engineering are available. This, in most cases, would mean innovative compensatory programs, which have higher per pupil instructional and support costs for minority students than do current programs for nonminority students. Such programs might also have to be longer, with more learning opportunities provided for minority students within the educational system itself.

The major thrust of a national effort should be to make the development, conservation, and effective utilization of minority citizens' scientific and engineering talents an integral part of a comprehensive national manpower policy. Such an effort should be viewed within the context of federal programs to aid the disadvantaged and broaden the base of opportunity for education and training. Minority scientific and technical personnel comprise a critical and scarce national resource that can and should be nurtured and strengthened by sound policies, programs, and projects.

References and Notes

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4. From an internal National Science Foundation memorandum.
5. A 1963 follow-up study of recipients of science and engineering baccalaureates showed that only 35 percent of them entered the work force directly with that degree [L. M. Sharp, *Education and Employment: The Early Careers of College Graduates* (Johns Hopkins Press, Baltimore, 1970)].
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NEWS AND COMMENT

The Sloan-Kettering Affair (II): An Uneasy Resolution

William T. Summerlin spent the night of Monday, 25 March, in New York on a cot in his laboratory at the Memorial Sloan-Kettering Cancer Center. It had become his habit to spend Monday and Thursday nights in the lab. He needed the time, he has said, to complete grant applications and get other paper work done.

That night Summerlin slept poorly, as he often had for several months. On the morning of the 26th, he got up at 4 a.m., shaved, and went upstairs to the animal room to check on his mice. Later that morning he would take some of them with him to a meeting with his boss and erstwhile mentor, immunologist Robert A. Good, president of the Sloan-Kettering Institute for Cancer Research.* It would be his downfall (*Science*, 10 May).

For 3 years, Summerlin and Good had been exuberant over experiments conducted when the two were in Minneapolis at the University of Minnesota—experiments that indicated that skin and other organs, when maintained for a time in tissue culture, lose their normal ability to provoke an immune response. If their observations proved true, it theoretically meant that one could transplant organs between genetically mismatched individuals

* The Memorial Sloan-Kettering Cancer Center is a composite of two related institutions, Memorial Hospital and the Sloan-Kettering Institute for Cancer Research.

without worrying about rejection. It was a stunning notion.

But lately, things had not been going well. No one, including a research fellow named John Ninnemann in Summerlin's own laboratory, could repeat the Minnesota experiments. Within Sloan-Kettering itself and the immunology community at large, that failure was raising suspicions. Good and Ninnemann had prepared a paper for *Transplantation* on the negative results of Ninnemann's efforts. Summerlin's name was to be included on that paper. He and Good were meeting to talk about the paper and the problems with the research. It was crucial that Good be convinced that morning that all was not lost.

Summerlin had been at Sloan-Kettering since April of 1973 but had spent little time in his laboratory, pressed as he was by clinical duties which included setting up Memorial Hospital's first dermatology unit. But by January 1974 he was devoting more time to research, and during the winter months he had transplanted cultured skin to what he estimates to be about 50 mice. Now, he was going to present the best of that lot to Good as proof that things could be salvaged.

After making a preliminary check on the animals, Summerlin went back downstairs to see if any of his assistants had arrived for work. He had asked a couple of them to come in early to help him change the animals' surgical dressings and chose the ones to be exhibited.

It was about 5 a.m. when Summerlin returned to his laboratory where he was greeted by a surprise. As a lark, his secretaries had brought in an elegant breakfast of crepes and champagne. It became a festive dawn, and Summerlin joined his staff in a couple of glasses of champagne.

Then, they prepared the animals. The mice were white (strain A-albino) that had received transplants of cultured skin from black mice (strain C57 black). The new skin had a dull, grayish look, Summerlin recalls. He chose 18 mice to show Good and put them in cages on a cart. Shortly before 7 a.m., Summerlin and the mice were in an elevator, heading for Good's office on the 13th floor.

