like. Not surprisingly, attendance hovered around 50%. This year, however, Tesler reports that attendance is up to at least 90%.

KEVIN KLEINE

Yes, you can! Indiana's Ken Pimple says his course, Teaching Research Ethics, aims to convince scientists in a week that they can teach ethics themselves.

He credits the turnaround to a seminar called Teaching Research Ethics he attended at Indiana University's (IU's) Poynter Center for the Study of Ethics and American Institutions. Tesler says the seminar exposed him "to different ways of teaching research ethics and different ways of thinking about it." When he returned to Chicago, he helped reshape the Northwestern research ethics course into one aimed at promoting the lively discussion of case studies.

Teaching Research Ethics, or TRE, is a week-long workshop for graduate science faculty at Big Ten schools, as well as the University of Chicago. It's led by the Poynter Center's Ken Pimple, who has a Ph.D. in anthropology, and David Smith, a professor in IU's religion department and director of the center. The motivation for TRE, says Pimple, is simple: Although the National Institutes of Health mandated in 1989 that institutions receiving NIH training grants had to provide instruction in research ethics to their young researchers, that did not necessarily mean that the scientists themselves would be prepared to teach courses in the subject. The goal of TRE, says Pimple, is to "convince scientists by the end of a week that they have enough training and life experiences as scientists that they can teach ethics, that it doesn't have to be taught by a philosopher, and then to convince them that it's worth their while to teach it."

The course runs for 5 days in May and includes 30 participants nominated by research deans of the participating universities. The deans are requested to look for respected research scientists who are fairly well established (although not necessarily tenured), credible in their departments, and able to act as leaders to disseminate what they learn.

The course weaves practical sessions on specific ethical issues in research—the use of human and animal subjects, for instance—with pedagogical sessions on the nitty-gritty of teaching ethics. This year Pimple is doing four sessions on teaching, including the use of short writing assignments and small group assignments; one on a software package he helped design that creates interactive case studies; and one he calls "Our Favorite Teaching Techniques," an open forum to allow the participants to discuss their own ideas and techniques. Special attention is given to teaching the participants how to assess student learning in ethics, with a series of lectures by Murial Bebeau of the Center for the Study of Ethical Development at the University of Minnesota.

Pimple and Smith also bring in outside researchers to lecture on issues in scientific conduct, including scientists who have designed well-regarded research ethics courses, to give the TRE participants "some models to work from," says Smith. For instance, Michael Zigmond of the University of Pittsburgh does a lecture on his ethics curriculum (see p. 1709). These researchers convey an important message just in being there, says Pimple: that research ethics should not be taught only by ethicists or philosophers.

What TRE does not try to do is teach the scientists specific rules and regulations or rights and wrongs in issues such as authorship, intellectual property, or misconduct. Instead, they provide a list of professional societies that have codes of ethics they can use. "Our assumption," says Pimple, "is that these are fairly good and responsible people, and they need a little bit of help getting started. ... What we try to do is show them that they can communicate to their students that there may be no absolute right answers, but there are certainly wrong answers—ones that are universally, indisputably accepted as wrong."

–Gary Taubes

The perplexity was deepened by data from Sayeeda Zain, also in Roberts' lab, who did not find the 11-mer on the adenovirus DNA where the mRNA predicted it should have been. Two other CSHL researchers, Ashley Dunn and John Hassell, had also found that fragments of adenovirus DNA from one region of the viral genome hybridized to mRNA that seem to come from a different region. Additional puzzling data were contributed by CSHL's James Lewis, Carl Anderson, and John Atkins.

All of these findings contradicted the dogma that genes are continuous, with each mRNA corresponding to one smooth strip of DNA. While paradoxes were piling up at CSHL, Sharp's lab at MIT was focusing on a similar mind-teaser unearthed by postdoc Susan Berget. Berget had been making electron micrographs (EMs) of adenovirus DNA hybridized to mRNA and had found pieces of mRNA inexplicably flapping at one end, unhybridized to any DNA.

