The Abdus Salam
International Centre for Theoretical Physics

United Nations Educational, Scientific and Cultural Organization

IAEA
International Atomic Energy Agency

One Hundred Reasons to be a Scientist

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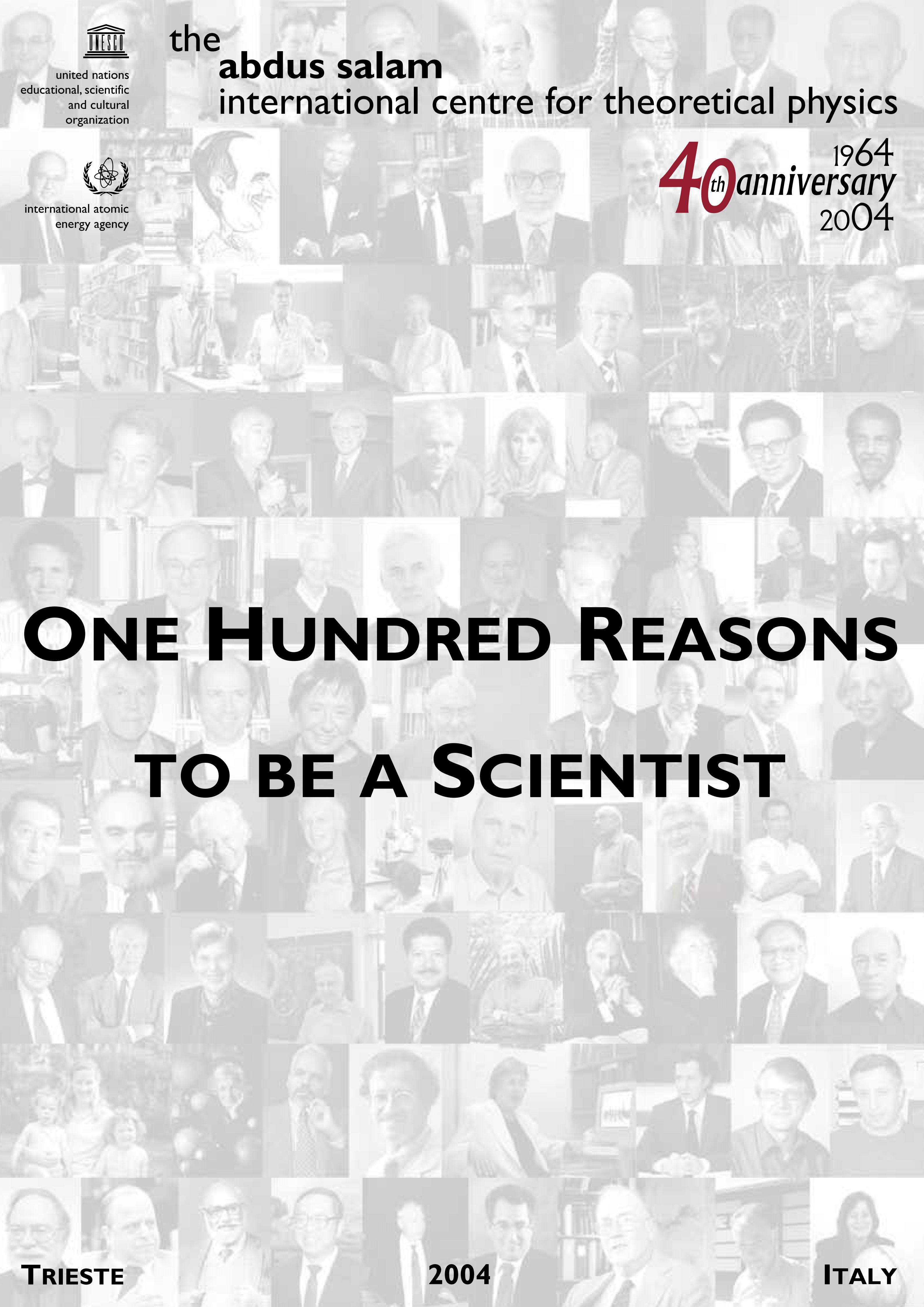

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40th anniversary
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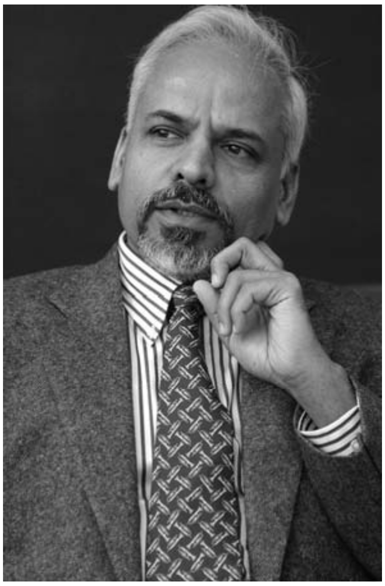
PREFACE

Katepalli R. Sreenivasan
The Abdus Salam International Centre for Theoretical Physics, Trieste



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A paradox of our times is that, while our societies have come to depend on technological advances as never before, the interest in basic sciences is diminishing at all levels. Particularly distressing is the lukewarm interest shown towards sciences by the brightest students at the high school level. This state of affairs holds true, to the lowest order, in developed as well as developing nations, and deserves our collective attention.

The International Centre for Theoretical Physics (ICTP) at Trieste, now named after its founding director, Abdus Salam, has been at the forefront of disseminating scientific knowledge to all segments of scientists. Taking as the occasion the 40th anniversary of our Centre, we thought it to be valuable to produce this book containing a number of brief and personal accounts by some of the most eminent scientists of our time, of what it was about science that captured their imagination as youngsters and kept it alive, and what main piece of knowledge they have added to the extraordinary lore of science. What message do they have for the budding scientists?

In choosing the writers, I have kept in mind high standards of scientific accomplishment and their connection to the Centre in some fashion. Slightly fewer than a hundred authors graciously honoured the invitation to contribute, and I am pleased to place before you the result of the effort. The emphasis is different in each article but the authors have all made efforts to be accessible without demanding any special knowledge or expertise on the part of the reader. Many authors speak from their heart and distil their experience directly. I have no hesitation in saying that these pieces reflect the seriousness with which the contributors took our missive and the affection with which they hold our Centre and its mission. I cannot imagine that they would be sufficiently motivated otherwise to take the time and make the effort needed for the task. I am grateful.

I hope that the readers will find these essays at least as inspirational as I have; I would have been pleased if I had the opportunity of exposing myself to similar articles as a young student. It is my belief that high school students and young college students, for whom the collection is primarily meant, will benefit as intended from spending some time with the book. Even the most seasoned researcher will find it interesting.

This seemingly straightforward task needed some work on our end as well. Without the diligent efforts of Mrs. A. Gatti, it would have been very hard to translate the concept into reality. Mr. E. Fratnik of the ICTP library lent his invaluable technical expertise and time. The concept itself arose in a conversation with Professor C.N.R. Rao. To them all, I am very thankful.

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SCIENCE AND SCIENTISTS IN DEVELOPING COUNTRIES[1]

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I was born in the country town of Jhang, then part of British India, now Pakistan, in 1926. My father was a teacher and educational official in the Department of Education and my mother was a housewife. I had 6 brothers and 1 sister. My family was by no means rich. My father took a vast amount of interest in my school work. He had great ambitions for me. I was destined for the Indian Civil Service, entry to which was by competitive examination. However, this was not to be—as events in my life took a different turning.

When I was at school in about 1936 I remember the teacher giving us a lecture on the basic forces in Nature. He began with gravity. Of course we had all heard of gravity. Then he went on to say "Electricity. Now there is a force called electricity, but it doesn't live in our town Jhang, it lives in the capital town of Lahore, 100 miles to the east". He had just heard of the nuclear force and he said "that only exists in Europe". This is to demonstrate what it was like to be taught in a developing country.

When I was 14, I won a scholarship to Government College, Lahore, with the highest marks ever recorded. I recall that when I cycled home from Lahore, the whole town turned out to welcome me. I wrote my first research paper when I was about sixteen years of age which was published in a mathematics journal but I wasn't actually hooked on research till I went to Cambridge University.

I was very fortunate to get a scholarship to go to Cambridge. The famous Indian Civil Service examinations had been suspended because of the war and there was a fund of money that had been collected by the Prime Minister of Punjab. This money had been intended for use during the war, but there was some of it left unuesed and five scholarships were created for study abroad. It was 1946 and I managed to get a place in one of the boats that were full with British families who were leaving before Indian Independence. If I had not gone that year, I wouldn't have been able to go to Cambridge; in the following year there was the partition between India and Pakistan and the scholarships simply disappeared.

[1] This article is put together from various interviews given by Salam, and the sentences are mostly his; we thought that it might be instructive for the reader to understand Salam's motivations in establishing ICTP.

At Cambridge, I achieved a First in the Mathematics Tripos in two years. I still had a third year free in the sense that I had the scholarship and the choice of whether to go on with higher mathematics—that's part III of the mathematics tripos—or to do the physics tripos. On the advice of my tutor, Fred Hoyle, who said "If you want to become a physicist, even a theoretical physicist, you must do the experimental course at the Cavendish. Otherwise, you will never be able to look an experimental physicist in the eye", I joined the Cavendish Laboratory where Rutherford had carried out his experiments on the structure of the atom. The Cavendish was an outstanding laboratory for experimental work and a focus for physicists around the world. However, I had very little patience with experimental equipment. To be a good experimenter you must have patience towards things which are not always in your control. I think a theoretician has got to be patient too, but that is with something of his own creation, his own constructs, his own stupidities.

The very first experiment I was asked to do was to measure the difference in wave length of the two sodium D lines, the most prominent lines in the sodium spectrum. I reckoned that if I drew a straight line on the graph paper then its intercept would give me the required quantity I wanted to measure. Mathematically, a straight line is defined by two points and if you take one other reading then mathematically that should be enough since you then have three points on that line, two to define the straight line and the third one to confirm. I spent three days in setting up that equipment. After that I took three readings, and took them to be marked. In those days the marking of experimental work in the class counted towards your final examination. Sir Denys Wilkinson was one of the men who supervised our experimental work, and I took it to him. He looked at my straight line, and asked "What's your background?" I said "Mathematics". He said "Ah, I thought so. You realise that instead of three readings you should have taken one thousand readings and drawn a straight line through them". I had by that time dismantled my stuff and didn't want to go back. So I tried very hard to avoid Denys Wilkinson during the rest of the year. I still remember the results came out in 1949. I was looking at the results sheets hung in the Cavendish and Wilkinson came up behind me. He looked at me and said "What sort of class have you got?" and I very modestly said "Well, I've got a first class". He turned full circle on his heel, three hundred and sixty degrees, turned completely round, and said "Shows you how wrong you can be about people".

I went back to Lahore in 1951 and taught there at the University. But as a physicist, I was completely isolated. It was very difficult to get the journals and keep in touch with my subject. I had to leave my country to remain a physicist. Now, it is the lack of this contact with others that is the biggest curse of being a scientist in a developing country. You simply do not have the funds, the opportunities, which those from richer countries enjoy as a matter of course. There are not the communities of people thinking and working in the same fields. This is what we have tried to cure by bringing people together at the International Centre for Theoretical Physics which I founded in Trieste in 1964. The Centre provides the possibility for scientists to remain in their own country for the bulk of the time, but come to the Centre to carry out research for three months or so. They meet people working in the same subject, learn new ideas and can return to their own country charged with a mission to change the image of science and technology in their own country.

I returned to Cambridge in 1954 as a lecturer and Fellow of St. John's College. Three years later, I accepted a professorship at Imperial College, London, where I succeeded in establishing one of the best theoretical physics groups in the world.

The pinnacle of my physics career came in 1979 when I shared the Nobel Physics Prize with Sheldon Glashow and Steven Weinberg for our unification of electromagnetism and the weak nuclear force in the 'electroweak' (a word which I invented in 1978) theory, one of the major achievements of twentieth century physics. This theory had made predictions that could be verified by experiment. The most revealing of these was that a new particle exists at extreme energies. To test this theory we had to convince the experimental physicists working on the great particle accelerators to build new equipment: To create, in principle, conditions that would be similar to those first few moments in the birth of the universe. In 1983 the final confirmation was obtained with the discovery that the predicted particles—the intermediate vector bosons—did exist. Called W^+, W^- and Z^o, these hypothetical particles were seen for a few fleeting moments under the cosmic conditions of the CERN accelerator. This temporary existence was enough to demonstrate that the unification theory was an accurate description of the fundamental nature of matter. This experimental verification led to the award of the Nobel Prize to Carlo Rubbia and Simon van der Meer in 1984.

I spoke earlier of the difficulties of doing science in developing countries. I would like to conclude with an appeal. Funds allotted for science in developing countries are small, and the scientific communities sub-critical. Developing countries must realize that the scientific men and women are a precious asset. They must be given opportunities, responsibilities for the scientific and technological developments in their countries. Quite often, the small numbers that exist are underutilized. The goal must be to increase their numbers because a world divided between the haves and have-nots of science and technology cannot endure in equilibrium. It is our duty to redress this inequity.

MY LIFE IN SCIENCE

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I was born in Athens, Greece, 1928 and was very fortunate in having a happy childhood and a very supportive family. My father had graduated from the University of Athens with a degree in Chemistry and then had specialized in Textile Engineering in Belgium. Although, like most women of her generation, my mother had not attended a University, she was a very cultured person and was fluent in several languages and in music, as was my father. I was again very fortunate to have been admitted to an exceptional high school, created by Americans and known as Athens College, where I received an excellent education especially in mathematics and in the classics. I was very much of a "nerd" and invariably secured top grades in all subjects, except for sports due to the fact that, early on in life, I developed a strong allergy towards anything requiring physical exercise. My main interest in those years was history, especially Greek history, which I studied with a great deal of passion. In spite of the German occupation during World War II and the resulting severe hardships and privations, our school functioned, regularly more or less, thanks to the dedication of our wonderful teachers. Unfortunately, our science education suffered because of a lack of laboratory equipment and supplies.

I was again very fortunate that, thanks to the efforts of Homer Davis, the Principal of Athens College, I was offered a scholarship at Syracuse University where I arrived in August 1947 having traveled from Greece on a converted troopship full of immigrants and displaced persons of various nationalities ready to make a fresh start in the land of opportunity. I shall never forget my first glimpse of New York City and of the Statue of Liberty. Having decided early on that I could not possibly make a living as a historian, I chose to major in something practical, specifically Chemical Engineering which, according to my way of thinking, combined Chemistry (my father's subject) with Mathematics, a subject that I found fascinating. The University at that time was very crowded and I was often forced to try and learn the material on my own. I graduated in 3 years and, being a "nerd", received all A's except in mechanical drawing and how to operate a lathe, both required courses which I found extremely uninspiring.

In order to pursue my studies further, I decided to see a different part of the country and, purely by chance, I chose to do my graduate work at the University of Minnesota. This was a fantastic break for me because, in my new environment, I found exceptional teachers including Neal Amundson, who became my advisor, mentor and lifelong friend. I also met, in a graduate Thermodynamics course, an international fellow student from Cuba, Jennie Vivó Azpeitia, to whom I have been happily married for close to 50 years. My Ph.D. thesis consisted of a mathematical study of multi-component distillation (at the time, a hot topic in chemical engineering) where I had to invent a new integral transform in order to solve the relevant mathematical equations analytically (unfortunately, my invention does not seem to have had any further use).

After finishing my studies, I decided to try and stay in the US permanently but, in 1954, the employment opportunities for non-residents were extremely limited. Fortunately, I received another lucky break when Charles Wilke, the then Chair of Chemical Engineering at UC Berkeley, offered me a temporary Instructorship for 3 semesters while telling me, in writing, that my appointment was not going to be renewed under any circumstances (as it turned out, I was promoted the following year and was granted tenure shortly thereafter). It was during my years at Berkeley that I developed my interest in Fluid Mechanics which, subsequently, became my primary area of research. Curiously, I had very little background in the subject and was drawn to it on the encouragement of a friend of mine at the Shell Development Laboratory, Thomas Baron, who felt that my ability in applied mathematics would come in handy. Needless to add, I had to start from scratch and to study the material on my own. Fortunately, I was able to attract a number of exceptional graduate students and before too long I was publishing high quality papers many of which are still being cited today on a regular basis. I also received another big break when I spent my first sabbatical in Cambridge, England, where I met George Batchelor, at the time a rising star within the Fluid Mechanics community, who became my lifelong friend and mentor.

In 1962 I moved to Stanford where, with my senior colleagues David Mason (who hired me) and Michel Boudart (who followed me from UC Berkeley), we succeeded in creating a chemical engineering department which, although small, was of exceptional quality and became recognized, within a few years, as one of the best on an international scale. Here my research flourished thanks to another group of exceptional graduate students whom I was able to recruit. Fluid Mechanics is a truly fascinating subject because it is so visual and what you can see can be so beautiful. It often entails an almost perfect combination of experiments (some of them quite striking to look at), sophisticated applied mathematics such as asymptotic analysis, and, increasingly today, high powered numerical computations. For 16 years starting in 1982, I edited The Physics of Fluids, and there is no better place for the uninitiated to obtain a glimpse of the beauty and sheer diversity of the field than by enjoying the Gallery of Fluid Motion which appears yearly in the September issue of that journal. In my own research, I touched on various aspects of the subject ranging from boundary-layers, to the motion of drops and bubbles, to the flow of so-called "non-Newtonian" fluids such as molten polymers, but, perhaps, my most enduring legacy will be in the field of suspension mechanics which, curiously enough, was also a chapter in Albert Einstein's Ph.D. thesis. It is amusing in retrospect that this topic was considered quite dead for several decades; yet, my students and I, driven purely by curiosity, did some experiments, found results that were totally unexpected, did more experiments, did analysis as well as numerical computations, explained the experiments quantitatively, and then showed that suspension mechanics is a far more fascinating subject than even Einstein could have foreseen. In fact, during the past couple of decades, I have given numerous talks worldwide many of whom with same title year after year, e.g. "The Mechanics of Suspensions; Latest Variations on a Theme by Albert Einstein" but, of course, even though the title of my lectures often remains the same, their content changes continuously as new discoveries are being made and new insights are gained.

In 1988, after spending 25 years at Stanford, a new opportunity arose in that I was offered one of New York State's Albert Einstein Chairs as well as the Directorship of the Levich Institute at the City College of the City University of New York. Again this proved to be a very fortuitous move, because I found the City to be absolutely fascinating and many of the students at City (high achievers and with high ambitions) great fun to be with. Once more, I was very fortunate in attracting a number of top graduate students, to be able to move into new areas in fluid mechanics (such as the effects of electric fields on the motion of particles) and to continue making discoveries in suspension mechanics.

I remain close to many of my graduate students, several of whom have already attained international reputations of their own, and have received numerous awards and other forms of recognition. The two that gave the greatest pleasure, both totally unexpected, was my election in 1991 to the US National Academy of Sciences, and my receiving the 2001 National Medal of Science from the President of the United States at a White House ceremony the day before my 74th birthday.

FROM ELEMENTS OF RADIO TO ELEMENTARY PARTICLE PHYSICS

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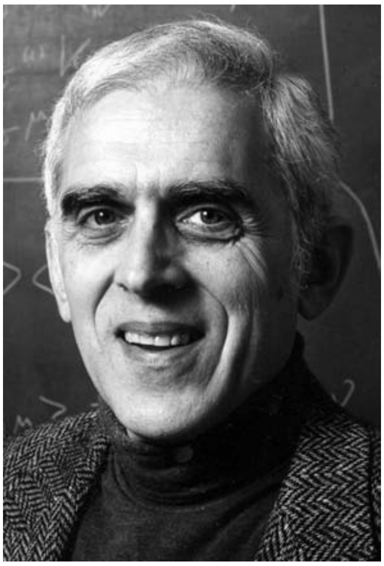

I was born in 1939 in New York City to Irving and Ruth Relis Adler. My father was a mathematics teacher and my mother had also majored in mathematics in college. I was directed towards science by my parents from an early age. When I was two years old my father built me a gadget box from pieces of hardware, and around the same time my mother made me a home-made version of the "Pat the Bunny" book, each page containing a different tactile or manual operation for me to perform. When I was older my father built me electrical toys such as telegraphs, a "burglar alarm" that rang a bell when a door was opened, and a miniature traffic light. We also engaged in nature activities, collecting snakes and butterflies. When I was eight I participated in a young people's astronomy course at the Museum of Natural History in New York, and my fascination with the fossils I saw at this museum led me to think briefly of being a paleontologist, but this interest soon waned.

My actual career path began in sixth grade of elementary school, when a classmate started to talk to me about his interest in radio; I visited him at home and saw his equipment and tools. This introduction developed into a serious interest in electricity, radio, and electronics while I was still in elementary school. I built various electrical devices, such as electric motors with rotor laminations cut from tin cans and permanent magnet stators taken from old radio loudspeakers. (I still have one of these on my bookcase at the Institute for Advanced Study). With encouragement from my father I read Marcus and Marcus's classic World War II text "Elements of Radio"; my father made a point of letting me be the family radio expert, while he was the consultant on the few bits of algebra in the text. Also at my father's suggestion, I started to canvass the neighborhood door to door, pulling a small wagon and asking for old radios, appliances, and television sets people were planning to throw away. I stripped the parts out of these, and used them to build radios, amplifiers, and even an oscilloscope using a salvaged 7-inch television tube. I also learned enough Morse code to get my Technician Class amateur radio license, and built a small rig using a war surplus aircraft receiver and a home-built transmitter. However, amateur radio activity did not interest me nearly as much as building electronic equipment, which I continued through various science projects in high school.

Given this exposure to electronics, it would have been natural for me to pursue a career in electrical engineering, but towards the beginning of my high school years I got a first glimpse of the fascinating world of high energy physics research. For two summers my family had vacationed in a state park near Ithaca, NY, and Phillip Morrison, an old friend of my father's, gave us a tour of the physics laboratories at Cornell, where Robert Wilson had built a succession of particle accelerators. I liked the ambience of these laboratories, and was impressed with the fact that if I pursued physics as a career I would learn and use electronics, but not necessarily the other way around. By my junior year in high school, I had decided on experimental physics as a career.

My first physics research laboratory experience came at the end of my senior year in high school, when I attended a two-week course in X-ray diffraction techniques for industrial engineers given at Brooklyn Polytechnic Institute by Isadore Fankuchen, who would every now and then include a bright high school student in his class. I was able to do all the theoretical and laboratory work, and learned many things, such as crystal lattice structure and Fourier transforms, that are standard physicist's tools. Almost immediately afterwards, I went to a summer job at Bell Labs in Manhattan, along with eight other science-oriented high school graduates. Many of them had already learned calculus, and so I decided to teach myself calculus that summer.

My father gave me his old calculus text, along with the sage advice to do every *third* problem—because I had to do problems to learn the material, but there was not time (and it would be too boring) to try to do all of them. So I spent my commuting time, and spare time at work, doing calculus problems. As a result, when I entered Harvard in the fall I was able to place directly into Advanced Calculus, which had a major impact in how fast I was able to proceed with my physics education.

I entered college intending to be an experimentalist, but my friendships with various classmates, among them Daniel Quillen (later a Fields medalist) got me interested in mathematics. I found that I was very good at the theoretical aspects of my classes, but although competent in the laboratory, I lacked the touch of the gifted experimentalist. So, by the middle of my freshman year, I had decided to shift my sights from experimental to theoretical physics. Along with Fred Goldhaber, who was to be my first year roommate in graduate school at Princeton, I took essentially the whole graduate course curriculum at Harvard during my junior and senior years. Memorable teachers at Harvard included Ed Purcell, Frank Pipkin, Paul Martin, and Julian Schwinger. As a result of my Harvard preparation, at Princeton I was able to take my General Exams at the end of the first year, and then to start thesis research with Sam Treiman at the beginning of my second year.

Treiman suggested that I look for calculations to do in the newly emerging area of accelerator neutrino experiments, and this was the beginning of my career in high energy physics. A major part of my thesis work was a calculation of pion production from nucleons (protons or neutrons) by a neutrino beam. Although this was a long and tedious project, it gave me a detailed introduction to the "vector" and "axial-vector" currents through which neutrinos interact with nucleons. This knowledge growing out of my thesis project was the foundation for my most significant scientific contributions during the period 1964 through 1972, which all involved in some way or another the discovery of further results connected with vector and axial-vector currents. These included various low energy theorems for pion emission derived from the hypothesis of a "partially conserved" axial vector current, various sum rules including the Adler-Weisberger sum rule for the axial vector coupling to nucleons and a sum rule for deep inelastic high energy neutrino scattering cross sections, as well as the co-discovery (along with Bell and Jackiw) of the "anomalous" divergence properties of the axial-vector current. The theoretical analysis of anomalies led to a deeper understanding of neutral pion decay into gamma rays, provided one of the first pieces of evidence for the fact that each quark comes in three varieties (now called "colors"), and has had a multitude of other consequences for theoretical physics over the last thirty-five years.

During the years since 1972, I have worked on a variety of other topics in theoretical high energy physics, including neutral current phenomenology, strong field electromagnetic processes (such as photon splitting near pulsars), and acceleration methods for Monte Carlo simulation algorithms. Throughout the last twenty years I have devoted about half of my research time to studying embeddings of standard quantum mechanics in larger mathematical frameworks. One aspect of this work involved a detailed study of a quantum mechanics in which quaternions replace the usual complex numbers. Another, more recent aspect, has involved the study a possible "pre-quantum" mechanics based on properties of the trace of a matrix, from which quantum theory can emerge as a form of thermodynamics. I have written books describing both of these studies. For the next few years, I plan to return to my original area of particle phenomenology, in the context of supersymmetric models for further unifying the elementary particles and the forces acting on them.

AFRICAN PHYSICIST, WORLD CITIZEN

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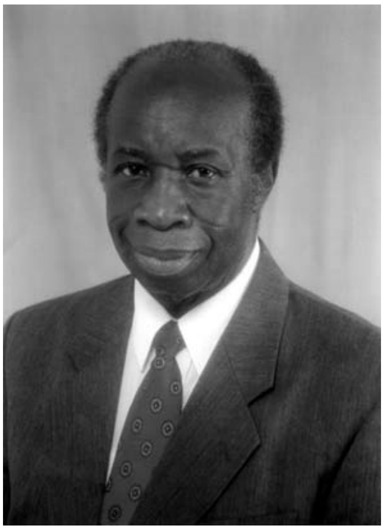

I was born on August 9, 1932 at Saltpond, Ghana, West Africa, along the coast of the Atlantic Ocean, usually called the Gulf of Guinea. My father was a general merchant. He owned a store and sold books, musical instruments, and fishing equipment. My mother was a dressmaker and my maternal grandmother was a fishmonger. As a young boy, my mother periodically sent me with provisions for my grandmother to a fishing village called Edumafa, in Ekumfi, a traditional area about six miles East of Saltpond. In those days, there was no motorable road between Edumafa and Saltpond and hence I had to walk. There I assisted my uncles in fishing.

I attended a Roman Catholic Elementary School at Saltpond. After classes my responsibilities were to dust and arrange books at my father's store. There I read books including biographies of renowned scientists: Newton, Einstein, Jeans, Hamilton, Gamow, Galileo, Maxwell and Rutherford. E.T. Bell's book "Men of Mathematics" had a profound influence on me. I decided that I would also be a great scientist to learn more about the workings of the cosmos and to contribute to its understanding. After elementary school I went to a high school, Ghana National College at Cape Coast. The school was founded by Dr. Kwame Nkrumah, the first president of Ghana. After high school, I proceeded to the United Kingdom where I attended Borough Polytechnic, now called South Bank University, and then Imperial College of Science and Technology. Among my Professors at Imperial College were Abdus Salam, P.M. Blackett, Harry Jones and Eric Eady. I returned to Ghana in 1960 to teach mathematics for two years at Kwame Nkrumah University of Science and Technology.

In 1962, I went to Princeton University to study Mathematical Physics for my Ph.D. The Department of Physics at Princeton in the 1960's was a very exciting place. It was full of distinguished professors among whom were Wigner, Wheeler, Dicke, Hopfield, Bargmann and Goldberger. Oppenheimer, Dirac and Yang from the Princeton Institute of Advanced Study regularly visited the Department. During my stay in Princeton, Fitch and his group were working on their Nobel winning experiments on CP violation. Dicke and his collaborators were measuring oblateness of the sun, the cosmic background radiation of the universe and the gravitational constant to the accuracy of one part in 100 billion. As a graduate student, I occasionally assisted in some of the measurements of Dicke's group.

My earlier research investigation was on the quasi-static theory of air motion in the atmosphere due to heat and angular momentum sources. Lately, I have been working on the theory of condensed matter physics. I was the first to introduce electron-hole scattering resonances effect on soft X-ray spectroscopy. The effect has been observed in lithium. I am also working on the theoretical studies of superlattices and microstructures including chiral carbon nanotubes.

Some of my recent publications were 'Photostimulated attenuation of hypersound in superlattices', 'Non-linear acousto-electric effect in semi conductor superlattices' and 'Differential thermopower of chiral carbon-nanotubes'. In Ghana, at the Kwame Nkrumah University of Science and Technology, I was Head of the Mathematics Department, Dean of the Faculty of Science, Founding Director of the Computer Science Department and Pro-Vice Chancellor. At the national level, I was Chairman of the Ghana Atomic Energy Commission on three separate occasions, Chairman of the Council for Scientific and Industrial Research, and Founder and National coordinator of the Ghana Energy Research Group.

Internationally, I was a member of the International Atomic Energy Agency (IAEA) Board of Governors and co-author of the Book "Comprehensive Study of Nuclear Weapons", a UN Secretary-General report in 1979. I am a member of the Scientific Council of ICTP. Currently I am President, Society of African Physicists and Mathematicians. I am active in the field of Information Technology. I have served in various capacities for the past thirty years in Africa, Australia, Europe, USA and Latin America. I was Chairman of the Williamsburg Conference on International Information Economy in Virginia, USA 1986; Chairman and Organiser of the Section on Computer Education in Developing Countries of the 1980 International Federation of Information Processing (IFIP) Congress in Melbourne, Australia; and Chairman of the panel discussion on the Financial and Quantitative Aspects of Computer Education at the IFIP Congress, Marseilles, France (1975).

Why science? Apart from understanding the universe, and perceiving new potentialities, science is an essential means of meeting society's needs for food, water, transport and communication, energy, good environment, health care, shelter, safety and alleviation of poverty. For example, utilizing science, less than 3% of people in the advanced countries are engaged in agriculture to produce sufficient food for their people. In Africa, because of the lack of scientific awareness, over 65% are engaged in agriculture and yet cannot produce enough food for their people. In fact, the development gap between the North and South is basically a manifestation of the technological gap.

As I wrote more than twenty years ago, "We (in the developing countries) paid the price for not taking part in the Industrial Revolution of the late eighteenth century because we did not have the opportunity to see what was taking place in Europe. We now see that information and communication technology (ICT) has become an indispensable tool. This time we should not miss out on this technological revolution".

I am involved locally and internationally on policies and issues related to science and technology for sustainable development. In the area of rural development, I have assisted in the establishment of two elementary schools at Edumafa and Owomasi in the Central Region of Ghana. I also founded the only library at Saltpond.

I am a fellow of several professional and learned societies such as the Third World Academy of Sciences, the British Computer Society, the Nigeria Solar Energy Society, the Institute of Physics (U.K.), the Ghana Academy of Arts and Sciences, the Ghana Institution of Engineers. I am the founding fellow of the African Academy of Sciences and the Ghana Institute of Information Technology. I am the Patron of the Computing Association of Ghana, the Science Teachers' Association, the Ghana Physics Students' Association, and the Africa Institute of Mathematical Sciences (South Africa).

For my role in the development and promotion of mathematical sciences in Africa, the African Mathematical Union gave me an award and a medal. I have also received awards and medals from the Mathematical Association of Ghana and the Ivory Coast Mathematical Society as well as the Prince Philip Gold medal of the Ghana Academy of Sciences for my contribution to physical sciences in 1973. I also received the Deserving Scientist Award from the Ghana Science Association and the First World Bank-IMF African Club Award in 1999. I was awarded the Martin Luther King Jnr/Cesar Chevaz/Parks Visiting Professorship at the University of Michigan, Ann Arbor, USA (1997) for my contributions to physical sciences and the promotion of international relations in sciences.

In 1979, I was invited by the government of India for a month. I gave lectures and seminars in several institutions in the following cities: Bombay, New Delhi, Calcutta, Hyderabad, Madras, Bangalore and Trivandrum.

While a student in London, I married Edoris Enid Chandler from Barbados, West Indies who died in 1981. I remarried Asie Mirekua Akuamoah. I have four children, two boys and two girls, and four grandchildren.

RNA AND THE ORIGIN OF LIFE

Sydney Altman
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Yale University, Office of Public Affairs

When I was about six years old, I was aware of the second world war (WW II) and the great feat of applied science that ended that war in the Pacific area. The atomic bomb, and its design and manufacture by great physicists, was a mystery to me, but a very fascinating one. The words "nuclear physicist" were also a magnificent attraction but nobody I knew was aware of the science or the people involved. At the same time, I had a marginal interest in the sun and the stars as interesting topics to read about.

