

VISCERAL VIEWPOINTS, FROM CAMBRIDGE TO NEW HAVEN

Howard Spiro

Introductory Musings

I was born in Cambridge, Massachusetts, in 1924 at the Mount Auburn Hospital, once known, less poetically, as the Cambridge Hospital. I hope to die in New Haven, for the nice symmetry of the tombstone I will never have – as, with Marian, I am planning to be cremated. I hoped, before it became illegal, to have my ashes scattered from an airplane, so I could get in people's eyes, to irritate them one last time!

In a nutshell, that is my trajectory, from birth to death. I hesitate at the word *intellectual*, as in general we physicians are artists and scientists, tradesmen and technicians, and rarely maybe philosophers and poets, but certainly not intellectuals. Watching Yale students, at seminars and as adviser to premeds and at one time even prelaws, I saw the best ones, captivated by ideas, go in for Ph.D.'s in chemistry, physics, philosophy, or English. Those like me – “otherly” gifted, with some intellectual pretensions or maybe just economic proclivities – chose the nearer good of medical practice. My colleagues at Yale Medical School have disagreed with this assessment, which may be why medical training has been made impossible for many. But as I have made my career by coming to correct conclusions from insufficient data, I maintain my stand.

Let me amplify. The Flexner Report of 1910 eliminated “nonscientific” medical schools, including most of those that trained women. Flexner assumed that all applicants to medical school had had a good general education and so he favored the German model of science for all physicians. Johns Hopkins was his ideal and, later, was to be the model for the reorganized medical school at Yale. Scientific medicine has come up with world-changing advances, but twenty-first-century patients love their doctors a lot less than they did sixty years ago, when I became a physician.

This state of affairs has many origins, one of them being physicians' aspirations to intellectuality and the consequent requirement for getting into medical school of

Howard Marget Spiro was born in Cambridge, Massachusetts, in 1924 and likes the nice symmetry of dying in New Haven – but not yet, please! Educated at Harvard and its Medical School, he had the good fortune of coming to Yale in 1955 to set up the Gastrointestinal Section at the Medical School. Although he and his wife, Marian, had anticipated staying in New Haven for a few years only, they have never left, except for sabbaticals. Probably the most influential was a year at Stanford's Center for Advanced Studies in the Behavioral Sciences, a respite which reminded him that he was a physician but that he was also, surprisingly, a human being. When they returned to New Haven, Howard began the Program for Humanities in Medicine in 1983. He founded the *Journal of Clinical Gastroenterology* and the electronic *Yale Journal for Humanities in Medicine*. He counts among his achievements the Yale Affiliated Gastroenterology Program and “The Geezers,” a group of elderly but hardly superannuated physicians. He here records his debt of love to Marian, who has put up with his faults and foibles for so long.

passing organic chemistry—the hated “orgo.” I cannot tell you how many young people who would have made excellent physicians have been eliminated by that requirement, which persists to this day. For years a friend has argued that a course in John Milton, or epistemology even, would prove a reasonable surrogate for persistence, intelligence, or diligence, but the medical hierarchy continues to insist on orgo. That friend has suggested sending out a helicopter or two to catch unaware a few internists of good repute to see how much organic chemistry they still know or rely on. But that experiment has not been carried out.

In getting together this account of what I would call my “transit” rather than “trajectory,” which has implications I do not claim, I found the process much like going to a psychiatrist. Long ago, I expected to become a psychiatrist; two of my children are psychiatrists, and so was my stepbrother. Despite this, my sense of myself is either too strong—or too weak—for me to seek analysis. Yet this chance to review where I have been may prove an exercise in recasting how I got there. Following the deconstructionists of the last century, I recognize that what I have not talked about may be more important than what floats to the surface, but let me claim a little privacy.

Early Life

How I came to be born in Cambridge may portend something. My folks, married a scant nine months before, were living in nearby Allston, adjacent land now coveted by Harvard. My grandfather, the poor immigrant child who had done well, wanted the “best” obstetrician to deliver me, and as he ran the family, he picked Dr. James Huntington, a prominent obstetrician in Boston who practiced at the Cambridge Hospital. Back then my grandfather could not know that I would be a boy, but as he had had four girls, he hoped for a reward from the Creator, in whom he had no faith. My mother, whose studies of Spanish in college led to her naming my sister Dolores, is said, when pregnant with me, to have gone regularly to the Boston Museum of Fine Arts, on a street conveniently also named Huntington (Avenue), to gaze at Greek statues in the expectation that I would resemble one of those figures. I am grateful to her for that advantage!

That birth in Cambridge suggests how cherished I was to be as a child, the first boy for my grandfather and a first-born son for the Jewish mother that Freud somewhere opined provided the confidence and love that made for a strong ego. A businessman who had started out as a barkeep, my grandfather was the great influence in my early life, even if paradoxically the only songs he ever taught me were Irish rebellion ones that I imagined he had picked up in bibulous Boston. That he wrote the names of his four daughters in a dictionary, rather than in a Bible, suggests the secular but assimilative trend of his mind. Much later, I came to realize that while he had taught me to be punctual and secular, and hard-working and ambitious, he might have wanted to manage too much. My father, a successful lawyer, had to wait in his shadow until my grandfather died; only then did my father find his own very

considerable and far more benign influence on me. Still, I am like my grandfather too, for our children complain that when I keep my mouth shut, my opinions pour out my ears.

My irreverence for authority—some would say my problems—stems from my grandfather, I imagine. My mother, as loving as Freud could have wished, had wanted to go to medical school, in the early part of the twentieth century an unlikely career for a woman. Needless to say, she was ambitious for me.

I grew up in Newton, Massachusetts, in those days still a WASP redoubt increasingly attractive to Irish Catholic and Jewish arrivistes. I skipped a grade, common back then, worked on the high school newspaper and magazine, ignored sports, and was one of the ten or twelve seniors at Newton High School who automatically went to Harvard College. I arrived there at a little over sixteen years of age.