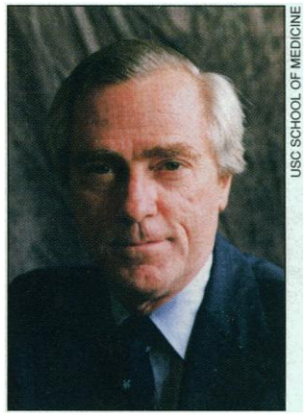
In March 1977, CSHL was buzzing with the rumor that Sharp's lab had made an important discovery related to adenovirus genes. Roberts believed it might be related to the puzzle of the 11-nucleotide sequence that wasn't hybridizing where he expected it to on the DNA. Spurred by his fear that he was about to be scooped by Sharp, Roberts hit on an EM experiment he hoped would explain the origin of the baffling 11-mer. Fortunately, CSHL's Louise Chow and her husband Tom Broker were world-class EM researchers who were very experienced in determining the location of adenovirus mRNAs on the DNA. Roberts gave his idea to the wife-husband team, who refined the concept, worked out the details of the experiment, and carried it out during the next few weeks.

The EMs revealed that Roberts' main hypothesis of how the 11-mer originated was wrong. But he wasn't disappointed, because the experiment was so fruitful: It confirmed that the beginning of the mRNA thought to contain the 11-nucleotide segment bound to a different part of the DNA from the rest of the mRNA. Even more startling, the DNA bound to this segment of mRNA had two loops of DNA extending outward, indicating that those portions of the DNA did not correspond to anything in the mRNA.

Somehow, the CSHL team concluded,

contrary to the orthodox beliefs, the genetic information in the DNA is not necessarily continuous: Large sections of the DNA in some genes—segments now known as introns—are snipped out as the mRNA is made (see diagram on p. 1706). At about the same time, Sharp and Berget (who had to convince Sharp that her EMs were not artifacts) came to the same conclusion.

In the world of molecular biology the discovery that introns are spliced out of certain genes before proteins are made was earth-shattering. And in the small world of the discoverers, the allocation of credit provoked just as large an upheaval. Even as the initial papers from four CSHL groups were being prepared for publication, credit began to be narrowed from the large group that had made scientific contributions. The first person to feel the squeeze was Zain—whose biochemical work showing that DNA was not simply translated into mRNA went unmentioned in the four papers submitted to *Cell* by 10 CSHL researchers. "I felt miserable," says Zain, who now studies tumor metastasis at the University of Rochester Medical Center in New York.



USC SCHOOL OF MEDICINE

Taking heat. French Anderson has taken criticism from colleagues in basic research who say the credit he gets for gene therapy is out of proportion to his scientific role.

A second narrowing occurred when it came time to present the data at meetings. "You quickly found out that there were certain people who were much more able to go around the world and talk about the discovery," says Klessig, now a plant researcher at the Waksman Institute of Rutgers University.

Although the winners' circle was already narrowing, at CSHL it was difficult to single out one individual to reap the rewards. Many of the principal CSHL players were postdocs or even grad students. In addition, none of the three senior figures—Broker, Chow, and Roberts—had been in charge of all the relevant work. "Rich Roberts was arguably the most senior figure and the one who had the broadest view, but in no sense was he directing what was going on in the labs," says CSHL's Michael Mathews, who was there at the time.

At MIT, Sharp was obviously the director of the research, but allocating credit was still a contentious issue. Berget acknowledges that the distribution of credit was not a perfectly smooth process, but says "Phil and I have made peace, and I would prefer not to discuss old issues." Yet she concedes that if she had it to do over, she would be "a lot more aggressive" in getting credit for her role in the discovery.

Sharp admits that when it came to presenting the work publicly, "it was mostly me." But he says Berget "presented it widely" and that he never tried to diminish her role. "It was fundamentally her postdoctoral work," says Sharp. "But I had been working on the problem for many years when Sue came to my lab."

Despite these early credit maneuvers, it wasn't until the Nobel Committee made its decision that only two were left: Roberts and Sharp. And that ultimate credit cut hit Chow and Broker, now at the University of Alabama, Birmingham, the hardest. Both believe Chow deserved to share in the prize. Several of their colleagues did, too, as became apparent in a *Boston Globe* article that appeared shortly after the 1993 award—an article that, by focusing on Chow's exclusion, helped to sustain the impression that credit for the discovery could be narrowed to a few individuals.