That is when it happened.

Summerlin whipped out his felt tip pen and painted the skin of the two mice on top. During the meeting he showed them to Good as evidence of a successful graft. But Good did not pay much attention and merely gave the animals a cursory glance. He had the *Transplantation* paper, from which Summerlin would withdraw his name, on his mind.

Good never caught on to the painted mice but James Martin, a senior laboratory assistant, did. Returning the animal cages to their places, Martin noticed that two of the black grafts looked "unfamiliar." He applied alcohol to the skin and discovered that the color washed away. Martin told William Walter, a senior research technician, what he had found. Walter told Geoffrey O'Neill, a visiting research fellow. O'Neill passed the news on to John Raaf, who, like Ninnemann, was a research fellow. Earlier that morning, Summerlin had shown these same two mice to Raaf, presenting them as successful transplants. Raaf told Good and Lloyd Old, second in command at Sloan-Kettering, what had happened.

By noon, Summerlin was back in Good's office where Good and Old were waiting.† He admitted to the paint job and was immediately suspended, pending further inquiry.

Ordeal in Public

Thus began the ordeal of William Summerlin and Robert Good, an ordeal that would be played out before all the world. Surely neither Summerlin nor Good sought to let the public in on this strange event but it was bound to be found out. And it was. Right after Easter, Barbara Yuncker broke the story in the New York *Post*.

Why on earth would any rational man paint a mouse? The answer, of course, is that he would not. Summerlin was "not himself" at the time of the incident. Nor, apparently, was he himself for the last few months, during which he also misrepresented data on corneal grafts in rabbits.

Lewis Thomas, president of the cancer center, in a formal statement on 24 May had this to say:

> The Trustees of the Memorial Sloan-Kettering Cancer Center have decided that Dr. Summerlin's relationship with the Center must be terminated. However, after discussion with Dr. Summerlin, his wife, and his personal psychiatrist, I have concluded that the most rational explanation for Dr. Summerlin's recent performance is that he has been suffering from an emotional disturbance of such a nature that he has not been fully responsible for the actions he has taken nor the representations he has made. Accordingly, it has been agreed that the Center will provide Dr. Summerlin with a period of medical leave on full salary [$40,000], beginning now, for up to 1 year, to enable him to obtain the rest and professional care which his condition may require.

Late last month, *Science* spent several hours talking about the "Sloan-Kettering affair" with Summerlin and his wife, Rebecca, in their home in Darien, Connecticut. He was asked why he had painted the mice. He said he was not entirely sure but that he had discussed the question with his psychiatrist. Summerlin said that he was in no position to give a solid, medical answer to the question but that he did have his own feelings about it. He was tired. For months he had been working too hard. He was not sleeping well, nor was he eating properly. Generally, he was on the verge of collapse. He admitted that on the morning of the 26th he had had a couple of glasses of champagne, not enough to make him intoxicated but enough, perhaps, to slightly affect his judgment. And he was angry, very angry at Good. He had been for a long time, since the fall when he began to feel that Good, whom he idolized, was turning against him, rejecting him because he was having trouble in the laboratory. Perhaps, says Summerlin, his painting of the mice was a kind of challenge, a gesture to test Good's attention and acumen, as if to say, "if you're so smart, pick out the phony mice." Perhaps it was a sort of game. If so, it was a deadly game that has hurt them both.

The mouse-painting episode brought into the open a series of problems—scientific doubts and personal antipathies—that had been stirring at Sloan-Kettering for some time. And it forced a recognition and uneasy resolution of some of those problems that might otherwise, by the institution's own admission, have lain dormant much longer.