I had put away a time capsule in junior high school when I took metalwork. My fellow students were turning out candelabras, while I could barely manage to fold over sheet metal. But I did write down my hopes for the future and soldered it up in a sheet-metal folder. That folder, misplaced after my mother's death and my father's remarriage, did not turn up for thirty or more years, long after I had come to Yale Medical School. I had written at age thirteen or so that I wanted to become not just a doctor but "a teacher in a medical school." I still don't know whose ambition it was, my mother's or mine. But my remaining at Yale Medical School for so long suggests how our early training creates mind by hard-wiring the brain with habits and hopes.

College and Medical School

College was an opening for me, as it ought to be, even though in the 1930s Newton High School ranked second only to Boston Latin—where my grandfather had forbidden my folks to send me. I lived at Harvard, ending up on the fourth floor of Grays Hall with a roommate from New York who had gone to a toney private school, and who claimed Margaret Mead as his aunt. Suffice it to say, by the end of that year I hated even the way he climbed up stairs, but in later years, as he also had become a physician, we shared only nostalgia for our freshman year. Fortunately, he never told me what he disliked about me.

Although I had been regarded as a smart kid in my high school class, at Harvard I kept meeting people who knew a great deal more than I about almost everything, and who seemed far more sophisticated than anybody whom I had known in Newton. Quite a few "preppies" had studied Greek as well as Latin, ancient history as well as American, were as familiar with T.S. Eliot as with Rudyard Kipling, and dressed quite differently from the middle-class kids I grew up with.

Those new vistas taught me to be more than a premed. I joined the *Harvard Advocate* and the Crimson Network (not a Commie group, but the primitive college radio station running through wires on the steam pipes), and had a go at volunteering in the South End of Boston in a settlement house, as they were still called. By the end of

my freshman year I had decided to major in English rather than in a scientific subject that would make getting into medical school far easier. Did I get any advice from mentors about this? I have quite forgotten. I was eager to advance beyond what then seemed to me to be a cramped high school education. I am grateful for that innocence and suspect that what my wife upbraids as my still adolescent sense of humor betrays how comfortable for me was my upbringing.

The Grant Study, supported by the five-and-ten-cent-store entrepreneur, entered my life at the end of my first college year. A longitudinal study of about four hundred Harvard men recruited from about four different classes, the Grant Study is still trying to ferret out, from probably antiquated sentiments, what made for “success” in the average Harvard student of the 1930s and 1940s. We were selected by three criteria, it was rumored: not having failed a course, not having seen a college psychiatrist, and having been seen at a dance. Each year since 1941 they have followed us with questionnaires, examinations, and, in 2006, a genetic analysis. This has given me the habit of reviewing my thoughts each year.

As one of the younger students in the class, I was an underachiever socially but remained independent, goal-oriented, and a hard worker, or at least a conscientious one. I have been little influenced by other people’s goals; for example, the idea that available money should direct research questions has never appealed to me, even when it came from the National Institutes of Health. I suppose it has something to do with that *conatus* of Spinoza as I understand it, for I have known my own mind and mistrusted most so-called mentors. Too many medical students veer from the path of their heart when a teacher or mentor advises them, “Do what I want you to do, and you will gain the world!” Few teachers can resist this “seduction of the innocents.” When I boast that more students who worked with me went into psychiatry than gastroenterology, I wonder what that means – and why.

Even in college I hated deadlines, and prepared for papers or exams a week or more ahead, a habit that has persisted throughout my life. Although deadlines act as the goad, I like to have everything arranged a week or more before. That’s one of the ways in which I avoid tension. I suspect that meeting my grandfather regularly in downtown Boston had something to do it. He was punctual to a fault. If I got to an appointment on time, he was upset at having to wait ten minutes or so for me. As a result, I learned to be even earlier – a trait that used to make me very impatient but, fortunately, has disappeared with age.

The summer of 1941, after freshman year, I ran a day camp for little kids from my grandfather’s beach house. But December 7 meant there would be no more summer vacations. After Pearl Harbor, we squeezed into one and a half years everything that would have been studied in three years, and the college had fewer amenities. At the *Harvard Advocate*, Norman Mailer taught me how to drop condoms filled with water out the window, but it was hard for me to take my colleagues at the paper as role models for my future. I suspect that my pretensions of studying English for a Ph.D. were

doused by that association. Moreover, aside from someone like F.O. Matthiesen, who emphasized the broad sweep, and Douglas Bush, who was said to have recited from memory *Paradise Lost* on the way to work and *Paradise Regained* on his way home, there were few in the English Department whose work appealed to me.

Later, when I took care of an erstwhile professor, Theodore Spencer, at the Brigham Hospital, I was able to talk with him more readily, and realized that part of my dissatisfaction with the English faculty had to do with my tendency to keep to myself and not to talk up the professors who gave the lectures.

231

The War Years

The war fired us all up. But my grandfather and my father both urged me to go to medical school, reminding me, convincingly, that I would make a poor soldier and a better doctor. Of course, they were right, but that did not keep me from massive feelings of guilt at not joining the war against Hitler. I have always had what Freud would have called a strong superego, and duty has always seemed very important to me. My son the psychiatrist has labeled me “the father of all guilt.” And so I was happier when the V-12 program for medical students disguised our apparent cowardice in uniform.

I graduated from college in June 1943, but medical school was not to start until January 1944, and so I was sent to Chelsea Naval Hospital as an orderly for the first five months of military service. Somehow I rather liked the freedom that came from just obeying orders. That would surface much later during a sabbatical year in Stanford at the Center for Advanced Studies in the Behavioral Sciences. There were so many dedicated achievers there that I could lie back to let them make all the decisions about where we would go and even what we would eat.

