"Purity" of Science

Many readers must have shared my dismay regarding the sordid "Sloan-Kettering affair" as reported by Barbara Culliton (News and Comment, 10 May, p. 644). However, this letter relates less to Summerlin's folly than it does to the preamble of the report. Culliton clearly implies in her introductory paragraph that if Summerlin did, in fact, paint the mice and falsify his data then he would not only have made a grievous error but would also have committed a cardinal sin. She then goes on to state that "the very thought of fakery threatens the powerful mystique of the purity of science."

Science has never impressed me as "pure," and it must by now be strong enough to fend off such acts, let alone mere thoughts, of fakery. Those who promote the mystique of science do so both to conceal its reality and to imply that scientists are demigods. One of the reasons I so enjoyed Watson's book, *The Double Helix* (*1*), was that the very man who was in the best position to cloak his accomplishment with mystique chose, instead, to "tell it like it was." For all its technical complexities, what he portrays is not a mystical event but a thoroughly human drama.

Summerlin's error was also human. However, his hanky-panky strikes me as more silly than sinful, since anyone who falsifies the facts bearing on an important scientific question will inevitably be exposed. However, if he must be judged guilty, surely the Sloan-Kettering Institute, the Conquest of Cancer Program, and Alfred Nobel should share the blame.

It is to be hoped that science will always serve as a refuge from dogma and high priests. Cloaked in mystique, it drifts toward scientism—which is hardly better than any pagan idol. Summerlin has paid dearly; cancer is still with us; let the pure of heart join with the tolerant and get on with the job.

JOSEPH STOKES III
Department of Community Medicine, School of Medicine, University of California, San Diego, La Jolla 92037

References

1. J. D. Watson, *The Double Helix: Being a Personal Account of the Discovery of the Structure of DNA* (Atheneum, New York, 1968).

Use of Grant Funds

Reidar D. Sognnaes (Letters, 31 May, p. 940) makes a proposal that some research grants be made for work already done as a reimbursement for out-of-pocket expenses incurred by the investigator. His suggestion has much merit. However, he appears to be unaware that this type of grant is already in use and has been for at least the past 15 years. That is, the study sections of the National Institutes of Health require grant applications to be so detailed so as to compel that they be for projects that have already been completed. The awards that are made in these cases are used to finance the next line of research, which will in its turn be used as the basis for another grant application. Rarely, if ever, is there any financing from the investigator's own pocket; rather the money comes from diversion of research funds from grants for work already accomplished. In fact, in view of the current situation, it is well to have the results already in the press by the time the grant application is sent in, since it seems that some members of the study section or their friends could begin doing the work on any good idea as soon as they read about it in a grant application. This means that, if the investigator waits until the award is made, he could be the second one to publish on that particular subject.

Thus, Sognnaes, perhaps without realizing it, is really proposing what is already the current practice. What we really need is a return to a research grant for work that has not been done, that is, something speculative enough so that the method of research and the outcome cannot be known in sufficient detail to write the kind of research proposal that is now being funded. Perhaps Sognnaes has a suggestion in that direction.

MYRON L. WOLBARSHT
Duke University Eye Center, Durham, North Carolina 27710

Letters of Recommendation

I wish to join Clyde Herreid (Letters, 5 July, p. 9) in deploring the failure of medical schools to respect the confidentiality of letters of recommendation. Two years ago a student of mine told me that he had actually been asked at one interview to read the confidential letters in his application folder and then to tell the interviewer what he thought of himself in the light of those letters. In the absence of direct proof (although I am confident of the student's honesty), I will omit the name of the medical school where this incident allegedly occurred. I will only add that the interviewer was a psychiatrist who apparently thought this procedure so clever and amusing as to forget that it represents a blatant breach of ethics.

GERALD L. GEISON
Program in History and Philosophy of Science, Princeton University, Princeton, New Jersey 08540

The letter from Herreid reflects a common view that continually puzzles and distresses me. Herreid writes that "medical schools depend upon honest, candid evaluations. A faculty member is much less likely to write such an open appraisal of the student if he knows that the information will not be kept confidential."

How can an evaluation be considered honest and candid if it is to be kept from the person evaluated? I agree that my criticism of my student should be a matter between him and me and people who have a responsible right to my views. They should not be available to the general public, but they must be available to the candidate himself if I am to be considered "honest and candid."

W. C. H. PRENTICE
Office of the President, Wheaton College, Norton, Massachusetts 02766

Like Herreid, I also write a large number of letters of recommendation each year for students applying to medical school. I have not had any direct indication from students that they have been allowed to see such letters during the interview procedure at the medical schools, but to the extent that such violations of promised confidentiality occur, I share Herreid's concern and agree that medical schools which are negligent in this regard suffer indeed from "truth in labeling" when they designate such information as confidential.

