



# A conversation with Donald Seldin

**T**oday we are joined by Donald Seldin from The University of Texas Southwestern Medical School (Figure 1). Seldin served as the chairman of medicine at UT Southwestern for nearly four decades and transformed the department and the school from a grouping of ramshackle army barracks to a world-class medical center, host to Nobel Laureates and members of the national academies.

He was also a major figure in the emergence of nephrology as a legitimate discipline, and in this role he was one of the founders of the American Society of Nephrology and served as its second president. Among his many attributes, Seldin has been described as one of the most magnetic and charismatic mentors academic medicine has encountered. The full interview can be seen on the *JCI* website, <http://www.jci.org/kiosk/cgm>.

*JCI:* Can you tell me a little about your path to medical school?

Seldin: I grew up in the midst of the Depression, which was particularly serious in New York, and I had to think of ways to earn a living following graduation; friends of mine who were studying philosophy were in terrible straits because of the financial crisis. I had to think of another discipline which would be of interest to me where I could forge a reasonable career, and I thought medicine would be exciting. So during the latter part of my attendance at NYU, I took a number of science courses so as to qualify for application to medical school, and I applied to a number of medical schools and was accepted at Yale.

*JCI:* When you were at Yale, how did your interest in laboratory science develop?

Seldin: Yale was a very interesting school at the time. At NYU, we used to have examinations daily, then surprise examinations weekly, then examinations monthly: exams were the rule of the day. When I came to Yale, there were only about 49 students in a class. There were no exams for two years. Attendance was not taken, and you could come or not as you pleased. This kind of laissez-faire program was at first startling to me, but I became very much attracted to it. It allowed me to read what I wanted, to explore things outside the standard curriculum. And when I entered the third year in medicine, I met John Peters. He was the head of the metabolic section, one of the towering figures in American medicine.

And I began to do research in the third year while I was taking my clinical clerkship. I chose to examine diabetic acidosis. We had patients in abundance. This department had a long history of studying diabetes and acidosis, and it seemed to me an exciting area. So a colleague of mine and I embarked on a study of patients with diabetic acidosis, exploring glucose and electrolyte metabolism in this disorder.

*JCI:* After your medical training you served as a captain in the army in Germany. Can you tell us a little bit about what that experience was like?

Seldin: I was assigned to the medical service at Munich, Germany, and in the course of my service I was summoned to a trial at Dachau, a concentration camp some 30 or 40 miles outside Munich. There a Nazi medical officer was on trial for having been associated with the death of some 30 or 40 inmates. I was summoned as an expert witness. My role was to testify about the status of liver biopsy at that time and shortly before that during the war. Apparently, these patients, inmates of Dachau, had died following liver biopsies. The Nazi medical officer refused American legal counsel and functioned as his own defense attorney. The questions the military court raised were whether liver biopsy was justified in terms of informed consent and whether liver biopsy was indicated for therapeutic or prognostic purposes. I was asked to testify about these matters, and I indicated that liver biopsy, at that time, was an experimental procedure and that informed consent would be necessary. I also said that the therapeutic or prognostic information that would be forthcoming would be trivial: liver biopsy was not an important guide to therapy at the time. The Nazi medical officer then challenged my status as an expert witness. He asked me what I knew about the status of liver biopsy in 1943 in Germany. And I told him: very little. He challenged my expertise in commenting on liver biopsy in view of the fact that no information about German practice was available in the US. I pointed out that liver biopsy had been developed largely in the United States, that the advances in liver biopsy were at the forefront there, and that one could infer from what was known about liver biopsy that to date it had no therapeutic implications. The officer then probed my competence by asking a series of techni-

cal questions: "How do you fractionate the plasma proteins? How do you do a BSP?" And he wanted to probe the details of these methods, not the general overall scope. This would be very difficult for an ordinary physician to cope with. It so happened that at Yale, where I was a student fellow, you had to go through the methods of the department personally. And in Munich I was responsible for setting up the clinical laboratory. So, I was able to cope with these questions reasonably well. And after a day and a half the judges decided that enough had been explored and that my qualifications were reasonable and the trial proceeded. He was convicted and sentenced to death.

*JCI:* After your time in Germany, you weren't exactly sure what you were going to do next, but there was a letter from Peters inviting you to come back to Yale. Did you return there mostly to do scientific work or did you think that you would be a clinical physician?

Seldin: The letter was very brief. It was almost the last day I was in Germany. By accident, I went back to the hospital to check the mail and there was a letter from Peters. Of its two sentences, the first said, "I would like to offer you a position as instructor in medicine at Yale at the salary to begin at \$2,500 a year." And the second sentence was, "Why haven't you written?" So, with this in hand, surprised and exhilarated, I came back to the United States, and we went to New Haven.

*JCI:* How did you end up at UT Southwestern?

