WITH GOOD FORTUNE, A DREAM OF YOUTH REALIZED

Pierre R. Demarque

I was born in Morocco in 1932, in the modern town built just outside the walls of the holy city of Fes. Morocco was at the time a French protectorate. My young parents had recently arrived from France. My father was a civil engineer, graduated from one of the elite engineering schools in Paris. He was involved in the construction of a large hydroelectric project, which was to open a portion of the Moroccan desert to agriculture. Our family then moved to the Algerian seaport of Oran for the construction of sea terminals. Algeria was then an integral part of France. Oran is the site of my first childhood memories. I have been told that some of the first words I spoke were Spanish and Arabic, learned from the young women who took care of me.

My parents enjoyed their life in North Africa, but my younger sister became very ill, and we soon returned to France to live in a pleasant suburb west of Paris. Only three years later, in 1939, the Second World War erupted. My father was called to serve in the military, as did my three uncles and one of my grandfathers (the second time in his life for him). During the invasion, my father was a sapper, blowing up bridges before the advancing enemy. He experienced firsthand the hell of Dunkirk and was eventually captured and taken to Germany as a prisoner of war. My mother, my sister, and I joined the massive exodus to south central France and eventually returned to occupied France, in Saint-Germain-en-Laye, for the rest of the war. I was as a child much exposed to the miseries and horrors of war. These events left a deep impression on me for the rest of my life. During these years, I experienced a number of serious health problems. I contracted tuberculosis and was fortunate to be sent to a Swiss sanitarium for recovery. Years later, as a university undergraduate, I had a recurrence of the disease.

Schools had reopened normally during the occupation. I am lucky to have been born into a family that attached much value to education, and I was expected to get good grades. My mother made sure that I did my homework, and most of the time I did well. I enjoyed writing essays, read history, learned the rudiments of English (instead of German, which was then very much encouraged), and excelled in Latin

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Pierre R. Demarque, Munson Professor Emeritus of Natural Philosophy and Astronomy, came to Yale as professor of astronomy and department chair in 1968, holding the latter post until 1975. During that time, he changed the focus of the department to astrophysics, laying the groundwork for making the department a leading international center in this area. His research has centered on the structure of stars, their ages, the evolution of galaxies, and the solar interior. He was named the Munson Professor in 1979. Demarque became the first director of Yale's Center for Solar and Space Research in 1985. Honors include the 1967 Warner Prize of the American Astronomical Society. He served as president of the International Astronomical Union's Commission on the Internal Constitution of the Stars from 1991 to 1994. He is a Fellow of the Royal Astronomical Society of London, and of the American Association for the Advancement of Science.

and classical Greek. I was considered a good student, one likely to qualify for advanced education. My leisure time was spent in long hikes in the nearby forest, either alone or with a few close friends of my age. I also enjoyed reading adventure stories, stories of great explorers and navigators, and of course the books of Jules Verne. There was no evidence in these early years that I would be a scientist. I was not particularly moved by the mathematics and natural science that I was exposed to in school. My mathematics teacher once told my mother that he did not think that my future was likely to include the use of mathematics.

There was, however, in the absence of my father, one role model in my life who exposed me to scientific research. My mother's youngest brother, still in his early twenties, had been released from captivity by the Germans and allowed back to his interrupted graduate studies in chemistry. He would talk to me about his research and once took me to his physical chemistry laboratory, in a deep basement at the Sorbonne. I was enthused for days about this visit.

The liberation, which took place in August 1944, was a long-drawn-out affair in Saint-Germain-en-Laye. The occupiers had abandoned the town, but for several days, there were no signs of the Allies. We were effectively in a no man's land. One could hear the continuous rumblings of artillery in the distance. On a couple of occasions German army motorcyclists sped through the streets, their guns blazing and causing great panic and commotion. It is impossible to describe the feelings of elation and relief when the first American tanks finally entered the town.

Soon after my father's return from the war, he decided that he had had enough of Europe and its turmoils, and in 1947 our family emigrated to Canada. We arrived in Montreal before my fifteenth birthday. I was sent to a French-speaking school to finish my secondary education.

