Chasing Viruses

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When my son was young, I coached his basketball team, and I was pretty good at it. I thought I could be a basketball coach if science did not pan out, although I have a good friend who is a basketball coach and he keeps telling me it is a hard life. I also had a dream that I might become a long-distance truck driver. I have always enjoyed long car journeys.

I have gone back in my mind trying to analyze why I chose to join Harvard's New England Primate Research Center 28 years ago. I guess I saw it as a place with opportunities to do good work and where I could perhaps be a big fish in a small pond. It is a relatively small institute, and there were not many people with my type of expertise in molecular virology when I joined. I am also big-city averse, so I liked the setting. We are located 25 miles away from the major metropolitan area of Boston.

I got into the AIDS field in 1985. At the time, we were studying herpes viruses in monkeys. The animals in one of our colonies were suffering from an immunodeficiency disease of unknown cause. I recall one day in 1981, when our scientific director at that time was walking in the hallways carrying the *New England Journal of Medicine* report of a disease among gay men in California. He was telling anyone who would stop and listen that the disease looked a lot like what our monkeys had. Of course at the time no one had any idea of how important a research area AIDS was going to be, but I decided on that day to devote some of my time to looking for the virus that was causing the immunodeficiency in our monkeys.

In 1984, my laboratory discovered a new type of retrovirus in these monkeys, but it was not the one causing the disease. In 1985, we reported the discovery of simian immunodeficiency virus (SIV), the monkey equivalent to HIV. That was a year after the human virus had been discovered. It was subsequently reported that one particular SIV strain was the likely origin of HIV type II.

The discovery of SIV was a life-changing event for me. My lab and I got thrust into a problem of enormous human importance almost by chance. Our research has been

very exciting ever since then. But the real excitement will come if and when there is a break-through discovery for the development of an AIDS vaccine. I don't think it has yet been made, and when it is, we will recognize it pretty quickly. I would like to play a part in the discovery process. That's what drives me.

The pharmaceutical industry has not been a key player in the development of an AIDS vaccine. As a result, the National Institutes of Health (NIH) has become involved in product development, clinical testing, and manufacturing of potential vaccine candidates—all activities that cost enormous amounts of money. Like many other scientists, I feel strongly that this strategy is an error. There are still major scientific hurdles that we need to overcome before we can produce a successful vaccine. NIH should be financing the necessary discovery research. I believe the AIDS field has suffered from having too much money go toward product development rather than basic research.

I am pessimistic that any of the products currently in the pipeline for an AIDS vaccine will prove effective. There are too many obstacles that cannot be overcome given our present level of knowledge of the virus, and I don't see any answers around the corner for any of these problems. But we should definitely not give up. It is enormously important to develop a vaccine for AIDS.

HIV has an incredible ability to avoid the host's defenses and eventually renders them ineffective. I am constantly amazed at the complexity of this virus, especially when I think that it contains such a small genome.

I fell into virus research somewhat by chance. I was a chemistry major at Boston University. I liked chemistry and it came easily to me, but I wanted to work on things more related to biology, so I joined a biochemistry lab at Michigan State University for my graduate studies. As soon as I started learning about viruses, I found them fascinating. I thought they seemed like a manageable system to study. You could change every nucleotide in their genome sequence and study what effects the change would have.

Today I spend 30% of my time being director and the rest of the time running my own research program. I became director in 1999. The new role came easily to me because of where I am. I am very comfortable here, and I understand what our mission is and what we need to do.

-As told to Laura Bonetta, a science writer based in Bethesda, MD.