Immersed in seventeenth-century English literature, I had considered combining my interests in literature with a career as a psychiatrist: psychoanalytic interpretation of literature was thriving in the 1930s. That idea was killed in part by a month as an orderly on the psychiatric ward of Chelsea Naval Hospital. I grew nervous about accepting as rational some of the delusions of the military patients. I also felt a horror, literally a shivering of my spine, at some of their outlandish actions and thoughts. But on the whole, I enjoyed my foray into the real world of the Navy and the forbidden delights of Boston’s Scollay Square, now cemented over—like my memories.

Medical Training

Medical school in 1944 was quite an experience. Those of us in the V-12 program wore Navy apprentice seaman uniforms, but aside from that cover, we had really no military duties. To switch from “Gray’s Elegy” to *Gray’s Anatomy* was hard, but dissecting a cadaver was our entering rite. We certainly had very little spiritual interest in the cadavers reeking of formaldehyde that clung to our clothes, stung our eyes, and irritated what Boston patients called our “bronickals.” Maybe that is why I regard dissection of the body as an unnecessary loyalty to the past, particularly when CAT scans give far better and more clinically useful views of internal organs.

I was trained in the era of paternalism – now renamed, in a frenzy of egalitarianism, parentalism. We did what we thought was best for the patient and for the family, and we were convinced that telling patients what to do was doing our duty – and doing good. “Beneficence,” it might have been termed, though I do not recall hearing that term. Beneficence was trampled under by the rush to truth-telling about thirty years ago. Today, many young physicians muzzle their opinions lest they reframe the patients’ choices. Still, I am not convinced that hard-nosed truth does more than strengthen the faith of physicians in their own rectitude. Sixty years as a physician has made deciding on the truth very hard for me to be sure about. Medicine is a profession, after all, and doctors know more than their patients, and I believe we have a duty to talk with them and then to guide them. I do not keep my opinions to myself.

Yet of my many clinical aphorisms, my favorite remains: “The eye is for accuracy, but the ear is for truth.” By that I mean that looking at a CAT scan to see what’s going on is quick and easy, but to interpret what is shown – to act as the mediator between the X rays and the patients – requires talking and listening and a lot of time. And unfortunately today there are not enough physicians to do that.

In the 1970s, physicians in developing countries could buy an endoscope to look around the stomach or colon, but they did not always know what to do with the information. Some advances, like CAT scans, are more important than others. For years I warned that whenever a patient came into the emergency room with abdominal pain, the physician had to ask whether appendicitis could be ruled out. Now physicians in the emergency department order a scan and the radiologist tells them what’s going on. That’s a big advantage for patients, but my old rule, once so valuable to the surgeon, is no longer needed – another clinical skill down the drain.

I gave up doing endoscopy when flexibility came along, because I had become convinced that technological pursuits required neither a college nor a medical school education. I left the new endoscopy to my colleagues with the excuse that I had no wish to sit at one or another orifice, pushing tubes into the innards. In no way do I belittle what was found, I just didn’t want to do it myself, and wasn’t sure that a medical degree was necessary to push the tubes. More than once I have suggested that we train technicians for such procedures, but my colleagues are wary.

Back to my medical school career: the first year was very difficult for me, for in the class of 125 men, there was only one other student who had not majored in science. He and I used to scan the pictures of our classmates, asking, “Who knows less than I do?” I could always find one or two such. But he could not – and flunked out at the end of the first year.

Second year was more fun: we felt like physicians as we learned about pathology and disease “entities.” Today, I would not argue that diseases should be considered entities; they are mainly diagnostic groups named by physicians for convenience. For example, calling a specific kind of indigestion “peptic ulcer” – for the hole in the mucosa that could be found at autopsy in the late nineteenth century – gave primacy to

the anatomic defect rather than to the symptoms of the patient. But insurance companies and third-party payers prefer to pay for an anatomical defect than one more vacuously subjective. I became convinced over the years that some reactions, even heartburn, occurred in families as acquired responses to stress, respectable because the elders had them as preferred reactions to frustration or other feelings. They were habits as well as genetic differences. Sadly, what is seen through the endoscope or at X ray scan trumps the patients' complaints. "I've had it up to here!" to a professor of English might seem a metaphor for frustration, but physicians regard that complaint as coming from delayed gastric emptying and as a reason to "rule out" gastroparesis, antral strictures, and the like.

Another good example comes in *H. pylori*, the bacteria that certainly cause gastritis and inflammation of the stomach, lots of ulcers, and even cancer of the stomach in some parts of the world. Until the 1980s, when it was still believed that stress had a lot to do with ulcers, we would talk with patients, and by the time they were better, they knew a lot more about themselves. Now we eradicate *H. pylori* and the patients feel better, but know little more about themselves.

The reliance on technology has brought many changes: in my day, we took the history and arrived at a potential diagnosis, then confirmed or ruled it out by the physical examination. Everything else was subsidiary, to confirm the possibilities. Now a presentation involves not only the history and physical exam but the lab reports and all the X ray findings, ignoring the clinical findings. Taking the X ray findings as the important criterion may be okay in an emergency, but certainly not in the office or clinic.

We had many famous teachers at Harvard. Among our teachers in pathology was Sidney Farber, who later won immortality in the eponymous Dana-Farber Institute. In those days he was an instructor known as "Slippery Sid" for the unctuousness that later achievement would rename graciousness. Later, after he and my grandfather had put together the Jimmy Fund, Dr. Farber offered me a job, but I had little interest in working with cancer and children, his area at the time. I was an optimist best suited for dealing with people who would get well.

The third and fourth clinical years were full of wonder and learning. A surgical lab where we operated on dogs confirmed that I would never make a good surgeon; something that metalwork in junior high had already made clear. That was part of a life-long pattern: I am glad to advise but not to do. At the Yale-New Haven Hospital I spent a good deal of time on the surgical service and, in my last fifteen years, made weekly bedside rounds with a surgeon. But I had no urge to cut.

