Legends and Legacies Book Chapters

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Margaret R. Spitz, M.D.

Professor and Chair of Epidemiology
Olga Keith Wiess Distinguished University Chair for Cancer Research
Margaret began her journey as an epidemiologist when she received a Master of Public Health degree from The University of Texas School of Public Health in 1981.

Drs. Joe Fraumeni, left, and David Schottenfeld congratulated Margaret when she received the 2003 Award for Research Excellence in Epidemiology or Prevention from the American Association for Cancer Research and the American Cancer Society.

Margaret and husband Louis Berman, M.D., in 2005. At left, daughter Elise Berman Engels, M.D., and husband Eric Engels, M.D., with Joshua and Emma; in the center, son David Berman, M.D., Ph.D., and wife Sasha (with Rebecca); right, son Howard Berman, Ph.D., (with wife Randee) and Sammy.

One of Margaret’s many favorite photos captured grandchildren Joshua and Emma Engels demonstrating an early interest in medicine.
Nowadays, my standard advice to young scientists or physicians is to “do as I say and not as I did.” This is because some of the most momentous decisions I have made in my life seem to have been conceived on the spur of the moment. This is absolutely not an approach I advocate, but I have been lucky in that most of these knee-jerk reactions worked out remarkably well for me.

By nature I am not rash. In fact, as the youngest of three children, I was the most practical. Every Saturday morning my mother would drive us to the synagogue and give my brother (the oldest) the bus fare for our return home. My brother, who enjoyed the finest things that life offers, wanted to splurge the money on ice cream, and then we would have to walk home; my sister, the kindest of souls, argued to give the money to the barefoot little children who would congregate by the strip shopping center, and again we would have to walk home. I voted to use the money as intended — to catch the bus home — but I almost never won that argument.

The truth is that I never grew up wanting to be a physician. Rather, I was determined to be a nuclear physicist. As a child growing up in the privileged all-white enclave of Lower Houghton, in Johannesburg, South Africa, that seemed to me an exotic and challenging career and one that offered to bring me a little nearer to the unattainable land of my dreams, the United States of America. How could I have even guessed what my chosen profession entailed? The harsh reality was that I attended an all-girls high school that did not even offer us the choice of studying either physics or chemistry — those subjects were considered suitable professions only for boys. Girls were relegated to French and biology.

As an aside here, Joshua, my 8-year-old grandson, has a mother (my daughter) who is a radiologist. He already knows that he wants to be a doctor but when asked if he will read X-rays like his mother, his response is immediate and definite — “No, that is only for girls!!!” How times have changed in only one generation!

Returning to my story, my father was a wise man. Born in Lithuania, he escaped being drafted into the Russian army by crossing the border disguised as a girl, overcame “Jewish quotas” to be admitted to study medicine in Germany, and immigrated to South Africa in the early 1930s. His prescience about the future of the Jewish community in eastern Europe was supreme. Life experience taught him (and that became the overarching theme and mantra of our childhood) that as Jews, we could never consider ourselves to be safe and settled and that we must select “portable” professions in preparation for the time when we would again need to emigrate. It was not for me to question why nuclear physics was not considered to be in this category.
As graduation from high school neared, I was interviewed by the local newspaper and asked what I planned to study at The University of the Witwatersrand (Wits) in Johannesburg. Without thinking I blurted out “medicine.” I had not even realized that I had already inexorably reached this decision. And thus was sealed my fate.

My first year at Wits Medical School was exceptionally challenging. I was one of a handful of women students and one who had never studied physics or chemistry. My days were miserable, and I barely made it through the first year. The next year, I was so uncertain about staying the course that my mother took me to a “guidance counselor,” who advised me to “throw out your books and go get a bachelor’s degree.” I was outraged by this inane advice (and even more that my mother actually had to pay for it) and decided to carry on just to prove the counselor wrong. The Hebrew word for this attitude is “davka.”