At about the age of twelve, I received a book to read that explained some nuclear physics and also presented the ideas behind Mendeleev's Periodic Table. The book was sufficiently elementary so I could understand it. I cannot remember who gave it to me. Nevertheless, I found the book spellbinding and I was very excited by Mendeleev's ideas about the elements and how he managed to predict the existence of elements that had not been found at the time he wrote the volume. That, to me, was a lasting and clear example of the power of science and its beauty. I was inspired to imagine becoming a nuclear physicist myself one day and that led to my future involvement in physics, and then molecular biology, when I went to college.

It is worth pointing out that I came from an immigrant family and it was only through the virtually free education in college, and my parents' interest in education as a way of furthering oneself, that I was able to think about getting a college education.

My contribution to science that might be remembered for a while, involves work on ribonucleic acid (RNA). RNA inside living cells is an exact copy of regions of deoxyribonucleic acid (DNA). Genes are composed of DNA. For many years it had been thought that RNA was reflection of the genetic material in DNA, although it was known that certain pieces of RNA carried out functions in side cells that were not enzymatic (they did not control chemical reactions inside cells). I and my coworkers discovered in 1983 that some pieces of RNA were actually catalytic, i.e., they controlled chemical reactions in side cells. This work, and that of several other individuals who made similar discoveries, has succeeded in showing that there are many such "enzymatic" RNAs inside cells. It has also changed the way in which we think about the origin of life on Earth and how the first chemical and enzymatic reactions (catalysis inside cells) got started.

MATHEMATICS: IMAGINATIVE LEAPS ACROSS DISCIPLINES

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University of Edinburgh, UK

Mathematics is a challenging but fascinating subject which has exercised the mind of mankind for thousands of years, across many cultures and civilizations. It was the intellectual challenge, embodied in subtle and elegant problems that always attracted me. Solving such problems required careful thought, but it gave great satisfaction. There is a beauty about mathematics that is difficult to describe to those who only see it as dreary computation. It is the beauty of a landscape whose terrain may be rough but the mountain peaks shine through.

At school I was, like many others, attracted also by the joys of chemistry. Concocting colourful mixtures in test-tubes was great fun, but in the end chemistry at that level lacked the intellectual appeal and coherence of mathematics.

Right at the end of my schooling our mathematics teacher introduced us to the fascinating subject of quaternions. These were discovered by the great Irish mathematical physicist of the 19th Century, Sir William Rowan Hamilton. Impressed by the power and beauty of complex numbers $x + iy$, Hamilton tried for many years to extend this to an algebra involving three real variables (x, y, z) that would provide a framework for the physics of space. Eventually he made his famous discovery that such an algebra (now called quaternions) had to involve four real variables (x, y, z, t), and moreover that the multiplication law would no longer be commutative. Thus, for two quaternions q_1,q_2 the products q_1q_2 and q_2q_1 could be different.

The story of Hamilton's discovery is one of the legends of mathematics. He claimed that the inspiration came to him while on a country walk and that he then wrote down his famous equations, for his "imaginary" quantities i, j, k

$$i^2 = j^2 = k^2 = -1 \quad , \quad ij = -ji = k \quad \text{etc.}$$

on the wall of the small bridge he was crossing at the time.

I became enamoured of quaternions, their intrinsic appeal no doubt enhanced by the romantic story of their discovery. I read with great interest the ways in which Hamilton and his colleagues applied them to 3-dimensional geometry and to mathematical physics.

When, a few years later, I went to University at Cambridge, I was sadly disillusioned to find that there was no mention of quaternions. Enquiries I made just yielded a negative response—quaternions was not the great discovery that Hamilton had thought, they were a minor side-line.

About thirty years later, when I was an established mathematician, new and exciting things were emerging on the frontier between mathematics and physics. I got deeply involved in this new interaction and, lo and behold, I found my old love the quaternions right at the centre of the new developments (with the mysterious fourth real variable interpreted as time).

This episode brings out many aspects of what makes mathematics and science so fascinating. First of all, good ideas have a long life and frequently make a dramatic comeback after having been ignored for many years. Secondly, good ideas straddle frontiers in the way quaternions, which start as algebra, move into both geometry and then physics. Lastly, ideas are the creation of individual human beings and are part of our common intellectual history.

It is the connection between different subjects, such as algebra, geometry, analysis and physics that has been the real focus of my own mathematical life. My main contribution linking all these areas was a formulae (now known as the Atiyah-Singer index theorem) giving the number of solutions of a differential equation in geometric terms. Often such links are very unexpected and bring quite new light to bear on old problems. For me this is one of the great attractions of mathematics, its ability to make imaginative leaps across disciplines. Long may it continue to do so.

SCIENCE OF THE XXI CENTURY

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Lawrence Berkeley National Laboratory, USA

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The leading nations of the XXI Century will be the countries where the great national and global problems will be understood and appreciated by the majority of their population. The heroes of these nations will be the scientists of great vision and ability to select and to explain the problems of primary importance, and to achieve the support (governmental and private) necessary to solve these problems. I remember my first step in becoming what I am now—an applied mathematician. I wanted to be a historian, and even to apply to Diplomatic School in Moscow. However, one evening, before going to bed, I took by chance a book, presented to me by my grandfather, a well-known geometer. I read there how a simple mathematical model explains the work of an important device in Chemical Engineering. I became excited: I achieved a great feeling that this is my future—to make the mathematical models of practically important phenomena! The only thing which remained from history in my life—a line in 'Who is Who': hobby—history reading. Scientists of great vision and organizing ability do exist, and in due course they reveal themselves and make the steps of historic importance. One example: Leo Szillard, an American scientist of Hungarian origin. It was he who prepared the text of the letter to President F.D. Roosevelt concerning the crucial importance of starting the work on the atomic bomb. This text was signed (not very eagerly) by Albert Einstein. However, when Roosevelt decided (difficult to believe now!) to decline this proposal, Szillard found a personal friend of FDR, explained to him the problem and persuaded him to interfere. The friend visited FDR and asked him only one question: "Frank, do you think, if in 1812 Napoleon had not turned down Fulton, the inventor of the steamer, the world map would nowadays be the same?" And FDR gave the order to start the work. The scale and value of this work—Manhattan Project—is well known. However, the common opinion of laymen, even scientific laymen, is that in our days there are no such problems of the scale of the Manhattan Project whose importance for the nation and the world is understandable for everybody, and where the leadership of the scientists of great vision and ability to unite the people around them can become a reality. This is deeply wrong! Such problems do exist, and they are understandable for everybody. First of all these problems are the large scale natural disasters. I will give two examples of them.

(1) **Tropical hurricanes**. The scales of these disasters are huge and moral and material losses formidable. The meteorologists are now able to predict them very accurately, and this is very important. A most important first step in modelling the tropical hurricanes was performed by the late great British applied mathematician Sir James Lighthill. Namely, he

was the first to mention the natural disasters as a problem of first importance for applied mathematics; he invited me to join him in his work on hurricanes. And the basic question is: is it possible to prevent, or at least to reduce the strength of, tropical hurricanes? The answer is affirmative. The tropical hurricanes have a characteristic feature: there always exists an air layer over the sea surface, up to a hundred meters thick, filled by large water drops. And it is very important that these drops strongly reduce the turbulence in the layer, and therefore the resistance to the wind of the sea surface becomes much less. It is as if the sea surface would become slippery. And therefore the atmospheric pressure differences which would be able to produce only weak or moderate winds without these layers produce the hurricanes. The technical problem is: how to suppress the formation of water drops in sensitive places? It is clear how to do it in principle, but laborious, although achievable, work is needed. I emphasize the work is one where the scientists will be the leaders, as it was in the Manhattan Project.

(2) **Prediction of earthquakes.** Again—tremendous losses, first of all—human lives, but also formidable material and moral losses. There exist a lot of **seismological**, as they are called, institutions, employing many learned people having high scientific degrees. However, their ultimate goal should be to **predict** the earthquakes. And here I take the liberty to ask a crucial question: are these specialists able to bring forward three, two, even one serious earthquake of large magnitude which were predicted **before** the event? I emphasize, **before**, not **after** the event, because there exist many articles, written **afterwards**, which prove, and very persuasively, that a certain big earthquake had to happen exactly at that very time (even half an hour before!) and in the very place where it happened. But where were the authors before the event? Why do the animals (dogs, snakes—this is known definitely) feel well in advance the coming earthquake, and the approved specialists still do not know it? I think that new leaders are needed—the scientists of wide profile, great vision and ability to organize and unite the people working for them. It is clear that the earthquake is a phenomenon determined by multiple factors. The important factors should be established and a proper monitoring should be proposed and organized. This is a gigantic work but it can be done and it should be done! And, this is very important, as a result of the work it should be accepted only as a real prediction of time and place of several earthquakes of large magnitude. Again, like it was in the Manhattan Project and its Soviet analog: the successful explosion was the only criterion! This is the work for years, perhaps for one to two decades, but not for centuries. And it should be done! It is a very important moment. Mankind is now hungry to harvest from science something that is really interesting without long preliminary explanations for everybody, even the not specially prepared. An example: I had a commission to answer the question: whether UFO's ('flying saucers') are a pure legend, a product of collective imagination, or is there something in it? Some serious work was performed—both theoretical and experimental—and a lot of information was collected. And we came to the conclusion that this is an interesting atmospheric phenomenon, which was not known before. We presented the model of the phenomenon and were able to explain observed features of the objects (regular discoidal shape, reflection of light, etc.). Our scientific paper was published (after a special consideration by the President of the Academy and some leading specialists) under some neutral title in the leading Journal of the Russian Academy of Sciences. I included the mathematical model in my book devoted to my

subject—applied mathematics. It leaked to the newspapers, and it is easy to imagine what started afterwards: requests for interviews, telephone calls, etc. However, I did not want to achieve a reputation of a person related to 'UFOlogists'—according to common opinion such a reputation is incompatible with the reputation of a serious scientific professional. Therefore, I did my best to escape the contacts with UFOlogists. And one time I was kidnapped! I received a telephone call, the caller introduced himself as a representative of the Moscow Society of the Explorers of Nature, a very respected old establishment. He told me that the Society was inviting me to speak before them about this problem; of course, a purely scientific talk was assumed. I agreed. At an appointed time a luxury car expected me near my house. We started to drive, but soon I understood that we were moving in the opposite direction. 'Why do you drive this way?' 'Oh, it will be in a different building, our permanent place is being repaired'. The driving was long, and it became dark. Finally we stopped at the door of a church close to a big Moscow Cemetery. The guard, escorting me, politely opened the door of the car, we entered the church, and I found the church full of people, expecting my talk. I understood—I am in the nest of UFOlogists! I decided to do the only thing which I could: I took my slides (there was a good projector and screen) and started to explain, purely scientifically, our mathematical model of the phenomenon, and its correspondence to the observations. The people listened to me with full attention and very politely. Soon I had forgotten where I was and how I was brought there. After the lecture I had to answer the questions and comments for more than an hour and a half. Many of the questions were interesting and appropriate. One listener corrected me: I had the idea that one of my pictures was a photograph, but in fact it was the drawing by an artist who was present at one of the events. This correction was of no crucial importance for me, but I took it into account. Absolutely no hostility. In conclusion, the Chairman, a leading Moscow UFOlogist, retired commodore Azhazha, as he introduced himself, said the following. 'We have to be most grateful to Professor Barenblatt for clearing our field. Yes, I am sure that 98 percent of the phenomena are explained by his model. But the remaining 2 percent are ours—with little green men, etc. Do you agree with that, Professor?' My answer was diplomatic—I do not know, perhaps even more than 2 percent, but I have no evidence of them! And I thought, how science thirsty is our community, and how important it is to direct this thirst for knowledge to really important problems. A great task and great responsibility for scientists!

As my conclusion, I can repeat what I said in the beginning. There exist gigantic problems standing now before mankind. These problems can be solved only by the teams led by scientists of great vision and ability to unite people. They should be supported by wise and strong authorities, able to participate in the selection of proper problems and proper people. Such scientists-leaders will be the real heroes of advanced nations of the XXI Century.

LIVING WITH PHYSICS

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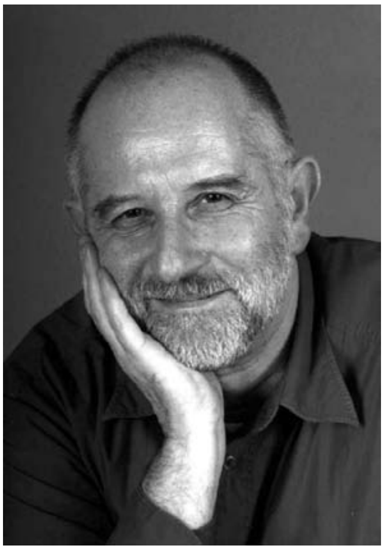

If you get your knowledge of science mainly from the TV, you might have the impression that it's a weird activity, very far from what most people care about. But science isn't remote at all: the world is connected in strange and wonderful ways. Think about this: many of you have a cd player. You can take it anywhere—on the beach, up a mountain, through the forests, in the deserts, at the north pole, even—and listen to music reproduced almost perfectly. That wasn't possible before in all of human history. In previous centuries, if you wanted to hear music, you had to go to live performances. But now we have this fantastic freedom that anyone, in any part of the world, can share the experience. In a way, it's the ultimate democracy: making available to many what could previously be enjoyed only by a few. How has this come about? Strange as it seems: through a physicist's dreaming.

Inside every cd player is a laser. Its light bounces off the bumps and pits on the disk, and electronics converts the signal into sound. The laser wasn't discovered by accident. It was *designed*, by applying our *understanding* of waves and particles of light that comes directly from *quantum physics*, which gives our deepest understanding of the strange tiny world inside atoms and smaller. The laser works on a principle discovered by Einstein nearly a hundred years ago. It was pure theory—dreaming while you're awake. He never dreamed that fifty years later other scientists would apply this principle to create bright pure light.

Nobody imagined that engineers would use little lasers to read music. It isn't only lasers: the electronic circuits that convert the music into sound contain millions of transistors—another device designed using quantum physics. And it isn't only physicists and engineers. To design how the bumps and pits on the cd represent the music involves mathematics: arithmetic, trigonometry, algebra—just those subjects where you hear people, who should know better, wonder if they're any use.

And of course it's not only cd players. Every supermarket checkout has a laser, and every mobile phone has millions of transistors. The point I'm making is that these are *quantum-physics machines*, applying very abstract ideas to practical inventions we use every day.

I'm a theoretical physicist, working at the abstract end of this chain of connections—a dreamer and scribbler, of mathematics, mostly. It's a mistake to think that only mathematicians do mathematics. Sometimes in physics you need mathematics that hasn't been invented yet, so we make it up ourselves, and mathematicians come later and tidy it up. Of course, it happens the other way too—we need to do some new types of sums, and find that a hundred years ago mathematicians have anticipated us, with pure thinking they never imagined anyone would find useful—the laser story again. My work is about waves—in light, on water, in quantum physics, and other sorts of waves. It's the connections that excite me—to start trying to understand why images in big telescopes get blurred, and then find you've explained the bright dancing lines of light on the bottom of swimming pools.

It's a good life, and suits me personally because I'm not a very competitive person. This might seem strange, because again the popular image, encouraged by the media, is of scientists at each other's throats, fighting to get their discoveries published before other people, competing for research money. As with any human activity, that does happen sometimes. But in all my years as a scientist I've almost always encountered the opposite: not competition but friendly cooperation, sharing results. This isn't because scientists are better than other people: in our private lives we're no different from anyone else. We cooperate simply because the ways that nature works are so well hidden that no individual can discover them by himself or herself. We're much cleverer together than separately, so it makes sense to cooperate. And the cooperation works across all cultures, nations, races, religions. Whether I'm in the UK, the USA, Africa, China, Lebanon or Israel, there's immediate communication and understanding (thank goodness every scientist speaks English).

When I started, I never knew any of this—about dreaming, about connections, about travelling, about cooperation. In my family, only one cousin had any education beyond sixteen. It wasn't a rich or a happy family: my father, who was a taxi-driver, was a violent man, and my mother spoiled her eyesight taking in sewing to make up for the money he gambled away. I was lucky to be born into a society where you didn't have to be rich to get educated. Education is the key.

Earlier, I wrote 'himself or herself'. Half the world's children are girls. Why is so much of their creative talent wasted? I have something to say about this. First of all, there's this image of science as a masculine activity. It's just wrong. I mentioned cooperation rather than competition. That's traditionally much more a feminine than a masculine characteristic.

Then there's this image of science as lots of gadgets: toys for boys. Well, I enjoy cooking, and I'm pleased to have a colleague who studies the science of cooking—he calls it molecular gastronomy. It's physics and chemistry, applied to what we call 'soft condensed matter'. He's collaborating with a leading chef, to create new and wonderful dishes—for example instant perfect ice cream, made by dunking the mixture in liquid nitrogen.

Again, there's a weird view that it's awkward being a scientist and raising a family at the same time. My wife is a biologist working in our Eye Hospital, trying to understand the miserable disease of dry eyes. She was studying when our babies were born, so for the first eighteen months of their lives I raised them in my office—an interesting experience, unusual for a man, which taught me a great deal (about the liberating technology of disposable diapers, for example).

Things are changing. Last year I was on two committees. One was to award the main prize in Britain for research in mathematics. After 150 years it went to a woman for the first time. The other committee gives grants to support the six brightest young mathematicians in Europe; our two top awards were for women. And in Britain, the best scientific research jobs for young scientists, in all subjects, come from the Royal Society of London—that's our Scientific Academy. They give several hundred grants each year. Last year, many of their grants went to women. Things are changing.

The excitement of scientific discovery is the *inner* knowledge it gives us, the quiet satisfaction at something understood. In science, when you discover something new, even a small thing, you're floating on a cloud for days. That's what delights me.

WHY I BECAME A PHYSICIST

Nicolaas Bloembergen
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As a teenager in the Netherlands, I attended a Latin School (gymnasium) in Utrecht, a city of about one hundred thousand inhabitants. I bicycled every day about six miles each way, from my parental home in a suburb. My parents always encouraged intellectual pursuits as well as participation in sports. The high school curriculum emphasized instruction in six languages and history, but fortunately good teachers in mathematics and the sciences captured my attention. I selected to specialize in the study of physics at the university, because I found this subject the most difficult and challenging. I was especially intrigued by the mathematical description of physical phenomena, such as the motion of matter and the direction of light waves.

I was fascinated by reading about the lives of Marie Curie and Albert Einstein, who were famous scientists in my school days. At the university I considered myself not bright enough to specialize in theoretical physics, and I learned that laboratory work often involves setbacks and negative outcomes. The success in making some new observations is, however, very exciting, although it is usually achieved with one percent inspiration and ninety nine percent perspiration. During World War II the Netherlands were occupied by German forces, and the Nazi authorities closed the University of Utrecht in May 1943. Nevertheless I continued reading physics textbooks for the next two years, until we were liberated by the Allied forces.

My perseverance was rewarded when I obtained admission to the Physics Department of Harvard University in Cambridge, Massachusetts, U.S.A., in early 1946. I became the first Ph.D. graduate student of Professor Edward M. Purcell, who was to share the Nobel Prize for Physics in 1952 with Felix Bloch for their discovery of nuclear magnetic resonance in condensed matter. My research work on nuclear magnetic relaxation was very exciting and involved a combination of experiment and theory. The results were published in a paper frequently cited as BPP, after the joint authors N. Bloembergen, E.M. Purcell and R.V. Pound. What we did not anticipate in 1948 is that the research results of that paper would a quarter of a century later turn out to be basic to the development of Magnetic Resonance Imaging. This medical diagnostic technique which was recognized by the award of the 2003 Nobel Prize in Physiology and Medicine, has now become about as important as x-ray imaging.

Any doubts I may have had about my decision to become a scientist during the difficult war years, vanished in 1946. The scientific enterprise had entered a golden age, and I remained an active participant in it for the next half century.

As a Harvard faculty member, I also enjoyed teaching new generations of students, and I have benefited enormously from their collaborations, discussions and criticisms. Since science is a universal, international discipline, I also enjoyed my trips to international conferences and I served as a visiting faculty member at Universities in France, Germany, India and the Netherlands as well as in various states of the USA.

My early work on magnetic relaxation phenomena led in a natural way to the invention in 1956 of a pumping scheme for amplifiers based on stimulated emission of radiation knows as masers and lasers. By concentrating the light energy in space and time extremely high intensities, or power flux densities, can be achieved. This in turn opened up a new subfield of scientific endeavor. This field is concerned with the optical properties of materials at very high light intensities and is called Nonlinear Optics.

Again, I experienced the good fortune that the field of lasers and nonlinear optics gave rise to important technologies. Lasers are used extensively in medicine and surgery, in global optical fiber communications, and the construction and materials process industries.

In retrospect my choice to become a physicist more than sixty-five years ago has been very rewarding. Perhaps the life sciences, geophysics and cosmology present even greater challenges at the present time. If you are curious enough, you should seriously consider further scientific training. New technologies have a profound influence on society in all countries, and they are all based on scientific principles. Every country will need further leaders with some familiarity of the scientific method. Perhaps you will be so intrigued by some questions posed by nature, that you will decide to remain a scientist.

A DUTY TO IMPROVE PUBLIC UNDERSTANDING OF SCIENCE

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"God and the soul, that is what I desire to know. Nothing more? Nothing whatever!" stated the philosopher Saint Augustine sixteen centuries ago. The structure of the world, from the celestial bodies to atoms and subatomic particles—the modern equivalent of God—was the object of my curiosity as a boy and as a young man. How the brain works and what really are those entities we call mind and behavior—the modern equivalent of the soul—have been, on the other hand, the subject of my work in the second part of my scientific life. After an initial passion for philosophy—mostly Spinoza, Kant and Husserl—I became deeply interested in the successes of modern physics. As a teenager I used to devour popular and semi popular books dealing with the ideas of relativity and the new atomic physics. It was then inevitable for me to study physics at the University of Florence, where I did some pioneering experimental work on laser sources of intense light beams. Nevertheless, almost as soon as I graduated I switched to biology. This was a big change, from physics to biology and from Florence to Naples. A move I never regretted.

It was a small book by Isaac Asimov entitled *The* genetic *code* that changed my life. In 1966 the genetic code was just being unveiled and the structure of only a handful of small proteins was known. The book was written like a detective story and opened up to me a world of intriguing unsolved biological questions. I applied for a fellowship and left for Naples, convinced that I would spend only a couple of years there. Instead I stayed for 23 years, from 1968 to 1991. I started studying fruit flies, the famous *Drosophila*, certainly the best known of all organisms from the point of view of genetics. The design of genetic crosses proved very easy for me, as compared to the solution of problems in physics, and I published some good papers on the characterization of some genes, just at the beginning of the Molecular Biology Era.

In 1981 I decided to move to the study of molecular genetics of mammals, including man, and a major change in my career occurred in 1985, almost by accident. On my way to Boulder, Colorado, to participate in a biological Congress I got trapped with other scientists for seven hours in the TWA terminal in New York. Walter Gehring from Basel, Switzerland, was among those scientists, and he had just made an incredible discovery in the study of *Drosophila* development. A family of genes, termed Homeogenes, was known in this insect, that play a major role in controlling the correct development of the various parts of the body. A mutation in one of these genes may cause the insect to have four wings instead of two, or eight legs instead of six or even a couple of legs on top of its head. Walter had just isolated three of these genes and observed that they shared a specific region, immediately christened Homeobox. This finding demonstrated that these genes had a common origin and acted through a common mechanism. In fact, their products are now known to be nuclear proteins that control the activity of many other genes. They are “master control genes” dictating the structure of the general “body plan” and imposing their decisions onto a number of “executor genes”.

Walter Gehring told me all this during those seven hours together, and I immediately realized that I wanted to study those genes, hoping that something similar might exist in mammals. Back home, in a few days my group demonstrated that this is in fact the case. In mammals, and actually in every higher organism, exist genes that are almost identical to those operating in the fruit fly and that exert the same function everywhere: *i.e.* to control the appropriate development of all parts of the body. I studied these genes, termed HOX genes, in mice and humans and proposed a number of models of their action.

In 1991 I moved to a bigger lab in Milan and abandoned HOX genes to study similar genes controlling in turn the development of the brain. Their identification showed beyond doubt that the development of the head is controlled by the very same genes, in humans, mice, frogs, flies, round worms and in the very primitive flat worms; a totally unexpected finding. More specifically, we were able to demonstrate that one of these genes, EMX2, plays a major role in the correct development of our cerebral cortex, the most precious part of our brain, singling us out from all other species of living organisms. The study of similar genes is likely to lead us to understand in depth the occurrence of brain disorders like epilepsies, mental retardation and some psychiatric illnesses. In the wake of these studies I became more and more interested in the way the brain works. This is no longer the subject of pure speculation, but may be studied experimentally in the framework of the so-called Cognitive Neurosciences.

Since 1996 I have dedicated some of my time to writing a few books aimed at popularizing some aspects of modern, and exciting, molecular biology, fully conscious of how important books of this type had been for me in my early days. I also lecture and write newspaper articles explaining to people what biology and science itself are today and what they can do for all of us. I consider this a duty and hope it will improve the public understanding of science, at least in my own country.

SIXTY-ODD YEARS OF FLUID DYNAMICS

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In the early 1940's every little boy in England thought that the Spitfire was the best aeroplane ever made, and one of those little boys is inclined to think so still. At any rate, my interest in airplanes was awakened early, and I joined the Torquay (Devon) Model Aircraft Club at the age of ten. I went through the usual stages of wanting to be a pilot and then wanting to be an aircraft designer, but after taking a degree in aeronautical engineering I decided I wanted to go into research. In those days, the late 1950's, there were all too many aircraft that never got beyond the prototype stage, if that, and this biased me towards "pure" research rather than "project-oriented" work which would die if the project did. Of course research, like truth, is "rarely pure, and never simple": I have preferred to do the sort of research which had a fairly obvious, predominantly peaceful, and if possible fairly immediate, application. The Press and public never seem sure of what counts as science and what counts as technology, but it's simple: science is seeking knowledge of how and why the universe works, and technology is making use of that knowledge—for good or ill.

The motion of fluids is governed by the Navier-Stokes equations which, although they are based on the simple principles of conservation of mass and of momentum, are complicated to look at and very tedious to solve—numerically of course: there aren't many non-trivial exact solutions. Engineers are concerned mainly with turbulent flow, the unsteady, eddying motion that forms cumulus clouds and produces cloud-like eddies when milk is poured into tea. Complete, numerically-exact calculation of all the whirls and eddies of turbulence for a worthwhile length of time is impossibly expensive today, except for fairly simple flows on small scales. Therefore fluids engineers need approximate, short-cut "semi-empirical" methods, not explicitly the exact equations of motion but depending partly on data from experiments. Fluid dynamics is still—to my mind—an exciting field because of this dual reliance on solutions of exact but difficult equations and on measurements at full scale or in the laboratory. Some other branches of science or engineering have either been taken over completely by computation or have to rely entirely on experiment for lack of quantitative theory.

Perhaps it is because turbulent flow is so complicated that I still like looking at clouds, water currents and even milk mixing with tea. When smoking was more popular, watching the smoke from one's neighbor's cigarette helped to pass the time in committee meetings.

One of the famous people in fluid dynamics, now deceased, gave a lecture at Stanford on his research work, and said that scientific discovery is more exciting than sex. His audience, composed mainly of graduate students, left convinced that either he was missing something or they were, but they were not quite sure which. The thrilling "eureka" moments aren't so common in a well-developed subject like fluid dynamics as in newer fields such as nanotechnology or biotechnology, but they do exist. Most satisfying of all is when a prediction proves to be true. I remember an occasion when I had an idea about wind-tunnel design which I rushed to test in the laboratory. It didn't work, which was a big let-down for me. I then found that in my haste I had assembled the test rig incorrectly, and when I repeated the test my idea proved to be correct after all. I was able to make a recommendation to wind-tunnel designers which is generally followed today.

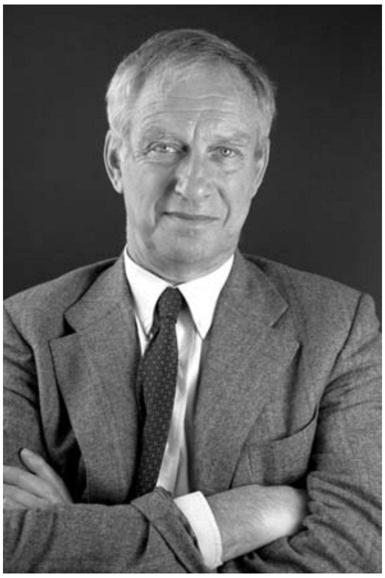
One of the most satisfying parts of working in science is that research workers are an international family. I have more in common with a fluids researcher from the other side of the world than with a physician from my own town. If I meet such a distant colleague for the first time in several years, we immediately feel at home with each other. Some scientists are better company than others, but very few are really objectionable—someone who is erratic or untrustworthy doesn't get far in science. Also, most scientific controversies are conducted objectively and usually settled quickly, in contrast to controversies about less factual topics.

Maynard Keynes once said that economists were the trustees, not of civilization but of the *possibility* of civilization. I think that this could also be claimed for most other branches of science and the technologies they serve.

Before doing an experiment or a calculation one should ask what use the results will be. In the case of fluid mechanics, one wants the work to lead to improvements in prediction methods for engineers, or meteorologists, or oceanographers, or astrophysicists or... These improvements come either from improved understanding, quantitative or even just qualitative, or from better or more extensive sets of data for calibrating or testing prediction methods.

ELECTRICITY WAS NOT INVENTED BY TRYING TO MAKE BETTER CANDLES

Edouard Brézin
Ecole Normale Supérieure, France



I went into physics… for the wrong reason. I was more interested initially by mathematics, but one day I started reading a textbook on quantum mechanics. I was fascinated by the fancy mathematical apparatus of the theory, and thus I decided to study physics. I am very glad that I made this decision, but it was clearly based upon a ridiculous analysis which shames me nowadays. Indeed the essence and beauty of quantum physics does not lie in abstract mathematics but reveals itself best in the simplest possible situations which illustrate the most striking features of the theory.

I was lucky to graduate at a time at which France was making a significant effort to build its research. In particular nuclear energy was to become a major component of our industrial society and I was given a position at Saclay, a national laboratory belonging to the Atomic Energy Commission, before I knew how to read or write. However I did not work on nuclear reactors, since there was enough intellectual freedom for doing basic physics.

I remember one of my teachers in Les Houches, a wonderful place in the Alps devoted to summer schools (they lasted eight weeks at the time), listed what he viewed as the main open problems:

- understanding nuclear forces, weak and strong
- understanding fluctuations near a critical point
- obtaining a good theory of turbulence
- reconciling quantum mechanics and gravitational forces

Thirty-seven years later I feel extremely privileged to have been the witness of so many advances during my lifetime, in particular concerning the questions just listed. The weak nuclear forces have been understood as part of a unified theory with electromagnetism; the strong nuclear forces, which bind permanently the quarks inside the nucleons, have been understood also on the basis of a generalization of the symmetry principle underlying electromagnetism (the local gauge symmetry).

Critical fluctuations have been understood through a beautiful set of new ideas, known as "the renormalization group", which influenced also many other areas of science in which self-similar (or fractal) phenomena take place. I spent several decades on those questions and I keep a wonderful memory of this exciting period of my scientific life. I have never worked on turbulent flows in hydrodynamics but a lot more about it is now understood.

The situation of quantum gravity is still far from its resolution, but if the ideas which are currently under study (the theory of "superstrings") turn out to be right, it would have deep consequences on our view of the world; for instance they imply that there are more dimensions of space than the three that we see.

The present stage of physics seems to me particularly open. After all we see only 3% of the energy content of the Universe. Most of it is "dark energy", an energy of nothing which stretches the Universe and speeds up its expansion. If the current ideas on a new kind of symmetry, "supersymmetry", required by contemporary views, are right a "mirror" image of our zoo of elementary particles remains to be found. It could be a large component of the missing "dark matter" of the Universe. The present understanding of the basic interactions leads to new investigations; for instance, are the constant of physics constant or do they change with time? Are their values accidental or fixed by some principle which remains to be found? Shall we know all the basic physical laws when quantum gravity is understood or will there be beneath a new layer of questions?

It would be a deep mistake to regard physics as limited to those "fundamental" issues. The slightest bit of matter is made of myriads of constituents and the more we understand various forms of order and disorder, the more new forms of matter that we do not yet understand manifest themselves. Nature knows remarkably how to go around mathematical theorems: for instance five-fold symmetries are forbidden for crystalline structures, but nature has presented us with quasi-crystals endowed with the just forbidden symmetry.

The XX century has been marked by quantum mechanics. Born with the understanding of atomic spectra, it has progressively become the language of chemistry, nuclear physics, solid state physics (with its world of information technology), lasers, etc. Regarded initially as a language for a few abstract specialists it became in some sense the heat engine of our times. A new era in the quantum world is now under active examination: it is called quantum computing which uses the seemingly paradoxical aspects of the quantum world, the intrication or non-separability of parts of a larger system. It may lead to significant changes in our information technology much later in our new century.

