

## LETTERS

### "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.

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#### 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.

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### 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.

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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."

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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