Letters

The Sloan-Kettering Affair

It was with surprise and dismay that we read Barbara J. Culliton's report "The Sloan-Kettering affair (II): An uneasy resolution" (News and Comment, 14 June, p. 1154). The following inaccuracies which appeared in that report call for correction. Culliton states that "it is a matter of one man's word against another's" as to whether we informed Summerlin that only single cornea experiments were being performed by ophthalmologists Bartley Mondino and Peter Laino. She further states that ". . . a couple of points are clear. Whether Ninnemann and Raaf did or did not tell Summerlin in early October about the rabbits, they sat and listened to him talk about the double eye transplants on subsequent occasions without saying a word. Just why they did not speak up is not at all plain, to say the least."

These are serious and false allegations that unfairly raise questions about our integrity and motives. The truth is that each of us, after conversations with Mondino, independently advised Summerlin that he was misinterpreting the rabbit cornea experiments. At that time he appeared to accept our corrections, and we never again saw him present rabbits that he claimed had received double corneal transplants. Another statement by Culliton—"Apparently neither of the research fellows got along well with Summerlin . . ."—is also untrue and appears to be a further attempt to discredit us.

We cannot understand why Culliton failed to interview either of us (or Mondino) prior to writing her lengthy and widely circulated account. That she should have contacted us would seem to have been required by professionalism and sound journalism. To us, this is a clear example of a science writer's publishing prematurely before she understands or has fully investigated her subject. The facts in this affair make it inappropriate to spread responsibility for Summerlin's irrational actions. Care should be taken to present these facts accurately and thus prevent damage to those who were associated with him for a short time.

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I, too, regret not speaking with Raaf or Ninnemann about their role in the Summerlin affair. After repeated attempts to reach them at Sloan-Kettering, I was informed that they were unwilling to discuss the situation with *Science*.—B.J.C.

The "resolution" of the "Sloan-Kettering affair" is indeed a source of unease for anyone who is the least bit skeptical of the utopian image of scientific endeavor. Sociologists have given us a wide range of images of deviance in every occupation except their own and those of their cousins, the physical scientists. We know about deviance in almost every sphere of life; why should we assume, as we do, that such behavior does not occur among professional scientists? What makes the scientist immune from vulnerability to deviance? We seem to take it for granted that the efforts of scientists are so much more important, their motives so pure, that under these conditions, altering one's data cannot occur. We also assume that because of an "invisible college" there will always be another colleague who can replicate our work, and this will serve as a check on dishonesty in research. In the case of Summerlin, replication served this function; however, how frequently do we incorporate research results which have not been checked into the body of scientific knowledge. And if they were checked, how often would we discover what was revealed in the Sloan-Kettering affair. We are naive to believe that dishonesty in research in unique and aberrant. The rewards are just too tempting: prestige, ego enhancement, promotion, and, as in the case of Summerlin, a $40,000 salary and a home in Darien, Connecticut. Mighty tempting rewards for success. Not only are the rewards tempting but, while the process of socialization in graduate school may give credence to veracity, it nonetheless emphasizes success. The emphasis on scientific success creates a severe strain on the practicing researcher, who is torn between the norms established for the process of research and the penultimate rewards for success. Under these conditions deviance is likely to occur in any group, even among scientists.

The second source of my unease is the less-than-critical manner with which Barbara Culliton presented Summerlin's and Sloan-Kettering's explanations for Summerlin's behavior. As Thomas Szasz pointed out long ago, the label of mental illness or emotional disturbance is more and more frequently applied to explain behavior that is not normative. Yet, as Szasz has also argued, the reliance on such a label to explain behavior represents an abnegation of moral responsibility (*1*). ("After all we can't really blame the poor fellow, he is suffering from an emotional disturbance.") This raises the question of why Summerlin should be let off so easily, for he violated one of the principal mores of the profession. It seems to me that professional scientists are too quick to duck the real issue here. They seem too willing, on the most tenuous basis, to excuse the behavior of "a colleague"—even one who has broken their most sacred rule.

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References

1. T. Szasz, *Yale Rev.* **49**, 561 (1960).

Joseph Stokes III (Letters, 2 Aug., p. 399) confuses the "purity of science" with the purity of thought and action we seek as a supreme human attainment. That we all fail to be perfect should not mar our concept of science. Scientists can cheat or make mistakes; science is incorruptible.

I agree that the Conquest of Cancer

Program—which, after all, is a complex political notion as much as it is a plan to solve a human problem—may lead many astray, but it is absurd to blame the Sloan-Kettering Institute or Alfred Nobel for setting high standards.

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Unexplained or Esoteric?

