

For Charlie's, Patricia, Nina, Philip, Lisa and their Families

MY IDEA OF WHY I FAILED TO WIN A NOBEL PRIZE
Michael Heidelberger

The tale that I am about to tell is not a matter of conceit. I have been nominated several times for the prize and have been told innumerable times that I should have had a Nobel Prize--and my reaction has been that I would rather have my friends say I should have had one than to get the prize and have people say behind my back, "he got it but didn't deserve it." Moreover, I've received many compensating awards, the respect of my fellow scientists, and the abiding satisfaction of knowing that I have given immunology a few small pushes that have helped propel it into the persuasive and pervasive scientific position it holds today. I bear no resentment to the Nobel Committees and my sympathies are with them, for they cannot possibly reward more than a minute percentage of the vast amount of solid scientific discovery that has been increasing almost exponentially since the Prizes were established. So let me get on with my story.

In 1926 James B. Sumner, Professor of Physiology and Biochemistry at Cornell University, published the first astounding report that an important enzyme, urease, which breaks down urea to ammonium carbonate, could be isolated from jack beans in pure crystalline form and was a typical protein instead of a mysterious substance of unknown structure, as all enzymes were thought to be at that time. In full recognition of the importance of his finding he took a boat to Sweden and made the rounds, telling members of the Nobel committees what he had done and that they owed him a Nobel Prize. This egotistical direct approach so angered the Swedes that for many years they refused to consider him for an award. In the meantime my friend Arne

Tiselius, who had become director of the Biochemical Institute of the University of Uppsala, asked me to go with him to see Professor Liljestrand, the powerful secretary of the Nobel committee, of which Arne was also a member. Professor Liljestrand's consent was considered essential before anyone ^ocould be approved for an award. Accordingly we went to see the gentleman, but remembering Sumner's fate, I was very reticent about my own work, leaving it to Arne to do prop^aganda for me if he wished to. I had the feeling that the interview was going badly and this was confirmed when it had no result. But there is a sequel: years later, when Arne asked me whether they should give a prize to Wendell Stanley for crystallizing tobacco mosaic virus, a corn cob-shaped protein, and to John Northrop, who, with Kunitz, had crystallized several enzymes and their inhibitors, I said, "of course, but how can you do it without giving Sumner a share?" So, some twenty years after Sumner's epoch-making discovery, justice was done and I had the satisfaction of feeling that I had at least helped someone else get a Nobel Prize.

Nov. 23, 1984.