Following the mouse painting, Good appointed a committee of Summerlin's peers within the institute to "review the veracity of Dr. Summerlin's scientific work and his alleged misrepre-

† Summerlin told *Science* that, at about 10:30 on the morning of the 26th, he himself called Good's office, having decided to confess what he had done. He was told, he says, that the earliest he could see Good again would be late afternoon. By then, events had already caught up with him.

sentations concerning that work."‡

On 24 May, the committee's findings were made public at a press conference at the institute. The conference, at which Thomas, Good, and Chester Stock, chairman of the review committee, would be present, was to begin at 10 a.m., but the doors to the press room opened at 9 so that the 15 or so invited science writers could read the 22-page report ahead of time. *Science* arrived about 9:15 and was asked for identification by an armed guard at the head of the corridor leading to the press room. Inside, several reporters were seated at long tables, quietly reading through what the committee had to say. They looked like students cramming at the last minute for an exam. There were none of the usual boisterous greetings. People read quietly and spoke in low tones.

Then, there was a bit of a commotion out in the corridor. One of the scientists from Summerlin's laboratory wanted to sit in on the press conference. A guard came and stood resolutely at the door. Some of the reporters went out into the corridor to see what was going on. "I want to hear what *they* are going to tell you happened," she said. Eventually, she was persuaded to go away. Another guard came to help out. During the press conference itself, the doors to the room were locked from the outside. Thomas and Good said later, when asked, that the lady was "disruptive."

Within the confines of the press room, however, no one was. The press displayed an unusual measure of restraint, asking direct and pointed questions but with none of the aggressiveness or hostility that some had anticipated. Thomas, Good, and Stock tried to put as positive a light on things as possible but appeared to be answering with reasonable candor. The questioning went on for 2 hours. In the end, of course, there were still unanswered questions. There probably always will be.

The committee in its review focused on two aspects of Summerlin's work—the grafting of cultured skin, particularly to mice, and the grafting of cultured human corneas to rabbits. On both counts it found Summerlin guilty of "misrepresentation." They never used the word "fraud"—not in print anyway, or in official statements.

If one sets aside the clear-cut fact of the mouse painting, there remains the question of Summerlin's previous mouse work, especially that which was carried out in Minnesota and which formed the basis of both Summerlin and Good's public pronouncements of a revolution in immunology. The committee found a "lack of properly organized and analyzable data" regarding the Minnesota work. Although there were, presumably, experiments on hundreds and hundreds of mice, "One or two mice bearing grafts of a different color is all that any witness appears to have seen at any one time," its report states.

The Old Man a Hybrid

There is is only one graft-bearing mouse from the Minnesota days that is still around, a brown creature with a patch of white hair that is called the "Old Man." Originally, the Old Man was thought to be a pure C3H mouse, a member of an inbred strain of brown mice. Were that so, for her (it is a female animal in spite of its name) to accept a graft of cultured skin from a white mouse would mean that normal histoincompatibility or immune barriers were overcome, and that, by culturing, the white skin had lost its immunogenicity. Edward Boyse, one of Sloan-Kettering's experts in mouse genetics, tested the Old Man. She is not a pure C3H but a hybrid. The fact that she accepted and retains a graft of skin from a white mouse has no immunologic significance at all.

This does not mean, however, that Summerlin knew all along that the Old Man is a hybrid or that he deliberately hybridized his mice. No one ever formally accused him of that, and it is presumed it was an accident. At the time Summerlin was working at the University of Minnesota, other investigators on the same floor were working with hybrids. A mouse mix-up apparently occurred.

After learning about the Old Man, Summerlin wanted to know whether other C3H mice he had brought to New York from Minnesota and that were in his Sloan-Kettering laboratory were also hybrids. On 6 May, Summerlin's lawyers wrote to Stock, asking that those mice also be tested by Boyse, in order that Summerlin might be able "to assure his colleagues of his sincerity in representing these mice and others as purebred mice. . . ." As it turns out, those mice were never tested. The committee, Stock told *Science* recently, saw no purpose in it since none of those animals had been grafted and therefore had nothing to do with the immediate question. Furthermore, Stock said, on 10 May, when Summerlin met with the committee for 8 hours, he withdrew that request. Summerlin disputes this. There are no records on the subject; the committee's inquiry was, Stock says, "informal"—no tape recorder, no stenographer.