Everyone can see how much science, physics in particular, has changed our view of the world and influenced our technology; communications, energy production, medical imaging are just a few obvious examples of this fact. Those advances did not occur by trying to improve over existing technologies: "electricity was not invented by trying to make better candles" and there is no reason to believe that our century will be any different in that respect.

A LIFE OF LITERATURE, SCIENCE, ENGINEERING, BUSINESS AND PUBLIC POLICY

D. Allan Bromley
Yale University, USA

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I was born on a farm in northern Canada and received my elementary and secondary education in one-room and two-room schools respectively. I am the only member of my generation in my family and those of the region who, in large part because of the Great Depression, proceeded to any higher education.

With the help of my grandfather I had learned to read fluently by the age of four and had formed a lifelong interest in science and technology before I left secondary school not because of any exceptional teachers but because I had been given the keys to the equipment cabinets, the chemistry and physics laboratory manuals and urged to teach myself.

The only reason that I was able to continue my education at Queen's University was because I won a national competition for the best essay on the evils of alcohol—a subject about which I then knew absolutely nothing. This together with several Queen's scholarships in English and General Proficiency provided the financial support that was impossible for my family.

Given this WCTU essay, everyone, including me, assumed that I was destined to be an English major and so I was registered for my freshman year. At the end of that year I was invited—a high honor—to transfer to engineering physics that included the entire program of both physics and electrical engineering.

Graduating with highest honors I had already spent summers as an operating engineer for Ontario Hydro at Niagara Falls and had concluded already that I would transfer to surgery or physics, and on invitation from Professor J.A. Gray—one of the giants of Canadian science—deciced to join his group. I did research to my master's degree in nuclear physics. A summer appointment at the National Research Council of Canada in Ottawa on cosmic ray research resulted in my going to the University of Rochester, immediately after my marriage to Patricia Brasser, to join the cosmic ray group lead by Professors Helmut Bradt and Bernard Peters. Within a week of my arrival in Rochester, Bradt died and Peters was deported as a potential communist, killing the cosmic ray program.

Somewhat at a loss I transferred to nuclear physics on the largely defunct second 27-inch cyclotron ever built that with another graduate student we rebuilt to operating status. I was one of the first persons in the U.S. to study stripping reactions to prove that nitrogen 14 and carbon

14 both had positive parity—at the time providing critical support for the validity of the nuclear shell model.

Joined by Professor Harry Fulbright from Princeton we transformed our cyclotron into the world's first variable energy one and continued our nuclear research, myself as an assistant professor until, in 1955, I joined the Chalk River Laboratories of Atomic Energy of Canada Limited (Canada's Los Alamos). With superbly qualified colleagues we demonstrated, using a 4MV Van de Graaff and the world's only liter of 3He gas, that the collective nuclear model and the deformed shell model introduced by Bohr, Mottelson and Nilsson in Copenhagen to describe nuclei heavier than iron, were even more successful in describing nuclei such as neon 20 and magnesium 25.

Working with High Voltage Engineering we designed and constructed the first 5MV Tandem Van de Graaff and with it my group did some of the earliest precision heavy ion measurements and discovered the first nuclear molecular complex in carbon plus carbon. We later learned this to be an important feature of all heavy ion reactions. This work was only possible because while waiting for the tandem, McKay and I had developed the first silicon based precision detector for charged particles.

Moving to Yale University in 1960 it rapidly because clear that its HILAC heavy ion accelerator had been designed by and for nuclear chemists and I began working with High Voltage Engineering on the design and construction of the world's first 10 MV tandem and installed it in the new A.W. Wright Nuclear Structure Laboratory at Yale.

In the following 25 years this laboratory graduated more nuclear scientists than did any other in the world. We continued study of heavy ion phenomena and demonstrated that the nuclear shell model developed to describe light nuclei worked even better with heavy nuclei in the lead region.

During the period 1970-1977, after becoming an American citizen, I was Chairman of the Physics Department, Director of the Wright Laboratory and held the Henry Ford II Chair of Physics. I have served as a Director in several New York Stock Exchange Companies and have learned an enormous amount from this exposure.

In the latter part of this period I became ever more active in national and international science policy matters serving as Chairman of the National Academy's Physics Survey Committee, as a member if the National Science Board, as President of the American Association for the Advancement of Science (AAAS) and of the International Union of Pure and Applied Physics (IUPAP), and as a charter member of the Reagan White House Science Council where I chaired the U.S. side of the Indo-U.S., the Brazil-U.S. and the Soviet Union-U.S. bilaterals on Science and Technology.

In 1988 President Reagan awarded me the U.S. National Medal of Science and in 1989 I became the first Cabinet-level Assistant to the President of the United States for Science and Technology. During this period we published the first public statement of U.S. technology policy, and greatly increased the cooperation between the federal government and the U.S. private sector in the development of generic technologies, as well as expanding the cooperation and communication among the more than twenty government agencies having substantial research and development portfolios. We also expanded international cooperation worldwide.

In 1993 I returned to Yale intending to establish a public policy think tank aimed at improving the understanding and cooperation between the federal government and our private sector but this activity was put on hold while I became the first Dean of the Yale Faculty of Engineering in more than thirty years charged with rebuilding this faculty. In 2000 when I stepped down as Dean we had added a new Department of Biomedical Engineering and programs of Environmental Engineering and of Applied Mathematics, destined to become departments. I had raised over $50 million for engineering projects and professorships. During this period I served as President of the American Physical Society. In 1999 I became the first and only Sterling Professor of the Sciences at Yale.

I have published more than 500 papers, authored or edited more than twenty books, and have received 33 honorary doctorates from universities in Canada, China, France, Italy, South Africa and the United States. With the benefit of hindsight I would not change any of my career embracing English literature, physics, engineering, business and public policy. And most importantly, I have enjoyed all of it. Changes of field are actually important to maintaining intellectual interest and acuity and I would urge all scientists and engineers to consider spending some time in public policy and government activities as a small return for all the opportunities and activities that the public provides.

IT WOULD BE WONDERFUL TO PROVE SOMETHING

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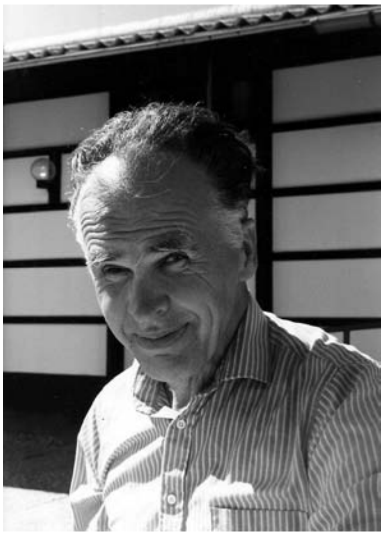

My decision to become a mathematician matured only slowly. As a child, I learned to read at 4 (upside down) sitting opposite my 3 years older sister. My parents let me show guests that I could multiply 2-digit (probably not 3-digit) numbers in my head. My first encounter with more serious mathematics came when I was 16 and took over some university textbooks. I remember most being fascinated by statements such as $1-1/3+1/5\pm...=\pi/4$. At the age of 17 I started studies at the university of Uppsala and it was natural to choose subjects such as mathematics, theoretical physics and statistics. At 19 I got my BS. It all seemed very easy and I still had no idea what mathematics was all about. I also had no clear picture what I would do in the future. Arne Beurling was the professor in Uppsala. He gave a course in complex analysis and here I met a subject which really mystified me and a personality that impressed me deeply. This was when I decided to continue my studies in mathematics. I was still uncertain if I would be clever enough for a university career but in those days (1947) a university degree always led to a job so I did not need to worry. What also helped the decision was that Beurling offered a job at the institute at 1/4 time with salary $40/month which soon became a full time job at $160/month.

Soon my studies for a doctor degree began. Beurling suggested that I should read the books in "Collection Borel", about 10 books on then modern analysis, written in French by the best French mathematicians (Borel, Lebesgue, la Vallee Poussin, Montel). I don't know what Beurling had in mind, but the method to read science books without worries about learning or examinations is a very good system just for really learning. So I went to the book store and ordered them. I also bought Zygmund's book on Trigonometric Series where I learned that one still did not know if a Fourier series of a continuous function need converge at any point—a problem open since Fourier's days in 1807. Of these books only Zygmund's book printed in Poland 1935, remains with me. The books all had soft cover and one of our dogs loved and ate the glue of French books while apparently the glue of Polish books is no good.

I got my degree in 1950 and a permanent position as professor in 1954. Looking back, I can now say that I still did not know what serious mathematics or problem solving really meant. It would take me another four years, till 1958, at the age of 30, when I for the first time wrote a paper that I still consider of some interest. There are two types of mathematical research and therefore two types of mathematicians even if the distinction is not sharp. On one hand, building systems of ideas or unifying concepts of different origin is important. Problem solving is another direction where hidden consequences are extracted. This type attracts most attention, even if it is not more important than the first. A recent famous example was "Fermat's last theorem": $x^n+y^n=z^n$ has no non-trivial solutions in integers x,y,z if $n \geq 3$. The proof by Wiles uses a lot of earlier ideas of system type. My own interests have been along problem lines and I shall concentrate on that.

Fourier's problem mentioned above was generally considered to have a negative answer, i.e. it was believed that there is an everywhere divergent Fourier series of a continuous function. In theory, to solve this problem one could divide time equally between trying to prove the positive result and to prove the negative. In practice, this does not work. You can only concentrate your efforts if you are really convinced that you work on a correct statement. When I finally could prove the positive statement, in 1965, in contradiction of conventional wisdom, it was because I had found a non-rigorous argument which was convincing, I thought.

In the case I just mentioned, it had taken me almost 20 years to solve the problem. Of course I had not spent all my time on this, but this long process is rather standard. It also says something about what it takes to be a mathematician.

The conventional picture here is that a good mathematician is a person with really outstanding intellectual power who solves the problem through his superior genius and the solution comes to him (or her, of course) as a bolt of lightening. Many mathematicians, naturally, like this description but my experience is that it is completely false. There is a very, very small group of geniuses, with very special minds for mathematics. Gauss is an example for problem solving and Newton, Einstein, Hilbert or Grotendieck for systems building. I have only met a few persons like that. For most of us—Nobel Prize winners included—the most important quality—besides of course a good intellectual capacity—is perseverance and capacity of concentration. Even Newton himself explains that he found his gravitational law "by constantly thinking about it". The psychology of this is to consider what you are trying to do as all- important and to believe that you can do it. This is very much what happens in athletics and here is a favourite story of mine which illustrates my point.

Sweden has no tradition in downhill skiing—we have the snow but no mountains that can compare to the Alps. On the continent, however, the best skiers are national heroes. Nevertheless, in the 1970's Ingmar Stenmark from the little village Tärnaby in Sweden became the leading skier in Europe. A few years later Sweden had 3 skiers among the 15 best and they all came from the same village! This is now history. However - and this may be sign in the sky also for mathematics—this year (2004) the leading skier in the women's competition is again Swedish and she also comes from Tärnaby!

The story illustrates that many of us can obtain amazing results if we are willing to concentrate our efforts on one goal for a very long period and if we believe in ourselves. The young people in Tärnaby knew Ingmar Stenmark as one of them and thought if he could do it, they can also. What is the reward? In athletics there is fame and now also money, much less of these in science and mathematics. From my own experience I would say that what really drives you is the challenge itself and the wish to prove yourself to yourself. On a more solemn level, it is of course rewarding to add a piece to the wonderful puzzle of science. Here is a missing piece in mathematics. To add two numbers, N digits long, a computer program needs Const.N steps when N is large. Is this false for multiplication? The method we usually use requires the order of N^2 steps but much more efficient methods exist. Try to find one! Probably there is no method, with CN steps. Nobody knows. Wouldn't it be wonderful to prove that?

ADVENTURE WITH COLD ATOMS

Claude Cohen-Tannoudji
Ecole Normale Supérieure, France

© Courtesy of Claude Cohen-Tannoudji

I was born in 1933, in Constantine, Algeria, which was then part of France. I did all my high school studies in Algiers. My parents were very much concerned by the education of their children and they were following very closely what I was doing at school. I think that it is very important for a young child to feel that his parents pay attention to his education. I remember also my school teachers who were excellent pedagogues and who knew how to motivate the interest of their students.

I came to Paris in 1953 as a student of the Ecole Normale Supérieure which is a higher education institution in France where students are admitted after a very selective examination. I spent 4 years there, attending a series of fascinating lectures given by the best mathematicians and physicists of France. I was initially more attracted by mathematics but met at Ecole Normale Supérieure a physics professor, Alfred Kastler, whose lectures were so stimulating and whose personality was so attractive that I decided to change to Physics. I deeply believe in the influence that an outstanding personality can have for arousing a scientific vocation.

I joined Alfred Kastler's research group to do a diploma work and then, after my military obligations, a Ph.D. thesis. I did this thesis under the supervision of Alfred Kastler and one of his first students, Jean Brossel who was also an outstanding physicist. I keep wonderful recollections of this period of my life. We were a small group and the equipment was rather poor but the enthusiasm for research was exceptional. We had endless discussions on how to interpret our experimental results. I learned there that a researcher remains a student forever. He has always something new to learn, some new tools to master. I remember that I was very much impressed to see my thesis supervisor, Alfred Kastler, following lectures at the university among his students, because he wanted to improve his understanding of matrix theory or quantum mechanics.

After my thesis, I got a position at the University of Paris. I very much enjoyed teaching. I think that research and teaching are complementary activities which cannot be dissociated. If one gives lectures without doing research one becomes rapidly obsolete because the lectures that one gives do not follow the progress of science. On the other hand, giving lectures is very important for improving one's research because when one tries to explain scientific concepts in the clearest possible way, one gets in general new interesting ideas and physical insights which can stimulate new directions of research.

I started also at that time my own research group and that was a great experience for me to work with young bright students joining me every year, to introduce them to the most recent development of physics, to start new investigations, to discover new physical mechanisms. Our general theme of research concerns the interaction of light with atoms. Observing the light emitted or absorbed by atoms gives useful information on the structure of these atoms and on the interactions which determine this structure. One can also use light, and in particular laser light, to exert forces on atoms, to manipulate them. During the last decades, spectacular progress has been achieved in this direction. We have developed in our lab new methods for cooling atoms with laser beams to very low temperatures, about 300 million times lower than room temperatures. At these ultralow temperatures, atoms move very slowly, with velocities on the order of a few millimeters per second. They can be observed for a very long time and this increases dramatically the accuracy of the measurement which can be performed on theses atoms. It is in this way that very precise atomic clocks have been recently built, the most precise ever made, which would be off by less than one second after 100 million years of functioning. New states of matters have been also discovered, where a macroscopic number of ultracold atoms condense all together in a matter wave, forming what is called now a “Bose-Einstein condensate”. These condensates have fascinating properties that we try to understand and they could have very interesting new applications like “atom lasers”, analogous to usual lasers where optical waves would be replaced by matter waves.

Science is a fantastic adventure. Every new discovery changes our vision of the world where we live. It is an integral part of the human culture, like painting, music or poetry. Understanding the basic laws which govern the huge variety of phenomena that we observe is the greatest achievement of mankind. In addition, the applications which result from the progress of basic science can bring a solution to the various problems that we have to face; finding new clean sources of energy, protecting our environment, providing enough food to everybody, improving human health... Everybody should understand that we will be able to solve all these problems by doing more science, not less. Finally, I think that science can contribute to improve moral standards, to eliminate intolerance and fanaticism by developing a critical mind, a sense of dialogue and mutual respect. So my most sincere wish would be to see all young students, all over the world, becoming enthusiasts for scientific studies.

POTENTIAL SCIENTISTS ARE BORN EVERY MINUTE

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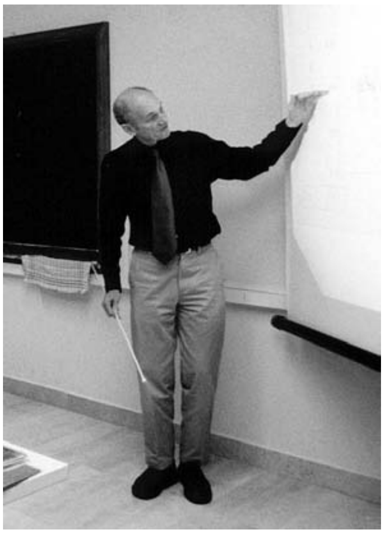

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There are many accounts by accomplished scientists that are moving because great difficulties had to be overcome, either because of an abusive regime, or because of an education system struggling in an impoverished country or because of "separate but equal" school systems as in the United States. I believe that in any spot in the world there are potential scientists born every minute. We lose them because of the lack of opportunity or deliberate discouragement, especially for women in many countries. The International Centre for Theoretical Physics is an institution which tries to help overcome the enormous disparities in the access to pure science. I shall never forget the statement of the former director of the ICTP, Miguel Virasoro: "…the opportunity to participate in pure science is a basic human right!"

It is with these warnings that I recall my experience in becoming a scientist. I was born into an academic family, not wealthy but comfortable. My father was a professor of classical languages at Southern Methodist University (SMU) in Dallas, Texas. We lived in an affluent neighborhood with an excellent school system, provided one was white. I suppose that I had a natural interest in science that was just part of my nature, but so did many others. We had chemistry sets and built crystal radios. With these kinds of interests, it would have been natural to study engineering when one finally got to college.

My interest in physics was really stimulated by an extraordinary teacher at the Highland Park high school. His class was infamous for its alleged difficulty. Consciously or unconsciously, he frightened all the young women away. Mr. Marshall demonstrated to us that physics was an experimental science, and there was a great deal of laboratory work expected in the class.

I might note parenthetically that the ICTP might well think of broadening its focus to experimental physics, as one can be sure that every minute at any spot in the world potential experimental scientists are born as well as theoretical scientists.

I will give two examples of Mr. Marshall's class. We were requested to build an electric motor, finding parts in junkyards and second-hand shops. It had to produce a part that rotated when six volts was applied. The variety of creative contraptions that were offered was memorable. The second project was to build a step-down transformer to take 120 volts AC and produce taps for 12 volts, 6 volts, and 3 volts. In addition, the transformer had to handle a load that consumed 10 watts. Most of us went to a junk shop to get a transformer core and some wire. One had to count the turns. Most of us got a husky core but one student took a core from the transformer for a discarded loudspeaker. This produced the correct voltages but when the power test was applied the transformer went up in smoke. An unforgettable lesson was transmitted. The particular student was in tears, but he was a fine pianist and went on to the Julliard School. I discovered through this high school physics class that I had a great love of analyzing data, any data—the deviation of a pendulum period from a constant when the amplitude was too large, or the details of the approach to equilibrium in a calorimeter. While in high school, I read serious science books for youngsters: I especially liked the book by George Gamow, entitled *One Two Three... Infinity*: *Facts and Speculations of Science*.

When the time came to attend college, which was SMU, I had planned to study engineering. My father wisely suggested that I major in physics and mathematics as an undergraduate and then study engineering, if that was still my interest. On completing my undergraduate studies, it seemed natural to continue in physics. I was accepted for graduate study at the University of Chicago. At that time, 1951, Chicago certainly had the best physics department in the world. I had classes with Enrico Fermi, Edward Teller, Murray Gell-Mann, Richard Garwin, Valentine Telegdi, Marvin Goldberger, and Gregor Wentzel. The atmosphere generated in all the students a passion for physics and, being just after World War II, a golden age for physics began. I could combine my love for data with a sense that one should do experiments that produced data of importance. I also learned that physics is basically an experimental science. Unless one had the brilliance of a Gell-Mann or Feynman, it was better to do experiments.

As physics was a growing field at the time, there were plenty of employment opportunities. I ended up at Princeton University, where in 1964, with colleagues Jim Christenson, Val Fitch, and Rene Turlay, we made a discovery of fundamental importance, namely that universes of matter and antimatter will behave slightly differently. This was not a theoretical discovery, but an experimental one, carried out with homemade equipment always on the verge of failure. It is a never-ending fascination for me that a jumble of equipment, wires, detectors and magnets fed by a beautiful accelerator can produce a result that is relevant to our deepest understanding of space and time.

How I Became a Scientist

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I was born in Amsterdam on December 3, 1933, the son of Anna Gurk and Jozef Crutzen. I had one younger sister. My mother's parents moved to the industrial Ruhr region in Germany from East Prussia towards the end of the 19th century. They were of mixed German and Polish origin. In 1929 at the age of 17, my mother moved to Amsterdam to work as a housekeeper. There she met my father. He came from Vaals, a little town in the southeastern corner of the Netherlands, bordering Belgium and Germany and very close to the historical German city of Aachen. He had relatives in the Netherlands, Germany, and Belgium. Thus, from both parents I inherited a cosmopolitan view of the world.

In May 1940 the Netherlands were overrun by the German army. In September of the same year I entered elementary school. My six years of elementary school largely overlapped with the Second World War. Our class had to move several times to different premises in Amsterdam after the German army had confiscated our original school building. The last months of the war, between the Fall of 1944 and Liberation Day on May 5, 1945, were particularly bad. During the cold "hongerwinter" (winter of famine) of 1944-1945 there was a severe lack of food and heating fuels. Also water for drinking, cooking, and washing was available only in limited quantities for a few hours per day, which caused poor hygienic conditions. Many died of hunger and disease, including several of my schoolmates.

In 1946, after successful exam, I entered the "Hogere Burgerschool" (HBS), Higher Citizen School, a five-year long middle school which prepared for University entrance. During those years, chemistry definitely was not one of my favourite subjects. They were mathematics and physics, but I also did well in the three foreign languages: English, French, and German. During my school years I spent considerable time at sports: football, cycling, and my greatest passion, long distance skating on the Dutch canals and lakes. I also played chess. As a child I read especially books about astronomy, geography and discovery expeditions around the world. I was fascinated about the world of the mountains, of which Holland has none, of course. During the war we could not travel anywhere and so I imagined that clouds were snow clad mountains. At home we had a book about Yellowstone National Park in the U.S.A. I read it many times and was fascinated by the pictures. First several years after the war, when I was 17 years old, I saw for the first time real mountains. I met my wife Terttu, a Finnish girl, on a mountain in Switzerland, when I was 21 years old. Yellowstone National Park, together with my family, I visited for the first time in 1975, when I was 42 years old.

Unfortunately, because of a heavy fever, my grades in the final exam of the HBS were not good enough to qualify for a university study stipend. As I did not want to be a further financial burden on my parents for another four years or more (my father, a waiter, was often unemployed; my mother worked in the kitchen of a hospital), I chose to attend the Middelbare Technische School (MTS), Middle Technical School, later called the Higher Technical School (HTS), to train as a civil engineer. Although the MTS took three years, the second year was a practical year during which I earned a modest salary, enough to live on for about two years.

From the summer of 1954 until February 1958, with a 21-month interruption for compulsory military service, I worked at the Bridge Construction Bureau of the City of Amsterdam. After marriage, my wife and I moved to Sweden where I worked in a house construction bureau. However, I was not happy. I longed for an academic career. One day at the beginning of 1959, I saw an advertisement in a Swedish newspaper by the Department of Meteorology of Stockholm University announcing an opening for a computer programmer. Although I had not had the slightest training for such work, I applied for the job and had the great luck to be chosen from among many candidates. On July 1, 1959, we moved to Stockholm and I started with my second profession, that of a computer programmer. The great advantage of being at a university department was that I got the opportunity to follow some of the courses that were offered. By 1963 I could thus fulfil the requirement for the “filosofie kandidat” (corresponding to a Master of Science) degree, combining the subjects mathematics, mathematical statistics, and meteorology.

Around 1965 I was given the task of assisting a scientist from the United States to develop a numerical model of the ozone distribution in the stratosphere, mesosphere, and lower thermosphere. This project got me highly interested in the photochemistry of atmospheric ozone, and I started an intensive study of the scientific literature. This showed me the limited status of scientific knowledge on stratospheric chemistry by the latter half of the 1960’s, thus setting the “initial conditions” for my scientific career.

I picked stratospheric ozone, and later atmospheric chemistry and climate studies, as the subject of my research. I was very lucky that there was so much to discover. An important facet of my research was the impact of anthropogenic activities on our atmospheric environment. I discovered that air pollution is not only created by industry and fossil fuel burning, but also by biomass burning in the developing world of the tropics and subtropics. A review of my research is found in my Nobel Lecture[2]. In this choice of research topic I was left totally free. I cannot overstate how I value the generosity and confidence that were conveyed to me by my supervisors Professor Georg Witt, an expert on the aeronomy of the upper atmosphere, and the head of Meteorological Institute, Professor Bert Bolin.

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[2] http://www.nobel.se/chemistry/laureates/1995/crutzen-lecture.pdf

THE MAKING OF AN ACADEMIC ECONOMIST

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© Courtesy of Enrico Fratnik

I can't remember a time when I did not wish to be an academic. My father was a university professor of economics, many of my parents' friends were academics, the guests at meals in our home were mostly academics, and what is more, I enjoyed conversing with them. So, I took it for granted that I too would pursue an academic life.

But it wasn't meant to be in economics. My father had explained to me why theoretical physics is the loftiest of all disciplines. At the University of Delhi in the late 1950's, the students I admired most read physics. So, I joined them. And I stuck to the subject on moving to the University of Cambridge as a mathematics undergraduate, to prepare for graduate work in particle physics.

It was in my third year at Cambridge that I changed my mind. For three reasons: First, even though I had been reading physics for a long time, I still hadn't been taken to the frontiers of the subject. Secondly, the last course I had attended, on scattering matrices, seemed to me to be a litany of computations; I couldn't fathom the physics in it. And third, as the Vietnam war went into full swing, I found myself spending much free time with fellow students in the social sciences, trying to detect the economic origins of war.

In those days it was possible at Cambridge to enrol as a Ph.D. student in economics with but a perfunctory training in the subject. Although I obtained my doctorate within two years from the time I began work on my thesis, my lack of training in the subject showed. On joining the faculty of the London School of Economics, I couldn't hold my own when conversing with colleagues. This made me diffident, in that I soon began consciously to avoid working on problems that were regarded as "hot" (to use a term familiar among theoretical physicists). It meant though that I could work at a leisurely pace, ruminating mostly on problems my very best contemporaries felt weren't worth thinking about; which meant, of course, that the problems hadn't even been posed. It is only recently that I have developed sufficient self-confidence to realise that my contemporaries hadn't appreciated that there could be gold hidden in the problems I chose to investigate.

In the early 1970's, a form of pure-mathematical attitude held sway among economic theorists generally. The best young theorists worked on problems that had already been formulated, but were tough to solve. Often, they involved generalising existing theoretical results in certain well-trodden directions. Now, the need for *general* results (for example, identifying conditions sufficient for a market system to sustain an efficient allocation of resources) is enormous in the social sciences. This is not only because the social world is very hard to read, but also because it changes over time. Since the actual world is at best a blur, theory has to cover *possible* social worlds. However, my training in physics told me that even if you greatly generalise a finding in one direction, if the underlying model remains very special in its many other directions, not much is gained. I was attracted to an earlier practice among economists that involves constructing *strong* special models, those that are lean and also transparent as to the directions in which they have to be extended if they are to cover more general ground. This viewpoint, which has shaped pretty much all my work, and the chance factors that are endemic in research, have led me repeatedly to identify formal connections among seemingly unrelated social phenomena.

In fact, during my years as a Ph.D. student, I developed a taste for unusual problems.[3] In my thesis I had developed a language for studying *ideal* population and investment policies for a nation. In the model I constructed to study the question, an economy's *carrying capacity* was not a datum. Because investment in capital assets can be made to expand productive capability, my model economy's carrying capacity had to be deduced from both ethical and ecological considerations. I showed, nevertheless, that if Classical Utilitarianism were to be used as the ethical basis for choosing among population and investment policies, the optimum population size would not be much lower than the economy's (optimum) carrying capacity. To be precise, I found that the ratio of optimum carrying capacity to the optimum population size was bounded above by e (≈ 2.71).[4] What I liked about the finding was not only that it was (to me at least!) altogether unexpected - and a blow for Classical Utilitarianism as an ethical doctrine - but also that it was based on an economic model that took Nature seriously.

However, in the early 1970's *ecology* was not a familiar term among social scientists. Nor was there ready access to the subject for someone teaching in a social science institution, as I was. Nevertheless, I must have been drawn instinctively to what is now called *ecological economics*, because my principle research during that period (conducted jointly with Geoffrey M. Heal, now of Columbia University) was to develop the economics of exhaustible natural resources (such as oil and natural gas) in a comprehensive manner. There was little ecology in our work, but in a treatise we published on the subject[5], I constructed a game theoretic model to show that there can be a problem of the "commons" even if entry into a common property resource is restricted. I also showed how a community could in principle circumvent a "tragedy" of the commons without creating private property rights to the resource. I didn't

[3] This taste has persisted. Over the years I have studied the economics of those human aims and activities that go toward the production and use of such elusive "goods" as knowledge, freedom, trust, health, children, and various categories of ecosystems that are listed under the label "the natural environment". In what follows, I report only that line of my research which has been directed at the interface of rural poverty in the world's poorest regions, their local environmental-resource base, and reproductive behaviour.

[4] The chapter was published as, "On the Concept of Optimum Population", *Review of Economic Studies*, 1969, 36(2): 296-318.

[5] P. Dasgupta and G. Heal (1979), *Economic Theory and Exhaustible Resources* (Cambridge, UK: Cambridge University Press).

know then, but the model offered the basis for our current understanding of an important aspect of village life in the world's poorest regions.[6] Since that book was published, one recurring motivation behind my work has been to uncover links between socio-economic pathways involving the lives of the rural poor in the poorest countries and such fundamental natural processes as those that relate nutritional status to human productivity and those that shape the evolution of local ecosystems.[7]

But there was a social problem I faced, something I had not anticipated, but which, until very recently, prevented my work from being absorbed into the mainstream of economic thinking. The problem was that *development economists didn't take the economic basis of the natural environment seriously*.[8] Why? I believe this was in large measure because environmentalists and economists in the United States and Europe saw environmental economics as consisting of problems of industrial pollution. The view led to a clash between environmental and development concerns. It was felt that a nation could afford to take Nature's services into account in economic calculations only after it became rich (as judged by GNP per head), a point that was endorsed implicitly in the World Bank's annual *World Development Report* as late as 1992.

To me though, *pollutants* and *resources* were merely two sides of a coin. Natural resources for me were not only oil, gas, the atmosphere, and the oceans as sinks for pollutants, but also ponds and rivulets, swidden fallows and threshing grounds, grazing lands and local forests, coastal fisheries and wetlands. I felt the latter simply had to be the basis of economic life among the world's poorest.[9] During the 1980's and '90s I explored several lines of attack to better understand the pathways that perpetuate extreme poverty among households in poor countries. In joint work with Debraj Ray (now at New York University), I used findings of nutritionists on the (non-linear) relationship between someone's nutritional status and their capacity for work to develop a theory of household *poverty traps*. The intuitive idea we pursued is simple enough: someone who is undernourished is incapable of earning the wages required to improve his or her nutritional status. The prior question was why that someone was undernourished to begin with. The trick therefore was to make both the nutritional status and labour productivity of a person endogenous in the analysis. We showed that in a poor, market economy the ownership of physical capital (e.g., land) matters crucially: those who are assetless are particularly vulnerable to being caught in a poverty trap. Our theory identified a mechanism by which large classes of people could be caught in a poverty trap even when an economy's gross national product (GNP) per capita grows. In a subsequent research

[6] The empirical literature on the use of common property resources in poor countries has grown enormously during the last fifteen years.
[7] In 1980, by pure chance I came across (and read!) the pioneering treatise on modern ecology by P. Ehrlich, A. Ehrlich, and J. Holdren (*Ecoscience: Population, Resources and the Environment* (San Francisco: W.H. Freeman, 1977). Since 1991 I have been taught ecology on a regular basis by a number of the world's leading ecologists, at meetings organised by the Beijer International Institute of Ecological Economics, Stockholm.
[8] To confirm this, one has only to browse books and articles on the economics of poor countries and development economics that were published before the mid-1990's.
[9] I developed this unified line of thinking in a book, *The Control of Resources* (Cambridge, MA: Harvard University Press, 1982). But the book bombed: It has only rarely been cited by either development economists or environmental and resource economists.

programme, I extended the theory by exploring the possibility that high fertility and degradation of the local natural-resource base in poor countries are linked to household poverty. In the models I constructed, none of the three spatially localized variables (*population*, *poverty*, and the *natural-resource base*) is a prior cause of the other two; rather, each influences, and is in turn influenced by, the other two. The trigger that pulls households into a poverty trap can be institutional failure, which may arise, for example, due to a breakdown of social norms of behaviour over the management of local common-property resources belonging to communities that have very little assets of other kinds. The models also identify public policies that would reduce the vulnerability of such communities. Several recent microeconomic studies have analysed village level data from Nepal and sub-Saharan Africa that are consistent with this theory.[10]

A recurring motivation of my work has been the search for comprehensive measures of human well-being. In an early work on the subject[11], I showed statistically that political and civil liberties have been beneficial for economic development in the poorest countries, implying that such liberties are not luxury goods. In joint work with Karl-Göran Mäler (Director of the Beijer International Institute of Ecological Economics, Stockholm), I have recently shown that a comprehensive index of *wealth* can be used to judge whether societal well-being is sustained along an economic programme. The index operationalises the concept of *sustainable development*. It includes as its ingredients not only manufactured assets, but also human capital and natural capital. In a book that develops the theory in a complete form,[12] I used World Bank data to show that over the past three decades, the average person in the countries of South Asia and sub-Saharan Africa have grown poorer in terms of wealth. The finding suggests that even though South Asian countries have grown in GNP per head (and shown improvements in the United Nations' Human Development Index), that growth (and those improvements) may have come in tandem with mining their natural capital assets, to the point where the countries are now poorer per capita. Their development policies have been unsustainable.