The article detailed a Nobel "lobbying effort" organized by CSHL Director Watson on Roberts' behalf. Central to the effort was a 15-page history Roberts says he wrote at Watson's behest that Watson distributed to people who might have contact with the Nobel Committee. This 15-page history was a painful eye-opener to Chow and Broker. The statements in the document, Chow and Broker wrote in a critique they provided to *Science*, "emphasize [Roberts'] efforts to diminish the contributions of Chow and Broker and the role of the EM mapping, despite the obvious fact that *EM was the one technique which came up with the correct definition of the phenomenon, and allowed the biochemistry to be reinterpreted* [italics in original]."

> "CREDIT IS A BOTTOMLESS PIT—THERE'S NEVER ENOUGH FOR MOST PEOPLE."
>
> —PHILIP LEDER

Watson, along with several other scientists interviewed by *Science*, now says Chow should have shared the prize. "Louise did it, and it's terrible that she didn't win," says Watson. The problem at the time, says Watson, is that Berget was also deserving, and including her would have made four—one too many for the Nobel rules. "There was a discussion of giving the prize to the Cold Spring Harbor Lab, and that would have been infinitely more appropriate," Watson says.

Roberts, who now works at New England Biolabs in Beverly, Massachusetts, disagrees. The key experiment, he says, was his idea, stemming from work he and Gelinas had been pursuing independently of Chow. In fact, Roberts argues that his mistake was in being too generous with credit by pushing for the package of four *Cell* papers from CSHL. "It seemed the appropriate thing to do was to get as many people involved with the story, and they could all get credit," says Roberts. Later, he says, he "felt so let down by so many people," because "they were all there to grasp all the credit for themselves."

Hassell, now a researcher at McMaster University in Ontario, Canada, calls this view "total nonsense." "We were all fighting to make sure no one got more credit than anyone else," says Hassell, who stresses that he does not begrudge Roberts the Nobel. "The *Cell* papers came out together for a good reason, and it wasn't because Rich Roberts was magnanimous."

Doctor's dilemma

In the gene-splicing story, the hot-button issues, at least initially, had everything to do with credit among peers and little to do with credit in the public eye. But in other scientific fields, the scramble for public recognition is a stumbling block from the very beginning. Consider the nascent field of gene therapy, where many basic researchers are bewildered that three clinicians—W. French Anderson, R. Michael Blaese, and Steven Rosenberg—are portrayed in the media as "pioneers" of the technique. And it doesn't salve those feelings that the trio has won a broad patent naming them alone as inventors of gene therapy, a field many researchers feel they have had a hand in developing.

In interviews with *Science*, insiders in the field stress that the path to the landmark human trial of gene therapy by Blaese and Anderson in 1990 was a long, winding road, with most of the crucial steps being taken by other researchers. Those insiders told *Science* much of the scientific credit for the field should go to pioneers such as Richard Mulligan of MIT, Dusty Miller of the Fred Hutchinson Cancer Research Center in Seattle, and Eli Gilboa of Duke University. In the early 1980s, these pioneers and their colleagues in basic research created the "vectors," or transfer vehicles, for gene therapy by figuring out how to scoop out the innards of a harmless virus and replace them with a foreign gene. That core group and others also worked to develop critical cell lines—"packaging cells"—in which the viral vectors could grow.

Anderson, who has been a proponent of the concept of gene therapy since the 1960s, was with the National Heart, Lung, and Blood Institute in 1984, the year he hooked up with

Pittsburgh: Interwoven With the Fabric of Learning

LINDA M. GOLDER

Ethical core. Neuroscientist Michael Zigmond is working to integrate ethics into the core life sciences curriculum.

Few researchers who take on a second career in pedagogy get the rave reviews received by Michael Zigmond of the University of Pittsburgh. Zigmond's course on Survival Skills in universities has already become famous (*Science*, 4 November 1994, p. 872), and now his ethics curriculum is wowing researchers as well. Zigmond, a neuroscientist at Pittsburgh since 1970, got into the ethics business 3 years ago, prompted by the National Institutes of Health mandate. Since then, he has attacked it with the single-minded resolution and creativity most researchers reserve for scientific problems. Or as one biologist who has heard Zigmond's presentation on his curriculum put it, "Zigmond is unbelievable."