Although Summerlin's behavior with regard to the mouse work is something the committee held against him, it was, in their opinion, secondary to his behavior with regard to claims of successful grafts of cultured human corneas in rabbits. After reviewing a wealth of information on the subject, they said, "The only possible conclusion is that Dr. Summerlin was responsible for initiating and perpetuating a profound and serious misrepresentation about the results of transplanting cultured human corneas to rabbits." Summerlin acknowledges that the information he gave out about the rabbits from last fall on was wrong, but he denies willfully promulgating untruths. He says it was all a misunderstanding.

The issue involves experiments in which Summerlin was collaborating with Peter Laino and Bartley Mondino, ophthalmologists at New York Hospital. According to one experimental protocol that the three of them worked out in June 1973, Laino and Mondino would perform bilateral corneal transplants on rabbits, first grafting a fresh human cornea to the left eye of a rabbit and, subsequently, a cultured cornea from the same human donor to the right eye of the same rabbit. As it turns out, Laino and Mondino, both practicing clinicians who did their work on experimental animals at nights and on weekends when they could, never actually did any double eye experiments. At no time did Summerlin ever have really close contact with Laino or Mondino; in fact, just the opposite seems to have been the case. Nevertheless, he presumed that they were following the protocol for bilateral transplants after he saw some rabbits (they were housed at Sloan-Kettering) that had clear right eyes and opaque left eyes. Under the double eye protocol, this observation would mean that transplants of cultured corneas, to the right eye, were successful,

‡ Good appointed six men to the committee, five of whom had been at Sloan-Kettering long before he and Summerlin came in 1973. They were: Chester Stock (1946), chairman, Edward Boyse (1961), Joseph Burchenal (1946), Bayard Clarkson (1958), and Martin Sonenberg (1949). The sixth was John Hadden, a contemporary of Summerlin's, who also joined Sloan-Kettering in 1973. On 8 May he was asked to withdraw from the committee by Summerlin's lawyers who claimed he was withholding information that was to their client's advantage. He did so the next day, without agreeing to the merit of their objection. By then, deliberations were almost complete.

whereas transplants of fresh corneas, to the left eye, were not.

Armed with no more information than his eager presumptions, Summerlin went around telling people about the astoundingly successful rabbit experiments. Whether he did so deliberately, this is, with full knowledge of the fact that Laino and Mondino had never gotten to the double eye experiments, may never be clear.

Ninnemann, who had been unable to reproduce Summerlin's skin transplant work, testified before the review committee that he learned from the ophthalmologists that they were doing only single eye experiments and that the cloudy left eye represented an unsuccessful rather than a successful experiment. He further told the committee that he informed Summerlin of this early in October. Raaf told the committee the same thing. And Summerlin denies it, saying they never told him any such thing.

In this case, it is a matter of one man's word against another's. Apparently neither of the research fellows got along well with Summerlin, who was particularly at odds with Ninnemann. Summerlin says Ninnemann was unwilling to communicate with him, going directly to Good instead. Ninnemann says it was Summerlin who made communications impossible. It is one of those situations that is almost impossible for an observer to figure out. However, 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. It is also clear that the review committee chose to accept Ninnemann and Raaf's version of the story. There were two of them against Summerlin, whose credibility was seriously in doubt. And it is clear, in any case, that Summerlin had no business making the claims he did in view of the fact that he did not know what he was talking about. As far as Sloan-Kettering is concerned, that amounts to "irresponsible conduct."

Basically, Summerlin acknowledges the validity of the point but places part of the blame for the whole unfortunate mess on Good. In a formal statement Summerlin said:

> My error was not in knowingly promulgating false data, but rather in succumbing to extreme pressure placed on me by the Institute director to publicize information regarding the rabbits. . . . In fact, for a considerable period of time I have been under extreme personal and professional stress which led to both mental and physical exhaustion.

