

Gray's experimental procedures. Copperplate engraving from a 1744 text by J. G. Döppelmayr.

Why did Gray not discover the electric shock? Dufay, as he himself points out, used an identical experimental setup. Perhaps the explanation is that Gray, as the excellent scientist he was, had developed a "standardized" experimental procedure. He electrified his object and measured the degree of electrification by means of the leaf-brass. He must simply always have kept the glass tube between himself and the experimental subject. Bearing this in mind, it is plausible that an assistant could have discovered the phenomenon by accident as he/she adjusted the leaf-brass stand while Gray held the glass tube.

However, if Williams really discovered the electric shock while working with Gray, why did he not publish it? Was he unwilling to acknowledge his assistant's insight before it was too late? Gray was said to have a difficult character. His close acquaintance Desaguliers wrote of him: "I was unwilling to interfere with the late Mr. Stephen Gray, who had wholly turn'd his Thoughts that way; but was of a Temper to give it intirely over, if he imagin'd, that any thing was done in Opposition to him" (14, pp. 186–187).

An alternative explanation is that Anna Williams was simply a fraud; she may have observed "the electrical spark," but not until after Dufay, or she may not have been truly aware of its importance until after she had read his paper. If this is the case, she must be one of the earliest examples of a woman trying to "steal" scientific honor.

Perhaps the real truth lies somewhere in between. From the following passage in Boswell, one might infer a certain tendency in Williams to overestimate her own achievements: "[On the death of Stephen Grey] appeared to me to be undoubtedly

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Johnson's. I asked Mrs. Williams whether it was not his. 'Sir (said she, with some warmth,) I wrote that poem before I had the honour of Dr. Johnson's acquaintance.' ... I mentioned it to Johnson... His answer was, 'It is true, Sir, that she wrote it before she was acquainted with me; but she has not told you that I wrote it all over again, except two lines'" (15, p. 26).

The full truth about Anna Williams's contributions to science will probably never be revealed, but—rightfully or not—it was important for her to let the world know that the "emission of the electrical spark from a human body" was her discovery.

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## The Paucity of Grants Among Young Scientists

A RECENT NEWS FOCUS ARTICLE ("NIH grantees: where have all the young ones gone?", E. Goldman and E. Marshall, 4 Oct., p. 40) and Editorial ("Getting older," D. Kennedy, 11 Oct., p. 323) document and decry the dramatic decline in NIH grant support to scientists below the age of 35 during the past two decades. Let me raise some anecdotal evidence for another concurrent phenomenon that may have contributed to the problem.

I believe that some academic research groups in the top 20 U.S. research universities have grown disproportionately during the

past 20 years. In my own discipline, chemistry, in the 1970s, a research group of 20 under a single P.I. would have been considered very large. Now plenty of "superstars" are the sole P.I.'s for groups ranging from 35 to 50 graduate students and postdocs. Universities and grant-giving institutions have tolerated or even promoted this tendency, while ignoring the heavy associated nonfinancial penalty. Converting this personal impression into hard figures could be accomplished quickly with little cost by requiring the top 20 research universities to determine the current size of the largest five research groups (responsible to a single P.I.) in each department of relevance to the NIH. If my impression is substantiated, then imposing an upper limit of 20 to 25 members per single P.I. might liberate several million dollars annually. To provide a true incentive, allow the "savings" to remain within the university by diverting them exclusively to peer-approved grant applications by young faculty members or for initial start-up funds for new junior faculty members.

The benefit of such a step goes beyond the financial "spread the wealth" factor to the impact it would have on the conduct of academic research in those universities that represent the pool from which the future superstars are generally drawn. Every P.I. will testify that the raising of funds, the writing of grant proposals, accounting requirements, and the myriad new bureaucratic burdens of the past 20 years have eaten into the productive time of senior investigators. Add to this the time dedicated by these superstars to increasing involvement with industry; the time demands of the many outside lectures, consultations, and travels in addition to the standard teaching and committee requirements of the university; and 5 to 6 hours of daily sleep and perhaps half a Sunday for weekly downtime: Barely 2 hours per day would be left for proper mentoring by senior investigators. For a research group of 30 graduate students and postdocs, this would leave 4 minutes per day per person.

If the top 20 research universities could be persuaded to carry out the suggested survey, why not go a step further and ask the members of the five largest research groups in each relevant department to estimate the weekly time available for one-on-one meetings with their P.I.'s? Indeed, why not attempt an experiment I have proposed twice before (1, 2)? Most American universities now require detailed evaluations by undergraduates of their teachers. Why not institute the same procedure for graduate students and postdocs in terms of the mentoring qualities of their preceptors? I have outlined (1) a brief questionnaire that could be answered in a few minutes. After having done this experiment myself, I suggested it to the chairs of some major chemistry de-

partments, who felt that opening such a Pandora's box (their words) would be undesirable. Given our increased attention to ethical and behavioral practices of the scientific community, perhaps the time has come to realize that this box is already wide open.