In my new Canadian identity, I became a different person. Under the influence of an enthusiastic, dedicated, and eccentric science teacher, math not only suddenly made sense, but also I discovered that I had a talent for it. I became very good at solving geometry problems and at calculus. At the same time, learning about physics became great fun, and I developed a passion for reading about modern physics and quantum theory, mostly what would be considered works of vulgarization, often written by famous scientists, such as Louis de Broglie, the inventor of wave mechanics. Bell's *Men of Mathematics* (which by the way includes one woman, Sonja Kowalewski) made a particular impression on me. The list of these books included astronomy books by A.S. Eddington and George Gamow, and introductions to general relativity and cosmology. To this day, I continue to have a fanlike fascination with Albert Einstein and his extraordinary genius.

Perhaps the strongest influence came from the Cambridge astrophysicist Fred Hoyle. I first heard him in his BBC radio series, "The Nature of the Universe." It was later a great excitement for me to become acquainted with him during his visits at the University of Chicago, in California, and later in England. Sir Fred was known for the steady-state theory of cosmology, for bold insights in nuclear astrophysics, and later for rather wild ideas about many topics (such as the origin of epidemics). We had very different scientific temperaments, but he always encouraged me in my work both privately, and publicly at conferences, in the most generous way.

I chose to study at McGill University as an undergraduate. The Macdonald Physics Lab was still basking in the glory of illustrious pioneers in radioactivity research, Nobel laureates Ernest Rutherford and Frederick Soddy, in the early part of the century. On the basis of my background, I was allowed to enter in the second year. But I fell ill in the middle of the year with a relapse of the tuberculosis that I had contracted in France during the war, resulting in effect in the loss of two academic years. I was confined to my bedroom for an extended period. The forced inactivity obliged me to do a lot of reading, much of it nineteenth-century literature such as the novels of Balzac. The much-looked-forward-to return to physics turned into a disappointment. I found the environment foreign and unfriendly. The lecturers appeared remote and dismissive of students. No feedback was provided in classroom homework. I was never assigned an adviser throughout my undergraduate days. We were exposed to mountains of material that I could not assimilate. I got my degree, did well in some math exams, but the "sink-or-swim" experience was a truly devastating blow to me.

One happy experience in my final year was a summer job at the Dominion Observatory in Ottawa. At the time, the Dominion Observatory concentrated on solar and geophysical research. The director, Dr. C. S. Beals, took a special interest in summer students. He had previously made a reputation at the Dominion Astrophysical Observatory in Victoria, B.C., in stellar spectroscopy and had then switched to conducting a survey of meteorite crater candidates in northern Canada. This involved seismological expeditions in the field. I was assigned to a team that explored the possibility that the Gulf of Saint Lawrence was the result of a meteorite crater. My team was sent to Prince Edward Island to detect seismic signals from depth charges dropped at sea by the Canadian Navy. The field seismographs we used were very sensitive, and one of our problems was having to deal with stray signals from the cows walking in nearby meadows. The results turned out negative, but this was a marvelous learning experience that nurtured my enthusiasm for science. Back in Ottawa, Dr. Beals offered me scholarship support to study for a Ph.D. in terrestrial seismology. I was tempted, but was still dreaming about an astronomical future.

Upon graduation, I decided instead to take some time off to think about my future and to save some money. I found a job at Canadair, then a major aircraft manufacturer in Montreal. On the basis of my mathematics background, I was assigned to a computational group. Aircraft design involves numerical tasks (such as the inversion of large matrices), which had until then been performed by hundreds of human computers working with desk calculators. My group had been set up to convert IBM accounting machines to these numerical tasks. I learned how the computers of the time worked, how to program in machine language, and about the design of suitable algorithms. As a bonus, during lunch break, I would take pleasure in watching production line workers on the assembly lines for Sabre jet fighter planes.

Not knowing any better, and thinking it was the best preparation for theoretical astronomy, I made the decision to study applied mathematics and to apply to the University of Toronto graduate program.