This is, however, not a real problem or direct concern for the faculty member who routinely makes letters of evaluation available to students as a matter of course. I have come to feel personally that such letters are probably of the

greatest potential value, not to the admissions committee to whom they are addressed, but to the students about whom they are written. Accordingly, I have adopted the convention of routinely making copies available to students upon request. I take issue with Herreid's contention that a faculty member is much less likely to write an open appraisal of the student if he knows that the information is not to be confidential; an evaluation (admittedly subjective) of the letters I have written before and since adoption of an open-file policy fails to reveal any obvious changes in tone, honesty, or candidness of the appraisals. Indeed, I have on occasion found this a useful medium to communicate to a student my concern about an area of deficiency which in all probability would have otherwise gone unmentioned.

Thus, while I agree with Herreid that medical schools (and other admissions committees as well) have an obligation to the writers of reference letters either to guarantee confidentiality or to adopt and announce publicly a policy to the contrary, I at the same time recommend that colleagues consider the virtues of making such letters available to the student as a matter of course, thereby making the requesting and writing of letters of recommendation less of a cloak-and-dagger operation than it too often is at present.

WAYNE M. BECKER
Department of Botany, University of Wisconsin, Madison 53706

Evolution of the Brain and Intelligence

Ralph Holloway (10 May, p. 677) has used his review of Harry Jerison's *Evolution of the Brain and Intelligence* (*1*) to pursue a debate of long standing between the two men. He devotes almost the entire review to criticizing Jerison's opinions on the evolution of the brain in *Homo sapiens*. Some criticisms are well taken, others are not; but this is not the point. Jerison has assiduously collected data on the evolution of the brain in all vertebrates living and fossil, and the bulk of his book is devoted to these larger issues. Moreover, he treats them with the finest and most coherent set of data ever available, and with a biometrical sophistication that avoids a century of pitfalls caused by misunderstanding of the relationship between brain weight and body weight. Among his fascinating conclusions are the following: (i) dinosaurs were not small-brained but had brains of the bulk predicted for reptiles of their body size; (ii) *Archaeopteryx* had a brain midway in size between those of average reptiles and birds; (iii) early Tertiary primates had smaller brains than average modern mammals of the same body size, but some of these primate brains were larger than those of any other early Tertiary mammal; thus, brain size has increased within the Mammalia as a whole, but primates have always been ahead of other orders.

A review in *Science* is the most widespread notice that technical books receive. Criticisms of particular and peripheral points should at least be accompanied by an adequate account of a book's main thrust and general content.

STEPHEN JAY GOULD
Museum of Comparative Zoology, Harvard University, Cambridge, Massachusetts 02138

References

1. H. J. Jerison, *Evolution of the Brain and Intelligence* (Academic Press, New York, 1973).

1984

I believe that it is somewhat farfetched to use the terminology of the Pentagon or the statements of Daniel Ellsberg as evidence that "there is still a danger that before 1984 we shall enter a tunnel at the end of the light," as was mentioned by Leonard M. Rieser, retiring president and chairman of the Board of Directors of the AAAS, in his presidential address in San Francisco (26 Apr., p. 486). Events from the Senate censure of Senator McCarthy to the present Watergate investigation present ample evidence of the general success of the checks and balances which are available to us to prevent an Orwellian 1984 in the United States.

Certain events in societies other than our own, however, closely approach some of the predictions of Orwell for 1984. Rieser quotes Orwell: "Whoever controls the past controls the future. Whoever controls the present controls the past," noting that Orwell's "IngSoc" history is rewritten with complete thoroughness on a daily basis.

It is common knowledge that continuous "revision" of history is one of the underlying facets of the thought control practiced in the Soviet Union. Selectivity in the release of news to the masses further strengthens the "control of the present," and the publication of historical facts or actual events not in agreement with current ideology can entail criminal prosecution to the disseminator.

While constructive self-criticism is necessary for any society, including our own, a consideration of events in other societies may help to put conditions in our society into better perspective. While we are far from perfect, others seem to be much closer to "1984" than we are.

PAUL GRIMINGER
Department of Nutrition, Rutgers University, New Brunswick, New Jersey 08903

What We Have Yet to Learn

Norman Hackerman deserves hearty congratulations for his brilliant editorial "Ignorance as the driving force" (8 Mar., p. 907), in which he explicitly brings out the meanings of "pure" ignorance, the ignorance explosion, and the information explosion.

In this context, I quote a famous Tamil verse of the gifted poetess Saint Avvaiyar, who is popular in every home in Tamil Nadu, India. She lived in the first century B.C., ranking first among the many poetesses who flourished during that period.

Poetess Avvaiyar says,

Kattrathu Kai Mann Allavu,
Kallathathu Ulakallavu.

In essence, it means,

What we have learnt, is like a handful of Earth,
While what we have yet to learn, is like the whole World.

This verse, sung 2000 years ago, even now holds true, and it is certain that the knowledge which we have today is only an infinitesimal part of what we have yet to know.

S. S. IYER
B 7/1, Niralanagar, Lucknow-7, India