Zigmond started with what he calls "more or less what everybody else is doing"—six sessions of 90 minutes, each concentrating on a specific topic, and a few case studies for discussion. At first, business boomed: Students had to be turned away from the first few classes. Then, he says, "attendance started to wane. I attributed that in part to the fact that it didn't seem terribly relevant. It was more of an intellectual exercise than anything else."

So Zigmond set about making research ethics "more real." First, he decided that "anything that's going to make sense for our students had to involve the active participation of someone like what they wanted to be—i.e., a bench scientist." And second, "if what we're teaching people is of essential importance, then it shouldn't be separated out from the rest of what we're teaching them."

Zigmond and Beth Fischer, who helps coordinate the program, went about integrating ethics into the curriculum of the Survival Skills workshop, which is open to the entire university community and consists of eight 1-day workshops covering everything from how to write a paper and give a seminar to how to get and keep a job. "Throughout each of these workshops," he says, "we deal with the ethical dimensions of each activity. We talk about plagiarism when we talk about writing, and we talk about misleading graphics when we talk about giving a talk. We talk about intellectual property and who owns the data when we talk about getting a job—[when you leave] what goes with you and what stays behind in your old laboratory." These workshops now include a lunch in which faculty members and students discuss fictitious case studies created by Zigmond.

Part two of Zigmond's master plan is to integrate ethical issues into the core curriculum in life sciences, something he is doing in his own neuroscience department and hopes will spread to others. Each of the directors of the core courses, he says, agreed to spend at least two 1-hour periods per term discussing an ethics topic relevant to the course subject. For example, he says, "the use of animals in research is something we would talk about in a course with a lot of data generated from animal experiments. Informed consent is something we talk about in a clinical neuroscience course. Ethical dimensions of gene therapy or genetic counseling or university-biotech relations are all issues that can come up in a molecular biology course."

The students seem to relish the idea, says Pat Card, who teaches the systems neuroscience course, if for no other reason than that ethics now provides 90 minutes of "a quite different digression from the basic information they're receiving" in these core courses. Although Card had never taught ethics before and hadn't studied it since his undergraduate days, he says he had no trouble keeping students interested for the full 90 minutes, and they "could have gone a lot longer had we had the time."

Zigmond has one other goal for his emerging program: a comprehensive exam on research ethics in which students demonstrate proficiency. "It's a way of telling them," he says, "that this is very important. Because in the end everything important we have an exam for. The way to tell students it's not important is to have a beer and pizza discussion at night—and that seems to me what ethics typically is."

–G.T.

Gilboa. The same year, Anderson stepped up his effort to test gene therapy in humans by beginning a collaboration with Blaese of the National Cancer Institute (NCI). Blaese, a pediatric immunologist, could help Anderson advance the technology Gilboa and others were developing and take it into the clinic against adenosine deaminase (ADA) deficiency, a rare but devastating genetic illness causing AIDS-like symptoms in children.

In 1988, Anderson and Blaese began collaborating with NCI's Rosenberg on a potential cancer gene therapy. The two proposals survived a punishing series of reviews by regulatory bodies. To meet those reviews, the National Institutes of Health (NIH) team switched from Gilboa's vectors and packaging cells to those developed by Miller. With the changes, they won approval; the ADA trial began in September 1990 and the cancer trial the next January.

The media came out in droves, anointing Anderson in particular as the father of gene therapy. Anderson also received the harshest criticism, much of it from research colleagues who argued that the trials were premature and that the main motivation for them was credit. "The only urgency is competition of labs," pediatrician Stuart Orkin of Harvard Medical School was quoted as saying in the *Los Angeles Times* in 1987. A week after the first child began receiving gene therapy for ADA, Columbia University hematologist and geneticist Arthur Bank told an international genetics conference that the main motivation for the trial "is the need for French Anderson to do gene therapy in man."