As I have already noted, I like to be at the margin, bringing disparate areas together. A dilettante at heart, I am happy to know a little about a lot, like Isaiah Berlin's fox. In general, some of my best friends have been surgeons who live relatively uncomplicated lives uncovering the mysteries under the skin rather than just thinking about them. One of the major differences between surgeons and internists, at least

before the discoveries of CAT scans, seems to be a willingness to tolerate uncertainty. I have often told patients that if we can make them feel better, ruling out serious diseases as we do so, that we do not have to pinpoint the specific cause of their dyspepsia, for example. I worry that focusing on a specific pain may bring the pain too far into consciousness, deepening the channels through which pain is carried to the brain.

I did like gastroenterology, however. It was personified at the Massachusetts General Hospital by Chester Jones, a somewhat militaristic martinet from Maine whose office was in the old wine cellar in the basement of the Bullfinch Building. It is hard to bring back to memory what drew me to him. I remember his terrorizing medical students and residents with his questions, and I learned from him what I should not do. As I have said, this exercise has brought me to a kind of self-psychoanalysis, and so I see connections in my head between the gray-headed WASP from Maine and that grandfather I keep talking about. In fact, they had been brought together when my grandfather developed intractable hiccups. He was cured by Dr. Jones, who came to his house and passed a large-bore rubber tube filled with mercury to stop the diaphragmatic spasms. Was that enough to bring me to gastroenterology? Memories are mixed motifs with motives.

I have been a gastroenterologist since 1948 and have always loved the field, and it has been good to me and good for me. Yet in 2007 if I were a medical student, I would choose neurobiology, as it now is called, mixing psychiatry with images of the brain that we hope will show the mind.

As a medical student I fell in love with the old Peter Bent Brigham Hospital, named for a brothel keeper dead many years before. It was not yet forty years old, and its traditions had not yet hardened. The skimpy out-patient department was known as the "Outdoor Department" and was closed at night. Anyone coming there after six o'clock would ring the doorbell and the telephone operator would respond through a microphone to the supplicant: "Chest or belly?" "Chest" brought the medical folk; "belly" brought the surgeons. That was triage in the 1940s.

The Brigham was known for clinical research and its coterie of excellent clinicians. I did my third-year medicine there, did a trial month of internship, and hoped that would be my home. Fortunately, I was taken up by two or three of the senior faculty, so that I ended up there as an intern, resident, and GI fellow.

In 1947 physician-soldiers were returning from the war to be inserted back into the training programs. To make room for them, we more recent graduates were sidetracked into our chosen subspecialties after our internship. I was twenty-four when I first started training to be a gastroenterologist. You could do both clinical and research work at that time, and so I worked in the new Biophysics Lab at Harvard, where I met Marian, who became my wife somewhat later. In those days, laboratory work required making up your own reagents and working out the techniques that you wanted to do, but I was intrigued by the combination of clinical work and research derived from clinical observations.

I took up the new tools of my trade, gastroscopy and sigmoidoscopy. In the 1940s it took three people to pass a gastroscope into the stomach. The physician held the front of the tube, an underling supported the rigid working part, and a third held the patient's head to move it into a straight-line with the esophagus when the metal tube was inserted. Parenthetically, we kept the scope covered by a cloth so the patients would not see what we were to insert. In the 1960s this cumbersome technique, which showed us only part of the stomach, was supplanted by more flexible options.

I was much interested in stress as a cause of "ulcers," but I wanted to study gastric secretion without passing a tube into the stomach, which seemed likely to stimulate gastric secretion and vitiate our measurements. I ran across some work measuring pepsin secretion in the urine. As the head of medicine was studying the effect of a new agent, ACTH, on hormones in the blood and urine, it was easy to get a measured sample of urine to measure the amount of pepsin. Stress was simulated by ACTH, so that a rise in urinary pepsin after ACTH, reflecting what was happening in the stomach, would buttress the relationship of stress. I did the work in my spare time in the evening, when Marian helped me, in those days out of duty and not love. We were delighted to find that uropepsin excretion markedly increased with ACTH, paralleling the idea that stress might be causing ulcers.

ACTH was such a hot item that I was asked to report our findings at a national conference. Imagine my surprise/chagrin when the chief of gastroenterology came to me to ask that he give the paper, because he needed to fill out his bibliography in order to be reappointed. He played on my loyalties in ways I will not repeat, but I was young and confident enough to let him take credit for the work. When later I learned that this was his pattern, taking credit for work others had conceived, I lost respect for him. To summarize what took some eighteen months to develop, I escaped back to clinical training on the wards, determined to handle matters entirely differently if I ever got into academic life. But as I planned to go into practice in Boston, as all respectable physicians did, it made no difference.

The Korean War started in June 1950, but since I had previously, in a surge of loyalty mixed with guilt, agreed to spend two years in the military Medical Corps, I was allowed to finish my residency training while my peers were drafted away. Marian and I got married in 1951, and Dexter, my chief resident, was my best man.

Madigan Army Hospital

Later that summer, Marian and I drove out to Madigan Army Hospital in Tacoma, Washington, where I became chief of gastroenterology at age twenty-seven, one of five in the entire U.S. Army. I found I was happy to take on the responsibility for making decisions, and I am sure that the adjective "arrogant" might have been applied to me. More important, I was impressed with the virtues of a salaried medical service. Those two years were important to my political development: I was a registered Republican, but my satisfaction with the military medical service, and the failure of the Republicans

to nominate Sen. Robert A. Taft for president, brought me closer to being a Stevensonian Democrat. I have been nominally an independent ever since. I have also remained a firm believer in a national health service. I know the faults, but I cherish the virtues.

Shortly after we arrived at Fort Lewis, I saw the chief of medicine as a patient and helped him out of some digestive problems, largely by getting him to stop drinking. Ordinarily, I believe that some alcohol is good for most people, unless there is a family history of alcoholism. Medical students used to say that I was one of the few doctors at the medical school who advised patients to have a drink every night. I always quote A.E. Housman to the effect that “malt does more than Milton can, to justify God’s ways to man.”

