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NEWS AND COMMENT

The Sloan-Kettering Affair: A Story without a Hero

There is no sin in science more grievous than falsifying data. There is no accusation that can be made against a man more serious than that he is guilty of such a sin. The very thought of fakery threatens the powerful mystique of the purity of science. It stirs deep and contradictory feelings of incredulity, outrage, and remorse among the entire scientific community—feelings it is experiencing now in the very complex and unresolved matter of William T. Summerlin.

The case has received widespread attention in the press, and by now it is well known that Summerlin, a young investigator at the Sloan-Kettering Institute for Cancer Research in New York, is alleged to have falsified the results of an experiment intended to prove that skin, when grown in tissue culture, loses its ability to provoke an immune response. Late in March, he was "temporarily relieved of his responsibilities" by institute president Robert A. Good, who has promoted Summerlin for the past couple of years. Good appointed a committee of Sloan-Kettering scientists to investigate the situation.

It is understood that the committee is investigating not only the mouse painting incident, which is alleged to have happened during the last few weeks, but the whole of Summerlin's work which has been cast into doubt.

A spokesman for the institute says there will be a "full disclosure" of the review committee's findings when they are complete, but, as yet, the full facts of the case are unknown. The institute and the committee are unwilling to discuss the matter, even declining comment on precisely what it is they are investigating. According to Summerlin, even he has not been informed about what is going on. "I know nothing about the review committee, and I don't know what they are reviewing," he told *Science*. He does, however, expect to appear before the committee before it concludes its work. "There are two sides to this story, and I want to tell mine [to the public] after the committee is done and I've had a chance to talk to them," Summerlin declared.

The Summerlin case raises issues that go far beyond the question of whether one man has or has not literally falsified data. That is important, but the stakes are even higher.

First, there is Summerlin's own reputation, already seriously damaged by an accusation, reportedly made by a Sloan-Kettering laboratory attendant, that he painted black patches on the skin of white mice to make it appear that he had successfully transplanted skin between genetically incompatible animals. When asked by *Science* whether he had painted mice, Summerlin said, "I have never willfully misrepresented my data. I look forward to continuing in science."

Beyond the alleged mouse painting incident is the question, now on many persons' minds, of the validity of the whole of Summerlin's work during the last 4 years, work which potentially has enormous implications for research in immunology and cancer.

Second, there is the reputation of Good, Summerlin's boss and mentor, a tremendously powerful and persuasive man who has lent the prestige of his own stature to the work Summerlin has been doing.

Third, there is the reputation and internal stability of Sloan-Kettering itself, a troubled institution that has been struggling for the last couple of years to find its identity and its future.

The present crisis could turn out to be ruinous for all three.

The simplest of the questions to be resolved in the Summerlin case is whether or not he really painted mice for the purpose of deceiving his colleagues. Possibly the answer to that is already known to the review committee.

The other questions are not even potentially easy to answer. They have to do with the environment in which research is conducted, with pressures to succeed in a spectacular way, and with who properly gets credit for what. They also have to do with how research should be presented to the scientific community and to the public and what one does when, seemingly all of a sudden, one can no longer repeat one's own experiments and it is possible

that previous claims were overblown.

Both Good and Summerlin have been ballyhooing the tissue culture work for the last year or more. In the fall of 1973, however, their initial confidence was somewhat shaken when Summerlin himself was unable to repeat his experiments. Subsequently, explicit attempts at repetition by others at Sloan-Kettering failed. Nevertheless, both the scientific community and the press continued to be told enthusiastically about the importance of the tissue culture work as if nothing were the least wrong.

Summerlin, with Good's consent, agreed to an interview with a reporter from *Medical World News* in February. And, at least twice during the weeks before the current scandal broke, he dazzled scientific audiences, apparently without ever mentioning the known problems of duplication.

A member of the National Cancer Advisory Board heard Summerlin deliver one of those talks, in Cambridge, Massachusetts, about 2 weeks before the 18 March meeting of the board. "He was very persuasive and convincing," this board member told *Science*. "There was no mention during the talk that anything was wrong. None at all." In fact, the board member was so impressed by what Summerlin had to say that when he subsequently learned that the National Institutes of Health (NIH) had only recently rejected a grant application Summerlin had submitted, he asked the cancer board to rereview the application with a view to funding it with cancer money. The application was brought up in closed session at the last board meeting but no action was taken. Now, the matter has been shelved, at least temporarily.