With NIH promoting his work, Anderson's star rose; in 1991 he was featured in both the *New York Times* and the *Washington Post* magazines. His celebrity rankled colleagues who felt that the basic researchers responsible for the system Anderson used had received pitifully little credit. Says one basic researcher in the field who insisted on anonymity: "His contributions have been organizational, not scientific. Other people are burning away trying to solve [the scientific] issues, and French is out there talking about it."

Anger from the research community was also directed at NIH for heavily promoting the gene-therapy trials. "A lot of good people had left, and this was the one thing they could trump up," says a gene-therapy researcher who also requested anonymity.

In the end, gene-therapy credit issues upset not just competitors of the NIH trio but also their collaborators. That became apparent in March, when Miller and Kenneth Culver, a former postdoc in Blaese's lab who played a central role in the ADA trial, were outraged to learn that a patent on the basic

technique in gene therapy issued to NIH listed the trio as the only inventors (*Science*, 31 March, p. 1899).

For many researchers, the most disconcerting behavior of the team that did the much-ballyhooed ADA trial is that they have published only scant results—although they continue to "keep their image up on the circuit," as one critic puts it, by discussing their "success" at meetings. "The thing I resent the most is how little has been published," says Stanford University's Paul Berg, who won a Nobel Prize in 1980 for his recombinant DNA work. "There's a lot of talk. You'd like to see the publication to say whether the outcome is valid or not. ... The major fault is there are people who think we are much further along than we are."

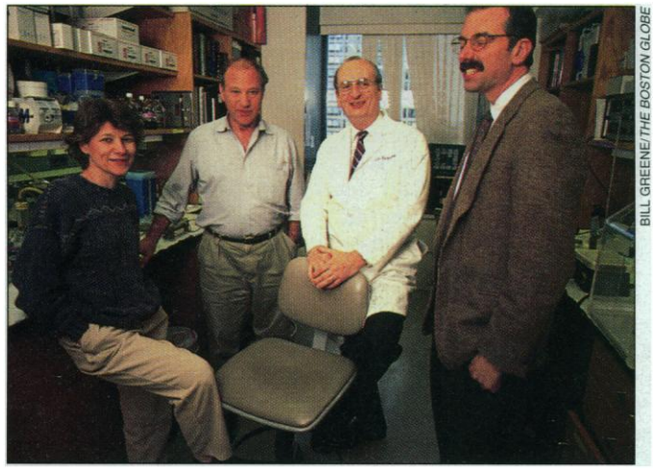
Blaese, Rosenberg, and Anderson all say that, in contrast to what the critics say, they moved ahead cautiously in carrying out the gene-therapy trials and that the primary motivation was human health—not credit. "I wasn't all that concerned about credit," says Blaese, who has moved from NCI to the National Center for Human Genome Research. Blaese takes responsibility for the delay in publishing, but says the motivation was scientific: "I felt it was important to get a relatively comprehensive view of what was happening." A manuscript is now about to be submitted, he adds. As for the patent, Blaese says he does not understand the law well enough to decide who merits being on it. Rosenberg adds, "I never have fewer than 10 patients in the hospital, all of whom are dying of cancer. My concern is finding things that can help those patients."

Anderson, now at the University of Southern California, agrees that even though many contribute to developing any field, credit is inevitably assigned to a few: "A lot of people put in a lot of very hard work, but in the end, one or two or three people get recognized." Anderson also stresses that his motive is helping mortally ill patients, not getting credit. But Anderson concedes that "we've all got a black part to our hearts—all of us are competitive." Indeed, says Anderson, "you can't battle for funding and publications for years and not be."

Folkman wisdom

While some researchers are quite comfortable on the covers of magazines, others have to be dragged into the limelight. On 30 April of this year the *Boston Globe* Sunday magazine ran a cover story on cancer researcher Judah Folkman, head of a large group of independent labs at Boston's Children's Hospital, a part of the Harvard Medical School. In the middle of the laudatory piece was an unusual aside: "It was only after arm-twisting by hospital officials that Folkman agreed to be interviewed and photographed for this story, according to people in his lab."