Right after that, I saw the commanding general at Fort Lewis and took care of his dyspepsia as well. A week or two later, when the chief of medicine asked if I wanted to be transferred to Paris, warning me that it was out “in the field,” in tents, and that my wife could not come along, I demurred. His response – “Well, if you had wanted to go, I would have used one rule, but since you don’t, I will use another one.” – has remained an important lesson for me. Authorities can always find a reason for what they want to do.

My two years in the Army were liberating and instructive. I wrote a few clinical papers and worked for the first time with a black physician, a formal, aristocratic southerner. While Marian and I loved the Northwest for its informality and open spaces, family reasons drew us back to Boston. I turned down a job at the Brigham Hospital because of that odious chief of gastroenterology. Today, I might have been more foreseeing and less forthright. I considered going into practice in Boston but liked the combination of clinical care and clinical research that academic medicine offered. Besides, going into practice was “falling away” and giving that erstwhile chief a victory. If I showed first that I could do academic work, I could go into practice of my own free will.

Yale Medical School and Academic Medicine

The Gastrointestinal Section, 1955-82

And so I went to the Massachusetts General Hospital to work with Chester Jones for a while. Shortly thereafter I heard from Stuart Finch, with whom I had been friends at the Brigham, that Yale was looking for a gastroenterologist. George Thorn, my chief at the Brigham, got in touch with Paul Beeson at Yale, and I drove down with Marian to see him. We talked for half an hour, shook hands, and our lives were changed forever. No contracts, no letters of recommendation, no arduous search; it was a simpler time for those lucky enough to get into the inner circle.

It was not a time, however, of much openness in the medical profession – neither for women nor for minorities nor for those of a different stripe. In the 1940s, Harvard Medical School each year took four to six Jews, maybe a few more Catholics, no blacks, an Asian every few years, and, until 1945, no women. Yale Medical School

was little different: even under Milton Winternitz, himself a Jew, a quota for Jews, Catholics, blacks, and even women was part of the order of things. But once past that barrier, there were many opportunities.

We came here in December 1954, expecting to stay a few years only. There was nothing to build on, so I set up a lab and offices in the abandoned Howard Building, whose name at least was auspicious. With a technician I made reagents, set up the studies on gastric secretion that I had started in Boston, began to see patients, and made rounds on the wards. I had also to think about the clinical activities of the Gastrointestinal Section.

One of the wonderful things about Yale and the developing academic medicinal scene throughout the 1960s was that you could develop an idea from seeing a patient, then go to the lab and try to work out some of the mechanisms. Our “labs” were not only rooms with benches and hoods, but also rooms with patients and beds. Patients could also be our subjects for study. There was no such thing as “informed consent,” so much burnished by Jay Katz at Yale Law School in the 1960s.

As an intern at Harvard, I had learned of an unspoken though implicit medical compact between the ward patients and the hospital doctors. We took care of them for free, and in return they gave us their bodies to study. If I wanted to find out the effect of raising the serum calcium level on gastric secretion, I had only to tell the patients that I wanted to see what taking Tums for their dyspepsia did, and I could put calcium into their veins and a tube into their stomachs without further ado. We felt neither guilt nor doubt, for we were doing what academics were supposed to do – to advance medical practice.

In the early 1960s, I think it was, a British report told of what academic physicians in Britain and America were doing to patients, implicitly comparing those studies to the atrocities of German physicians in the concentration camps. Among many other matters, motives were entirely different. The Nazi doctors were deliberately killing subjects who they knew were going to be killed. The discussions of the late 1960s, about patient autonomy, and renewed attention to the Nazi crimes with the Eichmann trials, changed our minds and actions.

In the 1950s, the National Institutes of Health, which I had earlier turned down for the Army, enthusiastic for a few years of a less sheltered life, was beginning to hand out research grants. They gave grants more for the people involved than for the ideas presented in the two or three pages that were required on the application. Moreover, nobody cared whether you changed direction: if you had a grant for studying gastric secretion and hypercalcemia, let’s say, and you decided somewhere along the way that you wanted to study gastric secretion during the heartburn of pregnancy – as I did, because Marian suffered from it – nobody much cared. Indeed, nostalgia reminds me how wonderfully little regulation there was. As long as we were productive, that was okay with Paul Beeson and the NIH. Much changed after about 1970, but, fortunately for me, my laboratory work ended about that time.

I will spare you the niggling details of how one sets up a Gastrointestinal Section. I have never been one for much formal planning, but rather liked to try something out and see if it worked. I have even less affection for committees. I do not know whether it was my intransigent personality, but I was on only two committees in all my years at Yale, although I have gotten a lot of things done. I look on committees as a cunctatory arrangement for authorities to postpone actions they do not like. Anything they want to do, they do, forthrightly. Fritz Redlich, later dean at Yale Medical School, told me he set up a lot of “couch committees,” to keep his would-be critics busy and off his back. A couple of them are still going several decades later, and I assume they survive by supplanting disagreement with conversation.

I like to work with other people but have avoided committees because I expected that I could get more done following my own inclinations. Fortunately, Yale Medical School in the 1950s was a pretty open place – for piracy, that is. If you didn’t ask for permission, you could get quite a bit done. That was true even of space for labs. When a faculty member was leaving, his rooms were up for grabs – within reason. Over the next three or four years, the Gastrointestinal Section occupied space from the basement to the first and second floors, and even to the fifth floor, simply by getting there first. Those first few years were full of action, NIH money was lavish, academic life was burgeoning, and all of us young section chiefs were busy and happy – and collegial, with Paul Beeson our god and caretaker.

I was aware that the clinical possibilities at Yale were somewhat limited. The priorities at Yale Medical School even in the 1950s did not include clinical practice. I was sure that scholarly clinicians could come to reasonable conclusions with, let us say, five patients rather than fifty. That meant that the academic gastroenterologists did not need to take care of the patients as long as they could see them in a teaching capacity.