Why did the NIH—specifically, the immunobiology study section of the Division of Research Grants—flatly reject Summerlin's grant application after its review which included a visit to Summerlin's laboratory by a team of study section members?

Why did Summerlin and Good continue to behave in public as if everything were in order after they knew doubts had been raised? Why was there what appears to be a cover-up? Perhaps the answers to these questions will be made known during the next few weeks.

Summerlin's story begins in 1965 when he was with the burn and trauma unit at Brooke Army Medical Center in San Antonio. As a surgeon and dermatologist, who was particularly interested in skin-grafting, he has recalled in the past, he "became increasingly impressed by the fact that, not only were there gaps in our knowledge regarding skin-grafting processes but, moreover, in our approach to skin preservation as well." Summerlin has said that it occurred to him that tissue culture might "lend itself nicely" to a study of the problems. So, after 2 years in Texas, he moved to Stanford University where he worked with Marvin A. Karasek who was already working with cultured rabbit cells. At Stanford they demonstrated that whole human skin could be maintained in culture "for extended periods of time" and subsequently transplanted back to the person from whom it had come. That made Summerlin wonder about transplants between unrelated individuals. "I found that after human skin is maintained in organ culture for 4 to 6 weeks it becomes universally transplantable without rejection," he said in the spring of 1973 at a meeting of the American Cancer Society.

That was a fairly startling finding, one with major implications for the entire field of immunology. If you could damp the antigenicity of tissue by growing it in culture—well, the possibilities seemed endless.

His work at Stanford was primarily clinical, and by 1971 Summerlin realized that the phenomenon he observed "required more definitive laboratory dissection." In August of that year, he moved to the University of Minnesota to work in Good's vast immunology laboratory. In so doing, he was following up on an idea that had been in his mind for a couple of years.

Summerlin first met Good in December 1969, when Good spoke in Miami at a meeting of dermatology researchers. "I introduced myself," Summerlin recounts, "and said I was interested in immunology. We had breakfast the next morning and he invited me to Minnesota, but I said it was probably premature." Later, feeling more confident that his observations were real, he contacted Good and made plans to move to Minneapolis.

Good's laboratory was practically an institution in itself with its large staff of scientists and technicians and its considerable resources. Summerlin, who was on the way to becoming one of Good's special finds, settled into what his colleagues from those days call a rather independent existence. One of them recalls that "Bill pretty much worked alone. As far as his research is concerned, he was not really part of any group. But when he had something interesting or a problem of some kind, he would frequently talk with me or someone else about it."

Good's personal contact with the individuals in his laboratory is probably best described as occasional. He traveled a lot; when he was home, dozens of people were vying for his attention.

In spite of what appears to be a great deal of circumstantial evidence to the contrary, it is by no means certain that Summerlin has fashioned an immunologic Piltdown Man. Work that was done in Europe during the 1920's and 1930's, for example, tends to support the basic concept of tissue culture altering antigenicity, even though that work is not precisely the same as what Summerlin is doing now. Henry Gans, a surgeon at Cornell Medical College in New York, referred to it in an unpublished letter to the editor of *Science*, written in response to *Science*'s 7 September Research News review of the subject in general, and in a conversation with this reporter. He knows of at least five investigators who reported successful transplants of tissues that were cultured prior to transplantation, although none of them was as scientifically dramatic as Summerlin's culturing of normal tissue and transplanting it to unrelated recipients.

Another, contemporary indication, that the phenomenon is real was reported in the 26 April 1974 issue of *Science* by immunologists Paul Terasaki and Gerhard Opelz of the University of California School of Medicine at Los Angeles. They have cultured human blood lymphocytes and find that after culturing, the cells lose their ability to *stimulate* an immune response but retain their ability to *respond* immunologically. They wrote:

We describe here the surprising finding that, upon aging in vitro, lymphocytes lose their ability to stimulate allogeneic cells while they retain their physiological ability to respond. The stimulating antigenic structures may therefore be labile in tissue culture, as has been suggested by the work of Summerlin on cultured skin.

(Neither of the UCLA researchers has had any part in Summerlin's experiments.)

"Good's role [in the actual conduct of an experiment] was minimal, at least in my case," says one of his former staffers. "He reviewed what I was doing with me from time to time but not frequently."