Folkman, a leader in angiogenesis (the sprouting of new capillaries that form blood vessels, a subject important in wound healing and cancer), is praised by his peers for trying to live up to the collaborative ethic in science. Experimental pathologist Cecil Fox, head of Molecular Histology Labs in Gaithersburg, Maryland, who worked at NIH for 20 years, has high praise for Folkman. "I'm not given to extreme statements about the nobility of my fellow man, but Judah Folkman is an all-time generous guy," says Fox. "He's the kind of creative scientist everyone should strive to be, but he's also a successful human being."



BILL GREENE/*THE BOSTON GLOBE*

Reluctant role model. Colleagues of Judah Folkman *(in white)* say he had to be persuaded to be in this *Boston Globe* photograph.

Researchers who work in the same department as Folkman agree. "What marks Judah Folkman is his extreme attention to fairness," says Bruce Zetter, a cell biologist who has worked with Folkman since 1977. "And fairness includes extending credit broadly when it's due—or even just perceived to be due." Zetter, who trained in Folkman's lab, describes his mentor as extremely scrupulous in the days when they published together: "He made it clear that he only wanted his name on manuscripts where he actually contributed."

Donald Ingber, a cell biologist at Children's who collaborates with Folkman, agrees Folkman is a master at allocating credit. "Obviously no one's perfect, but on the bell curve, he's way out on the side," says Ingber. "When he presents lectures, he'll even put slides up with a picture of each of us."

Folkman traces his ethos of credit sharing to his days in the lab of Robert Gross, a renowned Harvard pediatric heart surgeon, where Folkman developed a pacemaker for dogs. Folkman recalls that when it came time to publish the pacemaker work, Gross said "It's just going to be Folkman on the paper. They'll never know who you are otherwise. They'll think you're my technician. ... You can pay me back and do it for your students."

Those who work with Folkman say he takes this debt seriously. And he doesn't leave repayment to how he feels on a given day in the lab. On the contrary, he's developed a system for tracking who generates ideas: He fills notebooks, marking who had what ideas during the day and plugging them into his computer at night. Although the system isn't perfect—"We all have our own selective memory," says one co-worker—the attempt is appreciated.

Folkman says fair distribution of credit is vital because science batters the ego. "When you do research, most things are a failure," he says. "When young scientists get credit, it's terrific for their self-confidence."

Family values

A Judah Folkman can sometimes set the tone for an entire academic department. And occasionally one scientist can even set the tone for an entire field. Take the study of the small roundworm *Caenorhabditis elegans* as a model for developmental biology, a field started from scratch by Sydney Brenner of the University of Cambridge 3 decades ago. Today, the worm community prides itself on the free sharing of information, materials, and credit. Unpublished data are often printed in the *Worm Breeder's Gazette*, the community's newsletter. Reagents are frequently available before formal publication; researchers go out of their way to avoid working in the same area as their colleagues. "It started as a small family," says Brenner. "Nobody wrote any rules down, but once [the sharing ethic] got started, it worked out that way."

Although this ethic is now widespread in the small field, many nematode researchers point to one person—Brenner protégé John Sulston—as the Johnny Appleseed who planted the ethic of cooperation wherever he went. "John Sulston has been instrumental in setting the tone," says developmental biologist Judith Kimble, a Howard Hughes Medical Institute investigator at the University of Wisconsin, Madison. That tone, says Kimble, is not ego gratification, but a "spirit that the major goal is that science go forward as rapidly as possible."

Kimble should know, because her Ph.D. thesis was built on a technique Sulston developed and then gave away, encouraging Kimble's graduate adviser to exploit it for his own purposes. "It was always a major concern

to him that people share their results and reagents with each other," she says, and he has been "terrific over the years in giving credit to the people who work with him."

Paul Sternberg of the California Institute of Technology, another Hughes investigator who works on nematodes, also emphasizes Sulston's influence. "Everybody knows most of the time what others are doing—that's the ethic," says Sternberg. "We're doing the work not just for our ego's sake. You being first isn't the only motivation. You really are doing this to advance human knowledge."

Sulston's influence hasn't been felt only in isolated acts of generosity. With Cambridge's Alan Coulson and Robert Waterston of Washington University in St. Louis, he organized the nematode genome project, building in principles of cooperation. Kimble notes that one of Sulston's major concerns was that the database be open. "He set up a beautiful series of rules to ensure that nobody would have access to information before anyone else," says Kimble. "This was clearly his ethic, and he thought very hard about how to instill that ethic into the nematode community as a whole."