I brought New Haven clinicians into the academic enterprise and gradually enlisted clinicians from all around the state. Where other academics at Yale Medical School kept the clinicians in the community from using their facilities, I felt that we should compete on intellectual levels – oops, brain levels – and that physicians at Yale should be diagnosticians, experts in complicated matters, but not caretakers over a long period of time. I saw Yale as competing not with the folks in practice on York Street but with Harvard, Columbia, and Johns Hopkins.

That was one of the reasons why, in 1963, I set up the Yale Affiliated Gastroenterology Program, first at the Griffin Hospital, then at St. Raphael’s, finally including even Hartford Hospital and St. Francis, before the University of Connecticut established its medical school. At its peak, our program encompassed about twenty Connecticut hospitals, receiving financial support from the NIH, and we wrote a lot about it. That network, unique in those days, made it possible to set up large-scale clinical research, such as one on cancers of the pancreas and the stomach with the Mayo Clinic, Harvard, Columbia, and other schools. It was the model, until last year unrecognized, for the ongoing Yale Affiliated Hospital Program.

As I look back, that program enormously enlarged my clinical experience. Say what you will about physicians' intellectual life, practicing medicine is not like riding a bicycle, or even reading Homer. I was traveling to several other hospitals each week, accompanied by five or six medical students in my Volkswagen microbus, and at those hospitals I had the chance to see and review and discuss the most interesting and challenging patients. Since I believe that the patient-physician encounter fleshes out a developing narrative, I teach medical students and residents to start thinking about what might explain the patient's problem from the very beginning of the interview, changing diagnostic considerations as information develops. In clinical presentations to a group, I follow that approach, which mimics how clinicians work, as a good way to teach.

Academic physicians talk of the three-legged stool: research, teaching, and clinical care. The last two are inseparable, and as I am a kind of casuist, I have always felt that clinicians – whom in my franker moments I have compared to tailors – learn best by the case method. For that reason, in my clinic students, residents, and GI fellows saw the patient first; they would then tell me the story and findings, in front of the patient. I would ask questions of the patient to amplify some points. I answered questions from the patient as well as from the students, and finally offered my thoughts to the patient. I never heard complaints about this teaching exercise, but when I stopped being a teacher, I realized I had given patients no chance to decide. I made no exception for the rich and well-connected, so that even the heads of large corporations were treated the same as the poor working man. Was that right? Convinced of my rectitude, I never asked, even though I feared that my system could not be popular with referring physicians or their patients. But I saw my first duty as being a teacher. If I were to do all that over again, I would make clear to potential patients ahead of time how I worked.

I was learning so much from all this patient activity that it was difficult for me to resist writing a textbook, especially when Macmillan offered me the chance to do so. I was seeing, by my criteria, ten or so extra patients each week, cases that I would not have seen at Yale. Most evenings I could sit down at my dictating machine to spew out everything that I knew about a topic or whatever clinical problems I had seen that day.

Then my secretary typed up, triple-spaced, whatever I had dictated. Sometimes, mistakes in her transcription gave me my wittiest remarks. I would go over that hard copy, adding material from my "literature" files to amplify what I had stated. She then retyped the manuscript double-spaced, for me to put into what I generously told myself was better English: for doctors like me, that means sentences that can be understood without much thinking.

In 1970 the first edition appeared, all sixteen hundred pages out of my mouth – although some friends remarked that another orifice seemed more likely. That book changed my career direction in that there had been no good clinical textbook of gastroenterology available, certainly not one written by a single author. I had been doing

research for twenty years, as yet unaware of my adolescent yearning to be a teacher, and the success of that volume, which was taken up as a gift for graduating students and residents, made it irresistible for me to accept opportunities to speak roundabout. I could even think of myself as a St. Paul, bringing the gospel of academic gastroenterology to the gentiles out there in the community.

I tried to right the unequal relation between science and practice in my book and in my talks, reminding clinicians of what they should remember, but bringing them in on one new advance in our thinking. Nowadays there is a lot of talk about transfer of information from scientists to clinicians, usually on a one-way street, with clinicians learning about science and feeling that they have fallen away if they don't keep up. And yet that science is not always something that clinicians use; it is rather an affirmation of faith that they ought to be scientists.

Then too I had had an epiphany of sorts. Sometime in the late sixties, I was busy in the lab, probably telling other people what to do, when a telephone call asked me to see a patient in the Memorial Unit, as it then was called. I found myself annoyed, reluctant to leave what I was doing, until I said to myself, "Howard, you went into medicine to take care of patients. What is academic life doing to you?" I went to see the patient then and there.

Although I had been conscientiously responsible for much of the research the gastroenterology section published for a long time, by the 1970s – and doubtless even earlier – I had become aware that I no longer understood everything that was being done. I consoled myself with the idea that my name was helpful in getting something published. But without much hesitation – it took a year or so – I stopped trying to oversee any laboratory research and turned to clinical practice and teaching, with a great deal of writing thrown in. In exculpation, I should point out that this meant no difference in my salary, as all clinical income went to the dean. I was following my bent.

I must have been an entertaining speaker, and since I have an asymptotic relationship with ideas, I often found myself throwing in allusions to matters other than gastroenterology. Most of those animadversions were greeted with a laugh. As gastroenterology was still somewhat mysterious, a number of students from other countries were eager to come to New Haven, and mostly the Medical School was happy to have them. But when students from South Africa, then still tormented by apartheid, came to visit, some students protested enough for the authorities to forbid students from that country. I never did understand the reasons. But I saw a number of students from wherever I went and a number of foreign graduate students came to work with us, bringing their own support.

I skip all the next years. I was established, the various institutions regarded me as an opinion maker, I kept writing for different kinds of publications, and I traveled a good deal. I had little time to think. Although I was active against the Vietnam War, I was mainly a passive observer of the other social changes of the 1960s and 1970s –

the student rebellion, the black revolution, “women’s lib,” gay liberation, and wider equality for minorities – that so changed our lives in many ways.