He cites what he calls pressure to reproduce his experimental work, coupled with the responsibility of heading a laboratory while serving as chief of a new clinical service. It was, quite simply, too much for him. In retrospect, he admits this, as does Good who concedes that he never should have brought Summerlin in as a full member of the institute (the equivalent of being a full professor) when he did. Summerlin, at 35, would have been better off with fewer responsibilities.

Is this bizarre affair representative of science? It is a question a lot of people have asked; there is unlikely to be a consensus on the answer. However, Thomas, for one, thinks the answer is no. "Summerlin was under great stress," he observed at the press conference, "as are we all. His illness came upon him in this environment."

It does seem that the Sloan-Kettering affair has more to do with the personalities of the individuals involved than it does with "science" in the abstract. Good is aggressive, flamboyant in his way, with a driving ego. Summerlin, as his wife said, "followed Good like the Pied Piper," even to the point of adopting some of his characteristics, including a penchant for getting up at dawn. In his identification with Good, Summerlin resented the feeling he was being rejected, of having to make appointments instead of "just swinging in" to Good's office at will. Good says he was in no way rejecting Sunmmerlin, that he had as much, or more, contact with him as with anyone else.

It does not really matter. The fact is that somewhere, somehow, Summerlin and Good failed to get together over issues of considerable importance. Good failed to give Summerlin the scientific guidance it is now apparent was warranted. The two of them continued to speak positively about their work long after they should have quieted down. And so, two mice were painted and there was trouble.

The review committee officially acknowledges that Good bears some responsibility in all this, though it takes him to task ever so gently:

> The committee feels that Dr. Good shares some of the responsibility for what many see as undue publicity surrounding Dr. Summerlin's claims, unsupported as they were by adequate authenticated data. Dr. Good was slow to respond to a suggestion of dishonesty against Dr. Summerlin at a time when several investigators were experiencing great difficulty in repeating Dr. Summerlin's experiments. However, the usual presumptions of veracity and trustworthiness on the part of coworkers would have made it difficult for anyone in Dr. Good's position to entertain such a notion.

Besides, the committee said in essence, with his new job as director, Good was a very busy man—too busy, perhaps.

(Although it would be an exaggeration to call Summerlin a sacrificial lamb in this whole affair, there is evidence that Sloan-Kettering was not about to sacrifice Good for his share in all this. One consistently is told, "Good is too valuable a scientist. We can't let him be dragged down by this.")

Good, of course, did not want to believe that Summerlin's work, which had such exciting implications, was false. Since 1971, he had trusted Summerlin unquestioningly and even wrote of him on 18 February 1972, "I must say I am deeply impressed [by him]. I am certain he is honest to the core." Now, he says, "I just don't know why it took me so long to disbelieve Summerlin." He admits, however, that there may still be something there, as far as the research is concerned. "You have to separate the science from the man in the present situation," he says. "We have to look at the science further. At the very least, I continue to be impressed with his clinical observations."

The Sloan-Kettering affair has been an agonizing personal ordeal for both Summerlin and Good but it may be one that will have some positive effects, once the wounds heal. At the moment, Summerlin is resting in Connecticut, "getting to know [his] family again." and trying to sort himself out. Though he would not again pick this way to do it, he now says he has a "rare opportunity to get to know himself and his professional needs better than many people ever do," and early enough in his career to anticipate making use of what he learns. He told *Science* that he expects to get over his present intense dislike of Good and hopes they may be reconciled one day. Eventually, he plans to return to medicine and science.

Good says that he is "sadder and wiser" for this trying experience which will, inevitably, affect his behavior in the future. The whole situation has certainly provided many people in addition to Summerlin and Good a lot to think about.—BARBARA J. CULLITON