Yet as the worm field expands, credit conflicts are not as rare as they once were, especially among younger researchers. "The field is not free of credit issues," says one young researcher. This researcher suggests the pattern in nematode biology results partly from the fact that in the British system funds are often distributed by research institutions rather than through competitive grant applications: "If they had to write grants, they'd do things differently."

There's little chance the competitive grant system in the United States will change. Indeed, most researchers argue that the benefits of that system in producing excellence are so great that no thought should be given to changing it. It's also unrealistic to think the culture of credit will disappear; nor will the collaborative ideals of science change. In one form or another, they will continue to clash, and researchers will have to struggle to keep those collisions from tearing communities apart.

–Jon Cohen

Stanford: Bringing In the Big Guns

"It is the editor's everyday experience that unethical or improper behavior occurs all the way down from the most senior to the most junior researchers," says Drummond Rennie, an editor of the *Journal of the American Medical Association*. "The only difference is that when it's the most senior people doing it, that's what's being emulated." For this reason, says Rennie, any effort to teach ethical behavior that doesn't include the most senior people "may be doomed."

Once a year, Rennie, who is also a professor of medicine at the University of California, San Francisco, drives 45 minutes south to the Stanford University School of Medicine, where he gives the introductory lecture in a course entitled The Responsible Conduct of Research. Rennie is one of four of the most senior people in their fields who give the keynote lectures in the course. The others are David Botstein, world-famous geneticist and a former vice president of Genentech, teaching on conflicts of interest at the academic-commercial interface; Ernle Young, Stanford professor of biomedical ethics, on ethical theories and the Stanford guidelines on human subject research; and Donald Kennedy, former president of Stanford and a professor of biology, on issues of authorship, intellectual property, and peer review.

STANFORD UNIVERSITY

Lecture circuit. Immunologist James Maguire initiated Stanford's course in research conduct, which features eminent guests.

Kennedy says his experience as Stanford's president from 1980 through 1992 persuaded him of the need for the course. "Remember," he says, "I spent all those years as the last stop on the appellate chain for all the grievances that arise between faculty members and students on matters of responsible conduct. That's enough to convince anyone that there's a need."

The course is organized by Young and James Maguire, an immunologist and associate dean at the medical school, who initiated it in 1992. Stanford administrators, says Maguire, did not suffer from the delusion that a course in research ethics would stop the kind of person who might be tempted to "totally falsify data," because "those people will never listen," he says. Instead, the goal was to centralize discussions of ethics at Stanford and concentrate on "ethical issues of day-to-day living within major universities doing research and patient care." After the 1989 National Institutes of Health mandate, Stanford made Maguire and Young's course a requirement for every postdoctoral fellow on a training grant. The following year, it spread to all postdocs, and this year, to all trainees in biological sciences.

The Stanford course meets for an hour a week for 7 weeks, then intersperses what Maguire calls the "didactic" lectures of Rennie, Botstein, and company with breakout sessions. In the breakouts, students meet in groups of 15, led by principal investigators in the department. Each session begins with a loose forum discussing the subject of the previous keynote lecture, then moves on to discussions and even play-acting of cases.

"It's funny," says Maguire; "as we've expanded the number of people taking the course, the number of principal investigators wanting to be involved has greatly expanded as well. A lot of P.I.s were doing their own mini–ethics courses in their labs. Now that they see this really works, they're more than happy to teach in it." The sessions cover everything from the analysis of well-publicized cases of misconduct to ethical issues in clinical trials.

But what really sets this course apart is the presence of the big guns. And the motivation for having them there is twofold, says Maguire. On the one hand, they're the most knowledgeable people in the field: They have written extensively on the subjects and can bring a strong sense of reality to what could otherwise be a dry topic. Then, says Maguire, when you have a course with some of the biggest names on campus giving the lectures, you'll get good attendance. "It's probably no different than having [John Kenneth] Galbraith talk at Harvard," he says. "People will come."

–G.T.