I was accepted for graduate studies in the Mathematics department. Upon arrival in Toronto, I was interviewed by Professor H.S.M. Coxeter, whom I found out later was world renowned for his research in geometry. I still remember quite clearly this interview, which had a decisive effect on my intellectual trajectory. After quizzing me at some length about my interests, Professor Coxeter explained to me that the University of Toronto had begun a Ph.D. program in astronomy, the only one in Canada at the time, and he put me in contact with Professor J.B. Oke, an assistant professor in astronomy fresh out of Princeton. Bev Oke, who eventually became a Caltech professor, convinced me to transfer to astronomy. He and Leonard Searle, another astrophysicist who like Oke had studied under Lyman Spitzer at Princeton, and who became my thesis adviser, made a deep impression on my research future.

My graduate years at the University of Toronto turned out to be some of the happiest and most stimulating years of my life. This is where I met my wife, Marlene, then a graduate student in classics. We married while we were both still graduate students.

I studied and benefited from the graduate physics courses offered at Toronto in quantum and statistical mechanics, electricity and magnetism, classical mechanics, and relativity. I also learned much general astronomy, about the latest frontiers in astrophysics, and discovered the study of stellar structure and evolution, the field in which I have nearly exclusively worked until now.

Before discussing my research, let me relate an amusing anecdote. At Toronto, all students were expected to participate in the spectroscopic observing program at the David Dunlap Observatory, located a few miles north of the city. While the instrumentation was up to date, personal comfort and safety for observers were considered secondary. The dome was unheated, and the work involved climbing on a high ladder in the dark. During one of my first observing nights, while searching for a target star in a complicated stellar field, I lost my sense of direction and stepped down on the wrong side of the stepladder, fell to the concrete floor, and smashed my right kneecap. The physician who treated me taught in the medical school. He invited me to meet his class and displayed my leg to a large group of medical students. He explained to them that I was a graduate student in astronomy and that I had fallen from a ladder while observing at the David Dunlap Observatory. “This is a perfect example of a stellar fracture,” he explained, to the laughter of the whole class. The accident confirmed my intention to work on theoretical rather than observational research from that time on.

But even theory also requires modern facilities. Originally, the only computational tools I had access to in the department were desk calculators operated manually with a crank. It took some doing being provided with a Marchant electric desk calculator.

Eventually I was given access to the university's electronic computer, called FERUT (for Ferranti Electric Company and University of Toronto). This enormous machine occupied a large room filled with thousands of huge electronic valve tubes of the kind we all remember in our old radio sets. In those days, one had to learn machine language. I communicated with the computer with paper ticker tape. The operation of the machine required frequent human attention, and I spent many nights sleeping by it to get my stellar models done. These first computers were slow and not very smart, and the data were laboriously assembled graphically and plotted by hand. But I thought that they were wonderful, and I was able to construct state-of-the-art stellar models for a master's degree and then my Ph.D. thesis.

When I finished in 1959, Sputnik had just been launched, but the scientific expansion it stimulated had not yet reached most astronomy departments. Jobs were hard to find. My first jobs at Louisiana State University and the Universities of Illinois and Toronto required a heavy teaching load and limited time for research, but I managed to write a few good papers that attracted the attention of Professor S. Chandrasekhar at the University of Chicago, where I was offered a tenured position. While there I made enormous scientific strides from my frequent interactions with Chandra, with other colleagues at the Fermi Institute, and with such astronomy luminaries as W.W. Morgan and W.A. Hiltner.

I joined the Yale faculty in the summer of 1968, when I was invited to chair the Astronomy department and to help rejuvenate its program. After some hesitation, I accepted the offer. I think that I was recommended by Professor Rupert Wildt, whose work was familiar to me, and whom I admired personally. Rupert, who had pioneered the study of the interiors of the major planets Jupiter and Saturn, and had many common interests with Karl Turekian, was the only senior astrophysicist in the department, and he was then approaching retirement.