Things improved dramatically in the clinical years. Not that we as women weren’t told consistently and often that we were taking the rightful place of men who needed the training to earn a living for their families and, moreover, that as women, we were destined to drop out and amount to little. For several, this in fact became a self-fulfilling prophecy. But not for me — I would prove them wrong. In the last two years of medical school, we were to form teams to rotate through various clinics. I was invited by some of the brightest and smartest of the male students to join their team, and we became known as the “A” team. Parenthetically, all but one now live in North America and one in Israel. The competition was fierce but manageable, and I seemed to thrive in this environment.

As graduation approached, we faced the prospect of choosing our internships. Dr. du Plessis, the professor of surgery at our medical school, was a brilliant surgeon but dictatorial and tyrannical. His internship was considered to be the pinnacle of prestige and highly sought after. One applied and he chose. He tolerated women in the operating room only if they were nurses. No woman had ever been selected as his intern. I doubt whether any had even had the temerity to apply.

I received a surprising phone call from his second-in-command, Dr. Bert Myburgh, asking me to formally apply for this honor. Without thinking twice, and knowing that this “honor” carried with it brutally long hours, a total absence of any hands-on experience, servile duties and prohibition of free expression, I unhesitatingly declined the “honor.” There was a collective gasp of disbelief, and I was blackballed from all other surgical internships. There was no one to tell me that in all likelihood I had also dealt a setback to women’s lib, and perhaps I had. This is also perhaps the reason why I am so in awe of the talented and dedicated women surgeons at M. D. Anderson today.
Marriage, three children and the decision to leave the land of our birth for America followed. This part of my story is in itself a long chapter, best left for another time, so I am flashing forward to the inexorable stream of events that led to our arrival in America.

A phone call from my physician-husband Louis Berman told me that he had taken a position as a rheumatologist at The University of Texas Medical School in Houston. First, the mad dash to a globe to locate Houston, as we knew only of Dallas at that time. Next, the trip to Austin to take the Boards, my 7-year-old daughter’s school essay, “we are going to live in Texas, which is in Houston” (for which she received a perfect score), and the dreaded Visa Qualifying Exam. At first I was outraged when told that only one member of a family needed to pass the VQE. I wanted to earn this right to a visa on my own. And so my husband and I both sat for the English part of the exam, which was multiple choice. The questions were read by the wife of the American consul general, whom I later learned hailed from New Orleans and spoke with a very heavy Southern accent. I could not understand whether she was articulating “ankle” or “uncle” or “angle.” As a result, I, who had always excelled at the King’s English, almost failed the exam. I immediately decided to leave the science part of the VQE to my husband — another wise decision.

After arriving in Houston in 1979, I soon found it financially necessary to get a job and took a position as a physician in a home for the aged. I did not enjoy the way the American medical enterprise operated, so when a new director was appointed, and I found that I did not agree with his approach, I made a spot decision to resign. At this point, a friend suggested that I enroll in the UT School of Public Health.

A career in public health had never been a consideration. At medical school, our initiation into public health was through sewage farms that we were required to tour for credit. While I surveyed the optimal diameter for a latrine, the male students surveyed the cleavage of Dr. Erasmus, our voluptuous lecturer. If this was public health, I wanted no part of it. But eventually I did register at the UT School of Public Health, and I selected my classes based on their time slot, rather than on content, since I was then juggling priorities in order to supervise my three children, all under the age of 10. Car pools every day meant I could not take afternoon classes that ended after 2:30 p.m. This clearly was not the optimal way to learn epidemiology.

Initially, I was horrified at the casual atmosphere in the classroom: the way the students dressed, the freedom with which they brought drinks and food into the class, the impunity with which they challenged and argued with the lecturers and seemed to enjoy the interactions. This was definitely not what I was accustomed to. But gradually I learned to accommodate,
and, two years later, I received my M.P.H. in epidemiology. My thesis was on pancreatic cancer in the Beaumont/Port Arthur area of Texas, and, to this day, the data have not been published. They lie on my shelf in a neat but faded green binder. This sobering, mentorless experience has driven me to encourage all my undergraduate and graduate students to select realistic topics for their theses/dissertations and to consider the work as just a beginning rather than as a career in itself. I always tell them, “You need to finish off, grow up and get on with the rest of your life.”