While re-reading some of my past works in preparation of this essay, I noticed that I have rarely ever published an article in which the bird I was ultimately able to catch was in fact caught. This is because I have rarely ever known what it was that I was really after. Maybe I knew it subconsciously, but I doubt that. In my case, a discovery has meant a *growing* realization, not a blinding revelation. Usually, it has taken me several publications, brick by metaphorical brick, before I was able even to understand what the phenomenon I had been working on was, let alone to uncover the pathways that give rise to the phenomenon. I don't know whether this is a common experience among scientists, but I doubt it. I suspect there is nothing common among the processes by which we gain an understanding of the world around us.

[10] This work was summarised in my *An Inquiry into Well-Being and Destitution* (Oxford: Clarendon Press, 1993). A brief account of the theory was published in an article, "Population, Poverty, and the Local Environment", *Scientific American*, 1995, 272(2), February, 40-45. My understanding of the pathways linking undernutrition, infection, and low capacity for work was shaped by an extended correspondence I had, in the early 1990's, with Professor John Waterlow, who patiently taught me the subject.

[11] "Well-Being and the Extent of its Realization in Poor Countries", *Economic Journal*, 1990, 100(Supplement): 1-38.

[12] *Human Well-Being and the Natural Environment* (Oxford: Oxford University Press, 2001; revised edition, 2004).

TO BE A SCIENTIST

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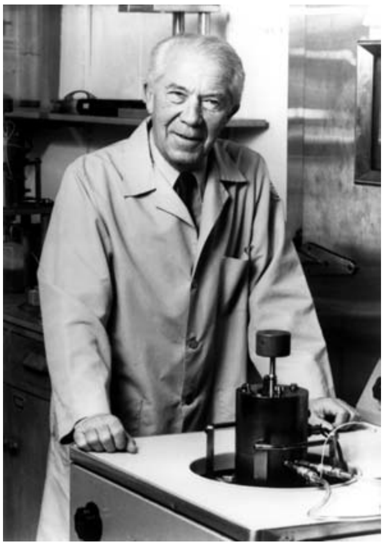

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Scientists are often described as persons who know a lot. This is not entirely wrong. To do good science, you must be trained in some discipline, like mathematics, physics, chemistry, or biology, sometimes in more than one. In addition, you must know what others have been doing in your field. But that is not enough. A "know-it-all" is no more a scientist than a collector of paintings is an artist. What counts is the generation of new knowledge or, better said, understanding. The true aim of science is to understand the world.

Not everyone, however, can be a Newton, Darwin, or Einstein. Most of us do not grapple with cosmic issues and have to be content with adding a little brick to the edifice. On a day-to-day basis, scientific research deals mostly with small problems. You are faced with some intriguing fact or observation that tickles your curiosity. Thinking about it, you let your imagination run, using all the available clues, all the bits of relevant knowledge you happen to have in store, trying to come up with some plausible explanation. This is the truly creative part of scientific activity, what it has in common with the arts. But it is only the first step. Then comes the hard job of confronting the hypothesis with facts. Does it fit with all observations? And, especially in the experimental sciences, how can you best test its validity? Not by trying to prove it right, incidentally, but by doing your best to prove it wrong—and failing.

This aspect of science is what makes it fun, like any other game of problem solving, like crosswords, chess, or conundrums of one sort or another. It has the same intellectual appeal, with the added benefit that it may tell you something about the world. What it tells, however, may become clear only after the fact.

This is another aspect of scientific research. Its results are unpredictable. Science explores the unknown and, therefore, cannot, by definition, foresee what it will discover, even less tell whether the discovery will be useful or profitable. This obvious fact is often overlooked by the politicians and administrators responsible for the funding of research. Because money is involved, they reason in terms of accountability and profitability. This is not only logically wrong; it amounts to ignoring the true value of science, namely its contribution to human culture.

All this I only dimly perceived when, as a young medical student, in the fall of 1935, I entered the research laboratory of my professor of physiology as an apprentice. I had time on my hands, and the blend of manual skill and mental gymnastics that seemed to be required appealed to me. It is only through practice and, especially, through the stimulating atmosphere of the laboratory and inspiring example of my mentors that I slowly grew to realise that I had become involved in one of the most creative and exciting human endeavours.

Contrary to many of my fellow scientists, I was not initially attracted by a special field or problem. This turned out to be useful, because it left me free to allow whatever I discovered to dictate the next step of my research, without preconceived ideas. More or less by accident, I first became involved in insulin research and spent a dozen years, somewhat complicated by the war and the occupation of Belgium, becoming acquainted with the topic and acquiring the training I felt was needed to address it fruitfully. This included learning chemistry after completing my medical studies and spending two years in Sweden and the United States to specialise in biochemistry.

Then, when I finally started on the experiments that I hoped would eventually lead me to elucidate the mechanism of action of insulin, a chance observation, in the shape of a hidden enzyme, challenged my curiosity and drew me in an entirely different direction. I never found out how insulin works but, instead, became a cell biologist and discovered lysosomes and peroxisomes, two important cell organelles. The lesson for me has been: Whatever you have in mind, follow the facts. You may not discover what you were looking for. But what you discover may be more interesting than what you were looking for.

It has been my good fortune that, while I was working in my little corner of research, truly revolutionary advances, such as no other generation has witnessed, have been made in our understanding of life. I have been blessed with enough years to ponder these matters after leaving the laboratory. Thanks to this gift of chance, I have been granted a glimpse of the "bigger picture", a deeply rewarding experience.

A RANDOM WALK IN PHYSICS

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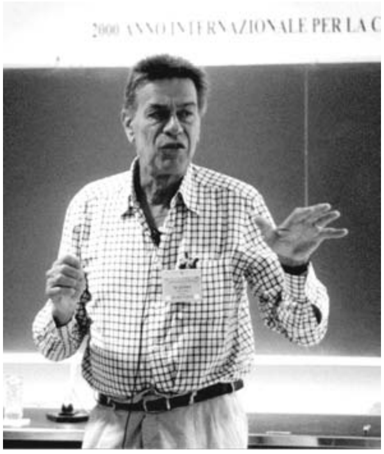

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Once upon a time, very long ago, a young man was fond of Physics. He had learnt a little from A. Kastler (in Optics), and from P. Aigrain (on Solid State). He then met a magician: R.P. Feynman. Actually, the young man never saw Feynman alive, but he started reading his papers on superfluid helium, on rotors, and later on vortices. Here was a theorist who handled equations, but who saw far beyond them! The young man was thoroughly transformed by this encounter.

He then became a theoretical assistant for a group that was studying the scattering of neutrons by solids or liquids. This led him to work on correlations in liquids, and in magnetic systems—nothing very profound but the field was educationally good.

Later, he went as a young professor to a young university (Orsay, near Paris) and he did a stupid thing: he, a theorist, launched an experimental group (on *superconductors*)! The conditions were difficult (there were bad witches around) and the experiments had to be done in a shack. But there were also good fairies. The lab, with the help of some colleagues, got some amusing results on what is now called the surface field Hc3, and on gapless superconductors.

Later, superconductivity became a heavy industry, requiring a strong metallurgical background, and our man left the field—taking with him most of his former students. They ended up on another green pasture: *liquid crystals*. This was not an easy migration. The culture, the tools, the concepts were very different. But they convinced other teams (~7) to join in, bringing in many techniques: chemistry, crystallography, optics, nuclear resonance, defect science, and even some theory! This ended up in a strong action, with many interesting novelties showing up.

Then, there was another migration, towards *polymer science*. Here, the cooperation was between three centers (Collège de France in Paris, The Polymer Center in Strasbourg, and the Neutron Center in Saclay). Later migrations reached *interfacial science* (adhesion and wetting). All along there was a very close cooperation between experimentalists and theorists—in each group the optimal ratio being ~1 theorist/5 experimentalists.

Now, our man has gone old—but he still retains the dream of finding new partners. Since he works now (for the first time) in a medical environment, he gets interested in biological subjects: a) cell adhesion and cell motion; b) the nature of memory objects in the brain. As in all fairy tales, our man has had many children (7) and grandchildren. He always loves physics, provided that it is a rational mixture of theory and experiments.

KINDLING AND SUSTAINING PHYSICS

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As a child, I was introduced to science through music. My early acquaintance with people having exciting professions came while in elementary school, when I was a scholarship student in violin at the Greenwich House Music School. It was here that I met children and parents with comfortable lives and exciting futures, very different from the lower class society in which I grew up. Stimulated by the environment at the music school and in reading books like Paul De Kruif's "Microbe Hunters", I was motivated toward self-study of math and science, which led to my entry to Hunter College High School, the only public high school of especially high academic standards available to girls in New York City at that time. This led to my access to a very strong pre-college academic program.

I got into physics through strong encouragement by my teachers at Hunter College in New York City, where I matriculated to become a school teacher, and where I first met Rosalyn Yalow. Professor Yalow, ten years older than me, taught me a course in Modern Physics in my second year at Hunter College. Through this course Rosalyn Yalow strongly engaged my interest in physics and strongly influenced me and encouraged me to become a physicist and to pursue graduate studies in physics. These studies started with a Fulbright Fellowship to Cambridge University, and continued at Harvard University and at the University of Chicago. Graduate study at the University of Chicago was carried out by students independently, but in my case it was even more independent because my nominal thesis adviser did not believe that women should pursue careers in physics. In my favor to actually become a physicist were two factors; first was the discovery in my Ph.D. thesis work (1958) of a highly anomalous magnetic field dependence of the microwave surface impedance of a superconductor which could not be explained by the recently published Bardeen-Cooper-Schrieffer (BCS) theory of superconductivity (1957). Second was the advent of Sputnik (1957) leading to a large federal increase in research funding in the physical sciences.

My marriage to Gene Dresselhaus (1958) and an NSF postdoctoral fellowship brought me to Cornell University to be with my husband. My postdoc allowed me to continue work on microwave studies of superconductivity for 2 years and to gain teaching experience in giving a course on electromagnetic theory to Cornell juniors. But after the 2 years of the NSF fellowship were over, there was no job opportunity for me at Cornell or anywhere else in Ithaca, N.Y. Therefore Gene Dresselhaus and I both left Cornell for the MIT Lincoln Laboratory, which would hire both of us, a very unusual situation at the time in view of the widespread use of nepotism rules which prevented two members of the same family from having staff positions in the same organization.

The 7 years that I spent at the Lincoln Labs were extremely productive scientifically. It was during this period that I started research in carbon science, and illuminated the electronic structure of graphite using the magneto-reflection technique. But the advent of 4 children created problems with my strict adherence to the scheduled working hours, requiring arrival at the Laboratory at 8 am. The harassment associated with these scheduling requirements, led to my gaining a visiting professor appointment at MIT in 1967, with the thought that one year of child development would help to resolve the scheduling problems. Instead, the visiting appointment led to my appointment as a full professor of Electrical Engineering at MIT, this is where I have been for the past 45 years, where I trained about 65 Ph.D. students and 30 postdocs, and from where I worked with an even larger number of collaborators worldwide.

I am best known for my contributions to carbon science, including graphite, graphite intercalation compounds (GICs), carbon fibers, ion implanted graphite, liquid carbon and more recently fullerenes and carbon nanotubes, but I have also contributed importantly to a number of other areas of condensed matter physics. Much of this work has been done in collaboration with Gene Dresselhaus, post docs, visiting scientists and various graduate students in their Ph.D. thesis work. Some of the earlier work on graphite, carbon fibers and other topics, has recently attracted greater attention because of the current interest in fullerenes, carbon nanotubes, nanoscience, and nanotechnology. The main *recent* contribution to the carbon nanotube field has been the discovery of single nanotube Raman spectroscopy on isolated single wall carbon nanotubes. This work grew out of two earlier discoveries, the first showing that the light scattering mechanism was a resonant process between the laser excitation and the nanotube electronic states that was selective of the nanotube diameter, and the second showing that metallic and semiconducting nanotubes could be distinguished by the different lineshapes of their Raman spectra. These early reports led to the successful observation of Raman spectra from an individual isolated nanotube, which is possible because of the singularities in the electronic density of states for a 1D system, and the strong electron-phonon coupling occurring when the laser excitation energy matches a singularity in the joint density of electronic states. What is important about this discovery is that, at the single nanotube level, the Raman spectra can be used to uniquely determine the nanotube geometrical structure, because the singularities in the electronic density of states, which are in resonance with the laser excitation, are at unique energies for each nanotube depending on its detailed geometrical structure. New physics has been discovered for each feature in the Raman spectra of SWNTs at the single nanotube level. The availability of a structural characterization tool for individual nanotubes makes it possible to study many other physical properties of nanotubes at the single nanotube level as a function of their diameter and chirality.

Throughout my career, I have focused on research, family, and service to the physics and broader science community. Public service has been an important part of my professional life and this has included being President of the American Physical Society, President and Chairman of the Board of the American Association for the Advancement of Science (1997-98), Treasurer of the National Academy of Sciences (1992-1996) and I now serve as Chairman of the Board of the American Institute of Physics (2003-). I also served as Director of the Office of Science in the US Department of Energy, at the Assistant Secretary level in the Clinton Administration (2000). I have been especially active in women's issues, first at MIT and later nationwide and internationally.

Over the years I have received numerous awards and honors, including the National Medal of Science in 1990, the Nicholson Medal of the American Physical Society (2000), the Karl T. Compton Medal of the American Institute of Physics (2001), 19 honorary doctorates, and membership in the National Academy of Engineering (1974), the National Academy of Sciences (1985), the American Academy of Arts and Sciences (1974), and the American Philosophical Society (1995).

PLAYING WITH NUMBERS

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I write this piece in honor of my friend and hero Abdus Salam, founder and moving spirit of the International Centre for Theoretical Physics. Salam was great as a scientist, greater as an organizer, greatest as the voice of conscience speaking for the advancement of science among the poorer two thirds of mankind. He did not play with numbers. He had more important things to do.

When I was a child, I loved playing with numbers. My interest in science was not driven by any noble desire to understand the mysteries of nature, discover new particles or cure diseases. I never thought deep thoughts about the universe. Science was exciting because it was full of numbers that I could calculate.

When I was fourteen, I won a prize in high-school. The prize was a book that I could choose myself. I chose "The Theory of Numbers", by G.H. Hardy and E.M. Wright, which was then newly published. It is a wonderful book, written at the right level for a bright teen-ager in love with numbers. I read it through from beginning to end. My favorite chapter was chapter 19, with the title "Partitions". A partition of a number n means a set of positive numbers which add up to n. For example, there are five partitions of 4, namely 4, 3+1, 2+2, 2+1+1 and 1+1+1+1. Chapter 19 was full of beautiful theorems about the partition-function p(n), which counts the total number of partitions of n. The function begins with p(1)=1, p(2)=2, p(3)=3, p(4)=5, p(5)=7, p(6)=11. The most beautiful theorems had been discovered by the Indian mathematical genius Ramanujan twenty years earlier. Ramanujan's theorems say that p(5k+4) is divisible by 5, p(7k+5) is divisible by 7, and p(11k+6) is divisible by 11, for any value of k. Ramanujan discovered them by looking at a table of the numbers p(n), and proved them by some clever arguments that are explained in chapter 19. Ramanujan died in 1920 at the age of 32. I recognized him as a kindred spirit, even more in love with numbers than I was.

After reading chapter 19, I took a closer look at the partitions of 4, 9 and 14, the first three numbers of the form 5k+4. I thought, it is all very well for Ramanujan to say that in each case the number of partitions is divisible by 5, but it would be even better if we had a way to divide the partitions into 5 equal sets. That would explain why his theorem is true. I asked the question, whether we can find a number R(P) for each partition P, with the property that the partitions of 5k+4 divide into 5 equal sets C(0), C(1), C(2), C(3), C(4), where C(r) is the set of partitions whose R(P) are of the form 5k+r for r=0,1,2,3,4. I called R(P) the rank of the partition P. I also looked at the partitions of 5 and 12 and tried to find a way of dividing them into 7 equal classes.

Four years later, when I was an undergraduate at Cambridge University, I was still playing around with partitions. After trying many candidates for the rank R(P), I found one that worked. The winning candidate turned out to be simple. The rank is the largest part in P minus the number of parts. Thus the partitions 4, 3+1, 2+2, 2+1+1, 1+1+1+1 of 5 have ranks 3, 1, 0, -1, -3, and there is one in each of the sets C(3), C(1), C(0), C(4), C(2). I checked that this rank also works for the partitions of 9 with 6 partitions in each set, and for the partitions of 14 with 27 partitions in each set. This could not have happened by accident. If you assigned the partitions of 14 by random chance into five sets, the odds against an equal division are greater than ten thousand to one. After I checked the partitions of 9 and 14, I knew that the equal division into five sets by rank must work for the partitions of 5k+4 for all k. Next I looked at the partitions of 5, which are 5, 4+1, 3+2, 3+1+1, 2+2+1, 2+1+1+1, 1+1+1+1+1, with ranks 4, 2, 1, 0, -1, -2, -4, one in each of the seven sets C(r) with ranks of the form 7k+r. I checked that the rank also works for the partitions of 12, dividing them into seven equal sets with 11 partitions in each set. So I was sure that the rank would work for the partitions of 7k+5 for all k. But the rank did not work for dividing partitions of 11k+6 into eleven equal sets. It failed already for the partitions of 6, since the partitions 3+3 and 4+1+1 of 6 both have rank 1, and there are no partitions with rank 4.

It was quite a triumph for a second-year student to discover something beautiful that Ramanujan had missed. I would have loved to tell Ramanujan about it, but Ramanujan was dead. Two other things made me sad. I could not find an explanation of Ramanujan's theorem for 11k+6, and in spite of long-continued efforts I could not find a proof of the equal division for partitions of 5k+4 and 7k+5. Finally I gave up trying and published my unfinished work in the student magazine "Eureka". My paper, with the title, "Some Guesses in the Theory of Partitions", contained only conjectures and no proofs. This was my first published paper. I stated two conjectures which I called the Rank Conjecture and the Crank Conjecture. The Rank Conjecture said that the partitions of 5k+4 and 7k+5 could be divided by their ranks into five or seven equal sets. The Crank Conjecture said that there must exist some other property of a partition, called the crank, which would divide the partitions of 11k+6 into eleven equal sets. I summarized the numerical evidence that supported both conjectures. But I had no idea how to find a plausible candidate for the crank.

This story has a happy ending. Eleven years after my paper was published, Oliver Atkin and Peter Swinnerton-Dyer, two mathematician friends of mine, proved the Rank Conjecture, using some powerful ideas invented by Ramanujan. Thirty-four years after that, the Crank Conjecture was proved by two other friends, George Andrews and Frank Garvan. Andrews and Garvan proved that the same crank will divide the partitions equally for all three cases, 5k+4, 7k+5 and 11k+6. The definition that they found for the crank is weird. Suppose that a partition has s parts. Let t be the biggest part minus the second biggest, and let d be the t+1'th biggest part minus t, with the convention that the t+1'th biggest part is zero if t+1>s. Then the crank is defined to be d if t>0 and s if t=0. It is easy to check that this crank works for the partitions of 6. The partitions are 6, 5+1, 4+2, 4+1+1, 3+3, 3+2+1, 3+1+1+1, 2+2+2, 2+2+1+1, 2+1+1+1+1, 1+1+1+1+1+1, with cranks -6, -4, -2, -3, 2, 1, -1, 3,4,0,6, one belonging to each of the eleven sets C(r). Andrews and Garvan found this definition by following a long trail of analysis also originating from Ramanujan. I would never have found it using my pedestrian method of random search. I was lucky to live long enough to see my conjecture proved after forty-five years.

All my life, I have worked as a scientist looking for situations where a little elegant mathematics can help us to understand nature. I found problems that I could solve with a teaspoonful of elegant mathematics, in physics and engineering and astronomy and biology. I never worried whether the problems were important or unimportant. So long as the mathematics was beautiful, I was happy. My work on the Ramanujan partition theorems was the least important of all, and the most beautiful. That was where my life as a scientist started, with a school prize at age fourteen. Playing with numbers was a good way to start.

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A LIFE IN SCIENCE

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My earliest memories are connected with trying to understand how things worked, and there has never been a time when I did not see myself as a scientist. My parents gave me scientific kits to make mechanical models (*not* Lego, but real spars to be joined with nuts and bolts), electrical models, before the time of electronics, and eventually a small chemistry laboratory. The war came to stop the supply of these things, but this coincided with my transferring to an excellent school with a strong scientific tradition. I found myself more attracted to the scientific side rather than technology, i.e. what were the basic laws of physics rather than its applications in engineering, what were the basic pathways of chemical synthesis rather than purposeful design for materials and drugs. This is not a matter of which thing is important, it is a matter of what one can do best. I remember being taught Euclidean geometry first. I could do it but it always required guesswork. Then I was taught Cartesian geometry, in which once you have a clear idea of what you want to do, you always win. So I am a Cartesian: set up your problem in the way you sense will be a good representation of nature, then crunch to the inevitable answer.

At seventeen I went to University and studied theoretical physics. Like most students one moves around several places in one's career, gaining experience and eventually giving experience to younger people. I won't discuss my travels but only my science. Young people naturally gravitate to difficult and fashionable areas and so I gravitated to quantum electrodynamics and made a contribution. I learned two lessons from this period. Firstly that theory is useless unless it can be checked against experiment (quantum electrodynamics can), but also that one has abilities which are not universal; one can do some things much better than others. Since science prospers from the efforts of people doing what they can do best, one should seek out areas where oneself can do best. So when my first choice moved to studies in symmetry which I found difficult, I moved into using the skills I had learned into an area where they had not yet been applied. This was in solid state theory, in particular the effects of impurities and flaws in the 'perfect solid'. This taught me a great lesson. One branch of science can make discoveries which can be adapted to solve problems in another area. Thus my study of the structure of the electron allowed me to produce a new way of calculating conductivity. Even more surprisingly the mathematics of these two problems is exactly what is needed to understand the behaviour of very long molecules, the polymers, and I was able to use the mathematics of the electron, the Schrodinger equation, to solve the behaviour of rubber and rubber-like liquids. Again the mathematics of rubber solved the problem of alloying copper in gold, and that mathematics was used by many other people to solve problems which although scientific are far from physics, e.g. how does one design the placing of oil wells in an oil field.

My latest move comes from the realisation that there are common features between cold glasses and granular systems—sand in a box say. That one can do good descriptions of granular systems, was treated with some scepticism when I first put it forward, but the last ten years have developed a mass of experimental computer modelling to show the ideas are sound.

It is clear to me that there are new areas for physics to conquer, in the mechanisms of life, the way the brain functions, can the world economic system be understood as a physical ensemble, can the environment? It is also clear that I am now too old for this; but there is plenty to do.

HOW DID I GET FROM HERE TO THERE?

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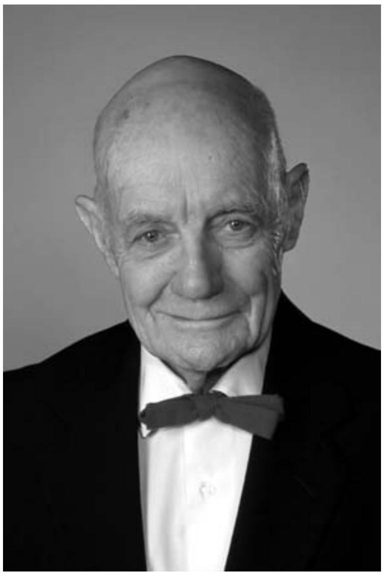

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"How did you get here from there?" is a question often asked of one who has achieved what passes for "success". That question assumes there is a rational explanation which, once understood, can help others become "successful". Moreover, many if not most species of animals and birds have developed procedures aimed at teaching its young the secrets of survival which, in a very fundamental perspective is the *sine qua non* of "success" in almost every sense of that word. Indeed, all species of fauna seem to engage in teaching their young the secrets of survival which, in a very fundamental sense, can be considered the cornerstone of any meaningful success. Indeed, it can be persuasively argued that Nature itself teaches species how to "succeed" by allowing only the fit to survive!

Now I, along with others contributing to this project, have been asked to explain how and why we have achieved some success in science. The easy and most truthful, but least satisfying, response to that question is simply: "I don't really know!" In fact, Werner Heisenberg's uncertainty principle, in which most modern scientists believe, asserts the impossibility of predicting in detail the future physical behavior of any particular atom or molecule. The reason is that the very act of interrogating such an individual particle about its behavior inevitably affects that behavior by an uncertain amount. Indeed, one can perceive an analogy to Heisenberg's "uncertainty principle" in Sigmund Freud's concept of the "unconscious mind" by which the behavior of an individual human can be governed by mental processes of which that individual is completely unaware!

Fortunately for science, the macroscopic behavior of a large population of individuals can often be quite accurately predicted because the tyranny of large numbers averages out the uncertainties in the microscopic behaviors of its component individuals. Moreover, the fraction of that population that occupy any particular state at some future time can also be accurately predicted even though the particular individuals that will occupy that state cannot be identified ahead of time. In what follows, I try to recall some of the factors that I *think* may provide clues to the reasons I have been asked to explain why I am here! But I urge any reader *not* to be tempted to consider anything I say as a recipe for repetition of my good fortune. As has often been said, "One man's meat is another man's poison!"

My father was raised on a farm which his father ran but did not own. In my experience people raised on a farm are likely to be resourceful because they were often forced to solve problems that arose without any help from "experts." Some of my friends who are engaged in experimental research in universities insist that graduate students who have been raised on a farm are particularly welcome in their laboratories because such students seem more able to cope with problems in experiments or with equipment than students raised in the city! I know that my Dad was extremely resourceful, seemingly able to fix almost anything mechanical or electrical that wasn't working properly. True, his college degree was in electrical engineering but I strongly suspect that his resourcefulness stemmed more from his experience on the farm when he was growing up than from his formal education. I know that during my childhood and youth I had implicit faith in his ability to fix anything. He was a carpenter, mechanic, electrician and photographer of no mean ability who had a great love of music. Our basement was home to years of issues of Popular Mechanics and Popular Science Monthly which I read and reread many times. My mother was also a very intelligent and caring person. The seventh of ten children of a country doctor she firmly believed that a mother should be in the house when her children, I and my brother Norman, three years younger but much bigger and stronger, came home from school in the afternoon. A book-lover who spent endless hours reading to her children, she was very active member of the Parent Teachers Associations in their schools. Much more liberal than Dad, her subscriptions were to "pink" periodicals like The Nation and The Christian Century. I too became a book-lover like my mother, and from the 5th grade on, was a frequent "customer" of the public library and read an average of three or four books a week till I finished college. One of the earliest books I remember was called "Stories of Everyday Wonders" out of which my parents would read to me at bedtime. It described and explained the intricate and marvellous systems that supplied the vital components of our lives which we so often take for granted like hot and cold running water, electricity, gas, coal and the blocks of ice that kept food and drink cool before the days of mechanical refrigerators. When I was a about seven or eight, Dad and Mother invested what was for them a considerable sum in a 20 volume encyclopedia for young people entitled "The Book of Knowledge". I became so entranced with those books that I often say, without undue exaggeration, that "I got through college on the Book of Knowledge!"

The first ten years of my life were spent in Hackensack, New Jersey, some ten miles from New York City. Dad was the manager of a small company engaged in waterproofing cotton duck and located in the adjacent town of Lodi, N.J. The company was sold in 1926 and Dad was unceremoniously released by the new owners in 1925. Approaching 50 and finding equivalent jobs scarce, he was paying the bills by working as a temporary draftsman at the Fokker Aircraft Company in nearby Teterboro, NJ. The value of having a trade as a back-up for a profession was an enduring object lesson for a youngster on the eve of the Great Depression and was the reason that I later went to welding school and became a certified electric welder! In 1928 we moved to Berea, Kentucky, the home of a remarkable institution with the official title of "Berea College and Allied Schools." It comprised a student body of some 1700, divided among four schools including: (a) The Foundation Junior High School, with an ungraded program through which students with as little as two years of formal school could progress at their own rate up through the equivalent of eighth grade, finishing with an accredited ninth grade curriculum, (b) the Academy, which provided an accredited curriculum for grades ten through twelve, (c) the Normal School with a two-year program leading to a

Teaching Certificate, and (d) the College, which offered accredited programs leading to B.A. degrees in the liberal arts and sciences as well as B.S. degrees in Home Economics and Agriculture. Dad's new position was teaching Auto Mechanics and Electricity in the Industrial Arts Department of the Foundation School and the Academy.

In later years Dad and Mother both said time and again that losing his "good job" in New Jersey was the greatest blessing that could have happened to the family. To this day, my brother and I share those sentiments and count ourselves privileged to have been reared in what was a truly remarkable community whose then soul was its President, William J. Hutchins, father of Robert Maynard Hutchins, the "boy wonder" of the American education scene who became Secretary of Yale at the age of 24, Dean of its Law School at 26, and President of the University of Chicago at 31! "William J." as the father was affectionately known at his own institution was truly one of Nature's noblemen. A striking man of vision and patrician to the core, his Berea was a singular stage on which the play was always provocative and the message meaningful. Name an outstanding person of letters, the arts, science or religion of those days and the odds are high that he or she came to Berea and talked at the thrice weekly "United Chapels." Attendance was required of all students and resented by most, but at my 50th college reunion there was a remarkable consensus among my surviving classmates that the Chapels we used to hate had become the most memorable components of our school experience. A most remarkable feature of Berea was that it charged no tuition! It admitted only those students who could prove that their families could not afford to pay any tuition. The whole institution, including several "industries" such as large vegetable gardens, a dairy, a sheep farm, a broom factory, and a variety of handicrafts, was run by student labor under the supervision of permanent staff. Every student had a job. The average gross out-of-pocket cost of a year at Berea in the 1930's, including room and board, was about $300! The standard workload was two hours a day, which was sufficient on average to cover about half of that amount. An appreciable fraction of the student body was the so-called "half day" students who earned enough to pay all of their expenses by working four hours a day instead of two.

When I entered college (with the class of 1938) I had decided to major in Chemistry, probably because of my affection and respect for Julian Capps, Professor of Chemistry. He and his charming wife Hilda, along with our next door neighbors, George and Eleanor Bent, were my parents' closest friends so I got to know Julian very well. He was a great raconteur who could recite Milton and Shakespeare at length. He had a rare sense of humor and an amazing storehouse of knowledge of all kinds, from natural history to soap-making. Any doubts I might have had were swept away in his freshman chemistry course. He was a magnificent teacher who made his subject live, in part because he had worked in industry and could relate the classroom to the real world of both commerce and everyday life, much more convincingly than did any textbook. I really looked forward to going to class, a rare sentiment among today's first year students in chemistry. As now taught in too many universities, introductory chemistry courses have become crucibles in which interest in the subject is cremated rather than ignited. After summer sessions at the University of Iowa and Purdue, I spent three years in graduate school at Yale. I thoroughly enjoyed those years in New Haven—but in spite of chemistry, rather than because of it. I found the courses in that subject dull and my research project boring, but even so, my teachers were kind and caring people.

After leaving Yale I spent the next 12 years in industry during which the pleasant memories of my school years in Berea fostered a growing interest in returning to the groves of academe. When I finally found an opening, through a back door at Princeton, Julian Capps was my role model. Of course, he was only one of the many teachers who, along with my parents, molded my life. I then moved to Yale where I stayed until my formal retirement. Later I moved to Virginia.

In sum, for me—and probably for most other people who have been fortunate enough to taste some of the fruit of what passes for success—it has been my parents and my teachers, from kindergarten on, whom I must salute. They molded the raw material.

SUPERNOVA AND SUPERGRAVITY

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I am happy to join the ICTP's project to present the early motivations and key achievements of practicing scientists in their own words. In my case, the story of how I chose theoretical physics as a career path is hardly exciting, but the truth should be told. I grew up in a middle class family which was not well off financially. My mother and father were loving, and encouraged my education, although for both of them education had ended at the high school level. Looking back at my own high school education, it was true that I had some natural talent for science and math. But I cannot recall now any specific motivation. In fact I recall being rather unsatisfied by my year of physics. The text was "Modern Physics," by Dull, Metcalf, and Brooks, which students rephrased as "modern physics is dull." Indeed I left high school with the intention of going into medicine.

