

Editor's Preface

I first met J.P. Trinkaus on a warm autumn afternoon in New Haven, in 1964. At the time, I was completing my undergraduate work at Johns Hopkins and was looking at graduate schools. I had been inspired to become a developmental biologist by one of Trink's scientific competitors and friends, Malcolm Steinberg, who was at that time a faculty member in the Biology Department at Hopkins. I was fortunate enough to have had the opportunity to work in Mal's lab, where he set me to work learning to dissect pigmented retina from the eyes of 6-day-old chick embryos cleanly enough to produce pure cell populations. This assignment was my first real exposure to an active laboratory in developmental biology research and I was having the time of my life. Malcolm knew Trink's style well and knew my idiosyncrasies from our interactions. He suggested that I interview with Trink to see if he and I would be compatible. So off to Yale I went to have a look at the Department of Biology and to meet Trink.

I walked around the campus a little, being suitably impressed by the imposing gothic buildings. Yale was a huge university compared to my relatively cosy undergraduate experience. I can remember being a little intimidated and a lot impressed. I met with Professor E.J. Boell, who used his elegant verbal style and stentorian baritone voice to describe Yale's virtues. Professor Boell's style and tone brought to mind then Senate Minority Leader Everett M. Dirksen. Then it came time to meet with Professor J. P. Trinkaus.

Professor Boell introduced me to him. Professor Trinkaus told me to call him Trink and welcomed me, motioning for me to sit down on a couch facing his desk. He was dressed in a pair of dark gray slacks, a white shirt, and a long black knit tie casually knotted loosely at his open collar button. He sat down at his desk, propped his feet up on his desk in a relaxed way and asked me to tell him about myself. He smiled often and warmly, laughed at my jokes, and made me feel comfortable. I knew immediately, from his informal style, and from the warm reception that he gave me, that I liked him. He had an ingratiating way of making me feel much more significant than I was. He also described his philosophy of training graduate students, basically giving them tremendous latitude in the choice of research projects, as long as it was in his general area of

expertise. I had read many of his papers and the papers of some of his well-regarded students with great interest in the intimate Biology Library in Mergenthaler Hall at Hopkins. At the time, I was intrigued by cell sorting in mixed cell aggregates (Steinberg's influence) and issues of how changes in cell adhesive and motile behavior drove early morphogenetic cell movements. Trink was certainly expert in those areas. At the time, I imagined that I knew what I was doing in science and had some specific ideas about research projects to pursue in graduate school. I don't think Trink was impressed by my ideas, for they were pretty simplistic, but he had a wonderful way of reacting to my naïveté that made me feel valued and significant. I think he liked my chutzpah. He spoke to me as if I had said something important. His voice had a timbre that conveyed enthusiasm and optimism. I decided about 15 minutes into the hour-long interview, that, if accepted, I would come to Yale to work with Trink. It was a no-brainer for me.

My career as a graduate student at Yale was difficult. I arrived thinking that I was the best thing since sliced bread, but soon realized that I was somewhere near the bottom of the heap of my cohort of new graduate students. They all seemed smarter, more competent, polite, politically astute, and disciplined. I quickly found myself quite intimidated by the competition. I started working in Trink's lab my first semester at Yale, mostly making messes in the lab and invading the personal space of other students. I casually used Kimwipes (wiping tissues) from one student's "personal" box. It never crossed my mind that anyone would even think of having a personal supply of Kimwipes. In spite of myself, several other veteran graduate students seemed to appreciate my spirit and sense of humor, and they treated me very well, indulgently helping me learn some discipline. Professor Boell gave me several much-needed talks about "collegiality" and "departmental citizenship." I would meet with Trink only rarely, to describe what I was up to. He would listen, make me feel like I was important again, and wisely pique my interest in studying amphibian gastrulation, a topic that came to consume my scientific interests for my entire research career. He saved me from myself on more than one occasion and was crucial in keeping me from being expelled from Yale (see p. 256). Finally, thanks to help from many excellent peers (who came to Yale because Trink was there), and my mentor's deft guidance, I was able to produce a satisfactory dissertation. I got the only two academic positions I have ever held because of his and Yale's reputation. Over the years, I always enjoyed reconnecting with him. He was always enthusiastic, supportive, had good suggestions, and made me feel welcome.