The Last Years at Yale, 1983-99

In 1982, I gave up the job of chief of gastroenterology after twenty-seven years, after a moderate amount of friction with the then chairman of medicine. I was offered a sabbatical at Stanford’s Center for Advanced Studies in the Behavioral Sciences, largely, I suppose, because of some of my nonmedical effusions. There Marian and I spent a refreshing year with no responsibilities except for me to attend lunch everyday with the other forty-nine fellows, and to limit my travel. Being there was like being born again, or at least returning to my first year in college. Academic medicine is very demanding and I had been much focused. I worried that on my tombstone could be carved, “Born a man, he died a gastroenterologist.” Incidentally, that phrase was one I made up for some essay years ago. I thought it was funny and realized it was trenchant when I read that a well-known writer claimed to have seen it in a cemetery in Vermont. I guess she mixed memory with reality, as we all do.

241

At Stanford, chatting with people in other disciplines, from philosophy to history, led me to abandon a project of writing a book on law and medicine. (For five years or so, I had been running informal private seminars for practicing physicians and academics to discuss malpractice and other problems with lawyers both from the Law School and in private practice.) Instead, I read widely and thought about a book on placebos. The Stanford Center remains my ideal, a kind of Plato’s Academy.

When I came back to Yale in 1983, people who used to come into my office every day, to kiss the ring and ask advice, now waved at me from across the way. While I recognized that they had found others to look to for promotion and help, a very human turn, I was saddened at the transitory nature of relationships, even if several of the old GI faculty came to talk with me. But I had more time to contemplate what I wanted to do.

In my new office, removed at my suggestion far from the GI group (now renamed the Digestive Disease Section), I turned to writing the book around what I had learned to think of as the “placebo response.” Faith in what the doctor does – or gives – elicits a response from one- to two-thirds of patients, and then they feel better. During my sabbatical, I came to understand that I was a placebo myself, that not doing any procedures had removed me some from the mainstream. So I began to think in a more focused way about the physician-patient relationship and about the “healing” that it could bring. I repeat: the placebo response resides in the patient and not in the physician. We simply stimulate, deliberately or not, psycho-physiological reactions that help people to feel better. There is no magic in the placebo response, which occurs in as many as two-thirds of people regardless of education or status.

I emphasize the difference between “heal,” which is how the patient feels, and “cure,” which is what the doctor sees, but that is a talk in itself. My exposition brought

me in touch with the fringes of mainstream medicine, politely described as “complementary” medicine rather than “alternative.” In many ways, that was not a revolution for me, or even an evolution, because I had always been very much interested in the psychic aspects of disease. For many years, a psychiatrist had made ward rounds with us and a psychiatric social worker helped out in my clinic for about twenty years. Explicitly recognizing the power of suggestion and affirmation was not a wild foray. The Alternative Medicine Office at the NIH liked my opinions enough to suggest a paperback edition of my book, and I took the opportunity to turn it into an *apologia pro vita mea*, as *The Power of Hope*.

Coming back from Stanford, I had another project that never came to fruition: to get financial support for a newly minted Ph.D. in the humanities, where candidates would spend half-time immersed in medicine and medical practice for two years or so. I also wanted to find money to support a physician who had just finished clinical training to work half-time at Yale College in some nonmedical area. I hoped that in our projected Program for Humanities in Medicine, such scholars moving back and forth across the chasm between the university and the Medical School would provide a bridge that might humanize the increasingly technical medical training. But I never could get the money to do it. Even one of my wealthy benefactors commented, “Howard, I don’t want to spend money to entertain physicians!” His idea, shared by so many others, was for specialization to make advances, but he had been trained as an engineer.

Instead, Enid Peschel and I settled on a lecture program, with a committee of medical students playing a strong role and getting a chance to talk with speakers. Ours was one of the first in the country. The emphasis on students lasted for some years, but gradually disappeared as required “professional” activities took up more of their time. I did have time to take on a few new writing stints, and to get into trouble by commenting on what I saw as lapses. The program gradually turned largely into a lecture series, which has been quite popular with older physicians and the general public, but has strayed far from what I had hoped. We did come out with a few books, on empathy, death, and physicians straying from their work.

When I retired in 1999, mindful of my father’s advice not to try to rule beyond the grave, I turned the program over to Tom Duffy, who has taken it in other wonderful directions. But I still dream that our original plan might have given life to Milton Winternitz’s vision of the Institute of Human Relations, whose name is still carved above the entry to the Medical School.

Two episodes made me think about the nature of medical ethics. The first concerned a stellar professor of medicine at Yale who had moved to Columbia Medical School as chairman of medicine, but returned to Yale in disgrace a few months later. At Yale, he had supported a junior colleague who had taken data from a paper he was reviewing, and had planned to promote him at Columbia. The details are not important, though they touch on the whole problem of peer review both in editorial

work and at the NIH. The scandal involved several important leaders at the Medical School, but it sank from public discussion, as so many matters do. We had planned to consider the issue at our legal seminar, but the dean called to forbid even such private discussion. When I resisted, he warned that he would attend our meeting. Of course, his presence squashed free conversation, as you might imagine. After a year or two at Yale, and a hegira in the wilderness of Sinai for while, the protagonist finally fled to private practice on Park Avenue in New York and, in all honesty, academic medicine lost a star.

His fall—the greatest since Lucifer, I used to say—and, more important, the reasons for it have been completely forgotten at the Medical School. A few months ago, I asked one of the faculty in the medical history department what she knew of the incident and learned that she had never heard of it. The public believes that we physicians bury our mistakes, as we certainly do, if only metaphorically. But if we do not rehearse or remember the bad things that some of us have done, how can we prevent them from happening again? Bioethics and medical ethics give matters like the now infamous experiments at Tuskegee deserved reiteration, but less dramatic issues—kitchen ethics or venial sins, if you will—are glossed over. My reluctance to bring this matter up says something. I wrote an editorial on the topic some years ago, giving no names or details, but it was turned down by several national medical magazines.