Happily my true awakening to physics came in my first year at Wesleyan University, a liberal arts college in Connecticut with only 650 students at that time. Wesleyan offered a special joint math-physics course to a small group of first year students. I decided to join that program in which classes in calculus and physics were separate but closely coordinated and taught by dedicated teachers. It was eye-opening and astonishing for me to learn that that one could use calculus to describe physical phenomena in a precise way. The basic laws of physics could be formulated mathematically! In this form the laws were, in my view, simple, elegant, and beautiful. At this point I embraced fundamental physics completely. With some ups and downs, my formal education ended with a Ph.D. degree from the University of Wisconsin in 1964. My doctoral thesis was entitled "Regge Poles in Boson-Fermion Scattering." Little of current value survives today, but it was good training.

I want to discuss two of my own research contributions. The initial work was done in the mid-1970's. One of them has some practical implications for the violent explosions of stars known as supernova explosions. The other was the discovery (with my collaborators Peter van Nieuwenhuizen and Sergio Ferrara) of supergravity in which Einstein's general relativity is combined with supersymmetry. This has led to a broad theoretical structure which could have applications to the structure of space-time, and to particle physics at accelerators currently under construction.

The path toward my work on supernova began when the Weinberg-Salam-Glashow theory of the weak and electromagnetic forces was confirmed experimentally. The beautiful idea of that theory was that, despite very different properties, the weak and electric forces could be combined in one single framework. The theory predicted that conventional weak processes, such as the beta decay of a neutron, proceed by exchange of a heavy charged particle, called the W-boson. This was expected. What was not expected was the prediction of a new type of weak process, the scattering of neutrinos from protons or neutrons which required the exchange of a heavy neutral particle called the Z-boson. Such "neutral current" processes were discovered experimentally in 1972, and the confirmation of the electro-weak theory brought a lot of new excitement to particle physics.

Applying simple ideas from scattering theory that I learned in graduate school, I realized that there would be a coherent effect in the scattering of neutrinos from a nucleus such as iron which consists of 26 protons and 30 neutrons. Roughly speaking the coherent effect meant that the scattering from the iron nucleus would be 56 times stronger than that from 26 individual protons and 30 neutrons. Knowing little about astrophysics, I discussed this with my colleagues who mentioned supernova explosions. The energy source of stars is the nuclear reaction occurring in the center of the star in which small nuclei fuse into bigger ones. Fusion reactions liberate energy which slowly diffuses toward the surface of the star, and we see that energy as emitted starlight. This process takes many millions of years until the center of the star consists largely of iron. Since iron is the most stable nucleus, further fusion toward still heavier nuclei is not possible. Nuclear reactions stop in the stellar core. The dominant force is then the gravitational attraction of the iron nuclei. This causes the sudden collapse of the core into a neutron star or a black hole, and the emitted energy blows off the outer layers of the core and rest of the star. These supernova are observed and are among the most spectacular astronomical events known.

Theory suggested that neutrinos stream in vast numbers out of the collapsing core, and that the neutrino energy is transferred to the outer core layers causing them to explode. Physicists had tried to simulate that process on large-scale computers, but the problem was that known non-coherent neutrino scattering did not transfer enough energy. It was just too weak. I pointed out that the coherent process of scattering from the entire iron nucleus was much stronger and could solve this problem. My suggestion was a little naive, because the explosion process is very complicated. No single effect dominates, but the coherent scattering process was a new effect which does play an important role.

Let's move the clock forward to 2004. It is known that about 1/4 of the energy in our universe appears in the form of cold dark matter. This matter consists of neutral particles whose mass is somewhat greater than the W- and Z-bosons discussed above. It cannot be the Z-boson, since this is unstable. Cold dark matter must be made of new stable particles which interact very weakly with ordinary matter. It is a very important problem to find out the properties of these particles which make up so much of the mass of the universe. Indeed many of them are streaming through the earth at all times, but interacting too weakly to be detected. Nevertheless, there are experiments set up and others planned to try to detect these extremely weak interactions and thus confirm directly the existence of cold dark matter. The detectors used consist of heavy elements such as germanium, in which there is a coherent scattering effect which is very much the same as the one I discussed for neutrinos. The idea is that particles of the cold dark matter penetrate the detector. With low but non-zero probability some of them will scatter from coherently germanium nuclei. Energy is transferred to these nuclei, which recoil in the detector and then can be observed.

Supergravity involves rather abstract theoretical ideas, and is thus harder to explain, but I will try. In the early 1970's quantum theories with supersymmetry were first proposed. Supersymmetry brings together particles with different spin. The electron which has spin 1/2 should have a partner, called the selectron with spin 0. The photon which has spin 1 should have a partner, called the photino, with spin 1/2. Particles paired by supersymmetry are called superpartners. In fact all currently known elementary particles are predicted to have massive superpartners. The mass of some of them is in the energy range accessible to the LHC accelerator which is now being built near Geneva, Switzerland. Most heavy superpartners are unstable, but one of them is stable in most versions of supersymmetry. The stable particle is a leading candidate for the cold matter discussed above.

The principle of supersymmetry should apply to gravity. It implies that the graviton of Einstein's theory of general relativity, which has spin 2, should have a partner with spin 3/2, called the gravitino. This was expected on general grounds, and several groups in 1974 were trying to formulate a precise theory using extensions of Einstein's geometric ideas. I listened to their presentations at conferences. They sounded quite beautiful, but the physical content of their proposals was unclear. Early in 1975 our group began an approach to supergravity in which we tried to work with the simplest physical ingredients, namely the fields describing the graviton and gravitino. We worked very hard for several months. We wrote down possible equations for these fields and checked whether they satisfied the requirements of supersymmetry. In the end we found a unique and reasonably simple set of equations. Success was a real thrill!

Later more general supergravity theories were found in which the graviton and gravitino couple to superpartner pairs involving spin 0, 1/2, and 1, as discussed above. There are specific supergravity effects in these theories which can be tested by experiments at the LHC accelerator. One major application of research in supergravity is to develop specific models of this type. This is important, but there is a more fundamental, but speculative side of supergravity which we now discuss.

There are two main ideas. The first is that there may be "hidden dimensions" of our universe, beyond the 3 spatial and 1 time dimension that we are all aware of. We can't see the hidden dimensions directly, because they are too tiny. As a simple model one can think of a tiny sphere located at every point in the observable 3-dimensional space. Indirectly we might observe their presence if we have high enough energy to produce certain wave patterns on the spheres. These wave excitations would be seen as extremely heavy particles in 3-dimensions. Physicists investigated whether the ideas of supersymmetry and supergravity could be applied to higher dimensions, and they found that this could be done consistently up to a maximum of 11 dimensions, of which 3 space and 1 time were familiar to us, and up to 7 more are hidden.

The second main idea was that the most basic units of matter are not point-like particles but tiny vibrating strings or even vibrating membranes. The theories that contain them require spacetime dimension 10 or 11. Because of the uncertainty principle it would take a lot of energy to actually verify the string-like structure. In low energy experiments we would just be able to see the stringy particles as points. It is for this reason that, from the low energy viewpoint, the super-string and super-membrane theories are described by supergravity in dimensions 10 and 11. The properties of such theories are a major topic of modern research, and many theoretical papers appear each month. It is deeply satisfying to me that the basic ideas of supergravity which we developed nearly 30 years ago continue to have broad applications today and might eventually be confirmed by experiment!

EDUCATION, SCIENCE AND CHANCE[13]

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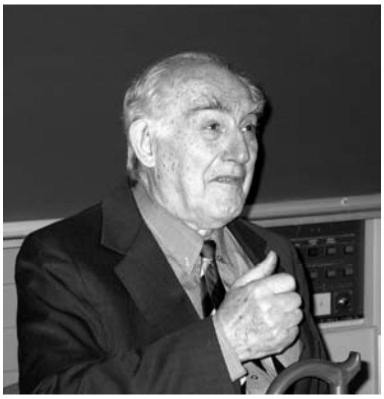

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My school years coincided with perhaps the most unfortunate period in the history of Soviet secondary education. Of the old school (gymnasium, etc.) there remained buildings. However, there were several old and supposedly skilled teachers. Chaos reigned over the rest. In 1931 I graduated from a seven-year school, our schooling having been "cut short" by the requirement instituted at that period to learn a proletarian trade in a factory. Finally a few years later this ill-founded system was changed in favor of one in which schooling went on for 10, and later 11, years.

The absence of the proper "educational" atmosphere, in the family in particular, had the effect that I am under the impression that I gained little from school. Nevertheless, the interest in physics emerged even in those years and a steady one, though I myself do not know why. I was fond of O.D. Khvolson's book "Fizika Nashikh Dnei" (The Physics of our Days), which I read even at school or immediately after graduation, it seems to me. All in all, I never hesitated about going in for physics, but I can recall neither the teacher, nor the textbooks.

On graduation from school, I somehow got fixed up in a job as a laboratory assistant in the Moscow Evening Machine-Building Institute. Initially, I was "in training" in A.A. Bochvar's laboratory of the Institute of Nonferrous Metallurgy and then I found myself in the X-Ray Laboratory. The chiefs were E.F. Bakhmetev and N.K. Kozhina (for some time also Ya.P. Silisskii). The major power was Venya Tsukerman. Leva Al'tshuler was also there. The three of us were on friendly terms and worked together. Of course, I ranked third; the lads were three years older and knew more. Ven'ka called us the 3V's: for Venya, Vitya, and Vladimirovich (that was Al'tshuler's patronimic).

The work in the Laboratory was of benefit to me: it taught me resourcefulness (following Venya's example) and experimental skills. In physics, to say nothing of mathematics, I made no significant progress. The year of 1933 saw the first "free" (i.e. "competitive" rather than by assignment) enrollment to the Moscow State University (MGU), and I decided to enter the Department of Physics. In three months I went formally through the 8th, 9th, and 10th form courses but I am convinced that the lack of a good, regular school had an adverse effect on me. While a schoolboy solves, say, 100 or 1000 problems on trigonometry, logarithms, etc., the number I solved was 10, or 100 times smaller. The same was true of arithmetic. And this

[13] You may read more in my book "About Science, Myself and Others" which will appear soon. (IoP Bristol, 2004).

told on me forever: I perform calculations badly, slowly, with effort, automatism is lacking. I have always feared and disliked calculations. Of course, behind it is the absence of ability for mathematics (in comparison with the corresponding abilities of the overwhelming majority of fellow-theorists). But this is precisely the reason why the lack of training had so pronounced an effect.

Of course, the lack of regular school adversely affected other aspects, too. At the age of about 30, I read for the first time "Byloe i Dumy" (The Past and Meditation) and many other works of literature (however, I am not sure it is a drawback). Of greater significance is "the Russian language". When I was in my 2nd year at the MGU, all of us took dictation and I made eight mistakes to get "unsatisfactory". Even now I write with mistakes. Making grammatical mistakes is not as significant as the ability for writing, the mastery of style and language. My language is somewhat poor and my phrases are frequently not quite literate. In this connection I recall my conversation with G.S. Gorelik. He had the ability to write well, and to my question "What helps you write so well?" he replied with a question: "How many times a week did you write compositions at school?" I answered, "something like once a week or once in two weeks, I do not remember". G.S. remarked that he had studied in Switzerland and wrote compositions every day. That is why I still have some gaps in my school knowledge. Disgracefully, I also do not know foreign languages, though, thank God, I have somehow mastered English (but I only can speak, though with mistakes, and make reports, while I am almost unable to write on my own without someone checking it). I am writing all that because I have firmly come to the conclusion that a person needs quite a lot so as to do real work and achieve success and satisfaction. Not knowing languages is, as a matter of fact, a disgrace, to say nothing of the harm to the business. The Europeans do not have such a problem. Any Dutch physicist knows English well and probably also knows German and French: having a facility for languages, one could master a language even without studying it at school—having started from childhood and so forth. But what if a person does not have linguistic abilities? These are specific abilities indeed. I, for example, am absolutely unable to remember poetry and in general am not able to learn anything by heart (as, for instance, a report). In childhood years at school, I would probably have been able to cope with all that. All my life, I have felt regret that I do not know languages, that I could know more about this and about that. However, when your work is in progress and there are so many interesting things in it, will you learn verbs or the names of constellations? I for one have never been capable of doing that.

All in all, no educational institution would make one into a very good writer, physicist, or mathematician, unless he exhibits the corresponding aptitude. However, first, inclinations alone would not suffice. How many talented people never "realized" their potentialities and what role was played by the shortcomings in education? Second, a good background, training, etc. are supposedly able to make a worthy professional out of a person of average abilities, who would otherwise be a drudge, become a failure, find no satisfaction in work, etc. On the whole, it is all clear. I write wherever I am led by my pen; and this topic has been touched on because I have often pondered over the question as to what losses I have "incurred" due to unfavorable conditions at school. Of course it is impossible to give a clear answer.

On the one hand, as I believe, I was extremely lucky as regards the "realization" of my modest abilities. But, on the other hand, what would have been possible if I had studied in a good ten-year school, to say nothing of “professional” family support (there was none)? Here, I would like to touch upon yet another “favorite” topic which I ponder over quite often. Take for example a sportsman who covered, say, a 100-m distance in 9.9 seconds to become an Olympic champion and a sprinter who did it in 10.2 seconds proved to be the fourth to miss even the bronze medal (the figures are, of course, arbitrary). Here, random circumstances might have played their part: how he had slept, what he had eaten, how he had pushed off the shoe, etc. Fortunately, in science this is not the case: the lot of the fourth is much better, he makes his contribution, writes good papers (with the understanding that the first writes very good ones). But the role of chance and of good luck may still be critical. This is not so for titans like Einstein, for too large is the “safety margin” and the outstripping of others. The talents of Maxwell, Bohr, Planck, Pauli, Fermi, Heisenberg, and Dirac were scarcely dependent as well on the fluctuations of good luck, accidental idea, etc. De Broglie, and even Schrödinger, were, it seems to me, a different matter, to say nothing of numerous Nobel Laureates. M von Laue was a well-qualified physicist, but they state that the idea of X-ray diffraction in crystals was a “beer idea” (Bieridee). Braggs, Roentgen, Zeeman, Stark, Lenard, Josephson, Penzias and Wilson, Hewish and Ryle, Cherenkov, Basov and Prokhorov, as well as 3/4 of the entire list were largely strokes of luck rather than “divine” revelations. I only want to emphasize that chances of success depend both on a lucky strike and a variety of factors, which include health, a timely read article or book, activity, ambition (as a stimulus), and perhaps many other things. An interesting topic.

LISTEN TO YOUR INNER VOICE

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Listen to your inner voice and do not give up too easily when an idea is criticized. My experience has been that if you think about it for a while before telling someone who happens to be very critical of new ideas, you might be able to defend it.

THE DELIGHTS OF STRING THEORY

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My interest in physics was stimulated in childhood by a combination of parents who were devoted to my education and an outstanding school physics teacher. Following an undergraduate degree in Cambridge I started my Ph.D. in elementary particle physics in 1967, also at Cambridge. This was an era in which there was no coherent theory of most of the fundamental forces. The theoretical framework known as quantum field theory, which had proved immensely successful for combining electromagnetism with quantum theory in the 1940's appeared to be of little use for describing the other three forces. In the 1960's there had been a large accumulation of experimental data concerning the strongly interacting particles such as protons and mesons. According to the quark model of Gell-Mann and Zweig, such particles are composed of quarks and anti-quarks but at that time there was no theoretical description of the strong force that binds the quarks together. Likewise, there was no consistent description of the weak force associated with neutrinos. The remaining force, the force of gravity described by Einstein's theory of relativity posed special theoretical problems. Even though gravity is too weak to be directly measurable in terrestrial particle physics experiments it should become the predominant force at ultra-short distance scales around the Planck scale of 10^{-35} metre. However, all attempts to unite general relativity with quantum theory at these microscopic scales had failed dramatically. For example, conventional quantum field theories that include gravity often make meaningless infinite predictions that indicate they are not adequate for describing physics at ultra-short distances.

For these reasons, by 1967 quantum field theory had been more or less abandoned in certain institutions, including Cambridge, despite its resounding successes in describing quantum electrodynamics. An alternative 'S-Matrix' approach had been suggested for describing the strong force while the other forces were considered to be impossibly difficult to consider. And yet this was the very year in which others were formulating the unified electroweak quantum field theory which led to the remarkable success of the Standard Model! Although the S-matrix viewpoint did not prove to be useful in its own right it eventually motivated Veneziano to boldly propose a rather simple formula that was supposed to described the scattering of mesons. This remarkable suggestion appeared in 1968 and had a huge impact on my subsequent career. It took a couple of years before it was understood that the Veneziano scattering formula can be derived from a theory in which mesons are pictured as extended string-like objects—this was the beginning of string theory. Although my early interests were phenomenological in the early 1970's I became more and more entranced by the novelty, elegance and richness of string theory. The essential idea of the theory is surprisingly simple—it is based on the premise that the fundamental particles are extended, string-like objects whereas all conventional quantum field theories assumed them to be point-like. Unlike a point, a string can vibrate. Its different modes of vibration have different frequencies and are observed as the different kinds of elementary particles. This provides a very attractive way of unifying a variety of particles.

However, the earliest versions of string theory had problems and general interest in the subject declined with the invention of a quantum field theory known as QCD that described the strong force and the subsequent formulation of the Standard Model, which described all three non-gravitational forces in terms of quantum field theory. I nevertheless remained transfixed by structure of string theory. In 1979 I started to collaborate with John Schwarz whom I had known since we were both in Princeton in the early 1970's although we had never worked together. We were very keen to understand how to construct consistent string theories that embodied gravity and might provide a unified description of all the forces, including the force of gravity. In such a picture all of the so-called fundamental particles—the graviton, the photon, quarks, the electron, neutrinos and other particles—would be identified with distinct modes of vibration of a single kind of string-like object. Furthermore, the fact that the string has a tiny, but non-zero, size has a radical effect on the short-distance structure of the theory which would be necessary in any consistent theory containing the Planck-scale effects of quantum gravity.

Very few researchers were interested in string theory at that time or for several years after so we were relatively free from competition. Starting in 1980, we published a series of papers over a four year period formulating various kinds of superstring theories—the prefix 'super' refers to the property of supersymmetry embodied in these theories. Quite soon it was apparent that something remarkable was afoot—we showed two types of superstring theory avoided the problematic ultraviolet infinities that had plagued all previous attempts to unite gravity and quantum theory. For the first time there was the prospect of a consistent quantum theory of gravity, although string theory was sufficiently unfamiliar to most of our colleagues that this result had little immediate impact. In the summer of 1984 John Schwarz and I met in Aspen Colorado and there we showed that superstring theories are also free of gravitational and Yang-Mills 'anomalies'. Such anomalies are inconsistencies that had appeared in virtually all interesting unified quantum field theories and had also been expected to arise in string theory. After we demonstrated the absence of anomalies in specific string theories there was an immediate explosion of interest in the subject and large numbers of theoretical physicists started to study superstring theory. Within a few months it was understood how the theory, which had been formulated in a rather abstract manner in ten space-time dimensions might make contact with the observed physics of our four-dimensional universe.

Until 1984 I had given only one five minute talk on string theory at a small ICTP workshop. Subsequently, I was inundated by invitations to talk at international conferences and workshops. I was particularly grateful to Abdus Salam who took a strong interest in the subject and gave me the opportunity of organising an annual spring school in string theory at

ICTP between 1986 and 1990. The new-found popularity of the subject was gratifying in many ways, but gone were the days when John Schwarz and I could spend a few months together and take up where we had left off a year later. String theory has progressed greatly in the past twenty years. We now understand very much more about the way the apparently different versions of string theory are all related to each other and to a theory known as eleven-dimensional supergravity. This set of relationships is currently called '*M*-theory'. But many aspects of the theory are still far from completely understood and a more basic formulation of the theory that captures the essential features of quantum space-time is still lacking. This is necessary, not only to formulate the theory more completely but also to understand its predictions for experiments in particle physics and cosmology.

From its obscure beginnings as a model for describing the strong force string theory has evolved into a central area of research which connects theoretical and experimental elementary particle physics, quantum gravity and cosmology. It also enjoys a remarkable symbiosis with some of the most interesting areas of modern mathematics. It is precisely because so much of this beautiful theory remains so puzzling that it will continue to be a source of fascination for many years to come.

Measuring Consciousness

Susan Greenfield
The Royal Institution of Great Britain

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At school, science wasn't particularly appealing in the way that it was taught. In those days, at least in biology, we were simply required to copy down from dictation from the teacher, telling us about the life-cycle of the amoeba, for example. The only way in which one could distinguish oneself was actually to be neat, and I have to say I wasn't very good in that department!

Chemistry involved learning about the distillation of water, although no one ever explained why. And meanwhile, physics was perhaps the most interactive: in those days, Nuffield Six had just been launched, and we were required to play with ticker-tape trolleys to give us a feel for time and space and, indeed, to skid dry ice across glass to gain a notion of friction. That said, the teaching was nonetheless very constrained and, compared to history, literature and language, there was, for me, no contest.

No one told me that in order to have insights into the excitement of science, one had to first master the basics—I thought the basics was all there was. At that stage, I thought that science wasn't for me. However, the lesson that I learned in retrospect, is that relevance to life and the ability to have your own ideas are vital for excitement in the subject—and this I was to learn later on that science could, after all, provide.

Once I arrived in Oxford, really to do philosophy, I found it very frustrating: we spun words and had ideas, but nothing could actually be proven and, indeed, in those days, the ideas also were somewhat desiccated. We concentrated on the structure of language, and I remember despairing, sitting in the new Bodleian Library having to read a whole chapter on the definite article.

Meanwhile, the other area of my study, psychology, was proving to answer more of the questions that I had asked. Since it was a relatively new subject in those days, and people were uncertain as to exactly what it might cover, the entrance requirements for it were less stringent than for one of the more established science courses—hence for the first time I encountered not just scientific method and the concept of controls, but also the idea that not everything was known and cast in tablets of stone. Far from putting me off, this uncertainty was actually exciting, as it meant there was room for discovery.

It was while I was doing psychology that I was introduced, for the first time, to human brain dissection. I can always remember the day when the plastic pots were put in front of us, and we had to role up our sleeves, put on surgical gloves, and immerse our hands into the evil-smelling fluid that preserved the brain, to pull out, and hold in one hand, what was once the essence of a human being. The thought struck me at the time, that if I got a piece under my fingernail, would that be what someone had loved with, or a memory, or a habit? And I think it was at that very defining moment, that I was hooked on science.

The idea that everything you feel, the very essence of you, and that marvellous inner subjective world that no one else can hack into, all this is generated in some way by the sludgy physical brain. This for me is the most exciting question, not just to scientists, but any human being might ask about themselves. And, of course, there is lot of subsidiary questions: such as 'what makes you, the individual you are?'; 'how you change and learn?'; 'how your genes influence your thoughts, or not'; and 'how thought can influence health'.

Perhaps the contribution that I personally have made most is the idea that these subjective sensations are ultimately tractable to scientific method. The problem has been that scientists hate subjectivity because we deal in things that can be 'measured'. My idea is that consciousness, far from being some magic quality, is actually also something that comes in degrees, and therefore can be measured. For me, this makes sense, since we talk of having greater or lesser consciousness, of raising our consciousness. I think in the future we will start to see ways in which this increase in 'degree' can be monitored in the brain, and correlated: matched up, with subjective inner states.

Although I am not sure if we will eventually turn the 'water' of neuronal activity into the 'wine' of subjective experience; it certainly is a very exciting quest to follow.

SOME PERSONAL REFLECTIONS ON BEING A MATHEMATICIAN

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1. EARLY INFLUENCES

I was drawn to mathematics as a young student in elementary school. I found that I enjoyed arithmetic; it came to me easily, and it was fun. Enjoyment turned to passion when I met my first great teacher. Mrs. Lottie Wilson was my mathematics teacher at Woodward Academy, a secondary school that was then known as the Georgia Military Academy. In those days there were few women in mathematics, but Mrs. Wilson was highly gifted. She was an outstanding teacher, and I am sure that if she'd had the opportunity to go on to graduate school, she would have been a full professor at the university level. She made me realize how satisfying it can be to solve mathematics problems, and she led me to understand the "beauty" of mathematics.

Later I received a partial basketball scholarship to Wake Forest University in North Carolina, which seems surprising when I look back. Although I could shoot pretty well, I wasn't as fast as many other players, and I couldn't jump as high. When I was advised that I wouldn't play a lot, I was asked to tutor some of the other athletes in mathematics. I'll always remember one of them, a huge quarterback named Sonny Jergenson, who went on to be a professional football star with the Washington Redskins. I don't think he learned much mathematics, but I learned that I enjoyed teaching mathematics, which I have been doing ever since.

2. THE NATURE OF MATHEMATICS

One reason mathematics appeals to me is that it is a flexible but precise language. There are many ways to describe the world, ranging from poetry to painting, but mathematical descriptions are unusual because they provide the vocabulary for the scientist's "rules" about how the world works. Mathematicians use this language to describe how a spacecraft moves through gravity, or a protein folds, or water flows around a rock. They may make up a hypothesis for such a phenomenon, and if it seems right, they will try to prove it. A mathematical "proof" is their way of explaining how the world works.

3. THE NATURE OF GEOMETRY

My special interest in mathematics is geometry, which literally means "measuring the earth." Early geometers wanted to measure features of the earth, such as the height of mountains and the width of rivers. They learned ways to do this with angles and distances, without having to climb mountains or cross rivers, so mathematics was respected by people who saw its power. The language of geometry is visual; you can do a lot of geometry just by closing your eyes and using your imagination on complicated questions, such as how many dimensions can matter have, or what happens to space inside a black hole. I like to go sailing, and I often lie on the deck and look up at the sky, thinking about geometry. Some of my best ideas have come while sailing or hiking or just looking at the Delaware River from my home in New Jersey.

4. CONTRIBUTION TO THE FIELD

I was fortunate enough, like other scientists, to "stand on the shoulders of giants" in learning my field. Some of these giants were Niels Henrik Abel from Norway, Don Spencer from the U.S., Emile Picard from France, and S.S. Chern from China. Scientists and mathematicians come from all over the world. My advanced education was largely a matter of understanding what these great mathematicians had done and adding in a small way to their work. When I was a graduate student, there were no good textbooks on the topics I was interested in, so I had to study the original work of the mathematicians themselves. This is a difficult but very rewarding path for someone who wishes to master a subject. Thanks to my early teachers and later collaborators, I was able to make contributions to mathematics in three general areas: complex analysis, algebraic geometry, and differential equations. One of the greatest contributions a mathematician can make is to be a good teacher, and I hope that I am able to do as much for some of my own students as my teachers have done for me.

5. CONTRIBUTION TO THE WORLD

One reason mathematicians take such pleasure in their field is that it has the power to contribute to the world. People who build a good foundation in mathematics can contribute more and more to the other sciences, where it provides the "language" to describe phenomena and analytical tools to give exact answers to complex questions. Mathematics is used every day in fields such as physics, computer science, materials science, epidemiology, and engineering. Mathematicians are also forming new partnerships with biologists to understand how genes and proteins work; with epidemiologists to find out how diseases spread; and with meteorologists to learn more about the ozone hole above Antarctica. Mathematics enriches many fields beyond science; for example, it allows financial experts to calculate risk in the stock market and Internet experts to develop security codes.

The usefulness of mathematics as a fundamental tool is increasingly apparent even to those outside the field. It is much harder for a non-mathematician to grasp what it means for mathematics to be beautiful, and it is unfortunate that so few people have the chance to see the world from this special perspective. Solving a problem is a little bit like making a complicated recipe that comes out just right, and then doing it again and making it even better.

Teaching students to understand mathematics well enough so they can appreciate its inherent beauty has been one of the greatest satisfactions of my professional life.

MORE AND MORE NUMBER THEORY IN TOPOLOGY

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I was born in Hamm (Westfalen) in Germany in October 1927. My father was the director of a secondary school teaching mathematics. Therefore I grew up in a mathematical atmosphere with mathematical books around me and a father who could always answer questions. Very early I enjoyed numbers. I studied the thermometer and discovered that there were numbers "below zero" and that one had the two operations of addition and subtraction. I had the help of my father, in particular to learn also the multiplication. It was great fun. Before I entered elementary school my father told me how to multiply numbers between 10 and 20 with each other:

$$17 \cdot 18 = (17+8) \cdot 10 + 7 \cdot 8 = 306$$

$$(10+a)(10+b) = (10+a+b) \cdot 10 + ab$$

This is simple enough. But it is a useful trick. When the teacher practised mental arithmetic with us, namely multiplying numbers between 10 and 20, my hand was up in the same moment when he shut his mouth. I liked handling numbers in this and other ways. Besides the regular training in elementary and secondary schools, I looked at interesting books. I loved the book "Von Zahlen und Figuren" by Hans Rademacher and Otto Toeplitz, which had been published in 1933. When I began studying this book, perhaps in 1938, I was not aware of the fate of the two authors who had to leave Germany because of the Nazi regime. Toeplitz died in Jerusalem already in 1940. An English edition of the book (translated by Herbert Zuckerman) appeared in 1957 with a preface by the author Hans Rademacher, who lived in Philadelphia, under the title "The Enjoyment of Mathematics". Rademacher and Toeplitz write in the introduction "...our presentation will emphasize not the *facts* as other sciences can disclose them to the outsider but the types of *phenomena*, the *method of proposing problems* and *the method of solving problems*." Their book shows that the beauty of mathematics, like the beauty of music, can be presented by small pieces to be appreciated by beginners or by amateurs. The students in high school should already realize the basic principles of mathematics and see its beauty.

Good teachers are essential. I was happy to have good teachers, my father and books like *The Enjoyment of Mathematics*. Let me mention examples of topics from the books which I especially enjoyed as a young boy and which are accessible to the majority of high school students.

The Sequence of Prime Numbers with the problem of infinitely many prime numbers in certain arithmetic sequences. *Incommensurable Segments and Irrational Numbers*. The old Greeks where shocked when they realized that side and diagonal of a square do not have a common measure. *A Minimum Property of the Pedal Triangle*. This is the solution of the following problem: An acute triangle is given. Inscribe in it a triangle whose perimeter is as small as possible (playing billiards on a table of the shape of an acute triangle). *Pythagorean Numbers and Fermat's Theorem*. *Periodic Decimal Fractions* and the number theory determining the length of the period. These topics and many others make mathematics enjoyable in an easy way besides the regular course work in school. The students can be congratulated who have a teacher who presents to them occasionally such a beautiful gem, to have some recreation from the heavier work of regular training.

My happy life as a young boy and beginning mathematician was interrupted by the war, by bombs and military service, auxiliary and regular. Only until early 1943 my school instruction was relatively normal. I had to join regular military service in March 1945, when I was seventeen. I became a prisoner of war in April and went home in July 1945. I had such an enormous luck compared with others who were in prison camps for many years. Mathematics helped me through these times. In the prison camp we got American emergency rations of dry food. The packages also contained toilet paper and sometimes a pencil. Sitting in a self-made hole in the ground as a small protection against wind and rain (we were always in an open field) I scribbled mathematics on the toilet paper and tried to recall proofs of theorems I loved.

I began to study mathematics in November 1945 at the University of Münster. The city was destroyed. In spite of all difficulties and the awareness of the terrible German crimes *the enjoyment of mathematics* gave me and the other students (who had often returned from the war after many years of service) a strong moral backing.

In 1952 I received an invitation to work at the Institute for Advanced Study in Princeton, New Jersey. From 1952 to 1954 I was a member of the Institute and experienced the perennial discussion with mathematicians from all over the world. In this stimulating surrounding I did my most important work:

I learned the newest methods of topology and the theory of manifolds, in particular the very recent work of René Thom, and applied this to algebraic geometry, to those manifolds which can be given by algebraic equations. I was able to generalize classical theorems on algebraic curves (Riemann-Roch theorem) to algebraic manifolds of higher dimensions. The unexpected relations between different fields of mathematics remained always fascinating for me, also in my later work. Relations between number theory and topology developed. The index theorem of M. F. Atiyah and I. M. Singer is a strong generalization of my Riemann-Roch theorem. It has an "equivariant version" which leads to an interesting study of maps with isolated fixed points. As contributions from such fixed points expressions turned up which were studied before in number theory (for example by my beloved author Rademacher). This led to unexpected relations between number theory and topology. When the opening of a new mathematics building in Princeton was celebrated in 1970 by a symposium “Prospects in Mathematics”, I proposed as a theme “More and more number theory in topology”.

I was happy to see this as an example of the unity of mathematics. The different fields of mathematics interact which each other and with other fields. Research topics like algebraic geometry and index theory are by now a must for theoretical physics.