Seldin: I was very happy at Yale, but you must remember that at that time there were very few medical schools of a high academic caliber in the United States. Medical schools were just being developed all over the country, but they were new. One of the most established schools was Yale, but the section in the Department of Medicine I was in was very crowded. Almost the entire Department of Medicine consisted of the metabolic division. So when I received an offer from Charles Burnett, who had just accepted an appointment as chairman at Southwestern, I decided to take it. I was very naive at the time. I never looked at the institution, I never visited it. And when I came to Dallas it was quite a shock.

When I arrived in Dallas in my car accompanied by my daughter and my wife, I inquired at a filling station about where



**Figure 1**  
Ushma Neill interviewing Donald Seldin on April 28, 2012. Image credit: Karen Guth.

the medical school was located, because I wanted to see it since I hadn't visited it before. I drove down where he indicated and all I saw was a bunch of broken down shacks and garbage, so I thought that I had missed the school and I returned to the filling station and indicated to the attendant that I went where he instructed me, but all I saw was garbage and the shacks. And he said, "That's it. That's the medical school."

*JCI:* You were promoted to the chairman of the department at a very early age. Was there much resistance that you encountered to that?

Seldin: I was from the Northeast, and I had a view of medical education that was somewhat different from that which had prevailed at the school. When I was asked to assume the chair, there were obviously some people who were enthusiastic but some who thought this was a tragedy.

Within a year of my arrival, I came to Burnett's office to tell him that a paper I had submitted had been accepted for presentation at the main program in Atlantic City. This was a towering thing at the time. He congratulated me and then went on to say that he was considering going to the University of North Carolina. He had only been in Dallas six to eight months. Within two or three months he left for North Carolina and the Department of Medicine consisted of one full-time faculty member, me.

*JCI:* In those early days you had to do a lot of recruiting.

Seldin: Recruiting couldn't be done broadly because there was very little money available and the space was trivial and broken down. The people I worked with mainly were medical students and house officers. And it turned out that they were magical. We had Floyd Rector, who later became a major figure in electrolyte metabolism; Jean Wilson, who became a major figure in cholesterol metabolism and sexual development; Dan Foster, a key investigator in metabolism and diabetes; Mike Brown and Joe Goldstein, who elucidated the normal and deranged pathway of cholesterol metabolism for which they were awarded the Nobel prize; Jere Mitchell, who examined cardiac performance; Norman Kaplan, who worked on hypertension; John Fordtran and John Dietschy, who became major investigators in gastroenterology; Helen Hobbs, who explored various genetic determinants of cholesterol metabolism; and many others. Most of these individuals worked with me, and then I sent them away for training, largely to the National Institutes of Health. They came back and they constituted the core of the faculty.

*JCI:* All these people you mention claim that you were the most meaningful mentor to them. What sort of advice do you have for your colleagues about being a good mentor?

Seldin: It's hard to say whether there is a formula or a specific program for mentorship. I have always been impressed by Hans Krebs' description of Otto Warburg's program as an ideal example of inspiring mentorship. Almost every member of Warburg's department was a Nobel Laureate. To be sure, all of them were talented. But equally or more talented fellows worked in other laboratories without achieving comparable success. Krebs identified the key ingredient in Warburg's unit as the scientific climate that prevailed — a dedication to fundamental problems, rigorous standards, exciting interactions — all under the pervasive influence of the mentor. In a clinical department, it is vital that students and fellows interact with the mentor on a personal basis, so that the excitement of medicine might serve to animate the technology and stimulate intellectual exchange and debate.

*JCI:* What did your early mentors, like John Peters, teach you about being a mentor?

Seldin: Well, Peters was a man of unimpeachable integrity. He was a very deep scholar, dedicated to academic medicine. But he never neglected his clinical responsibilities and tried very hard to exploit his clinical experience for academic purposes. At the same time, he was extremely moral, very anxious to ensure that the best of medicine reached all the people. For this reason, he was in the center of a great deal of controversy when I was at Yale. This was during the McCarthy period; Peters was fired from an NIH study section on grounds that he was a security risk. The case came before the Supreme Court. The judgment was set aside, but the constitutional issue of whether he was a security risk was not settled. The court simply ruled that there was a technical error in his charge and therefore set aside the ruling. He continued at Yale for the rest of his career. He was a gigantic influence and his example — what he did, how he functioned — was an inspiration to all his students.

*JCI:* You have certainly a way with words. Some of your more classic witticisms include, "Education is what you have left after you've forgotten the facts." And one about surgery, "Does your mother know that you are cutting people open for a living?" And, "This is a beautiful example of therapeutic frenzy combined with abysmal ignorance." Where did you develop this way of talking?

Seldin: I like language, and I like formulating things in a coherent way.

**Ushma S. Neill**