Summerlin's experience was similar. "Bob and I were not really working together," he recounts. "In fact, it was often hard to get to talk to him. I used to have to get up at 4 or 5 o'clock in the morning to see him for a few minutes. But it did not matter too much then. The whole group there in Minnesota was very good, very friendly. There were a lot of people knocking heads together."

Summerlin's goal was to extend his observations about the transplantability of cultured human skin to animals and to other organs. He had to prove that what he observed about culture altering antigenicity was true and he wanted to explain why. By all indications at the time, his work was going extremely well, so well, in fact, that, when Good left Minnesota in January to head Sloan-Kettering, he asked Summerlin to come with him. Summerlin agreed.

On 30 March 1973, at the previously mentioned meeting of the American Cancer Society (a seminar for science writers), Summerlin reported that mouse studies confirmed his early clinical work. Using "well-defined inbred mice," he said he could show that skin maintained in culture for 7 to 10 days could be transplanted to genetically incompatible animals without being rejected.

> . . . Lymphocyte cytotoxicity studies revealed that such grafts elicited no thymic-dependent lymphocyte response and no sign of blocking antibody production. Also, we now have both human and mouse data showing that the classical histocompatibility antigens, both H-LA in man and H-2 in the mouse, maintain their integrity during the organ culture process and persist after subsequent allogeneic transplantation without rejection. This phenomenon has been extended to xenogenic skin grafting after culture, using mice as recipients and human, pig, guinea pig, and rat skin donors.

Scientists Talk of the Need for Conservation and an

Wildlife conservation still retains some of the elitist cachet it had in the days before man's biotic resources began to be perceived as finite. Yet, as evidenced at a recent symposium of biologists, zoologists, and ecologists, the rapid extinction of plant and animal species the world over threatens to narrow down future choices for mankind.

Biotic impoverishment was the subject of the conference, which was sponsored jointly by the Smithsonian Institution and the World Wildlife Fund (WWF). It was in the nature of a brainstorming session to figure out how the WWF, as the largest private international organization concerned with world wildlife preservation, could best apply its limited resources. WWF, since its creation in 1961, has spent some $12 million on its mission, including a highly publicized $1 million campaign to save the Bengal tiger.

The picture, in view of the proliferation of population and economic activities over the world, is very grim. As Herbert Bormann of the Yale school of forestry said, people and their activities are spreading over the world like a "sheet of molasses," and the WWF is in the position of "scurrying around trying to fence off little areas" as the engulfment proceeds.

The only good news seems to be that, despite the relative ineffectuality of their efforts, both scientists and politicians throughout the world are developing a keener awareness of the urgency of the problem.

"Species-by-species" protection has long been recognized by conservationists as an insufficient approach. The U.S. Congress finally realized this in 1972 when it passed the Marine Mammal Protection Act, the first law designed to maintain the optimum population of a number of species within the health of their ecosystem. The rationale for conservation is no longer argued only in terms of esthetics or cost-benefit ratios, but on the far more fundamental grounds that the future viability and well-being of man are dependent on preserving as many species as possible from extinction.

Lee M. Talbot, ecologist at the Council on Environmental Quality, points out that, despite concern in the United States over pollution, it "is about the least important aspect of environment" because it is, in most cases, reversible. But changing land use, such as leveling forests or filling in wetlands, eradicates entire habitats and causes some species to be lost to the world forever. Plants and animals that may now be regarded as dispensable may one day emerge as valuable resources.

The eradication of species in tropical lands is seen as particularly alarming. If formations in the Amazon continue to be cut over, for example, it is estimated that some 1 million species of flora and fauna will disappear (the worldwide extinction rate up to now is estimated at about 10,000 species per century). This is only a guess, however, because scientists don't know how many species there are to begin with. One and a half million species are known; estimates of the total number have risen from 3 million a decade ago to somewhere near 10 million.

Population ecology is a science whose rules are only beginning to be adumbrated. So scientists face two equally urgent tasks: gathering data on which predictions and decisions can be based, and trying to persuade governments that it is in their interests to preserve habitats before they are erased by commercial and farming activities.

Since no amount of money is going to do the job, a major theme of the conference was the need to develop an "ethic of biotic diversity," in which such diversity is perceived as a value in itself and is tied in with the survival and fitness of the human race.