Another example arose shortly after our return to New Haven. I read about a prize in the name of Hans Eppinger, a famous hepatologist of the 1930s and head of the University of Vienna Medical School from 1938 to 1945. This very famous man, at least a half-Jewish “honorary Aryan,” had killed himself rather than be tried at Nuremberg for experimenting on concentration camp inmates. I had heard of the story first in medical school: his name stayed with me because another Eppinger, Eugene, had been my advisor and model at the Brigham Hospital. The contrast between the two Eppingers stayed with me.

The prize had been sponsored by a pharmaceutical company in Freiburg and by Hans Popper, a professor at Mt. Sinai Medical School. In 1938 he had fled Vienna to go to Chicago, where he had been given a job by my father’s cousin, who ran the Chicago Medical School. I was disappointed that Popper had sponsored that prize and wondered why none of its recipients had asked about Eppinger. By coincidence, Popper was coming to New Haven to talk before the digestive disease group and I took advantage of our relationship to talk with him about the whole matter, which I considered disgraceful.

My exposé of Eppinger made public the whole sorry mess. *Time* magazine, the *New York Times*, and other news media took up the story, with the result that the prize was ultimately withdrawn. That made a lot of people mad at me, complaining that I had set back relations between Germans and Jews forty years or more. Nevertheless, getting rid of the Eppinger Prize made me feel proud, even if hepatologists still tell me it was a shame I brought the matter up.

Life After Yale

Since leaving the Gastrointestinal Section, I managed to keep busy, editing a few books, running around talking, and keeping out of trouble. After I retired from Yale in 1999, I would have been happy to work as a volunteer in the various clinics of the hospital. I ended up working not for the poor but for the rich, in my office at 40 Temple Street. I decided not to ask any residents or students to work with me, and I enjoyed the experience of working by myself. I had never allowed a buffer between me and anyone who came to see me: my desk had always been placed against the wall, so I could sit in front of it, in my academic office as well as in the clinic. Although I had lost the metaphorical buffer of students and residents, I continued my old habit of listening to the patient's story first without knowing information that had been sent. And I have continued that practice for the past few years. The numbers of patients referred has dwindled as my peers have retired, for doctors refer across age lines as enthusiastically as they play tennis. But as I am in my mid-eighties, aware of the bad image of elderly doctors – or of old folks in general – I do what I can. I do tell some of the patients that they are my psychotherapy, but only a few have suggested I pay them.

Following my father's advice, I gave up lecturing on gastroenterology to the medical public, editing the *Journal of Clinical Gastroenterology*, along with that program for humanities. I felt I ought to start over without a lot of baggage, but that has not been completely possible

That I am a workaholic searching for purpose even in old age I freely admit. That I derive my identity from being a physician I do not deny. After all, I started medical school at age nineteen and have had a liturgical life, since 1955 knowing approximately what each day would be like. So what?

I wish that society could find a place for us old doctors, those of us at least who want to continue working. We should not take care of patients in the hospital, where cures depend so much on the latest medications and the ever-advancing technology. But we should find a place in the out-patient offices and clinics, because our experience tells us that time cures many complaints, and that the right hand of fellowship helps 80 percent of patients coming to a physician. And we should have a place in the ICU's – vertical, not horizontal – asking questions about what is being done to the old and the dying. But, as I like to repeat, medicine is a field where the young rejoice to teach the old. It has always been that way, as the old poem reminds us: "New occasions teach new duties." The ways of the young replace the habits of the old, as they should.

Some of my friends do not like hospitalists, because they do not know the patients whom they treat. I disagree, because I worked like a hospitalist when I was a full-time gastroenterologist at the hospital. I knew better what to do about the acutely ill patients with ulcerative colitis than most of the practitioners. It would be folly for practicing physicians to try to substitute for hospitalists or ultraspecialists. It is too bad that the team does not know the patients as well as the doctor who took care of them in practice, but we should look for ways to improve the human aspects of medical care in these new and far more effective medical institutions.

Long ago, physicians looked at the patients and maybe their urine or the bumps in their skull structure. With advances in classification and examination, physicians began to focus on diseases. Later, technology let vision scrutinize individual organs or system of organs. After that, subspecialists took an interest in a component of the organ, and that was followed by evaluation of the separate cells. Cell biology fulfills Virchow's vision of cellular pathology, but advances have not ended. Now professors of medicine look at the genes responsible for enzyme changes that account for what are clearly now subcellular "diseases." We clinicians can easily talk of tyrosine kinase and our patients are grateful for the medications that command what it can do. And that, if I understand rightly, is simply the effect of a twist in an amino acid or two.

No wonder consideration of the patient is dropping out of medical vision. We old doctors may lament these changes, even as we boast of what subspecialization has done to improve the lot of patients with heart or gastrointestinal problems. If we can welcome experts in the esophagus or pancreas, why not cheer on experts in fibroblasts or in squamous cells? I grant you that no one has ever been nice to a fibrocyte or consoled an errant enzyme, but the microscopic vision will greatly help our patients even as they complain about the inhumanity of their doctors.

How can we restore the physician's attention to the person? Programs for humanities in medicine, like that at Yale, have been around for quite a while now and yet commentators continue to lament the peripheral relationship of humanities to the daily life of physicians, the narrowing of vision during medical education. We may be going about matters in the wrong way. Instilling a feeling for the human condition, which is what "humanities" is all about, should begin in college. Those premedical students who know that they want to care for the sick could have a program less focused on the hard sciences and "orgo" and far more on anthropology, history, and other topics with "big visions." Students who think about the humanities in their impressionable college years should be better able to bring very real human emotions to their later care of patients.

"Clinical educators" now teaching clinical work could by example teach about passion and empathy. Comments on the humanities need not be a separate program: the same physician should be able to raise questions about an alcoholic with a broken spirit as readily as about the housewife with a broken hip. These are matters which sensitive people think about, and if taught early and often in college life might not be left as part of a more distant world. Programs for humanities in medicine would take their place as a natural sequence in medical school.