Now I would like to say a few words about the research that my collaborators and I conducted at Yale on stellar evolution, a field that flourished throughout the last decades of the twentieth century. For more than thirty years, we carried out research on many specialized aspects of stellar physics, including stellar rotation, convection in stars, physics of the solar interior, and stellar pulsation. I played a role in most of these activities. In 1985 the Center for Solar and Space Research was created. Supported by NASA, it stimulated further interest in the solar interior, under the leadership of Professor Sabatino Sofia.

For the purpose of today's presentation, and to give you a flavor of some long-term themes in my research, I will now focus on a couple of prolonged controversies in which my collaborators and I were involved, that spanned several decades and coincided nearly exactly with my active years of research at Yale. The first one was about the age of the oldest stars, and the constraint it puts on the age of the universe. The second one was about the interior of the Sun, our own star, and the story of the missing solar neutrinos.

But first, let me briefly sketch the history of the field of stellar structure and explain how after 1950 it developed into the study of stellar evolution, whose influence still pervades most of astronomy. The modern field of stellar structure, or internal constitution of the stars as it was then called, dates back to the late part of the nineteenth century. Although clearly not a practical field, it is effectively a branch of applied physics; its development runs parallel to the extraordinary growth of physics during the nineteenth and twentieth centuries. Interestingly enough, the origin of the field has a Yale connection. The first modern paper on stellar structure, an investigation of the internal structure of the Sun, was written in 1869 by J. Homer Lane for the *American Journal of Science,* which was published at Yale. Lane was a graduate of Yale College, class of 1846. He later worked at the patent office in Washington. His contemporary Simon Newcomb described him as "an odd-looking and odd-mannered little man, rather intellectual in appearance, who listened attentively to what others said, but who, so far as I noticed, never said a word himself." Not quite the typical Yale undergraduate of our days.

In his pioneer paper, Lane considered the structure of a gas sphere in hydrostatic equilibrium, with the help of the gas laws known at the time. The field advanced with progress in statistical mechanics and electromagnetic theory later in the nineteenth century, then atomic and nuclear physics in the early and mid-twentieth century, respectively. By the early 1950s, pioneer efforts were made to think in terms of the next obvious step in the subject, the study of stellar evolution. Once the source of stellar energy had been identified as nuclear fusion of hydrogen into helium, the next questions were, What happens to the star as it undergoes nuclear transformations? How does it change in luminosity, in temperature, and on what timescale? An understanding of stellar evolution held the promise of explaining the observed differences in stellar populations in terms of differences in their chemical composition and ages.

Starting in the 1950s, the last decades of the twentieth century were an extraordinary period in astrophysics. As I entered the field, I found myself in the exhilarating position to address for the first time major scientific questions such as determining the ages of stellar populations; answering basic questions about the history of our own star, the Sun; establishing a chronology of our galaxy based on its fossil record; and for the first time beginning to shed some light, based on physical theory, on the age of the universe. Previous generations had prepared the scientific ground. Some mathematical tools had been developed. And computer technology was reaching maturity.

At Yale, in collaboration with young researchers and colleagues, I had the opportunity to carry on a variety of projects, large and small. Let me describe in a few words two controversial questions that dominated our efforts over several decades. By coincidence these two problems found their resolution around the time of my formal retirement from teaching (thus giving me the luxury to return to problems since my retirement that had previously been swept under the rug but have now become tractable, and have become relevant to the new field of exoplanet research).

One such problem was the ages of globular star clusters and their connection to cosmology. The twentieth century saw the beginning of the modern science of cosmology, or the study of the physical universe and its past history. It started with the extraordinary revolution in the theory of gravitation, Einstein's theory of general relativity, which provided a framework for describing the structure and evolution of the universe. Einstein's early models were artificially constrained to be static. He had introduced a cosmological constant, which he regretted and described as his "greatest blunder," because soon after the universe was found to be expanding, and the law of the expansion rate of the universe, now called Hubble's law, was discovered (it had in fact been first derived earlier theoretically from Einstein's equations by the Belgian scientist Georges Lemaître). In this picture, starting from the observed recession velocities, and with some knowledge of the distance scale and other cosmological parameters, one can derive an age for the universe, the so-called cosmological or Hubble age of the universe.