Scientists at the meeting advanced a number of ideas that might lead to more systematic efforts at preservation. Bormann noted that qualified leadership is in short supply and advocated the creation of some sort of training institute. Just as Sandhurst military academy was the seedbed of the talent that created the British Empire, so does the conservation movement need a "Sandhurst" which would bring together social, economic, and political as well as scientific disciplines for the creation of a sophis-

Summerlin went on to say that other organs, when cultured, also appear to lose their antigenicity. He specifically mentioned whole human and rabbit corneas and mouse adrenal glands.

It was all very exciting. Transplantation between unrelated individuals is a terrible problem, and here was someone suggesting a potentially simple way around it. Good, who was at the meeting briefly, enthused over the work.

Several weeks later, at the meeting of the American Society for Clinical Investigation, Summerlin was one of the lead speakers. His presentation was lucid and convincing. The audience was impressed. Good was excited, pleased, proud. Everybody looked good, including Sloan-Kettering, whose reputation as a place of scientific excellence Good was trying to rebuild.

In spite of the public displays of success, there were reasons to believe there was a long way to go before the tissue culture phenomenon could be said to stand on solid ground. Several immunologists were skeptical. It seemed too easy; they needed to be convinced. According to some of Good's colleagues in immunology, he himself tried to reassure them, putting the strength of his own very substantial reputation behind his words.

But there was another problem. Unknown at the time to the scientific community at large, there were investigators who, try as they might, could not repeat Summerlin's experiments. The ultimate test of proof in science, repeatability, had not been met. But Summerlin and Good were not emphasizing that.

Barbara B. Jacobs of the American Medical Center in Denver is among the investigators who has tried to duplicate Summerlin's work. In work that preceded Summerlin's, she demonstrated that cultured mouse tumors can be successfully transplanted to incompatible mice; however, her work differs from Summerlin's in several major respects (*Science*, 7 September 1973). In 1964, she says, she tried to extend her mouse tumor work to skin but

Ethic of Biotic Diversity to Slow Species Extinction

ticated and influential cadre of leaders. In the shorter term, Bormann also suggested that the WWF put some money into the creation of an institute, in a fast-growing country such as Nigeria, that would be staffed by natives and would take stock of the country's resources and make decisions on what needs to be saved at once.

Daniel Janzen, tropical biologist at the University of Michigan at Ann Arbor, observed that "the only way to fight the loss of a habitat is with the same power that is destroying it." Costa Rican rain forests, for example, are being decimated by a combination of foreign businessmen. Why not approach similar businessmen, suggested Janzen, and offer to sell them areas of valuable habitat on the same basis one would sell a valuable painting. In this way they could combine a good deed with a good investment. Thus, said Janzen, could be created a "museum of natural habitats." Roger Payne of the New York Zoological Society sprang to this idea. The best way to sell a new concept is to put it in an already accepted form, he pointed out. A natural habitat museum could be just that—with a board of directors, trustees, curators, and guards. Some of the collection would be privately owned, some would be on display, and some could be withdrawn from display. "Guards" could be altruistic adventurous types like Peace Corps volunteers who could live on the land for a certain period.

Talbot later told *Science* that none of the ideas advanced were new, novel as they may have sounded. This was not to denigrate the conference; rather, he said, it proved once again that people all over the world concerned with conservation all tend to come up with the same basic approaches. "What came out was really an independent endorsement of what we have been doing for a long time," says Talbot, who has been engaged in international conservation activities during the past 25 years. "It also shows that while we have been right we haven't been all that successful." Some of the approaches to which Talbot was referring are intensive lobbying of governments, which is a specialty of the International Union for Conservation of Nature and Natural Resources, a nongovernmental organization that acts as consultant to the United Nations; the idea of creating natural habitat museums; and campaigns to save a single species such as whale or tiger as a means both to attract public attention and to save an entire habitat, of which the publicized species is only a small part. Also a part of world conservation philosophy is the need to preserve "spectacles" such as the migration of wildebeests across the Serengeti Plain or the accumulation of flamingos around Lake Nakuru in Kenya, even when the species involved are not endangered. This, too, involves protection of vast areas and all their attendant biota.

Talbot's own belief is that if more effective approaches exist, conferences won't uncover them. What we need is to get some smart people from a mix of disciplines into some sort of think tank, he believes, people free from day-to-day concerns of conservation who can back off and take a hard look at the total picture.