GROWING UP IN 'SCIENCE'

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Children are naturally inquisitive, and will poke bugs to see how they respond, toss twigs in a stream to see how far they will go before they get stuck, will take apart a toy to see what its pieces are like, wonder where the water disappears to when it goes down the drain. I grew up in a household in which exploration was not just tolerated but encouraged. Activities that I can remember began on the kitchen floor playing with pots and pans, removing all part that could be unscrewed. My father repaired everything—the roof, the radio, plumbing, electrical wiring, tuned the piano, repaired the car, as well as doing the vegetable gardening. As a child I watched him whenever he was doing such things, and he would explain what he thought was wrong and how whatever it was worked. My mother had an ancient Singer sewing machine, and inside its drawer she kept a small screwdriver for adjustments. I was allowed access to the screwdriver, as long as I put it back, and anything that could be assaulted with it was fair game. My mother described to me many years later a visit of the family doctor, who had come to the house to see one of my sisters. He remarked to horror with a stern voice that I had taken apart the phonograph (the old hand-windup kind) and the parts were scattered around the floor of the living room—in short, that I was not being adequately supervised and was misbehaving. Her unastonished reply was 'well, *if* he can't put it back together, his father will'. I still remember the shape of her screwdriver. The other interesting piece of household equipment was the large magnifying glass, useful to examine ants, or make the sun burn a hole in a piece of paper.

A little later, my mother encouraged chemistry in the kitchen. I was given a few test tubes, corks, and children's books describing activities such as how to make hydrogen from zinc (taken from the casing of an old battery) and strong vinegar, or how to shoot a cork across the room using vinegar and baking soda; describing the unusual multiple properties of sulfur as it was heated to melting and beyond, and how to grow single crystals of sugar and salts. The hydrogen was identified by a satisfying 'pop' when ignited with a match. The crystals never came out as gloriously as the pictures in the books; yet one could see the symmetric forms, and wonder about how they came to be. Invisible ink was another surprising feat easily done in the kitchen. While most students first see a color indicator of acidity in chemistry lab, my father showed me that red cabbage was a fine indicator dye, turning blue or red depending on the acidity of its surroundings.

Electrical things began by being given a couple batteries, some wire, and a few light bulbs. The activity I most remember from these is winding wire around a cluster of nails to make an electromagnet, and happily figuring out things to do with it such as making a telegraph to signal from my bedroom to the kitchen.

Tinker toys and an erector set with which to build followed. My ambitions were always too large for my clumsy fingers and the available parts, but how I wanted to build things that would ‘work’, that would do something interesting. Birthday gifts would include simple items like pulleys and rope, a saw, hammer and nails, chosen to help me explore the world of making things.

I wanted a radio. My parents did not want the noise that it would produce. The compromise was that I should build a crystal set, a radio receiver with no vacuum tubes (it was before transistors). I was given a set of ancient headphones and an old bulletin from the United States Department of Agriculture on building crystal sets. The entire parts list consisted of headphones, a crystal of galena (lead sulfide), and wire that you wound into coils on cardboard tubes. Such a set could receive radio stations as far as 75 km away, without the need for batteries. (The bulletin was written in 1930 to bring radio to farms that did not yet have electricity). Wanting something that received more distant stations, I found a design for a single vacuum tube radio, and saved money to buy the vacuum tube. My introduction to electronics was very ‘hands on’, building very simple things, making modifications, seeing what worked. And it was very inexpensive. The one real mystery in the crystal set was how a piece of wire making contact with a crystal of galena resulted in rectifying the radio signal to yield the audio sound. I did not understand that until I was a graduate student in physics 12 years later.

A bicycle presented new opportunities. Spokes would break, or the coaster brake would go out of adjustment, and I would take things apart. Rescue by my father, and perhaps a trip with him to the repair shop, would often follow. The trip to the repair shop was not to get a repair done—that was too expensive—but to find out how to do it and to buy necessary parts.

I took up building model airplanes from kits. The early ones were rubber-band powered. Later ones were powered with a small gasoline engine, from which I learned much of later use when I first had an unreliable automobile. I read a little about science in magazines, and an occasional book about astronomy or but more than anything else, devoured what I could find on how the useful inventions of the everyday world work.

Science at school was dreadfully taught. Before I was 12, there was no science at all. In beginning science, my teachers emphasized memorizing the names of things, not doing things, not understanding. My grades in these classes were terrible. I had two good science teachers. One was a biology teacher who emphasized *organizing* facts, not memorizing them, and seeing the relationships between living things. It was my first experience with science in the field, sciences of observation. The other was a high school chemistry teacher who treated teenagers as adults, taught by giving real lectures, and gave a laboratory in which I did experiments of sophistication that I could only eagerly read about when younger. Suddenly I was the best student in the class.

Physics is an exploration of what is not understood about the way things are, in search of essential principles, facts, and quantitative description. Some fall in love with the mysteries of the origin of the universe, or the nature of the world at distance scales that are unbelievably small. For me, having been brought up curious about the world around me, and fascinated to understand and manipulate it, the most interesting physics involves the properties of things at the human scale, and how these are related to underlying more microscopic structure and properties.

From this background, it is obvious that my university studies would ultimately lead to condensed matter physics. The first ten years of research was on the interaction of light with crystalline solids, and how this was related to the electronic structure of solids and the quantum aspects of light. It was a marvellous era, for there were places which yet lacked zero-order understanding. Experiments were coming along at a great rate so that theory was quickly tested. It was, in hindsight, also an excellent training ground for acquiring a wide vocabulary of mathematical models of general use.

As the understandings of solids grew, I turned toward biological systems, where zero-order explanations in physical terms were chiefly not yet known, and where quantitative experimental facts of the sort that physics is built on were slowly being accumulated. The nature of my contributions was unusual, for I tended to ask a different kind of question. Indeed, while I am now chiefly known for my contributions to theoretical biological physics, the nature of the most significant contributions has *not* been mathematically profound. I have managed only to identify simple problems, to state them clearly, and to describe their solution in a way that makes them understandable and amenable to a physics investigation.

My most cited paper is the first I ever wrote on how the brain works. It links a known physics topic—magnetism and the spin glass—to the psychological phenomenon of associative memory, by way of a physics-type abstraction of the behavior of a network of interconnected nerve cells. It introduced the idea of computation in neurobiology as being carried out by the dynamical trajectory of a system with many degrees of freedom moving toward a (temporary) fixed point of its dynamics. Known now as the 'Hopfield model', this insight led many physicists into neurobiology by illustrating how close the questions of neurobiology could be to questions in physics, and how useful a physics-type modeling approach could be in neurobiology. It took more than two years of attending meetings and seminars on neurobiology to enable me to find that problem. My most cited paper in molecular biology described 'kinetic proofreading' (a general method of 'proofreading' at the molecular level) and was also the first I wrote having anything at all to do with tRNA or protein synthesis. Again, it was a matter of posing the right question. A biologist would ask 'how does the desired reaction happen?' I found a new principle by asking instead 'why does the undesired reaction not take place, when it is so similar to the desired one?'

My present enthusiasm in science might be described as 'how do we think?' It is the type of question I have always pursued, though with age the questions have gotten harder. Is it biology or physics? It doesn't matter. Perhaps physics is best defined simply as 'what those trained in physics do'.

A LIFE OF SCIENCE AND SOME POLITICS

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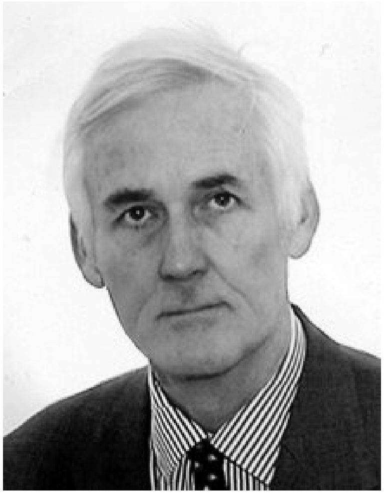

Scientists keep asking questions and, as the Roman poet Ovid so clearly expressed it, are happiest when they find some answers. They are particularly pleased if the explanation or theory answers several questions at the same time and if it has practical uses. Being the first to find an answer is what drives some scientists, but most of us do not see science as some kind of race. Rather, as Isaac Newton nicely put it after his theories had answered many questions about the movement of planets, the challenge is to stand on the shoulders of the earlier generations of scientists and try to see a little further into the distance.

I learnt about the value of questioning when in 1953 I and my brother stayed on holiday with my great uncle, the famous mathematician and scientist Lewis Fry Richardson and his wife Dorothea. After going to the village store to buy some paint, where we bought two different types, we came home; it was placed in a bowl and he poured various chemicals onto the paint, including sulphuric acid from his little chemical laboratory. The result was that one paint sample turned brown and filled with house with a horrendous smell, while the other did nothing. So we bought the latter! Richardson whose house overlooked a large Scottish loch was thinking scientifically every moment of the day. He described how he had recently been studying how floating parsnips spread apart by the action of turbulence eddies in the water just as balloons also spread apart in the atmosphere.

Scientific questioning and the experimental approach can also be very effective when they are applied to human behaviour and every sphere of life, as I have found in my career in science, administration and politics. I learnt from Richardson to be sceptical. He did not believe the history book explanations of great leaders causing wars, but showed quite accurately how they arise from the objective trends of how armaments build up and that their statistics follow certain general laws. This is still a highly controversial view of the world!

My own scientific interests were in mechanics and fluid motions, especially wind, eddies and waves. I was also stimulated by my grandfather Maxwell Garnett, a Fellow of Trinity College Cambridge, and a distinguished applied mathematician and administrator. As a boy I enjoyed planning layouts of my electric trains, constructing Meccano gear boxes, and building a radio crystal set in a cigar box. But it was sailing off Britain's south coast in rough waters in my grandfather's open dinghy, even in the snow on one occasion, that really interested me in how physics relates to the natural world. At high school (Westminster) my friends and I devised our own experiment to test the theory of why one turn of a rope around a bollard on a dockside or a cleat on a boat is sufficient to hold a huge tension. The mathematical formula for the ratio of the tensions proportional to $e^{2\mu\pi}$ proved to be quite accurate where μ is the coefficient of friction, one of the experiments that helped convince me that science was satisfying and useful.

In my engineering studies at university, where I specialised in fluid mechanics and thermodynamics, I became fascinated by research into new sources of power generated by heating gases so that they became electrically conducting and then propelling them (near the speed of sound) through a magnetic field. This involved magnetohydrodynamics, an exciting new subject that developed in the 1940’s for studying magnetic fields in the interior of the earth and in outer space.

Using the same astrophysical equations we found how magnetohydrodynamic flows in the laboratory or in power stations could move in unexpected ways, for example forming jets along the side of the pipe or even reversing. Then, as so often in science and technology, research problems lose their urgency while others become critical. Although MHD power and thermonuclear fusion, which also is based on MHD processes, continue to be of research interest, by the end of the 1960’s these power sources were not living up to their early promise of practical benefits.

I became more engaged in the scientific and practical problems of the environment, stimulated by photographs of our ‘blue planet’ taken from the Moon and the dangers of global chemical pollution and acid rain becoming of critical importance. Fluid mechanics research was able to make significant contributions, particularly as part of broad teams where other disciplines were involved such as chemistry, meteorology, geophysics and engineering. The solutions to many environmental problems first requires analysing processes affected by how the wind changes as it impacts on buildings, hills, sand dunes, mountains, forests, etc. The distortion of the turbulent eddies affects how particles, gases and heat in the atmosphere are diffused upwards and downwards towards the ground. This determines the levels of concentrations of smoke, dust, sand, pollen and the temperature. Sometimes adverse environmental conditions can be controlled, for example by reducing pollution or designing urban areas more sustainably, leading to healthier living conditions. I have been involved in many practical studies but often they make use of advanced mathematical research, for example the topology of flow patterns. I have found how mathematical thinking is also essential to understand the results produced by very large computers which can now simulate atmospheric and oceanic motions, ranging in scale from the smallest eddies at 1cm to the largest at 1km. Research has shown how the statistics of the turbulence (e.g. those which define the energy of the different scales of motion) have distinct features in similar environmental (and engineering) flows. These statistics are related to the form of the eddies (e.g. whether they are like vortices or jets) and to how they are positioned in relation to each other.

One could summarise this story in verse by building on the famous lines written by L.F. Richardson in the 1920’s. He focussed on the basic mechanics of eddy motion in turbulence. The new verse describes the new ideas about turbulence in complex flows.

Lewis Fry Richardson:

Great whirls have little whirls that feed on their velocity,
And little whirls have lesser whirls, and so on to viscosity.

Julian Hunt:

These eddying motions proceed like a symphony,
but hitting an object - then what a cacophony.
Large eddies impact, smaller ones stretch,
in water or air their dynamics they etch;
With a streak, or a swirl or sometimes a horse shoe,
the statistics transform and new patterns ensue!

TALENT MAY NOT ALWAYS BE EVIDENT EARLY

Daniel D. Joseph
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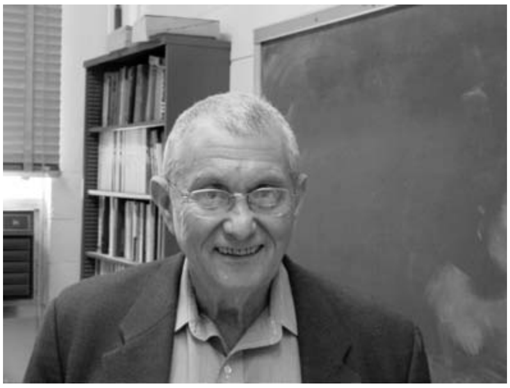

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I have had a very satisfying life of creativity in science and engineering. This could not have been predicted from my early life. Perhaps I had a talent for innovation which was not evident before the fact. This is a lesson which may be expressed by the idea that you should not discount your abilities before you know what they really are.

My father had a jewelry business in Chicago. He had fled to the United States in a wave of immigration from Russia. No one in his family or my mother's family attended University or was otherwise involved in learning. I was not expected or particularly encouraged to go to college. My father expected me to go into his business. I was an average student in high school and had no particular idea of what to do with my life. My community and school mates were largely second and third generation Jews who introduced me to a respect for learning absent in my immediate family. Among my friends were some intellectually gifted boys who greatly influenced my life.

After graduation from high school I enrolled in Roosevelt College in downtown Chicago. Many of my friends went to the University of Chicago, which I did only two years later. The University of Chicago was and is a great University which then, in the late 1940's, was organized in an unconventional way with a highly intellectual two year college followed by the divisions, equivalent to an accelerated graduate school. I enrolled in the division of social sciences with a specialty in Sociology.

I was a student there 1948-50. I lived in an apartment with five other boys: some of them went on to distinguished careers in social service, sociology and law. These were golden years for physics at Chicago, a controlled thermo nuclear reaction had first been achieved there during the war years and the faculty was populated with the greatest names in physics at the time. One of my roommates was enrolled in the physics program; we called him "Benny the shake". He lost his class notes in the supermarket just before the big exam and did not pass. I can't say that he failed because he lost his notes; graduate work in physics in those years was fiercely competitive and many students did not make it. Probably, if I had been enrolled in the physics program at that time, I would not have a scientific career now.

My record as a sociology student in Chicago was decidedly mediocre; the University forgot about me for forty years which is why in 1999 I had such an intense pleasure to receive the "Professional Achievement Citation of the University of Chicago" for my work in fluid mechanics.

The intellectual climate at Chicago in those years was vibrant, full of debate and discussion about society, politics and life. Some of us got very interested in Marxist politics, an interest which controlled my life some years after. After getting my Masters degree in Sociology, my wife and another married couple went to France; we went to communist rallies, had study groups and learned some French. This trip was financed partly from a book sale. We had bought a lot of books at an inventory liquidation at the book store and got some very good books at low prices; one of these "The Trobriand Islands" by Malinowski is an anthropological classic and fetched a good price but the real treasure were a few copies of the "Revolution Betrayed" by Leon Trotsky; Trotskyists would pay anything for this out of print classic.

We returned to the US in 1951, in the years of Joe McCarthy, with the idea of radicalizing the workers. After a period in New York where I worked in a factory, we moved to Berkeley California were I had two jobs. One was in an awful factory and after that in a machine shop as an apprentice machinist. In those days I drove a small Studebaker that got many miles per gallon but had a hard time with Berkeley hills. My coworkers drove big cars with tail fins. The whole adventure seems ridiculous now but would not be so ridiculous in a less forgiving country. For some years I became increasingly uncomfortable with the radical movement and I was greatly relieved when I quit at the time of the Kruschev speech revealing the crimes of Stalin.

My evolution to engineering and science has roots in my years as an apprentice machinist, which I loved. I liked making relatively complicated things with precision. The machinists were a good bunch. We had an apprentice school in a local high school and I was recognized as a star for my ability to do simple calculations fast and with accuracy. This was in fact the first indication I had that I might be able to follow a career in engineering. After the Kruschev speech I enrolled in a program of Mechanical Engineering at Illinois Institute of Technology in Chicago. I had no longer any interest in Sociology and I needed to make a living; I had no thought to follow an academic career.

The years in the late 1950's were "sputnik" years with lots of money available to catch up with the Russians. I was very highly motivated in my studies because I was older and had family responsibilities. I did very well, achieving nearly a perfect record and financial support through a generous fellowship. Now I was in a track which would lead me inevitably to a scientific career. In my IIT days I studied with very able professors with credentials in applied mathematics and fluid mechanics. I have to say that at this time and for years afterward I had grave doubts about my abilities in these subjects. My professors wanted me to pursue a graduate degree in applied math at Brown University but I stayed at IIT. I got my Ph.D. in 1962 and in 1963 I took a job in the department of mechanics at the University of Minnesota where I have been ever since.

My science life can be divided into distinct periods. In the first period I focused on stability theory and wrote many papers on applied mathematical topics. I worked for six years on my book on "Stability of Fluid Motions." This book taught me that when you write a book it is a good idea to have all the required research and not to try to create it on the run. I did a lot of the work on this book in 1969 while on sabbatical at the University of London. My kids said that Daddy wrote in the day and erased at night. My stability book has been well received even up to today. Not all of my works in this period were theoretical. With Gordon Beavers I wrote a paper on "Boundary conditions at a naturally permeable wall" which is an experimental paper building on my Ph.D. thesis. This paper spawned many new papers which exploited the new boundary condition including the next to last paper of the famous scientist G.I. Taylor.

My next period was more decidedly mathematical. I wrote papers on the mathematical theory of bifurcations, on a rigorous formulation of theory of perturbations of the domain of solutions of partial differential equations with applications to free surface problems in fluid mechanics, on the completeness and convergence of biorthogonal series, on the number of solutions of the nonlinear diffusion equation driven by positive sources, on the theory of heat waves and other topics. In 1980 I published a book with G. Iooss on "Elementary Stability and Bifurcation Theory" which is not so elementary but is more elementary than its competitors. It was a lot of fun to write this book and to introduce the subject in the simplest possible frame as the bifurcation and stability of plane curves.

During this period I developed an interest in the flows of viscoelastic fluids. These fluids are very interesting because they give rise to motions which defy intuition. When a rod is rotated in an ordinary fluid the fluid near rod sinks as it does in a bathtub vortex but a viscoelastic fluid will climb up this rotating rod and the height of the climb can be really dramatic. A contrarian point of view is appropriate in this subject; to inform intuition you should expect a behavior exactly opposite to what is expected from simpler fluids like water and glycerin. Viscoelastic fluids differ from others because the relation between stress and deformation is not linear and the present state of stress depends on the history of deformation. I worked on the mathematical theory of these fluids solving some important problems like the rod climbing problem and others where the nonlinear stress is important. I identified a mathematical consequence of viscoelasticity and memory as one that gives rise to decaying waves and I invented a meter to measure the speed of the viscoelastic waves and to deduce their times of relaxation. This work is summarized in my book on the "Fluid Dynamics of Viscoelastic Fluids". The citation for my election to the National Academy of Sciences in 1990 reads "… Joseph has set the mathematical foundations of viscoelastic fluids. His theoretical framework and elegant experiments have stimulated a generation of workers".

It should not be thought that a life in science is pleasure straight up; your colleagues are an aggressive group and the fight for recognition is fierce. It might be argued that the peer review system which works so well in science does so precisely because your colleagues do not wish you to succeed. In the early days I had a great mentor, later tormentor, Clifford Truesdell who was widely thought to be the arbiter of good taste in the field of continuum mechanics. First he wrote that I was one of the great minds of time and then, in the 1980's when my interests drifted more strongly to experiments and applications he changed his mind. It was deeply troubling because in a sense I had been demoted by the very peer group, led by Truesdell, that had supported me in the past. Life is tricky, to quote the Rolling Stones "sometimes you don't get what you want but you get what you need". It was after this that I was elected to all the academies and was awarded many medals from professional societies.

One project which we studied in the late 1980's and 1990's was lubricated and self lubricated pipelines. This is a very practical subject with important applications. The idea is that if you have water and viscous oil in a pipeline, the oil will go to the wall and lubricate the flow. This leads to huge savings in pumping power. The downside is that you might get continued buildup of oil on the wall leading to blockage. First we did many stability studies to show that the lubrication situation with water on the wall is the stable one. Then we did many experiments which were in pretty good agreement with the stability calculations. In the early 1990's we were contacted by Syncrude Canada who wanted us to evaluate water lubrication for transporting bitumen froth against two competitors, heating the froth and dilution with Naptha. The bitumen froth is a very viscous and stable emulsion of water in oil. They get this oil by mining it at the tar sands, then taking away most of the sand with a hot water process that leaves water in the oil. The froth is too viscous to pump. The water in the emulsion is produced from the tar sands; it is loaded with clay in the form of colloidal nano-particle clay platelets which give the water a milky color when it flows. As luck would have it, the emulsion breaks when sheared at the wall and forms a lubrication layer; we call it self lubrication because water does not need to be added. Now this clay water is great; it sticks on the oil and prevents the oil from sticking to itself, like powdering the dough. We did experiments for Syncrude on this, looking for show stoppers, and none were found. We found a scale up rule which allowed Syncrude to design a cost effective pipeline with a 36 inch diameter that we could not test. They run this pipeline today at a cost of six to ten times greater than what it would cost to transport the water alone, a huge saving.

Recently we have been working on the direct numerical simulation of the motions of particles in liquids. We developed powerful codes which allow us to move particles flowing in slurries, fluidized beds etc directly without models. Numerical simulations are like experiments; they are numerical experiments and we treat these experiments like real experiments processing data for correlations. The correlation method is the way to turn huge amounts of data into useful formulas, from data to formulas, without models. This is a great way to tie numerical simulation to engineering practice.

Nowadays I have become interested in a very theoretical topic called viscous and viscoelastic potential flow. The subject of potential flow is centuries old but everyone put the viscosity of the fluid to zero; they called it inviscid potential flow. If you write "potential flow" in search at Google you will get over eight million hits. All of them, except the 20 or so papers we wrote, have no viscosity. There is no fluid with a zero viscosity, there is no reason to put the viscosity to zero and it is not useful to do it.

I have had many wonderful students in my 40 years of research life. I love these students; whatever we created could not have been done without them. I think that they love me also and we form an academic family tied together by mutual respect and the joys of discovery. It is a great life.

Scientific Truth

Leo P. Kadanoff
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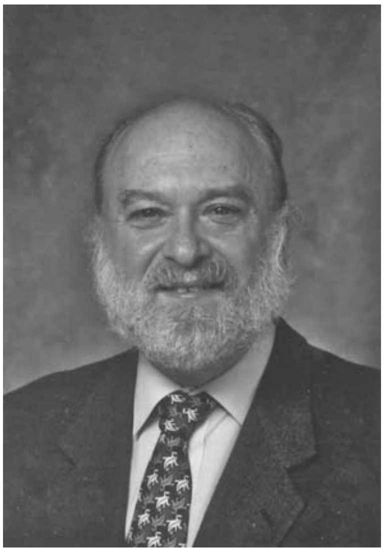

When I was a young man, I was first drawn to mathematics and then physics by its possibility for finding out and describing true things. In contrast to the confusion and complexity of my adolescent world, statements like "for a right triangle, c squared equals a squared plus b squared" are verifiably true. In addition, one can determine whether this statement is also true for other kinds of triangles. I found this certainty attractive, and in some sense amazing.

Later on, I became even more moved by the possibilities of finding truth about the world by using physics. From Schrödinger's equation one can say true things about the hydrogen atom. The possibility of finding something indisputably real, like an emission line from hydrogen, that is also within the possibility of accurate human prediction, attracted my imagination. I dreamed of saying things which were both true and new. In an adolescent fantasy, I would discover some hidden behavior of the world which would be my very own.

Now it is many years later, and I am not entirely an adolescent any more. I do feel that my life in science has indeed fulfilled some of my youthful fantasies. So in many ways, I am pleased and satisfied by my professional life, both in past elements and in its continuation to this day. However, I have also acquired a broader perspective. I do realize that science's ability to find and publish truth is obtained at the expense of a focus upon the less important things in life. We are better at finding true things than knowing the nature of love, justice, humanity or indeed Truth. So I have become more modest in my hopes for what portion of the world can be encompassed by science. But I remain steadfastly tied to my original view that the value of science is in its possibility to discover and state things which have a considerable content of verifiable correctness.

In doing that science might perhaps serve as an example to other parts of life. Our world suffers from an abundance of falsehoods, as in classifying a whole group of people as evil, or in listing a played out oil-field as productive, or in treating a parochial political view as universal, or in describing management theft as "protecting the interests of stockholders". One major benefit that might be provided by science and scientists is to serve as an example of an area in which such falsehoods are neither prevalent nor rewarded.

Alas, it is not so. Our scandals are comparable to those in other walks of life. When we get wildly optimistic about cold fusion, or about hot fusion, or find a need for developing a technology for shooting down asteroids, or argue for practical benefits from huge investments in impractical parts of science, then we are behaving in the same self-serving fashion as the community around us. So long as we make extensive use of unbelievable statements to raise money for science, we cannot justly complain that the United States government is misusing scientific facts to support its own interests. So long as we minimize the management failures which put the names of Batlogg, Bell Laboratories, and Lucent on the fraudulent work of Schon, we cannot claim that our world is managed better than say, the world of corporate accounting. And if we scientists don’t represent the truth, who will?

FORAYS INTO THE WORLD OF ASTRONOMY, TECHNOLOGY AND SPACE

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© Courtesy of Krishnaswamy Kasturirangan

My fascination for Astronomy could be traced to my childhood in the small township of Ernakulam in the Southern Indian State of Kerala in early forties. With practically no background effects of city lights, the nights in those years were virtually lit up by the star studded sky in its fullest grandeur, creating in my mind, a sense of awe and wonder. I still fervently recall the beautiful sight of the milky way that strung like a necklace across the night skies of Ernakulam, a spectacle difficult to experience these days in any urban site. This in turn kindled my imagination as to what it all means. By the time I left Ernakulam for high school education in the city of Mumbai, the urge to understand the secrets of the night sky had become a deep-rooted passion. My high school education and early college education led me to develop interest in physics and mathematics. To me learning physics was the sheer delight of understanding the things that you see and experience through the principles that govern them. Similarly, mathematics to me was a beautifully conceived game with its own set of rules; never a dull moment when it came to problem solving. I continued this interest by taking a Masters degree in Physics from Bombay University. But my interest in Astronomy never dwindled, all these years and if at all, only strengthened with better understanding of Physics.

Around this time, I came to know of an opportunity to work with Dr. Vikram Sarabhai, a well known Cosmic Ray Physicist who was creating initiatives in Astronomy at the Physical Research Laboratory in Ahmedabad well known for research in cosmic rays and atmospheric physics. When I communicated my interest to do research in Astronomy, at PRL, he promptly invited me for a discussion. I vividly recall the way Dr. Sarabhai went on to assess my interests. At the academic level, his questions covered both basic and advanced physics, many of them straightforward, but some intimidating—in an academic sense. He further tested my determination to do research by explaining the demands of rigor and hard work in a research career, the meager financial remuneration and above all the uncertainties of a research problem leading to Ph.D. I made convincing defence of my interests and my conviction to pursue research. He then outlined his own ambitious vision of making India a major space-faring Nation. Probably, he was indicating that he sees my role in a longer perspective. He agreed that I work in the newly emerging area of x-ray astronomy, hardly a year old since the discovery of the first x-ray star in the constellation ‘Scorpious’.

Dr. Sarabhai's way of initiating me to a research career and to shape me as an experimental physicist was rather unique. Through such diverse jobs as sheet metal cutting, making chassis for power systems, designing of simple power supplies, glass blowing, design of vacuum systems for preparation of Geiger Muller counters, design of electronic circuits and building finally balloon borne payloads for cosmic radiation detection, he made me get handsdown experience on a variety of techniques. For nearly two years, I spent learning these techniques. Also, I was made to make an indepth study of several aspects of physics right from nature of particle and electromagnetic radiation propagation and interaction in the atmosphere and in interstellar medium, charged particle motion in different configuration of magnetic fields, nucleo synthesis in stellar explosions, theory of gas discharges, radiative transfer in stellar atmospheres, production of x-rays under extreme conditions of temperature, density and magnetic fields and so on. In retrospect, I feel it was an extraordinary two years, the like of which I never experienced in the subsequent three decades of my academic and professional career. In the next two to three years, developing an x-ray telescope to measure diffuse cosmic x-ray spectrum and conducting one of the first few earliest measurements in the medium energy domain with balloons was a source of great excitement and satisfaction. The scientific importance of this study is that, the diffuse cosmic x-ray radiation is conjectured to originate from the superposition of radiation from a number of discrete sources. Another possible origin to it could be through non-thermal radiation resulting from collision of relativistic electrons with the primordial radiation from original big bang and thus of crucial importance to cosmology.

In those years, we graduate students working in astronomy, atmospheric physics and cosmic rays often discussed each other's research and the related new developments elsewhere. During one such interaction, I raised the possibility of detecting the effect of the newly discovered x-ray source Scox-1, through its ionization effects in the ionspheric D-region of the atmosphere in the altitude range of 50-80 km. This was based on the simple back of the envelope calculations of the relative energetics of the solar x-rays and that of Scox-1 in the earth's vicinity. Sun on the average emits x-rays, in the energy domain 2-10 kev, which is around 10^{-4} ergs per cm^2/sec. The resulting ionization in the lower D-region was monitored at Physical Research Laboratory by recording the field strength of 164 KHz transmission from Tashkant to Ahmedabad in a single hop reflection mode. One could estimate the level of ionization from the recorded field strength. Its continuous monitoring enabled study of the Solar x-ray variability as a function of Solar activity. Since x-rays from the newly discovered extrasolar object Scox-1 is equivalent to 10^{-6}erg/cm^2 sec. at D-region, the question I posed was whether in the night, during the season when Scox-1 transits over the region of reflection of the 164 KHz, one could detect its effects. The day time effects of Scox-1 will be swamped by the two orders of magnitude higher solar x-rays. Lo and behold! Search of records revealed the clear presence of its effect. Its unambiguous origin was doubly confirmed by the existence of the sidereal effect. This turned out to be a truly remarkable discovery considering that until that moment, no known extra-solar effect was seen in the earth's atmosphere whose influence could be detected directly. Soon many picked up this announcement and confirmed the discovery. In fact one group saw in this effect an explanation for the long standing puzzle of interference in the Loran radio navigation system. To me it was clearly an excellent example of the power of interdisciplinary research, and the ambience PRL provided for such an intellectual pursuit.

It was again Dr. Sarabhai who initiated me to the world of space technology, especially satellite technology, by providing an impeccable rationale. He argued that my background in Physics, familiarity with experimental techniques as well as my penchant for looking for linkages between different themes, could all add up towards creating an exceptional systems specialist. This aspect was not very well appreciated in early seventies, but whose value became evident when complex multidisciplinary systems had to be realized in the later years.

Here too, he proved truly prophetic! My grooming as a 'system specialist' made me play a very special role in the design and development of India's first Satellite "Aryabhata". The unique leadership that I gave for building the first two experimental earth observation satellites "Bhaskara I and II" and again directing the team that built the first of the world class series of Indian Remote Sensing Satellites IRS could be attributed, even more, to this background.

An interesting example of the linkage between science, technology and management, in resolving a problem we encountered in Space, can serve to illustrate the value of system specialization. When we flew a Television camera for imaging the earth, onboard Bhaskara-I, we encountered a serious problem of electrical disturbances in the satellite when this instrument was switched on after keeping it in storage for a few days in orbit. Simulation on the ground, involving testing a similar camera system in vacuum chamber and looking for such effects, quickly led us to the conclusion that there could be an electrical breakdown within some trapped gaseous medium between a high voltage point and ground plane. Further, the problem was seen only after the payload was in vacuum chamber for a few days, pointing to the possibility that the trapped gas could be slowly leaking, taking it to a pressure regime where such discharge could occur. We embarked on detailed experiments to understand the characteristics of gas discharge within the applicable voltage ranges and over a wide dynamic range of gas pressure. Through this experiment, we could identify precisely the pressure domain in which such discharges could occur. Interestingly, we also observed that the discharge ceased to be a problem when the pressure reached below a certain point. These off-line laboratory experiments on gas discharge, seen together with the analysis of the anomalous behaviour of the payload in vacuum, led us to some interesting projections about the evolution of this anomaly in orbit in the future. The most interesting outcome was that we could roughly estimate the rate of venting of the trapped gas over the next few months in orbit that could take its pressure to a level where discharge could cease as demonstrated in the laboratory experiments. This was strikingly proven, to our excitement, when the payload was switched on successfully returning beautiful images of earth, after six months of keeping the instrument dormant in Space. This entire episode is an excellent example of managing a team working in multi-disciplinary mode. Solution to the problem called for evolving suitable simulation techniques with high dose of input from physics, understanding the technology of fabrication, problems of material incompatibility in thermo-vacuum conditions and above all lots and lots of "scientifically inspired commonsense".