Next I needed a job. I interviewed for a position as an occupational physician for Texaco, Inc., and was flown to their headquarters in White Plains, New York. They offered me the job, with a handsome salary and benefits, but were inflexible about my hours. In another snap decision, I rejected the offer. Instead, in 1981 I took a position at M. D. Anderson as an assistant professor, non-tenure track, part time, under Dr. Guy Newell in the newly created Department of Cancer Prevention and Control, which was then within the Division of Medicine. Guy Newell hired me purely on faith. My resume was one page, double-spaced. Besides Guy, there was only one other faculty member in the department. There was no infrastructure at all. My office was a cubicle in the former kitchen facility for the Texas Medical Center (now the Smith Research Building).

Shortly after I arrived in my new department, we received a phone call from a patient with salivary gland cancer who reported that a co-worker in his factory also had been diagnosed with the same malignancy; he wondered whether there was a common causative occupational exposure. Of course, I had no sage answer for him, but this gave me an idea. Over the next year, I and two unpaid but supremely dedicated research volunteers reviewed and abstracted the medical records of 329 patients with salivary gland cancer and a similar number of matched controls with other diagnoses. As we were analyzing these case-control data, a new chief of Pathology arrived at M. D. Anderson: Dr. John Batsakis, a world authority on the pathology of salivary gland cancer. I was in the right place at the right time. But, as I tell my junior faculty, you also have to recognize that you are in the right place at the right time. Otherwise, it turns out to be neither.

Together, Dr. Batsakis and I wrote and had three manuscripts accepted in the early 1980s, and, thus, my career as a cancer epidemiologist was launched. Many years later, I had the opportunity to send reprints of these three papers to Dr. du Plessis, the former professor of surgery whose internship I had so arbitrarily spurned years before. Surgical treatment of these tumors was his specialty. We exchanged some very meaningful letters that I have treasured. All was forgiven!

When Dr. T.C. Hsu, a brilliant cytogeneticist, approached me soon afterwards to help validate a new assay, the mutagen sensitivity assay he
had developed to assess cancer susceptibility, I jumped at the opportunity. However, at that time I knew nothing about grant applications. No one had ever suggested I write and submit one. Moreover, I had no idea what a study section was, either. Could it be a chapter in a book? I did not understand anything about the workings of the National Cancer Institute (NCI) or the peer review process. Fast forward to the present — I have just completed two years on contract to the NCI, spending 10 percent of my time there working with both intramural and extramural scientists, and I’m now thoroughly conversant with the system. But I did prepare a small grant (my first) with Dr. Hsu to evaluate his assay in patients with head and neck cancer, and, with beginner’s luck, it was funded. This turned out to be one of the earliest molecular epidemiology studies. We did not even recognize then that we were at the head of the curve.

I was so excited by this heady feeling of success that I jumped into the fray again and quickly submitted another grant. This time, it did not receive a fundable score. I was devastated, stuffed the review in the back of a drawer and never resubmitted that grant! Now, I insist that all faculty develop thick skins and that every grant, even those unscored, needs to be revised and resubmitted. And since I had no help in revising, I make it my top priority to participate actively in all the grant revisions in my department.

Next, there was an announcement from the NCI for grants on minority populations. By then, Guy Newell and I had published data showing that, in 1984, lung cancer overtook breast cancer as the leading cause of cancer mortality in Texas. Much the same trend was being shown in other states. We also documented that African-Americans had higher rates of lung cancer and earlier ages of onset (despite lower smoking intensity) than their white counterparts had. Exploring the reasons for these trends in minority populations seemed to be a winning approach and, as a result, our lung cancer program was created.