In the News & Notes feature "Physical phantasmagoria" (12 July, p. 129) advice that may be misleading is offered to geophysicists and other scientists concerning "worlds to conquer." In the notice of the publication of *Strange Phenomena*—a compilation of "unexplained natural events" by William R. Corliss—at least two phenomena are graced with the description "unexplained," when in fact they are merely esoteric. Although I must confess my ignorance of Barisal guns and mistpouffers, neither the green ray nor the Brocken specter lies outside the scope of present-day science. *Sky and Telescope* has published several excellent accounts of the green ray, most recently in the issue of July 1974 (*1*). The Dutch astronomer Marcel Minnaert describes the physics of this phenomenon in *The Nature of Light and Colour in the Open Air* (*2*, p. 58). The optical principles of the Brocken specter are also explained by Minnaert (*2*, pp. 257–259); it is the result of the faint backward scattering of light by fine water droplets.

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References

1. *Sky Telesc.* **48**, 61 (July 1974).
2. M. Minnaert, *The Nature of Light and Colour in the Open Air* (Dover, New York, 1954).

Leukemia and Chromosomes

The Research News report by Thomas H. Maugh II (5 July, p. 48) on leukemia is a useful summary. We wish to correct several points and add some information about the cytogenetic research discussed by Maugh.

First, the Philadelphia chromosome, which is found in the majority of patients with chronic myelocytic leukemia, has been identified as chromosome number 22 (not number 21). An extra chromosome number 21 is found in Down's syndrome (mongolfor speculating about a possible chromosomal relationship between chronic myelocytic leukemia and Down's syndrome.

Second, our work discussed by Maugh on the genetic immune deficiency disease, ataxia telangiectasis (A-T), has not been on granulocytes. We and others have studied A-T lymphocytes and found (i) an increased tendency to chromosome breakage and (ii) clones marked by a chromosome rearrangement in some A-T patients. The chromosome rearrangements in these clones all appear to involve chromosome 14 and more specifically its long arm. The connection between this cytogenetic finding in A-T lymphocytes and the well-known tendency of A-T patients to develop lymphoid and other malignancies is not yet clear.

Third, the only A-T patient with leukemia whom we have had the opportunity to study (in collaboration with Ray Teplitz at the City of Hope National Medical Center, Duarte, California) has chronic lymphocytic (not myelocytic) leukemia. The leukemic cells in this patient have clearly evolved from a preexisting lymphocyte clone with a number 14 translocation. This provides further evidence for the single-cell origin of chronic lymphocytic leukemia.

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Copyright Laws

Curtis G. Benjamin's letter (28 June, p. 1331) in support of William & Wilkins' Supreme Court suit against the U.S. government for copyright infringement omits some of the problems on the other side of the fence. Just as publishing companies are faced with the financial squeeze attendant to inflation, so too are academic institutions. While costs have risen, departmental budgets have fallen further and further behind, and now new demands are placed on us to pay for the dissemination of information to our students. Publishers seem to be saying that if we are unable to pay, then our students have no right to receive information we deem necessary.

But let us examine this a little further. Funds that made our research possible did not come from the publishers. Nor did the publishers assist us in writing the manuscripts. Indeed, they charge us for reprints, presumably make a profit by selling their journals, and do not reimburse the authors for their efforts. Thus, the author does the fund raising, the thinking, the laboratory work, and the writing, and then the publishers claim ownership, apparently because it may make money for them. And to top it off, they now want us to pay for the privilege of using the articles we have published to teach our own students.

I agree that the copyright laws should be revised, vesting ownership of an article either in the name(s) of the author(s) or the scientific society responsible for publication—but certainly not the publisher.

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Imprisoned Soviet Mathematician

The Soviet Embassy in Washington, D.C., refused to accept a petition composed by the International Defense Committee of Mathematicians for Shikhanovich and Plyushch and signed by more than 650 American mathematicians. "The hostile and slanderous nature of those petitions compels us to reject them and return them to the sender," wrote V. I. Kuznetsov, Second Secretary of the Soviet Embassy, on 8 July 1974. Kuznetsov failed, however, to point out a single inaccuracy in the statement of the committee.

The Soviet mathematicians Yuri Shikhanovich and Leonid Plyushch were arrested (separately) in 1972 on charges of anti-Soviet propaganda were held incommunicado for nearly a year, and were declared mentally incompetent at trials at which they were not present.

It is a pleasure to report that Shikhanovich has finally been released. On the other hand, Plyushch is still confined in a "special" psychiatric hospital in Dnepropetrovsk.

He has been subjected to harsh treatment and involuntary chemotherapy.