Meanwhile, conservationists see a desperate need for immediate action. For the most part, they must stand by helplessly watching Bornean rain forests being flattened for pasture land and Costa Rican rain forests being turned into Swedish cabinets. Janzen predicted that within the next century only a few dozen tropical areas will have escaped the heavy hand of man, and these will be saved not as a result of any policy but from quirky circumstances in these areas.

The conservation movement has moved far beyond concern about furry creatures with warm brown eyes. However, until someone comes up with a better idea, the furry creatures will be used as the selling point to the general public (WWF's symbol is the panda). As one Indian official is quoted as saying, ". . . we are going to preserve the whole biological pyramid with the tiger on top." Conservationists are well aware that the real problem is the salvation of countless other species, some known and some not, the silent majority, as it were, upon whose continued survival the quality of future human life depends.—C.H.

NAS Elects New Members

The National Academy of Sciences has announced the election of a new foreign secretary, 12 foreign associates, and 95 new members. A 96th member was elected posthumously.

George S. Hammond, vice chancellor of natural sciences at the University of California, Santa Cruz, was elected to a 4-year term as foreign secretary of the academy. He succeeds Harrison Brown, professor of geochemistry and science and government at the California Institute of Technology, who had held the position since 1962.

The newly elected members of the academy, who bring the total to 1077, are as follows:

Richard D. Alexander, University of Michigan
Edward Anders, University of Chicago
Richard C. Atkinson, Stanford University
Karl F. Austen, Harvard Medical School
William O. Aydelotte, University of Iowa
John W. Backus, IBM Corp.
David Baltimore, Massachusetts Institute of Technology
Lloyd M. Beidler, Florida State University
Francis R. Boyd, Jr., Carnegie Institution of Washington
Myron K. Brakke, University of Nebraska
Eugene Braunwald, Harvard Medical School
Winslow R. Briggs, Carnegie Institution of Washington
Thomas C. Bruice, University of California, Santa Barbara
Solomon J. Buchsbaum, Bell Laboratories, Inc.
Orville L. Chapman, Iowa State University
John A. Clements, University of California School of Medicine, San Francisco
Gerhard L. Closs, University of Chicago
William G. Cochran, Harvard University
Columbus C. Cockerham, North Carolina State University
Lee J. Cronbach, Stanford University
Horace W. Davenport, University of Michigan
Clement A. Finch, University of Washington
Willis H. Flygare, University of Illinois
Heinz L. Fraenkel-Conrat, University of California, Berkeley
Ronald Freedman, University of Michigan
Jacob Furth, Columbia University
Daniel C. Gajdusek, National Institute of Neurological Diseases and Stroke
Ernest P. Geiduschek, University of California, San Diego
Ivar Giaever, General Electric Co.
Martin Gibbs, Brandeis University
Robert R. Gilruth, National Aeronautics and Space Administration
Herman H. Goldstine, Institute for Advanced Study
Leo A. Goodman, University of Chicago
Roy W. Gould, California Institute of Technology
Kenneth I. Greisen, Cornell University
Jerome Gross, Harvard Medical School
Roger C. L. Guillemin, Salk Institute
Charles F. Hockett, Cornell University
Hendrik S. Houthakker, Harvard University
Frederick S. Hulse, University of Arizona
Leonid Hurwicz, University of Minnesota
Jerard Hurwitz, Albert Einstein College of Medicine
John D. Isaacs, Scripps Institution of Oceanography
Ali Javan, Massachusetts Institute of Technology
Elwood V. Jensen, University of Chicago
Eric R. Kandel, New York University School of Medicine
Bessel Kok, Research Institute for Advanced Studies, Baltimore
Norman M. Kroll, University of California, San Diego
Harold D. Lasswell, Yale University
Paul F. Lazarsfeld, University of Pittsburgh
Wassily Leontief, Harvard University
Estella B. Leopold, U.S. Geological Survey
Abba P. Lerner, University of California, Berkeley
Richard Levins, University of Chicago
Dan L. Lindsley, Jr., University of California, San Diego
Frank J. Low, University of Arizona
Clarence R. Lynds, Kitt Peak National Observatory
Richard S. MacNeish, R. S. Peabody Foundation for Archeology
John L. Margrave, Rice University
Charles F. Mosteller, Harvard University
George D. Mostow, Yale University
Hans J. Müller-Eberhard, Scripps Clinic and Research Foundation
Hamish N. Munro, Massachusetts Institute of Technology
Theodore M. Newcomb, University of Michigan
Alex B. Novikoff, Albert Einstein College of Medicine
Jeremiah P. Ostriker, Princeton University
Chandra K. N. Patel, Bell Laboratories, Inc.
Ralph G. Pearson, Northwestern University
William G. Pfann, Bell Laboratories, Inc.
Leo J. Postman, University of California, Berkeley
David M. Prescott, University of Colorado
Clifford L. Prosser, University of Illinois
Allen E. Puckett, Hughes Aircraft Co.
Sarah Ratner, Public Health Research Institute, New York City
William H. Riker, University of Rochester
Hans Ris, University of Wisconsin
Herbert E. Robbins, Columbia University
Abraham Robinson*, Yale University
Glenn W. Salisbury, University of Illinois
Rudi Schmid, University of California, San Francisco
Theodore W. Schultz, University of Chicago
William R. Sears, Cornell University
Irwin I. Shapiro, Massachusetts Institute of Technology
Robert G. Shulman, Bell Laboratories, Inc.
Leon T. Silver, California Institute of Technology
Elias M. Stein, Princeton University
DeWitt Stetten, Jr., National Institute of General Medical Sciences
Philip Teitelbaum, Princeton University
Howard M. Temin, University of Wisconsin
Bert L. Vallee, Harvard Medical School
Kenneth M. Watson, University of California, Berkeley
John S. Waugh, Massachusetts Institute of Technology
George W. Wetherill, University of California, Los Angeles
Benjamin Widom, Cornell University
Jacob Wolfowitz, University of Illinois
James B. Wyngaarden, Duke University School of Medicine