The same systemic knowledge coupled with the culture of rigour of scientific analysis was brought to bear on a larger scale, in leading the Indian Space programme during the last ten years of my career. This period saw India repeatedly demonstrating its unique status as a vibrant space faring Nation among a handful of five or six nations. I had the satisfaction of over-seeing a space program, where science, applications and technology went hand in hand to carve out a unique niche for space in the context of national development, an objective which made Indian program truly one of its kind anywhere in the world.

Even in those hectic years of realizing space systems, I kept myself abreast of the developments in various aspects of Astronomy. In one such instance, I stumbled on a novel paper in Nature by Phillip Morrison and his colleagues at MIT, USA regarding the theoretical possibility of a permanent particulate ring structure around the Sun based on evolutionary, physical and chemical considerations. Taking advantage of the solar eclipse of 16^{th} February, 1980 and using a scanning infra-red photometer which we quickly rigged up, a search was made for its presence up to about 5 solar radius. Even though there was no positive evidence of such a particulate ring, this interesting experiment nevertheless led us to put specific constraints on the model of MIT group. Maintaining a certain level of interest in research topics, for me was a matter of intellectual imperative. At the same time such an attitude helped me to continuously rejuvenate the faculty of incisive thinking and in-depth analysis, a crucial need for an effective leadership.

No space program can succeed without the teamwork of a large body of dedicated and professionally competent scientists, technologists and engineers. When it comes to effectively leading such a team, the bottom line is that the top leader has to command the professional respect of the entire team. This may sound a tall order, but not impossible to achieve.

It is here, that I feel in retrospect, that the strategy of Dr. Sarabhai in moulding individuals early in their career has its true significance. Strong orientation in basic and fundamental research in the early phase of one's career, gives a special ability to think differently and be innovative. Realizing the necessary insights to understand a complex system, and to see the not so obvious linkages between its different elements, can be considerably enhanced, in my view, by strong research grounding in fundamental sciences and mathematics. It is interesting to note, that even innovative styles of management could be traced to such creative minds. Further, wetting one’s own hand in experimentation leads you to a remarkable level of confidence in independently assessing complex technical issues, review the same, and questioning other’s judgements, an imperative for being professionally respected by your colleagues besides making the system more accountable.

At this juncture, when we often lament the diminishing interest in basic sciences, it is not only the basic research, that is in danger, but also dwindling high caliber science and technology leaders. Without science leaders of vision, who can influence the course of science research and technology developments both at policy and implementation levels, the role of science and technology for socio-economic development can be seriously jeopardized. Needless to emphasise, this is of particular concern for the Third World.

SCIENTIFIC RESEARCH IS A TOKEN OF HUMANKIND'S SURVIVAL

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If you are so clever, why are you so poor?
/Popular expression/

Why is it that some of us still decide to become scientists, despite the fact that businessmen, lawyers, and doctors enjoy a much higher income?

A famous Russian writer L. Tolstoy once wrote that a writer is not merely a person who writes; a writer is a person who cannot live without writing. The same, I believe, is true for a scientist. Science is an exciting adventure where major reward comes from the discovery itself. What you get instead of big money is freedom, camaraderie, independence. The honours and promotions will depend on yourself more than in the other occupations. And you will have the overwhelming feeling of uncovering yet another one of nature's mysteries.

An instant understanding, the efficiency of thought and action, and a good feeling that comes when the like-minded people work together…
/F. Press/.

It was 1960, the height of the cold war. I was in Moscow doing research on the theory of seismic waves—tremors in the earth generated by an earthquake. I was absorbed in my problem; I enjoyed the mathematical challenge. I did not give much thought to how it connects with the real life.

The summons came from the President of the Russian (then Soviet) Academy of Sciences. He had received a message from the Palace of Nations in Geneva, where technical experts from the Soviet Union, United States, and United Kingdom—three powers possessing nuclear weapons—met behind closed doors. The President showed me a letter from Geneva: an American scientist, Frank Press, quoted my work while arguing with Moscow experts. And, to my great surprise, I found myself in Geneva.

At that time every man, woman, and child on the Earth lived under the threat of annihilation by nuclear weapon. Each superpower had more than enough nuclear bombs to destroy the others in the first strike. But in the 20 minutes it took for the rockets to reach their targets, the other side had plenty of time to launch retaliation strikes ensuring the destruction of their enemies only a few minutes later. This threat of "Mutually Assured Destruction", with ominous acronym "MAD", was for some years the only thin thread protecting all of us from the common fatal fate. Continuing nuclear tests meant development of even stronger bombs, introducing even more imbalance into the global nuclear standoff. The three nuclear powers were willing to come to an agreement, putting a ban on the nuclear weapon test. And hence, the technical experts were summoned to solve the problem that arose.

In formal terms, the problem was the following. *Suppose that:* (i) the nuclear powers had signed agreement to stop the test of the new nuclear weapons, and (ii) one of the participants had violated this agreement and secretly made an underground nuclear explosion. *The problem is*: how can the other powers detect the violation?

It turned out that this problem had a direct connection with the theory of seismic waves. Underground nuclear explosions produced earth tremors very similar to those generated by earthquakes. How could one distinguish the natural tremors from the ones produced by the explosion? Suddenly, my theoretical knowledge had a direct application in the area of survival of the humankind.

In the atmosphere of the ongoing Cold War, with political tensions hanging over our heads, scientists and engineers from the opposite sides of the Iron Curtain had to find a solution, which outwardly seemed impossible, given all our differences in cultural background. What saved us was a clear and obvious distinction. We were all scientists. We were able to work out a common language, based on respect to hard evidence, undisputable ranking by expertise only, and persistent self-criticism. We were able to work out a solution that eventually allowed politicians to reach one of the most important decisions of their times: nuclear test ban.

This episode taught me that as a scientist I have people all over the world who think and interact the way I do. It taught me never to feel lonely abroad. And, above all, it taught me that while there is science, there is hope of survival and well-being for all of us.

"Were Napoleon as wise as Spinoza, he would spend his life at the attic writing four volumes of essays"

/A. France/.

A common lore is that immersion in science does not go with practical sense. It is true that on occasions a greatest mathematician of out time was so involved in a current problem that he could leave home wearing shoes from two different pairs. However, if you look carefully, you will find another side to this, which makes scientists the most practical people in the world. All new technologies, all new brands of industry from defence to entertainment stem from fundamental research. Among past examples are antibiotics, electronics, biotechnology, synthetic fibres, the green revolution, and genetic forensic diagnosis, to name just a few. And now only the basic research could give us new sources of energy; new mineral deposits; efficient defence from terrorism; cure from cancer; new forms of transportation. People trained in theoretical physics are headhunted by financial institutions; those trained in frontiers of biological research become founders and directors in the pharmaceutical industry. So, knowledge of basic science will give you a head start in whatever career you choose.

/These dangers/ "are a threat to civilization's survival, as great as ever posed by Hitler, Stalin, or the atom bomb"

/J. Wiesner/.

It is commonly recognised, that the very survival of our civilisation is threatened by natural and man-made disasters. Among them are earthquakes, self-inflicted destruction of megacities, environmental catastrophes, economic and social crises. Today, a massive release of radioactivity from a nuclear waste disposal, an earthquake in the middle of a megalopolis, an outburst of mass violence, or any other global disaster, can cause up to a million of casualties, render large part of our world inhabitable, trigger global economic depression, or a war in a "hot" region. Such dangers keep growing, although trillions dollars a year are spent to contain them by all known techniques.

The hope and the responsibility for breaking the stalemate rest not on the money but on intellectual resources, though the money is more popular, according to the French proverb: "Nobody is satisfied with his wealth, everybody is satisfied with his wisdom." Only the basic research can create a springboard for developing new disaster preparedness industry.

"Ours is the time of contest over issues not completely understood"

/McGeorge Bundy/

Scientific research in an exciting venture into the great unknown and the token of humankind's survival. It is the scientists with their tools that are up to the challenge and can ensure that we all safely move with the time.

Finally I have to remind you that the science is not the beginning and the end. More important for the humankind and for each individual are the human qualities. However, if humanly used, science is their indispensable guardian and caretaker.

MY ENJOYMENT OF SCIENCE

Joseph B. Keller
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© Courtesy of Joseph B. Keller

Our father sometimes gave my brother and me puzzles to solve. Here is an example: A goose met a flock of geese and said "Hello 100 geese". The leader of the flock said "We are not 100 geese. But if we were twice as many as we are, plus half of that, and you, we would be 100." How many were there in the flock?

I enjoyed mathematics and did well at it in elementary school and in high school. Therefore, I planned to major in it in college. However, I took a physics course in my first year, and liked it so much that I changed my major to physics. But I also continued studying mathematics, so I ended up with a major in math, too.

The physics labs were fun because I got a chance to play with lots of electrical equipment, with microscopes and lenses, with interferomers, etc. I repeated Millikan's famous oil drop experiment to measure the charge of an electron. I learned to use x-ray machines, and took a picture of a block of wood with a bullet buried inside it. A friend and I worked on an old cloud chamber and got it operating so we could see the tracks of alpha particles emitted by a bit of radium.

It was equally exciting to learn the theory behind all these experiments, and to be able to predict the results by mathematical calculation. That really captivated me, and I have been doing that kind of thing ever since.

When I graduated from college, during World War II, I got a job working for the government on sonar and submarine detection. One of the problems was to calculate how strong the sonar sound signal scattered back from a submarine would be. Another was to understand how such acoustic signals were reflected and scattered by the surface and the bottom of the ocean, and absorbed by the seawater.

Later, I found that the same kind of mathematical methods I had learned to use in analyzing sonar could be used to study all types of waves. Therefore, I used it to analyze radio wave propagation, radar, light waves, surface waves in the ocean, earthquake waves, seismic waves from explosions, etc. The probability amplitudes governing electrons and nuclei in atomic physics are also waves, and I was able to analyze them, too.

I was fortunate to be able to devise a new method for calculating the behavior of waves of all kinds. It is called the Geometrical Theory of Diffraction, abbreviated GTD. In includes the rays of geometrical optics, as well as new rays diffracted by edges of bodies, by corners, by smooth surfaces, and so on. In addition to giving quantitative values, it provides a clear physical picture of how waves travel.

Using similar ideas in quantum mechanics led me to an improved method of calculating energy levels, now called the EBK method. It embodies results of Einstein and Brillouin.

I have had fun learning new things and solving problems throughout my life—problems concerning blood flow, nerve impulse propagation, brain development, fluid motion, running, weight lifting, option pricing, queuing theory, ...

OUR GREAT CONTEMPORARY[14]

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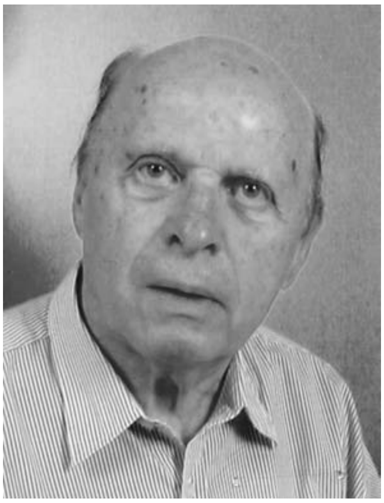

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In 1968 in Trieste (Italy), a ceremonial meeting took place that was devoted to the opening of a new building of the International Centre for Theoretical Physics. This Centre was founded by the Pakistani physicist Abdus Salam, a professor at Imperial College in London. Later on he won the Nobel Prize in Physics. The Trieste Centre is now well known: all the world-renowned theorists have visited it. The Centre was founded in 1964 simultaneously with the Landau Institute for Theoretical Physics. This was not a mere coincidence. This reflected natural processes in science which were related to the greatest achievements in theoretical physics of the 50's: quantum electrodynamics and the theory of superconductivity. According to Salam, the main goal of ICTP was the development of theoretical physics in the Third World countries (Asia, Africa and Latin America). Even though the Centre was created under the aegis of IAEA in Vienna, some 80% of its funding was provided generously by the Italian Government. In getting those funds a great role was played by Salam's Deputy, Paolo Budinich. By choosing this Italian physicist as his Deputy, Salam undoubtedly made a very successful move. On the occasion of the "inauguration" of the Centre, an international conference on theoretical physics was organized at a tremendous scale, characteristic of all that Salam did. More than a dozen Nobel Prize winners were invited, and the list of participants included all the most renowned theorists. As the Director of Landau Institute I was asked by the USSR Academy of Sciences to give my suggestions on the list of our delegation. Here is the list of theorists who took part in this undoubtedly historical conference: academician Fock and later on elected academicians V.L. Ginzburg, A.A. Abrikosov, E.M. Lifshitz, L.D. Faddeev, E.S. Fradkin and myself. In addition, two experimentalists from Moscow University were invited. All of them were scientists with a solid reputation but with no immediate relevance to the conference subject. They were included with my endorsement, so to say for the "balance of forces". In bureaucratic circles of the Academy of Sciences, the delegation for a conference outside the Soviet Union that consisted of only pure theorists was a sort of embarrassment: "why the theorists are ahead of experimentalists?"

[14] With minor changes, the original version of the article appeared in the magazine "Priroda", in 1996.

I think that such a strong delegation of soviet physicists-theorists has never visited the West before. But things did not go smoothly. At the last minute we learned that Ginzburg was again not allowed to leave the country. However, he appeared in Trieste a few days after the beginning of the conference due to efforts of Keldysh (President of the USSR Academy of Sciences).

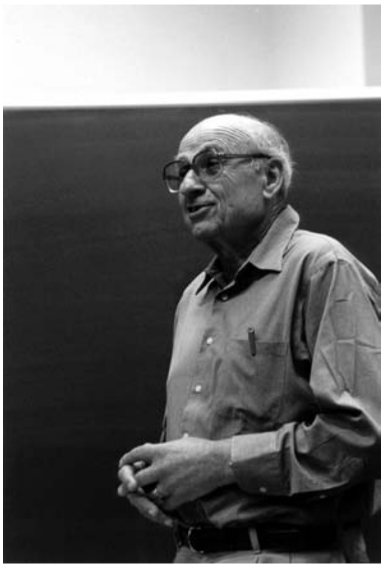
I remember an unusually high level of talks, lectures of the Nobel Prize winners every evening, including those given by the great P.A.M. Dirac, numerous excursions and going out for dinner. During one of those meetings of friends that I and Abrikosov organized, we managed to get the usually silent Dirac to speak. To begin with, Alyosha (Abrikosov) told his usual “touristic” stories, one of the n was about him facing a bear when walking alone in the mountains. Dirac was impressed by this story so much that he started to ask questions and then began to speak himself.

The four of us—Ginzburg, Abrikosov, Lifshitz and myself—undertook a car excursion to Venice and Florence and returned to Trieste through San-Marino. All this was organized by Salam (now this looks funny but one should not forget that soviet people were not allowed to have cash in hard currency and to travel without supervision, so that to rent a car was a problem). In Florence we had to stay a bit longer because of Lifshitz who could not visit the Pitti Gallery together with us. The point is that he was a passionate amateur photographer and took pictures of everything interesting. Those slides he then showed with pleasure to his friends and commented with his usual promptness. I have the impression that a passionate amateur photographer taking sightseeing pictures looks at the world only through his camera and sometimes misses the most interesting things.

Though the Trieste Centre was supposed to assist developing theoretical physics in the Third World, it played—at least during two decades—a role of an International Centre in a wider sense. I was lucky to participate in numerous conferences devoted to the most relevant problems of contemporary theoretical physics, to be a Director of schools on physics of condensed matter and to be a member of the Centre’s Scientific Council. Taking part in the Centre’s activities is an important and special part of my scientific biography.

FINDING A COURSE THROUGH ADVERSITY[15]

Walter Kohn
University of California, USA



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I was born in 1923 into a middle class Jewish family in Vienna, a few years after the end of World War I, which was disastrous from the Austrian point of view. Both my parents were born in parts of the former Austro-Hungarian Empire, my father in Hodonin, Moravia, my mother in Brody, then in Galicia, Poland, now in the Ukraine. Later they both moved to the capital of Vienna along with their parents. I have no recollection of my father's parents, who died relatively young. My maternal grandparents Rappaport were orthodox Jews who lived a simple life of retirement and, in the case of my grandfather, of prayer and the study of religious texts in a small nearby synagogue, a Schul as it was called. My father carried on a business whose main product was high quality art postcards, mostly based on paintings by contemporary artists commissioned by his firm. The business had flourished in the first two decades of the century but then, in part due to the death of his brother in World War I, to the dismantlement of the Austrian monarchy and to a worldwide economic depression, it gradually fell on hard times in the 1920's and 1930's. My father struggled from crisis to crisis to keep the business going and to support the family. My mother was a highly educated woman with a good knowledge of German, Latin, Polish and French and some acquaintance with Greek, Hebrew and English. I believe that she had completed an academically oriented High School in Galicia. Through her parents we maintained contact with traditional Judaism. At the same time my parents, especially my father, also were a part of the secular artistic and intellectual life of Vienna.

After I had completed a public elementary school, my mother enrolled me in the Akademische Gymnasium, a fine public high school in Vienna's inner city. There, for almost five years, I received an excellent education, strongly oriented toward Latin and Greek. At that time, my favorite subject was Latin, whose architecture and succinctness I loved. By contrast, I had no interest in, nor apparent talent for, mathematics which was routinely taught and gave me the only C in high school. During this time it was my tacit understanding that I would eventually be asked to take over the family business, a prospect which I faced with resignation and without the least enthusiasm.

[15] Reproduced, in this abbreviated form, courtesy of the author, from the Nobel website http://www.nobel.se/chemistry/laureates/1998/kohn-autobio.html

The Anschluss changed everything: The family business was confiscated but my father was required to continue its management without any compensation; my sister managed to emigrate rather promptly to England; and I was expelled from my school.

In the following fall I was able to enter a Jewish school, the Chajes Gymnasium, where I had two extraordinary teachers: In physics, Dr. Emil Nohel, and in mathematics Dr. Victor Sabbath. These two inspired teachers conveyed to us their own deep understanding and love of their subjects. Yet again, I take this occasion to record my profound gratitude for their inspiration to which I owe my initial interest in science.

I note with deep gratitude that twice, during the Second World War, after having been separated from my parents who were unable to leave Austria, I was taken into the homes of two wonderful families who had never seen me before: Charles and Eva Hauff in Sussex, England, who also welcomed my older sister, Minna. Charles, like my father, was in art publishing and they had a business relationship. A few years later, Dr. Bruno Mendel and his wife Hertha of Toronto, Canada, took me and my friend Joseph Eisinger into their family. Both of these families strongly encouraged me in my studies, the Hauffs at the East Grinstead County School in Sussex and the Mendels at the University of Toronto. I cannot imagine how I might have become a scientist without their help.

When I arrived in England in August 1939, three weeks before the outbreak of World War II, I had my mind set on becoming a farmer (I had seen too many unemployed intellectuals during the 1930's), and I started out on a training farm in Kent. However, I became seriously ill and physically weak with meningitis, and so in January 1940 my "acting parents", the Hauffs, arranged for me to attend the above-mentioned county school, where—after a period of uncertainty—I concentrated on mathematics, physics and chemistry.

However, in May 1940, shortly after I had turned 17, Churchill ordered most male "enemy aliens" (i.e., holders of enemy passports, like myself) to be interned ("Collar the lot" was his crisp order). I spent about two months in various British camps, including the Isle of Man, where my school sent me the books I needed to study. There I also audited, with little comprehension, some lectures on mathematics and physics, offered by mature interned scientists.

In July 1940, I was shipped on to Quebec City in Canada; and from there, by train, to a camp in Trois Rivieres, which housed both German civilian internees and refugees like myself. Again various internee-taught courses were offered. The one which interested me most was a course on set theory given by the mathematician Dr. Fritz Rothberger and attended by two students. Dr. Rothberger, from Vienna, a most kind and unassuming man, had been an advanced private scholar in Cambridge, England. His love for the intrinsic depth and beauty of mathematics was gradually absorbed by his students.

Later I was moved around among various other camps in Quebec and New Brunswick. Another fellow internee, Dr. A. Heckscher, an art historian, organized a fine camp school for young people like myself, whose education had been interrupted and who prepared to take official Canadian High School exams. In this way I passed the McGill University junior Matriculation exam and exams in mathematics, physics and chemistry on the senior matriculation level. At this point, at age 18, I was pretty firmly looking forward to a career in physics, with a strong secondary interest in mathematics.

I mention with gratitude that camp educational programs received support from the Canadian Red Cross and Jewish Canadian philanthropic sources. I also mention that in most camps we had the opportunity to work as lumberjacks and earn 20 cents per day. With this princely sum, carefully saved up, I was able to buy Hardy's Pure Mathematics and Slater's Chemical Physics, books which are still on my shelves. In January 1942, I was released from internment and welcomed by the family of Professor Bruno Mendel in Toronto. At this point I planned to take up engineering rather than physics, in order to be able to support my parents after the war. The Mendels introduced me to Professor Leopold Infeld who had come to Toronto after several years with Einstein. Infeld, after talking with me (in a kind of drawing room oral exam), concluded that my real love was physics and advised me to major in an excellent, very stiff program, then called mathematics and physics, at the University of Toronto. He argued that this program would enable me to earn a decent living at least as well as an engineering program.

I was fortunate to find an extraordinary mathematics and applied mathematics program in Toronto. Luminous members whom I recall with special vividness were the algebraist Richard Brauer, the non-Euclidean geometer, H.S.M. Coxeter, the aforementioned Leopold Infeld, and the classical applied mathematicians John Lighton Synge and Alexander Weinstein. This group had been largely assembled by Dean Beatty. In those years the University of Toronto team of mathematics students, competing with teams from the leading North-American Institutions, consistently won the annual Putman competition. (For the record I remark that I never participated). Physics too had many distinguished faculty members, largely recruited by John C. McLennan, one of the earliest low temperature physicists, who had died before I arrived. They included the Raman specialist H.L. Welsh, M.F. Crawford in optics and the low-temperature physicists H.G. Smith and A.D. Misener. Among my fellow students was Arthur Schawlow, who later was to share the Nobel Prize for the development of the laser.

During one or two summers, as well as part-time during the school year, I worked for a small Canadian company which developed electrical instruments for military planes. A little later I spent two summers, working for a geophysicist, looking for (and finding!) gold deposits in northern Ontario and Quebec.

After my junior year I joined the Canadian Army. An excellent upper division course in mechanics by A. Weinstein had introduced me to the dynamics of tops and gyroscopes. While in the army I used my spare time to develop new strict bounds on the precession of heavy, symmetrical tops. This paper, "Contour Integration in the Theory of the Spherical Pendulum and the Heavy Symmetrical Top" was published in the Transactions of American Mathematical Society. At the end of one year's army service, having completed only 2 1/2 out of the 4-year undergraduate program, I received a war-time bachelor's degree "on active service" in applied mathematics.

In the year 1945-6, after my discharge from the army, I took an excellent crash master's program, including some of the senior courses which I had missed, graduate courses, a master's thesis consisting of my paper on tops and a paper on scaling of atomic wave-functions.

My teachers wisely insisted that I do not stay on in Toronto for a Ph.D, but financial support for further study was very hard to come by. Eventually I was thrilled to receive a fine Lehman fellowship at Harvard. Leopold Infeld recommended that I should try to be accepted by Julian Schwinger, whom he knew and who, still in his 20's, was already one of the most exciting theoretical physicists in the world.

Arriving from the relatively isolated University of Toronto and finding myself at the illustrious Harvard, where many faculty and graduate students had just come back from doing brilliant war-related work at Los Alamos, the MIT Radiation Laboratory, etc., I felt very insecure and set as my goal survival for at least one year. The Department Chair, J.H. Van Vleck, was very kind and referred to me as the Toronto-Kohn to distinguish me from another person who, I gathered, had caused some trouble. Once Van Vleck told me of an idea in the band-theory of solids, later known as the quantum defect method, and asked me if I would like to work on it. I asked for time to consider. When I returned a few days later, without in the least grasping his idea, I thanked him for the opportunity but explained that, while I did not yet know in what subfield of physics I wanted to do my thesis, I was sure it would not be in solid state physics. This problem then became the thesis of Thomas Kuhn (later a renowned philosopher of science), and was further developed by myself and others. In spite of my original disconnect with Van Vleck, solid state physics soon became the center of my professional life and Van Vleck and I became lifelong friends.

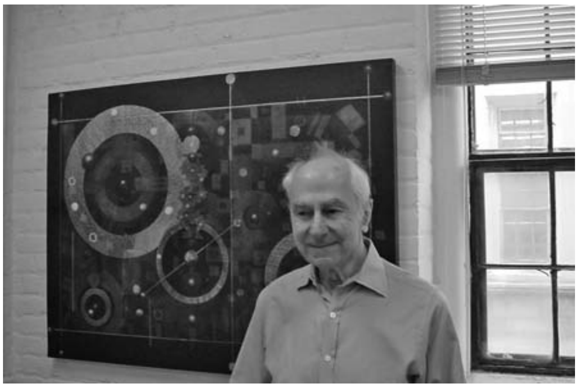
After my encounter with Van Vleck I presented myself to Julian Schwinger requesting to be accepted as one of his thesis students. His evident brilliance as a researcher and as a lecturer in advanced graduate courses (such as waveguides and nuclear physics) attracted large numbers of students, including many who had returned to their studies after spending "time out" on various war-related projects.

I told Schwinger briefly of my very modest efforts using variational principles. He himself had developed brilliant new Green's function variational principles during the war for wave-guides, optics and nuclear physics (soon afterwards Green's functions played an important role in his Nobel-Prize-winning work on quantum electrodynamics). He accepted me within minutes as one of his approximately 10 thesis students. He suggested that I should try to develop a Green's function variational method for *three*-body scattering problems, like low-energy neutron-deuteron scattering, while warning me ominously, that he himself had tried and failed. Some six months later, when I had obtained some partial, very unsatisfactory results, I looked for alternative approaches and soon found a rather elementary formulation, later known as Kohn's variational principle for scattering, and useful for nuclear, atomic and molecular problems. Since I had circumvented Schwinger's beloved Green's functions, I felt that he was very disappointed. Nevertheless he accepted this work as my thesis in 1948.

Looking back I feel very fortunate to have had a small part in the great drama of scientific progress, and most thankful to all those, including family, kindly "acting parents", teachers, colleagues, students, and collaborators of all ages, who made it all possible. It has been an interesting journey.

ISSUES OF RESPONSIBILITY

Serge Lang
Yale University, USA



© Courtesy of Serge Lang

Katepalli Sreenivasan, who was at Yale for many years, and is now director of the Abdus Salam International Centre for Theoretical Physics in Trieste, asked me to write a couple of pages of personal comments for a collection to be published and distributed in high schools throughout the world. I am so indebted to Sreenivasan for his past support that I could not refuse his request.

I wasn't a math freak when I was young. I was only sporadically good in math. I went to Caltech as an undergraduate when I was 16 years old in 1943. At the time, I was mostly interested in the humanities (including music). I got B's and C's in math, C's in physics, D's in chemistry, and A's in english, history and the humanities in general. I got a C in advanced calculus. (I deserved a B, but the professor wouldn't change the grade). During the war, there were no summer vacations, and I graduated in 1946. I was then drafted, and spent over a year in the American army, mostly in Trieste, as a private in the intelligence section of the Assistant Chief of Staff, 88th Division, so I know about that corner of the world. I went to Princeton graduate school in philosophy after I was discharged in summer 1947. I had been admitted on the basis of recommendations from my humanities professors at Caltech. However, the second term, besides two graduate courses in philosophy, I took two undergraduate courses in mathematics (linear algebra and complex analysis) to test things out. I had no trouble with them, and Princeton admitted me as a qualifying student in mathematics for the following fall. I then took graduate courses, and almost immediately became a student of the professor who had taught me that linear algebra course, Emil Artin. From there on, I got my Ph.D. in 1951 under his guidance, stayed at the university one year, spent a year at the Institute for Advanced Study in Princeton, went to Chicago two years as an instructor (postdoc they say today), and then permanent positions, starting with Columbia University where I remained for 15 years. I had made it securely as a mathematician with some big ideas and publications in 1956.

I want to recall one exchange with Artin just after my thesis. I said, OK, I got the thesis, it worked out, I had an idea, but later in life, what happens when I don't get ideas and I'm stuck about discovering mathematics? He answered: "That's the price you have to pay to be a mathematician." I was lucky. Although there always exists an apprehension whether one can do mathematics, i.e. discover theorems (or conjectures), I had continuous productivity throughout the rest of my life, with the exception of three years (1966-1969) when I supported the protest movement in the United States and I did not publish research papers.

Pure mathematics is not a science, whose main component is experimental, i.e. compatibility with the empirical world. My discoveries so far have been made as an art form, although one never knows when such discoveries turn out to be applicable. Furthermore, they are at a technical level which makes it impossible to describe concretely in an essay addressed to a broad audience, such as the present essay. Luckily, I can bring to your attention a beautiful piece of mathematics coming from deep mathematics, for which a proof understandable at the secondary school level is available: Noah Snyder's proof of the Mason-Stothers theorem about polynomials.[16] He discovered this proof when he was a high school senior in 1998.

On the other hand, to the extent some studies are claimed to apply to the empirical world, whether social-political studies, or physics, chemistry, biology, ecology, medicine, engineering, whatever, it is of primary importance that such studies be compatible with facts. Products in certain other areas make no such claims and are made to give us stimulation for its own sake, e.g. poetry and music. Mathematics may occur in both areas. Indeed, mathematical models may be useful to represent what happens in the empirical world. However, to the extent studies claim to describe the behavior of people, or photons, or electrons, or viruses, or drugs, ad lib, clarity of thinking requires that such studies keep clear the distinction between a fact, a hypothesis, an opinion, a belief, a theoretical construct, a model, and a hole in the ground.

Several times in my life, I have been involved in criticizing works which claim to be "science", i.e. works which claim to have empirical validity, and are actually false or just plain garbage. Cf. my book *Challenges* (Springer Verlag, 1998) for documented case studies of this type. Cf. also the book *Scienza e Democrazia* (Liguori 2003), edited by Mamone Capria. In particular, the whole last section pp. 429-540 is about me or by me, dealing with the maintenance of academic or scientific standards. The articles in this book are translated in Italian (no problem for Triestini!).[17]

Issues of responsibility arise. For instance, when mathematicians teach calculus, or biologists teach the use of mathematical modeling, to what extent do teachers warn students about passing off "mathematical modeling" as science, when a purported "model" is not based on empirical data, and is proposed (let alone accepted) quite independently of empirical verification? How does one document the warnings? Does one include a warning about making such assumptions explicit when teaching calculus and biology? What are the implications of holding up resp. not holding up in the classroom specific articles as models of so-called mathematical modeling not justified by empirical conditions? Can we, do we, shall we engage a calculus class in a discussion of such articles, bringing up documentation to the

[16]Noah Snyder, An alternate proof of Mason's theorem, *Elemente der Math*. **55** (2000) pp. 93-94, online. I have written a self contained account of the basic theory of polynomials, culminating with that proof: *Polynomials, a beautiful high school topic*, Springer Verlag, 2004. See also *Math Talks for Undergraduates*, Springer Verlag 1999

[17]For one of them in English, see the last item pp. 457-470 of my collected papers Vol. IV, Springer Verlag 2000. See also my three articles published in the *Yale Scientific*, 1994, 1995, 1999, on the evidence which exists incompatible with the HIV/AIDS orthodoxy.

attention of the class to justify criticisms? What would happen if we did so? The social, academic, and practical forces against doing so are multiple, and obviously very strong.

In my writings and actions attempting to maintain standards, I often refer to Feynman's works. Although he mostly kept out of the fray (he resigned from the National Academy of Sciences in 1969), he did get involved a few times in his life quite effectively, and he writes it up very well.[18] It's not a question of invoking his "authority", as some Yale undergraduate once interpreted my giving Feynman as an example. But we have to make decisions, de facto, about what, when, and how we get involved. The answer is, about different things, at different times, in different ways, according to a generalized principle of relativity.

[18]Check out his three books: *Surely you're joking Mr. Feynman*, *What do you care what other people think*, and *The character of physical law*. The latter contains the best exposition of the 2-slit experiment, which warns you about talking nonsense, incompatible with verifiable facts, in ways which might not have been obvious at the start, in a context which is as free of human emotion as is possible.

BE OPEN TO PROBLEMS

Peter D. Lax
Courant University
New York, USA

Many mathematicians become fascinated with their beloved subject early in life—I was hooked at age 12—and start their studies already in their teens. The likely reason for this, in addition to a brain wired for logical connections, is that in order to grasp and solve mathematical problems no understanding is needed for a wider context which can be acquired only through worldly experience. For this reason many mathematicians shy away from mathematical problems posed in a non-mathematical setting. I was fortunate to acquire a broad view of mathematics, thanks to my mentors and to some happy accidents of life.