I had read both editions of his book *Cells into Organs* quite closely, mainly searching for good research problems. In 1998, I telephoned him to urge him to produce a third edition of this important book. He was not enthusiastic about my suggestion because he wanted to focus his few remaining years to

continue his research. He also mentioned that he felt out of touch with some parts of the rapidly growing field and that Prentice-Hall was not eager to publish a third edition. I told him that I had an idea. I suggested that members of his extended scientific family should update his second edition of *Cells into Organs*, co-authoring new chapters with Trink. It seemed to be a good way to honor him for all that he had done for the world of developmental biology. By that time, I was head of a small but lucrative publishing company of my own, and indicated interest in publishing such a work, assuming that Prentice-Hall would turn the copyright back to Trink. He was a little interested in this idea and told me that he would think it over.

About a month later, he wrote to me declining my suggestion, saying that he wanted to complete several areas of research and publish the results as his last papers before he died. He was more interested in his beloved research of *Fundulus* than receiving further accolades. I accepted his decision but wondered how, after so many years of research, he was still able to muster enthusiasm for being at the bench. I pulled out a thick file of his reprints that I had gathered over the years and noticed that he started publishing in the late 1930s. I started counting the decades. Including the '30s, he had published in seven decades, and, if he continued to publish after 2000, he would have published in eight different decades. I suddenly realized that he was a true scientist, who was interested in solving complex problems. I felt tremendous admiration for his attitude. As I thought more about his life, I realized that his scientific career was a link between the classical period of embryology and the modern molecular biology that has been applied with such spectacular success to problems of developmental biology. I also realized that he had led a truly interesting life that merited a short memoir. I immediately knew that I wanted to convince him to write a memoir and that I wanted to publish his book. He was uncharacteristically unsure of himself in this different kind of writing. I reassured him that he should just write his life down and we would worry about the details later. Finally, he agreed to write this memoir. We worked up a contract, I sent him a small advance against royalties, and he set to work.

While he was writing, my publishing enterprise had fallen on hard times and my professional situation had undergone some dramatic changes. Trink was understandably concerned about the limited amount of time he had left before he died. I was slow in moving the publishing project along and he expressed his concern in his characteristic direct style, saying, "I want this thing published before I die." I wrote him a long apologetic letter saying that I just could not pick up the pace and explained the reasons for the delay, offering him freedom to take back the manuscript and try to get some other publisher to publish it instead of my company. In a recent letter, he declined my offer, saying that he wanted me to continue as his editor and publisher and that if he died before I got around to publishing his memoir, then so be it. J.P. Trinkaus was a smart, compassionate, and

loyal man. His confidence in me once again made me feel important, and lit a fire under me. My intuition about Trink, on that fall day in 1964 in his office in New Haven, 38 years ago, was correct. I sensed on a deep emotional level that I would derive life-long benefit from learning from him. His intuition about me, that my skills and enthusiasm could be cobbled into producing a decent dissertation and scientific career, also turned out to be true. He was like a good father to me. My efforts on this memoir are my own way of saying, "Thank you, Professor Trinkaus." The result, *Embryologist*, is before you. I hope you find as much pleasure in reading his memoir as I did while contributing to its emergence. My only regret in this entire undertaking is that Trink died before the book was published. He did see the final version of the text and expressed pleasure about the outcome. R.I.P. old friend.

Kurt E. Johnson, Ph.D.
Professor of Anatomy and Cell Biology
The George Washington University School of Medicine
Washington, D.C.
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