On a parallel track, our research group focused especially on the determination of the ages of the oldest stars, stars found in the halo of the galaxy, more specifically in the ages of the oldest known star clusters, the globular star clusters. The idea was not only to understand the formation of the galaxy but also to set a lower limit to the age of the universe. This so-called nuclear age would have to be reconciled to the expansion age of the universe within the constraints of a particular cosmological model.

For half a century, there had been two parallel, independent tracks of research regarding the universal timescale, and for most of that time, the two timescales were inconsistent with each other. To everyone's frustration, the nuclear age exceeded the expansion age. The oldest stars were older than the universe. Encouragingly, the two timescales did not differ by more than a factor of two, but as such they were useless for significant cosmological studies. On the cosmology side, the main problem was the establishment of a reliable distance scale. On the modeling side, the physics of stellar interiors, both macroscopic and microscopic, underwent many improvements, mostly thanks to the national laboratories involved in weapons research (stellar interiors have much in common with nuclear weapons).

For a long time, I feared that we might never know the answer for this age discrepancy in my lifetime. Cosmologists often discounted nuclear ages as too indirect and subject to too many uncertainties. In my view, the reason for the discrepancy was probably due to incomplete information and to the fact that some important piece or pieces in the cosmological puzzle were probably still missing. The solution came in the late 1990s when new observations overturned the simple cosmological model that had so far been favored. The need for a cosmological constant was restored. Einstein's self-admitted "greatest blunder" turned out not to be a blunder after all, and the timescale conflict disappeared. In 2011 this revolutionary discovery earned a well-deserved Nobel Prize to Saul Perlmutter, Brian Schmidt, and Adam Riess.

At this point, cosmology remains very much unfinished, but the cosmological age is now in line with the age of the oldest stars (which turned out to be correct). And globular cluster ages have now ceased to be part of the cosmological discussion.

Another prolonged controversy in which I took an active part was the missing solar neutrino problem. In a nutshell, the issue was the following: if we are to believe that the Sun derives most of its energy from the nuclear fusion of hydrogen, we should observe on Earth the flux of neutrinos that are predicted by the chain of nuclear reactions in the standard solar model. Very sensitive observations by Raymond Davis and his collaborators in the deep Homestake gold mine in South Dakota revealed no such signal. The controversy about the missing solar neutrinos lasted forty years and was finally resolved about fifteen years ago after it became possible to study the behavior of solar neutrinos in more sensitive deep underground experiments and to measure the sound speed in the solar core with helioseismology. The answer was a surprise: the problem arose from our faulty understanding of the nature of neutrinos, not from flaws in the Davis experiment, nor from defects with existing solar models. By the way, Ray Davis, a Yale chemistry Ph.D., won the Nobel Prize for his efforts.

Ironically, these two scientific questions that had occupied me for most of my career became moot at the time of my retirement in 1999.

After my retirement, I was much looking forward to revisiting unfinished business. The time was ripe to return to a problem that had always intrigued me, a perpetual source of headaches in calculations of stellar ages. The question is the competition between radiation and convection in transporting energy in stars, the source of many uncertainties in deriving age. This seemingly rather abstruse problem was first discussed in a classic paper by Karl Schwarzschild (best known for his pioneering work on black holes) in 1906. Now a hundred years later, with modern tools, this problem has become tractable, and with indispensable contributions, first from a postdoctoral fellow, and now from my Yale faculty colleague Sarbani Basu and an outstanding graduate student, the solution to Schwarzschild's classic problem is in sight. I have also had the satisfaction of continued long-distance interaction, thanks to the Internet, with several former students now established in Canada, Germany, and South Korea.

I'd like to end on a favorite quote from the great mathematician Henri Poincaré: "The scientist does not study nature because it is useful; he studies it because he delights in it, and he delights in it because it is beautiful."