* Elected posthumously.

The newly elected foreign associates bring the total to 138. They are as follows:

Pierre Aigrain, University of Paris
Aleksandr Braunsteyn, Academy of Medical Sciences, Moscow
David Catcheside, Australian National University
John G. D. Clarke, Cambridge University, England
Alan Hodgkin, Cambridge University
Ryogo Kubo, University of Tokyo
John McMichael, Royal Postgraduate Medical School, England
Jacques Oudin, Pasteur Institute, France
George Porter, Royal Institution of Great Britain
Igor Shafarevich, Moscow State University
Jan Tinbergen, Netherlands School of Economics
Nikolaas Tinbergen, Oxford University, England

failed. After Summerlin's work came out, she tried again, for a year, but was not successful.

Steven Codish of the Peter Bent Brigham Hospital in Boston is an investigator whom Summerlin has, in the past, referred to as someone who could duplicate his results. But Codish says he has "no information one way or the other" on the subject. Almost 2 years ago he did some experiments with skin transplantation. Out of a group of about 500 mice, there were two that looked as though they had accepted grafts of foreign skin. Summerlin, Codish recalls, was enthusiastic. Codish was doubtful. There was no histologic evidence to confirm a successful transplant, merely the presumption of a take based on gross appearance. He never published his results on the grounds that there was nothing to publish. Nor has he attempted to pursue the matter. Codish is a surgeon who says, "I have not been in the lab in a year."

Someone who has pursued the matter is Sir Peter Medawar, immunologist, Nobel laureate, and a member of the board of scientific consultants at Sloan-Kettering. Medawar and colleagues in his laboratory in London tried to repeat Summerlin's mouse experiments but were not able to. It caused consternation at Sloan-Kettering. Summerlin apparently attributed the failure of the Medawar group to repeat his experiments to technical difficulties. Some evidence that this might be the case was reported in a letter to the editor of *Science*, dated 17 September 1973 and published in the 2 November issue. Michel Prunieras, a French investigator on sabbatical leave at Sloan-Kettering, wrote that using the "classical technique of Billingham, Brent, and Medawar," he was unable to get a successful take of a graft. However, Prunieras went on to say that:

> In the last two series [of six experiments], in which we made some technical adjustments suggested to us by Summerlin, five black mice are carrying patches of white hair 60 to 75 days after transplantation. To us, this is confirmation that failure in attempts to duplicate Summerlin's results can be ascribed to simple technical difficulties.