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**I NOTED WITH INTEREST DONALD KENNEDY'S** Editorial "Getting older" (11 Oct., p. 323), as it is high time that someone recognized that today's postdoctoral fellows are not the twenty-somethings of the 1960s. However, I fear that the figures Kennedy quotes—7 years to earn a doctorate in the life sciences and 2 to 3 years as a postdoc—tend to downplay the investment of time that today's junior scientists put into the pursuit of a scientific career. First, although the average of 7 years sounds correct, this number hides a tremendous variance. I have met people whose doctorates were completed in 3 years and others who spent 20 years as registered full-time doctoral students, with no correlation whatsoever between the duration of the doctorate and the quality of the work. As for the figure of 2 to 3 years as a postdoc, that is almost certainly an underestimate. Ten years as a postdoc is not unheard of, and 5 years appears closer to the norm. Finally, Kennedy omits a significant contributor to the aging of junior researchers, that is, the time that today's students spend pursuing the Master's degree. The Master's has become a de facto prerequisite for entry into doctoral programs, and students often spend 3 or even 4 years obtaining a Master's. It is not unusual today to find postdocs in their late thirties who move on to become research associates in their early forties on their hopeful path to academic seniority.

We must face the fact that the ultimate goal of most doctoral students is to attain tenure. This is a feat that cannot even be mandated by law, thanks to Supreme Court decisions that prevent the forcible retirement of college faculty. Grants or no grants, what we have here is an academic "lost generation" who will be a lot older by the time things get any better.

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**AS A RECENT POSTDOCTORAL FELLOW, I AM** not surprised by the decline in new NIH awards to applicants 35 years old and under ("NIH grantees: where have all the young

ones gone?", E. Goldman and E. Marshall, *New Focus*, 4 Oct., p. 40; "Getting older," D. Kennedy, Editorial, 11 Oct., p. 323). All postdocs currently in the biomedical sciences have experienced this phenomenon firsthand; indeed, the general expectation among scientists in my age group is that when it comes to getting an NIH award the first time you apply, basically you can "fahgetaboutit!" Several factors that might partially explain this trend have been identified; however, I'm surprised that the most obvious and human one has not been mentioned. NIH award committees are made up of older scientists who have already proven themselves and tend to give awards to other scientists who have already proven themselves on the basis of past performance, i.e., older scientists. Young scientists are an unproven commodity, and no matter how many papers they may have generated during their increasingly long postdoctoral periods, they are still considered "riskier" when it comes to granting awards. There is a need for postdoctoral representatives on NIH award committees or, more properly, separate awards for first time applicants. This decline in awards to young applicants has had a marked and depressing effect, with many young scientists frustrated in their career paths and stuck in a postdoctoral limbo. In the end, many abandon academic research altogether and move into the corporate sector.

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**IN HIS EDITORIAL "GETTING OLDER," DONALD** Kennedy (11 Oct., p. 323) states that the average amount of time to earn a Ph.D. in the life sciences in the United States is 7 years. In the UK, the average time is 4 years. Although some might say that this is too short a time, it is surely an advantage to give scientists the opportunity to demonstrate their originality at an early age. If the Ph.D. training is prolonged, it may well take the edge off enthusiasm for original research. A shorter period of study would not solve the problem described in the Editorial, but it might help.

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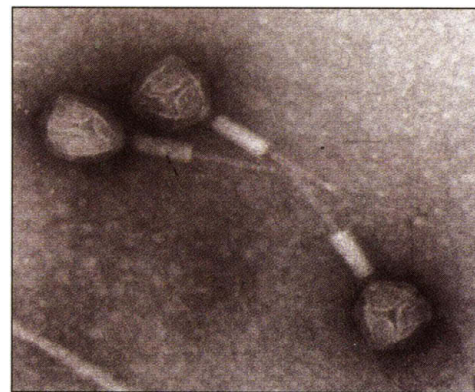
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## Phage Biology: Coming of Age

**I AM WRITING FROM THE PERSPECTIVE OF** one who spent many years studying phage biology—30 years ago, my laboratory purified and identified a protein phage receptor, the BtuB protein, which is the receptor for phage BF23. Phage biology is both a wonderfully exciting and a poten-

tially useful field, and I applaud the efforts of my former colleagues Elizabeth Kutter and Ry Young in helping to promote interest in the practical uses of phage to fight parasites and pathogens ("Stalin's forgotten cure," R. Stone, *News Focus*, 25 Oct., p. 728). In their remarkable diversity, much greater than the diversity in antibiotics, phage have already provided us with the tools to meet the most serious objections to their use, such as the rapid development of host resistance.

One excellent example is the receptor specificity system of the transducing phage p1. This phage encodes two different receptor specificities, each recognizing a different region of the host lipopolysaccharide (LPS) core. The genes for these specificities are read in opposite directions from an invertible promoter, allowing the phage to toggle back and forth between the two. Because the frequency of promoter inversion is several orders of magnitude greater than the host mutation frequency, the host can never mutate to develop phage resistance without giving up the



**Bacteriophage that kill *Salmonella*.**

LPS core entirely, a lethal condition in a natural environment.

We should be actively studying phage as alternatives to antibiotics and antimicrobial chemicals, just as we have studied microbial toxins as alternatives to insecticides and as tools for developing insect-resistant plants. This requires not only more knowledge about phage biology but also a change of thinking for study sections and funding agencies. In the past, research on phage has been justified on the basis of their ability to serve as models for human biology. For example, every successful lambda grant application began with the obligatory analogies between lambda and human cancer. Molecular biology has now matured to the point where phage research deserves funding on its own merit.

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