I felt very insecure about this lung cancer grant application, my first R01. Several other applications on this topic were also being submitted from M. D. Anderson. I needed a new twist. I happened to go to Employee Health for a TB test, and sitting next to me, also waiting his turn, was Dr. Jack Roth, chairman of the Thoracic Surgery department. We discussed some ideas and devised a plan to look at germline changes in the tumor suppressor gene p53 as a risk factor for lung cancer. A few months later, my NCI program officer called to tell me that my grant was approved for funding. (There was no electronic communication in those days.) I was so amazed that I assumed Reagan’s doctrine of “trust but verify” and called him back to verify that he had not made a mistake. How naïve I was then.

The lung cancer grant has been continuously funded now for 17 years. We showed that as many lung cancers were occurring in former smokers
as in current smokers, and this gave my great friend and supporter, Dr. Waun Ki Hong, the brilliant idea to launch an innovative chemoprevention program in former smokers.

One sour note. A leading oncologist opined publicly that epidemiologic research, and mine in particular, was “phenomenologic.” I had not heard the word before, and there was no Google engine then to search for the meaning. Could he have meant “phenomenal”? I did not think so, since I had detected a sarcastic undertone. I have spent the years since then striving — I believe successfully — to prove him wrong.

I have been extraordinarily fortunate in my career. As I walk through the expansive and bustling fourth floor of the Cancer Prevention Building, I not infrequently experience a flood of incredulity. Is this really the “house that I, as the founding department chair, helped build from its foundation?” I am exceptionally proud of the achievements of my department and its national reputation. I am equally proud of the talented and hard-working faculty, whose success is my success. I feel gratified that epidemiology is no longer considered a peripheral discipline but is now integral to most ongoing SPORE programs and many multidisciplinary programs throughout the institution.

I have been the recipient of many honors and awards, both at M. D. Anderson and nationally. Among the most meaningful are the Award for Research Excellence in Epidemiology or Prevention, from the American Association of Cancer Research and the American Cancer Society, and the Rosalind Franklin Award for Women Scientists, from the NCI. That this honor came from Dr. Joseph Fraumeni, whom I have so admired and whose textbook on Cancer Epidemiology became my bible early in my career, made it even more special.

I have learned everything I know on the job. I have met many amazing oncologists, surgeons, scientists, and of course, epidemiologists. I have learned so much from them: the importance of networking; how to prepare and edit a manuscript; how to make a formal presentation; to strike just the right tone in responding to a reviewer’s critique; to be a gentle but decisive mentor; and to be honest but fair. I had to learn how to juggle and prioritize my life as a mother and wife with the realities of an academic career. I am, therefore, exquisitely sensitive to the special challenges facing our women faculty. I have made many mistakes but hope that I have learned from all of them to do better next time. I have never placed my own career needs above the needs of my children. That my daughter is now “having it all” as a devoted mother and extremely successful practicing radiologist is my ultimate success story.

Most recently, I decided, after much soul searching, to submit my name as a candidate for the position of head of the Division of Cancer
Prevention and Population Sciences to replace Dr. Bernard Levin, who was retiring. Then, I had second thoughts. It was not the right time for me personally. It was not the right time for my department to undergo a transition in leadership. Would the extensive changes I wanted to implement be acceptable to all? I was ambivalent. But I received an unexpected call from Dr. Margaret Kripke, then the Executive Vice President for Academic Affairs, informing me that I was one of the three candidates on the short list. My instincts jumped into high gear, and I told her that I was withdrawing my name. Once again, until I said it, I did not realize that I had already made this decision. I feel good about this decision, but time will tell all.

Postscript: In reviewing what I have written, I may have left you with the impression that my whole life has been nothing more than a series of impulsive decisions. This is not so. As I have gained wisdom and perspective, I have come to the realization that these life-changing moments were, in fact, not spontaneously arrived at. Rather, they were deliberate decisions reached in the deepest recesses of my mind, only waiting to be expressed. I am, after all these years, still a pragmatist.