One can hardly imagine that the letter, coming out of Sloan-Kettering as it did, smoothed feelings.

Medawar's interest in the tissue culture experiments apparently had some indirect influence on the course of events to the extent that persons at Sloan-Kettering were anxious to persuade him of the validity of their finding. In October 1973, Medawar came to New York for a meeting of the institute's board of scientific consultants. Summerlin says that Good and other members of the staff discussed the research with Medawar, who is widely regarded as one of the world's great immunologists, but he was not invited to participate in those discussions.

In March, just before the alleged mouse painting incident, Medawar again came to New York for a meeting of the board. According to sources close to the situation, Medawar and his colleagues had said they would not believe until they saw a white mouse with black patches. Summerlin's friends speculate that *if* the mouse painting incident did occur, it must have been in some desperate effort to convince Medawar, at least temporarily—long enough to gain time to figure out why the experiments were not working any more.

(The mouse that Summerlin had been showing visitors to his laboratory was a black animal with a patch of white fur, a patch Summerlin says is the result of a successful transplant between unrelated animals. However, doubters say that it is possible that the patch represents something else, a genetic cross between a white and black animal, for example. There are known cases of persons believing they had successful transplants between incompatible animals who discovered later that there had been an honest mix-up in the laboratory and that animals they had thought to be genetically incompatible were, in fact, related. That can happen when you are talking about white patches on black animals. But it cannot happen in the reverse. Genetically, it is not likely one would get black patches on a white mouse. Therefore, there are only two ways to get black patches on a white mouse: skin grafting or paint. A successful transplant of skin from a black mouse would be pretty persuasive evidence that the Summerlin-Good idea is correct.)

Whatever the truth, Medawar went back to England unconvinced. In an interview with the *New York Times*, published on 18 April, Medawar is quoted from London as saying, "I have dropped this work in frustration and disappointment. We feel we have not gotten sufficient details of the techniques."

The inability of several persons to duplicate Summerlin's experiments is by no means prima facie evidence that they were faked. There are a number of other possible explanations. It is unusual for a person to be unable to duplicate his own work, but it is by no means rare. Some minor change in technique could do it. An undetected viral infection in the tissue culture could be responsible. There are many speculative possibilities but, whatever the answer, last fall things began to look dubious and people at Sloan-Kettering were concerned.

Good put graduate student John L. Ninneman to work trying to repeat Summerlin's work. He even sent him to Denver to Barbara Jacobs' laboratory to learn technique. But Ninneman could not repeat the work.

A paper bearing the names of Ninneman, Summerlin, and Good was prepared for the journal *Transplantation* in which efforts to repeat the experiments were described. The present status of that paper is unclear. Earlier, during the fall Good asked Summerlin to withdraw a paper from the *Journal of Experimental Medicine*, a paper to which Summerlin referred in his February interview with *Medical World News*. In the magazine's 15 March issue, it says, "Soon he will publish evidence in the *Journal of Experimental Medicine* showing that skin, the body's largest organ and the one most rapidly rejected after transplantation, loses immunogenicity in culture."

Both Good and Summerlin decline to comment on these matters.

In the minds of many observers the Summerlin case is really a Sloan-Kettering affair with Robert Good on center stage. Good is an exceptionally imaginative scientist whose contributions to immunology have been substantial, and even his critics acknowledge he has a genius for teaching. He is also a man of tremendous energy, drive, and ego. He has a talent for promoting himself and his ideas that is enviable or deplorable, depending upon one's point of view. When his picture appeared on the cover of *Time* a little more than a year ago, a few of his colleagues applauded him for getting science to the public, but many more were resentful.

There is no doubt that Good is a controversial man. In January 1973 he moved to New York from Minneapolis to head a controversial institution. One longtime Sloan-Kettering staff scientist assessed the state of the institute this way: There was a feeling within the scientific community that

the quality of research at Sloan-Kettering was deteriorating. Nevertheless, it was constantly in the news, often with claims that were more dramatic than substantive. Furthermore, at a time when many institutions were losing money, it looked as if we were not being hurt at all. It made people resentful.

Good was brought in, at an income reported to be in the $100,000 range, to turn Sloan-Kettering around. The first thing he did was reorganize the research program around what he considers the most important approaches to cancer. Now, instead of having disciplinary departments, they have eight divisions, including ones on cell surfaces, oncogenic viruses, immunobiology, and communication of scientific information. "This reorganization is intended to impart more flexibility, and focus attention on the free interaction of investigators in different disciplines . . .": says a document describing the administrative changes. Several persons think that whatever the merits of the reorganization, free interaction is not among them. One senior Sloan-Kettering scientist complains that he and many of his colleagues who were there before Good came now feel like second-class citizens in the face of the many new scientists Good has brought to the institute during the last several months. And, he says, access to Good is limited to those few persons close to him who are working in areas in which he has been involved. "It is," he concludes, "as though there were two institutions in the same building." Even Summerlin, presumably one of the in-crowd, felt the environment at Sloan-Kettering was somewhat chilly. Compared with what he thought was a friendly environment in Minneapolis, he finds Sloan-Kettering extremely "isolating."

Sloan-Kettering these days is not a happy place. It is rich, and getting richer, but not happy. In 1972, the research institute and its affiliate, Memorial Hospital, received about $7 million in government grants and contracts. According to the National Cancer Institute, the Memorial Sloan-Kettering Cancer Center will get about $20 million in fiscal 1975, more than any other cancer center in the country. For that amount of money, people are going to expect to see results, whether in clinical or basic research. If the present crisis generated by the Summerlin case is any indication, it appears that a high pressure environment that drives individuals to exaggeration and fosters hostility is not ideal for the kind of achievements in research that Good, like everyone else, would like to see. Sloan-Kettering may want to say what it is doing.

—BARBARA J. CULLITON

Breeder Reactor Debate: The Sun Also Rises

In effect, the AEC may be involved in another Washington cover-up—this time an attempt to cover up the sun.
—BARRY COMMONER, in a speech in New York, 30 March 1974.

Barry Commoner, the militant environmentalist and banner bearer of the Scientists Institute for Public Information (SIPI), was complaining that the Atomic Energy Commission (AEC) had withheld important information from the public. The AEC, Commoner said, apparently had suppressed an optimistic report on the potential usefulness of solar energy that undermined the AEC's case for proceeding with its nuclear breeder program.

The AEC promptly and stoutly denied that it had done any such thing, maintaining that the report in question had been available in the agency's public documents room in Washington since last December, shortly after the report was written. Commoner nevertheless repeated the accusation on 3 April in Los Angeles and again on 23 April at a Washington news conference called by SIPI. There is little reason to doubt the AEC's denials, but, as it happens, the AEC has sat upon or bowdlerized enough internal reports that were inimical to its interests over the years to lend plausibility to SIPI's allegations. And plausibility seems to be what this curious squabble is all about.

During the past couple of years a number of environmental groups that are either critical of or clearly opposed to nuclear energy—SIPI among them—have gradually intensified their advocacy of solar energy as a credible and preferable alternative to the atom. This is not an easy case to make, but if a federal agency covers up an optimistic report on solar power or other alternative technologies, then the agency must be worried that the environmentalists are on to something big. And therein lies a measure of plausibility to be gained for the alternative vision of a "clean energy" economy.

Hand in hand with the growing advocacy of solar energy and other renewable resources has come a subtle shift in criticism of nuclear power. Two years ago few nuclear critics of any prominence would openly acknowledge that his or her opposition was directed toward an ultimate goal of closing down reactors and, in effect, of abandoning the technology. Instead, the pattern was one of illuminating doubts among experts and of drawing much-deserved attention to uncertainties in the technology, such as the questionable adequacy of emergency cooling systems.

These days, perhaps in emulation of consumer advocate Ralph Nader's bold frontal assault on nuclear energy, there is a growing tendency among environmental activists to press not just for full and open debate but for a moratorium on conventional nuclear plants and death for the liquid metal fast breeder reactor (LMFBR).

Commoner, for instance, says he thinks that reliance on the breeder would be nothing less than a "disastrous mistake." It is a risky position to take, one open to accusations of blind opposition to progress in the manner of latter-day Luddites. Hence the search for a credible alternative.

The flap over the AEC's solar energy report is a direct outgrowth of the larger controversy swirling around the breeder program. Last year, through a suit brought by SIPI, the AEC was obliged to throw out its first attempt to write an environmental impact statement on the breeder program, as required by the National Environmental Policy Act (NEPA). The AEC said it was sufficient to discuss only the impact of the lone demonstration plant