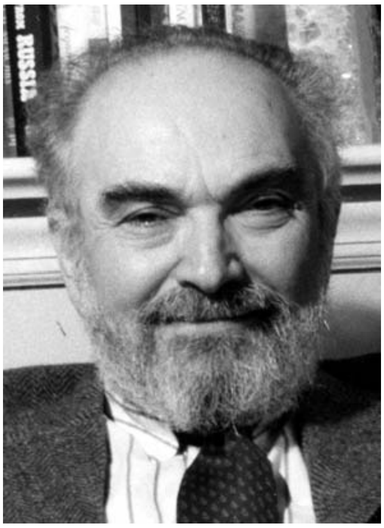
I was born in Hungary where mathematics has a long and respected tradition. I was encouraged and tutored by distinguished mathematicians and pedagogues, Paul Turan, Konig, Rose Peter, and later Paul Erdos. In 1941 my family and I fled to America to escape being murdered by the Nazis; I was 15 years old. In New York I was fortunate to join Richard Courant and his group, where I have stayed, except for some significant excursions. One of them was to the Los Alamos Laboratory, during and after the second world war. It is there that under the influence of von Neumann. I became interested in scientific computing, the royal road to using mathematics to tackle scientific problems.

I have worked on many problems, some of them applied, some purely theoretical. Most of them originated in the theory of partial differential equations, but have often taken me far from their source. Teaching was an integral part of my work; whenever I understood something well, I had the urge to explain it to others. I had more than fifty Ph.D. students, and I have written books on linear algebra, and on functional analysis; I recommend both to students of mathematics.

What do I advise to budding young mathematicians? To be open to problems, wherever they arise, and especially to be on the lookout for new mathematical phenomena that cry out for an explanation.

ON THE MICROSCOPIC ORIGIN OF MACROSCOPIC PHENOMENA

by Joel L. Lebowitz
Departments of Mathematics and Physics, Rutgers University



Science is the human endeavor to understand the nature of the world into which we are born. This quest for understanding is driven by practical needs as well as by an innate curiosity which, whatever its evolutionary origin and utility, goes far beyond the utilitarian. Even very young children have this basic urge to explore and examine. When maintained into adulthood it gives rise to all human creativity including that in the sciences, both theoretical and experimental.

I belong to the theoretical physics community. My main interest is in finding out how the dynamics of the microscopic components of matter, such as atoms and molecules, determine the behavior of macroscopic objects containing very many atoms, objects that we can see and touch, like a glass of water or a piece of metal. This is the subject of statistical mechanics which provides a mathematical framework for describing how well-organized higher level structures or behavior may result from the random, nondirected activity of a very large number of interacting lower level entities. Fortunately, an understanding of many aspects of the behavior of macroscopic systems—such as the boiling and freezing of water—can be obtained from simplified models of the structure and interaction of atoms. We can often take as our starting point Feynman's description of atoms as "little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another". (The degree to which this simple picture gives predictions which are not only qualitatively correct but in many cases also highly accurate, is remarkable, since the structure of real atoms is governed by quantum mechanics and is much more complicated than Feynman's rather crude classical picture).

Statistical mechanics explains how macroscopic phenomena originate in the cooperative behavior of these "little particles". Some of these phenomena are simply the effects of the combined actions of many individual atoms; for instance, the pressure exerted by a gas on the walls of its container is due to the continual bombardment of the walls by very many gas molecules. But other phenomena are examples of emergent behavior; they have no direct counterpart in the properties or dynamics of individual atoms. Particularly fascinating and important examples of such emergent phenomena are (1) the irreversible approach to equilibrium and (2) phase transitions in equilibrium. Both of these would be astonishing if they were not so familiar. Their microscopic understanding and analysis form the core of my own research. Let me sketch them very briefly.

The problem of irreversibility can be stated as follows. Most of the processes we can observe going on in the world about us are *uni-directional* or *asymmetrical in time*. They display an *arrow of time*: cooked vegetables cannot be uncooked, splattered eggs do not reassemble. Yet this manifest fact of our experience is particularly difficult to explain in terms of the fundamental laws of physics. Newton's laws, quantum mechanics, electromagnetism, Einstein's theory of gravity, etc., make no distinction between the past and the future—they are *time-symmetric* and completely reversible. It is only the secondary laws, those that describe the behavior of macroscopic objects containing many, many atoms, which explicitly contain time asymmetry.

A prime example is the second law of thermodynamics which states that an isolated macroscopic system evolves uni-directionally in time towards equilibrium—a state characterized by the maximization of a quantity called *entropy*. (The entropy of a macroscopic system is a measure of the number of microscopic states in which the system can find itself at a given energy or temperature—it can therefore also be thought of as a measure of disorder, suitably defined). The explanation of why and how such time-asymmetric macroscopic behavior arises from completely reversible microscopic dynamics is well understood in principle: it is due to the universe starting out in a state of very low entropy. The current problems concern the derivations and solutions of equations describing these phenomena quantitatively.

The second example of emergent phenomena, much investigated by statistical mechanics, is that of phase transitions in equilibrium systems, such as occur in the boiling or freezing of water. Here dramatic changes in structure and behavior of the macroscopic system are brought about by small changes in the temperature or pressure without any change in the structure of the individual atoms or molecules making up the system. For example the volume occupied by a kilogram of water molecules at atmospheric pressure, while changing only very little when the temperature increase is between 5^{o}C and 95^{o}C, increases by a very large factor when the temperature changes from 99.9999^{o}C to 100.0001^{o}C. Even more dramatic things happen in the freezing transition around 0^{o}C where there are essentially "infinite" changes in some properties, like fluidity. For more details see items 370, 383 and 434 in the publication list on my web site:

http://www.math.rutgers.edu/˜lebowitz .

Please look also at the Human Rights page on my web site. I believe that scientists have special responsibilities in this area. The scientific perspective makes differences between people based on nationality, race, religious belief or gender entirely trivial, while making the things humans have in common, such as the potential to comprehend many aspects of our universe, very special and significant. The scientific outlook should therefore make scientists work hard for a sustainable and just world.

SCIENTISTS ARE LIKE EXPLORERS

Leon M. Lederman
Fermi National Accelerator Laboratory, USA

© Courtesy of Ron Sherman

My interest in science, as best I can recall, came from reading about scientists. There were several crucial books written for young people. I was 10 when I read about biologists in a book called "Microbe Hunters". It told the story of how scientists solved problems to prove that diseases were caused by germs. What I remember most was the puzzles produced by a certain disease and how, by careful work and by "ideas", scientists were able to connect the disease to a microbe, a killer one could see in a microscope!

Then to make the story even more exciting, by identifying the culprit, one could cure sick people and save many lives. This was much better than being a baseball player! Another book was written by Albert Einstein and compared science to a detective story. Someone is murdered and there are lots of clues: a bloody knife, a barking dog, and other pieces of information that seemed disconnected, but which the detective (scientists) carefully wrote down. Eventually, when the murder was solved, each clue, each piece of evidence fitted together like a jigsaw puzzle—all was explained.

Later, in high school, I discovered that science had another incredibly wonderful thing—not only can science solve problems about the world, but that each "little" problem solved, added up together to allow humans to understand the world: why there is day and night, and the way our solar system works. (When I was told that I was standing on a planet that was whirling me around at 1,500 km/hr. I wanted to hold onto something!)

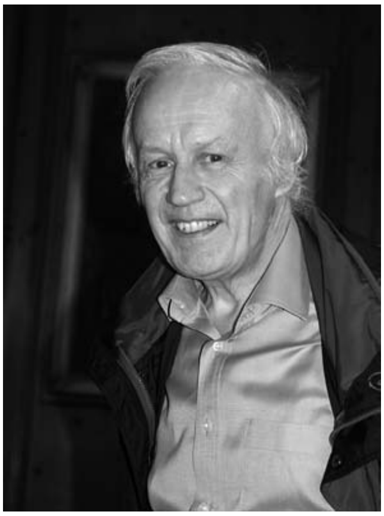
Then astronomers and physicists really could understand the stars (each one a sun with its own planets) and the way they were clustered into unimaginable huge collections of billions of suns. Other scientists used giant, powerful microscopes to look deep inside atoms and actually began to understand how matter and energy worked—just like the biologist who, long ago, studied bacteria.

I realized that to be part of the group of scientists who were exploring the world, way down at the level of a billionth of a centimeter and out to 10 billion light years, was the most exciting life one could imagine. I discovered that scientists were like explorers, like Christopher Columbus or Vasco da Gama. In those times the oceans and continents of Africa and America were unknown mysteries. Today, the unknowns are in our own bodies, our own minds, and on a winter night we see the stars and galaxies. And today we have so many mysteries: how did galaxies form, what is the dark energy that pushes the whole Universe?

And how do our minds work? There is still so much to learn! And scientists even get paid to do these things!

PHYSICS MEANS CONFRONTING THEORY WITH EXPERIMENT

Anthony J. Leggett
University of Illinois at Urbana-Champaign, USA



My entry into my eventual career in physics was extremely unorthodox. In fact, at my high school in Britain, I specialized in classical (Greek and Latin) languages and literature, and took no science courses at all. However, after I had obtained a scholarship (in classics) to Oxford University, I had two trimesters to kill at school. At some point in that period I was buttonholed in the corridor by a retired priest who was living on the premises, who had at one time been a university professor of mathematics; noticing that he and I both had time on our hands, he volunteered to give me a couple of hours' informal tuition a week in modern mathematics. Although at that time I had no inkling that I would ever need any such thing, I found to my initial surprise that not only could I do the problems he set me but that I rather enjoyed doing them. That early experience was, in retrospect, probably the turning point in my career.

The course I followed at Oxford was a four-year one; for the first five trimesters one studies classical languages and literature, then for the last seven an equal mix of “ancient” (Greek and Roman) history and philosophy. The philosophy component of the course is mainly modern, and I enjoyed it very much and did well on it (as well as on the ancient history); and one obvious choice of career was to go on to do a doctorate in philosophy and eventually try to become a faculty member in that subject. However, as I started, towards the end of my third year, to contemplate this prospect, I began to realize that somehow it was not what I really wanted to do in life. On the other hand, the prospect of an academic career was in the abstract very attractive. I therefore started to try to analyse exactly what it was about a career in philosophy that I found unattractive. And the answer I eventually came up with was that it was because, at least as the subject was practised in Oxford in those days, what counted as good or bad work in philosophy seemed to be so much a matter of fashion, and of the exact nuances of one’s phrasing; there seemed to be no objective, external criterion of whether what one did had any validity, and I felt in my bones that it was just such a criterion that I wanted. I did briefly contemplate going into pure mathematics, but rejected it on the grounds that in that subject, by its very nature, to be wrong means to be stupid; I wanted the possibility of being wrong without being stupid, and physics seemed to offer just that opportunity. So I applied to do a second undergraduate degree in physics, and after some vicissitudes was accepted and launched my new career.

I don’t think I have been disappointed; over the last 40 years I have been able to make a string of nontrivial conjectures about various parts of the physical world, and some of them have turned out to be wrong but hopefully not stupid (they are the ones you don't hear about), while others have apparently been shown by experiment to be along the right lines (and in one case have been recognized by the award of the Nobel Prize). Right or wrong, it is just this confrontation of my theoretical ideas with real-world experiments which I find infinitely exciting, and which has kept me going in physics; I have never regretted the choice I made.

WALK WITH RESPONSIBILITY

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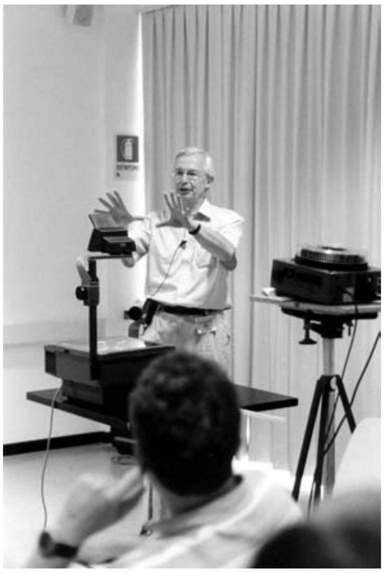

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Science offers most exciting perspectives for the future generations. It promises a much more complete understanding of the universe, an always greater creative power of chemical sciences over the structure and transformations of the inanimate as well as of the living world, an increasing ability to take control over disease, aging and even over the evolution of the human species, a deeper penetration into the working of the brain, the nature of consciousness and the origin of thought.

First, I would like to say a few words about my own field of activity, chemistry, about what is making it so attractive to me.

Indeed, chemistry plays a *central role* by its place in the natural sciences and in knowledge, as well as by its economic importance and omnipresence in our everyday lives. Being present everywhere, it tends to be forgotten and to go unnoticed. It does not advertise itself but, without it, those achievements we consider spectacular would not see the light of day: therapeutic exploits, feats in space, marvels of technology, and so forth. It contributes to meeting humanity's needs in food and medication, in clothing and shelter, in energy and raw materials, in transport and communications. It supplies materials for physics and industry, models and substrates for biology and medicine, properties and processes for science and technology.

In addition to the exploration of the molecules of life, chemistry seeks non-natural species, possessing a desired chemical or physical property. It opens wide the door to the creative imagination of the chemist at the meeting point of chemistry with biology and physics.

Like the artist, the chemist engraves into matter the products of creative imagination. The stone, the sounds and the words do not contain the works that the sculptor, the composer, the writer express from them. Similarly, the chemist creates original molecules, new materials and novel properties from the elements provided by nature.

The essence of chemistry is not only to discover but to invent and *above all to create*. The book of chemistry is not only to be read but to be written! The score of chemistry is not only to be played but to be composed!

Beyond the general progress of knowledge and the technological development, the most important impact science can and must have on society is the *spirit* that it implies, the scientific, rational approach towards the world, life and society.

Mankind is always at the risk of oppressive ideologies and dogmatisms, rigid traditionalisms, wars and aggressions. The scientific attitude shows the way, by its international, better, its supranational spirit for which there are no borders and no classes.

Science education in our schools, colleges and universities as well as for the general public must be a major priority, so as

- to train the researchers, discoverers and creators of tomorrow;
- to lift irrational fears and rejections;
- to develop the scientific spirit, the scientific attitude, in order to fight the obscure, the deceitful, the irrational.

A continuing and aggravating problem is the unacceptable *North/South imbalance*. It is the responsibility of the developed countries to offer solutions. Again scientific progress is crucial. One may hope that progress in medicine and advanced technologies resulting from research in the developed countries, provide means for fighting disease and allowing sustainable development in the less advanced countries.

A very actual issue concerns the attitude of the scientist with respect to *ethics and society*. It is my strong opinion that the scientist is first of all responsible towards the truth. Ethics is a relative notion and ethical evaluations change with time, location and progress of knowledge. Pursuit of knowledge and truth must supersede present considerations on nature, life or the world, for our vision of today can only be a narrow one and we have no right to switch out the lights of the future.

These perspectives for the future of science, for *our* future, have already been expressed in most fitting terms by this quintessence of the artist-scientist, *Leonardo da Vinci* when he wrote:

"Where nature finishes to produce its own species, man begins, using natural things, in harmony with this very nature, to create an infinity of species".

The future of science and of mankind lies in the hands of the coming generations. May they take up the challenge. Prometheus has conquered the fire and we cannot give it back. We have to walk, with enthusiasm, determination and a deep sense of responsibility, the way from the tree of knowledge to the control of destiny.

WHAT DREW ME TO SCIENCE

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Johanna M.H. Levelt Sengers

My encounters with the physical sciences began in my childhood in the Netherlands. My father was a Ph.D. chemist, who, in addition to running the family coffee and tea business, operated a small chemistry laboratory at home. My mother held a master's degree in physics and astronomy. She married shortly after, and I was the firstborn of what would be ten children, so my mother never practiced in her chosen field. My father loved to talk with us about science at dinnertime, explaining the movement of planets, earth and moon by using apples and oranges arranged on the tablecloth. When we children reached high-school age, he would not go to bed before having solved our math homework problems.

When I was twelve years old, my parents enrolled me in an all-girls' school, which had a college-preparatory branch called Gymnasium. In addition to classical and modern languages, history and geography, we carried a full load of physics, chemistry and biology studies. The school carried the identical curriculum as all-boys' and gender-mixed Gymnasia in my country. My first-year's encounter with geometry was love at first sight: a subject I did not have to memorize, but could actually reason out—facts that did not have to be believed, but could be proven. I was fortunate to have a number of outstanding teachers, with advanced academic degrees in their subject matter. The chemistry teacher was my favorite, and drew me to the field. I was intrigued by physics, as well. While a high school student, I read quite a few books on mathematics and astronomy and relativity. With my father, I worked on beginning calculus, which we did not get in school. Although the mindset in the Netherlands was that science and technology were topics for boys, I was never discouraged at school.

My Gymnasium years played out during the Second World War, when my native country was under German occupation. School was my lifeline, where I could forget the misery by immersing myself in topics of interest to me. So when I finished my high-school years by passing the final State-wide exam with flying colors, I wanted to head for higher education. At the time, however, there were eight younger siblings at home, six of them boys. Although our home life was steeped in science, there was ambivalence about paying to further educate a daughter needed as a mother's helper.

So I applied for a State scholarship, which I obtained. This allowed me to pay for daily travel to the University of Amsterdam, and for tuition. I lived at home, tutoring high-school students in the weekends, so as to minimize my financial dependence on my parents. Later, during my graduate studies, I taught high-school physics part time.

Since I had no strong preference, I chose both physics and chemistry as my undergraduate majors. This implied the heaviest load of lab work I could have chosen. I did not find the transition to the university easy. The undergraduate classes were huge, and most professors did not excel as teachers. Among my physics classes was thermodynamics, taught by Antonius Michels, who later would become my thesis adviser. I found this subject, having to do with the interaction of matter, heat and work, very difficult, but quite intriguing. After I completed my undergraduate studies, I started on a master's degree with physics as a major and chemistry as a minor.

My graduate-level physics professors included Jan de Boer, an accomplished teacher of statistical mechanics and quantum mechanics. Statistical mechanics relates properties of bulk materials to the forces between the molecules. Quantum mechanics deals with the behavior of very small systems, which manifest themselves as waves. I was one of three or four females out of over fifty students.

I carried out my experimental work at the Van der Waals Laboratory, a high-pressure laboratory headed by Professor Michels. I measured properties of fluids contained in heavy walled steel vessels, at pressures up to 1000 atmospheres and at cryogenic temperatures. After I obtained my master's degree, my Ph.D. thesis subject became measurement of the properties of pressurized argon, a non-reactive gas of spherical molecules (atoms). This gas has a critical point at 122 ^{o}C below the freezing point of water. Below that critical temperature, argon can be liquefied, and there would be part gas and part liquid in my steel vessel. My argon data became a test case for statistical-mechanical theories of molecular interaction. All fluids have critical points; for instance, that of water is at +374 ^{o}C, and that of helium at -269 ^{o}C. Critical points also occur in magnets and in many model systems of statistical mechanics; they were to become the focus of my later career.

After obtaining my Ph.D., I was a postdoctoral researcher at the University of Wisconsin in Madison for a year. After a few more years at the Van der Waals Laboratory, I married a fellow-researcher, Jan Sengers. We both applied for positions in the US at the National Bureau of Standards (NBS), presently the National Institute of Standards and Technology (NIST), where we were hired late 1963. NBS/NIST is part of the US Department of Commerce. It provides measurement standards and materials property data to industry and science.

During my first five years at NIST, I gave birth to my four children, which required a few months of absence each time. My managers were very understanding, and allowed me to work part-time until my youngest was nine years old. My husband joined the University of Maryland in 1968.

When my husband and I got on board at NBS, the field of critical phenomena was just taking off. Because so many different systems have critical points, this field is strongly interdisciplinary, with physicists, chemists, mathematicians, and engineers all studying aspects of criticality. The hypothesis was that many of these systems, after proper choice of variables, would behave in the same way: critical point universality.

My contributions were in the area of fluids. My colleagues and I tested fluid behavior near the critical point, and found that it conforms to the so-called scaling laws, whose universal form followed from theories then being developed. I applied the new theoretical insights, which led to a Nobel Prize for Kenneth Wilson in 1972, to fluids of practical interest in industry. Ethylene, for instance, a starting point for fabrication of polymers, has a critical point just below room temperature. I measured its properties, and found them to be consistent with the new theory. Throughout my career, I have worked on properties of water and steam, which are required by the electric power industry. I was the one to show that steam properties near the critical point conform to the scaling laws.

In summary, my work grew naturally from my early interests, and has formed a bridge between new theoretical insights and engineering applications. I feel very fortunate to have found my niche in the world of science and engineering.

I LOVE A PUZZLE

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I love a puzzle. As a child, it was mathematics that caught my interest, and I am still inclined to spend my free moments with a math puzzle, often one I make up for myself. As I come across problems I can't solve, I store them for future reference, and pull one out of my head when I have moments to spend. The more basic the problem, the better I enjoy it; I always feel that working through such problems deepens my understanding of the logical relationships that tie everything together.

This fascination with mathematics meant that mathematics came easily for me in school—I had had lots of practice with it, and with problem solving. It was natural, therefore, that I should study mathematics at university (The Johns Hopkins University), and then go on to graduate work. Before I knew it, I had a Ph.D. in Mathematics from the Institute for Fluid Dynamics and Applied Mathematics at the University of Maryland. But I wasn't quite sure what to do with it; even in graduate school, I had come to realize that the problems I would have to solve as a professional mathematician were too disconnected from the real world to satisfy me as my life's work. I wanted problems to solve that meant something to others, and that could help them to make sense of their worlds. Even while a student, I turned to biology, and sought for ways to unify the diverse perspectives that defined the subject. For postdoctoral work, I went to Berkeley to work with George Dantzig, famous for his invention of the simplex method to solve linear programming problems. Together, we confronted the problem of active transport of sodium across red blood cell membranes, using queuing theory to capture the properties of molecules moving through narrow channels.

My first real job was at Cornell, in the Mathematics Department. But Cornell was also a powerhouse in ecology and evolutionary biology, and I quickly gravitated there, drawn both by the importance of the problems being addressed, and by the fascination with trying to understand the grandest puzzle of all—how the diversity of the biological world emerged, and is maintained. Physics has its basic laws, the foundation on which theories can be built. Darwin and Wallace gave biology its own law, evolution by natural selection and other mechanisms, and my great avocation became trying to see how much I could learn about the organization of the biological world through the lens of evolution. At the same time, I was learning from other disciplines, like fluid dynamics and statistical physics and developmental biology, about mechanisms of pattern formation and self-organization. Obviously, these had something to do with one another: The evolution of life really was the ultimate experiment in self-organization, and the mechanisms of self-organization seen during development clearly had been shaped by evolution. It also was evident that processes as diverse as the successional development of plant communities, and the emergence of culture in human society, were manifestations of self-organizing processes that had been shaped in turn by evolutionary processes, and indeed were continuing to feed back to influence those evolutionary processes. Evolution is not something in the past, but something that continues to change the context for ecological and social interactions. I had to think about scales—of space, of time, and of organizational complexity, and about how the collective dynamics of huge numbers of individual agents pursuing their own selfish agendas led to the emergence of patterns and processes on broader scales, which in turn fed back to shape and constrain the microscopic processes. This was not so different from the standard fare in statistical mechanics, except that the diversity of kinds of agents was staggering in its complexity, and constantly changing in composition through the generation of new variation.

Over the years, as my ideas grew, they were enriched by what I learned from others. I collaborated broadly, because I could learn so much from others. Cornell and Princeton provided the perfect settings for doing that, because my colleagues had so much to offer. From some I learned mathematics and logic, from others I learned physics and natural history. In 1982, I made my first visit to ICTP in Trieste, joining with Tom Hallam to organize the first course in mathematical ecology. That added an important new dimension to what I was doing—the chance to share what I knew with people from every imaginable nation, and to learn from them about the unique environmental problems of their countries. I was now able to solve problems that mattered—in ecotoxicology, in epidemiology, in conservation biology, and in the new science of sustainability. By then, I had moved fully into the Section Ecology and Systematics at Cornell, spent 5 years as its Chair, and created the Ecosystems Research Center to tackle the environmental problems facing society. The interface with societal problems took my work closer to applied science than ever before; but somehow, this only made clearer to me the need for creating a relevant conceptual framework for addressing those problems, and for deriving a core set of organizing principles for ecological systems.

Princeton was the natural place for the next phase of my career. Ecology and evolution were equally strong there, but there were new opportunities to build bridges to economics and policy. At the same time, I became a regular at the Beijer Institute, in Stockholm, which more than any other institution was bringing together leaders from the ecology and economics communities. I was drawn to economics both because it was central to the solution of environmental problems, and because economic systems really were ecological systems—or was it that ecological systems were really economic systems. That is, in both ecological and economic systems, competition for limited resources shaped the interactions among agents, including cooperation and exploitation. Both are examples of complex adaptive systems, in which new diversity continually arises, and is shaped by the interactions among an effectively infinite number of agents. Almost simultaneously, I also began to spend whatever time I could at the Santa Fe Institute, a remarkable meeting ground for scientists of all persuasions, who independently had come to confront the same kinds of questions as I had. Santa Fe is a glorious natural and intellectual environment, where ideas are created and tossed out with the same reckless abandon that mutation and recombination employ to generate the diversity in natural systems. The unspoken philosophy is that one shouldn't put limitations on the generation of ideas. Rather, like mutations, they should be created freely, counting on the process of natural selection to winnow the wheat from the chaff. This runs counter to the view that many people have of science- that the ideas should flow largely from the results already obtained. There is much to be said for this view—hill-climbing is an important part of natural selection; but it is too constraining, and likely to limit advances. A generous sprinkling both of wild and original ideas is crucial to advancing knowledge. My own career has been a journey in trying to balance the radical and conservative approaches; there is a place for both in the life of any scientist.

CLIMATIC MODELS THROUGH COMPUTING

Syukuro Manabe[19]
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In 1958, I received a letter from Joseph Smagorinsky, inviting me to join his group at US Weather Bureau to develop a so-called general circulation model of the atmosphere. The model attempts to simulate, on an electronic computer, the behavior of the atmosphere based upon the laws of physics. It represents a very ambitious extension of the numerical weather prediction model of the hydrodynamical equations, which had been developed under the leadership of John von Neumann at the institute of Advanced study, Princeton, New Jersey. Although the computer technology was at an early stage of development, Smagorinsky already had a grand vision for the development of a comprehensive, mathematical model of the Earth System for the study of climate.

When I received the invitation, I was a graduate student, majoring in meteorology at University of Tokyo. Although I had almost completed my Ph.D. thesis by then and began to look for a job, it was very difficult for me to find a right kind of job in the postwar Japan. So, it did not take long before I accepted this very attractive invitation from USA, where computer technology started to develop rapidly. Nevertheless, this was the best decisions I made during my long research career.

In the fall of 1958, I started working as a staff member of the general circulation research section of US weather Bureau, which later became Geophysical Fluid Dynamics Laboratory (GFDL) of National Oceanic and Atmospheric Administration (NOAA). Immediately, I participated in the development of a general circulation model of the atmosphere. We had to work extremely hard to overcome many difficulties encountered. To my great relief and

[19] Manabe is a member of the National Academy of Sciences of USA, and foreign member of Academia Europaea and the Royal Society of Canada. He is an honorary member of the American Meteorological Society and the Japan Meteorological Society, and is a fellow of the American Geophysical Union and the American Association for the Advancement of Science.

Throughout his career, Manabe has been honored with many awards, including: the Fujiwara Award from the Japan Meteorological Society (1966), the Rossby Medal from the American Meteorological Society (1992) and the Blue Planet Prize from the Asahi Glass Foundation (1992). The American Geophysical Union presented him with the Revelle Medal (1993) and the Asahi Newspaper Cultural Foundation in Japan gave him the Asahi Prize (1995). Manabe has also received the Volvo Environmental Prize from the Volvo Foundation (1997) and the Milankovitch Medal from the European Geophysical Society (1998).

satisfaction, our model began to simulate successfully the broad-scale features of the atmospheric circulation and rainfall by the mid-1960's. This was the beginning of my long research career.

In the early 1960's, I started collaborating with Mr. Wetherald, a staff member of GFDL, constructing a vertically one-dimensional model of the atmosphere. The model was constructed as the first step towards the construction of the three-dimensional, general circulation model of the atmosphere. It computes numerically the vertical temperature profile of the atmosphere in "radiative, convective equilibrium". This model succeeded in simulating realistically the basic thermal structure of the atmosphere that consists of the convective troposphere and stable stratosphere.

Using the radiative, convective model, we estimated the magnitude of global warming in response to the doubling of atmospheric concentration of carbon dioxide. For the first time, we were able to correctly estimate the change in the convective heat transfer from the earth surface to the atmosphere, thereby eliminating the serious flaw in the earlier studies of global warming by Arrhenius and others. The study was published in 1967, and is the first study of global warming that stood the test of modern scrutiny.

During the 1960's and early 1970's, I spent most of my time involved in the development of a three-dimensional, general circulation model of the atmosphere. In particular, I played a major role in explicitly modeling the vertical heat transfer due to thermal radiation and moist convection, and the budget of heat and water at the continental surface. By the late-1960's, we succeeded in simulating the vertical as well as horizontal distribution of temperature in the troposphere-stratosphere system, and the global-scale distribution of precipitation and its seasonal variation. These simulation studies demonstrated that a general circulation model had become realistic enough for studying climatic change.

In 1975, Wetherald and I published another landmark study on global warming. Using a general circulation model of the atmosphere, this study simulated and evaluated, for the first time, the three-dimensional response of the atmospheric temperature and hydrologic cycle to the doubling of the atmospheric carbon dioxide. The response includes the intensification of the global hydrologic cycle that involves the increase in the global mean rates of both evaporation and precipitation.

To simulate satisfactorily the behavior of climate, it is essential to consider the oceans, which continuously interact with the atmosphere, serving as a huge reservoir of heat. It was the mid-1960's, when I began to collaborate with Kirk Bryan, leader of the ocean group at GFDL, to develop a coupled ocean-atmosphere model in which general circulation model of the atmosphere is combined with that of ocean. Our collaboration lasted during the most of our careers, eventually yielding a coupled model, which has become indispensable for the study of natural and anthropogenic variations of climate. It was a great pleasure to work with him, as he has always been open-minded and very generous in sharing his ideas.

In 1969, Bryan and I published a paper, which described the first successful attempt to develop a coupled ocean-atmosphere model. Using the model, we began to identify the influence of oceanic heat transport upon the broad-scale features of surface temperature and precipitation.

In the late 1970's, Ronald Stouffer joined our group, and began to make major contribution for the improvement of the coupled ocean-atmosphere model. By the late 1980's, we achieved another milestone, using a coupled model with realistic geography. In the study published in 1989, we described the simulated, time-dependent response of the coupled model to the gradual increase in the atmospheric carbon dioxide. Based upon the in-depth analysis of the simulation, we succeeded in elucidating the role of oceans and land surface in delaying and shaping the response of climate to gradual increase of greenhouse gases in the atmosphere. This study was cited extensively in the first report of the Intergovernmental Panel on Climate Change published in 1990.

In the 1990's, we extended the application of our coupled model to the study of natural, unforced climate variation. Performing the time integration of the coupled model over a millennium, we succeeded in simulating a major fraction of the variability of surface air temperature with inter-annual to multi-decadal scales. Based upon the extensive analysis of the result obtained, we successfully identified some of the basic physical mechanisms responsible for the natural variability of climate.

Throughout my career, I have explored large climatic changes of the geological past in collaboration with the staff member of GFDL. For example, we elucidated the critical role of greenhouse gases (e.g., such as carbon dioxide and water vapor) in maintaining the warm climate of Mesozoic and the cold climate of the last glacial maximum. More recently, using our coupled ocean-atmosphere model, we succeeded in simulating the so-called abrupt climate change, which frequently occurred during the glacial and de-glacial periods.

In short, we have conducted innumerable numerical experiments, identifying the physical mechanisms, which are responsible for past, present, and future changes of climate. Our studies have helped to substantiate our speculation that human activities have had significant impact upon the global climate.

Although I was trained as a specialist in the field of dynamical meteorology at the graduate school of University of Tokyo, I succeeded in opening new avenues of research through interdisciplinary collaboration, making a big leap forward. When I developed an atmospheric model of radiative, convective equilibrium for the study of the greenhouse effect, I learned from Professor Fritz Möller of Germany, a pioneer in the field of thermal radiation in the atmosphere, who was visiting our laboratory. It was also very fortunate that I was able to collaborate with Kirk Bryan, physical oceanographer at our laboratory for the development of coupled ocean-atmosphere model, which is indispensable for the study of climate. In my opinion, an interdisciplinary topic yields a new perspective and provides us the most promising avenues of scientific research.

As we go into the 21st century, it is going to be increasingly difficult, in my opinion, to place an even-handed emphasis between the rapid development of technology and the protection of global environment and eco-system. In order to make a prudent decision on our future course of action, it is highly desirable to have a reliable projection of global change in future. This is why it is necessary to construct a realistic model of the Earth system. The development of climate model, which is the subject of this essay, is the first step towards this very important goal.

As we know, the earth system is infinitely complicated. Obviously, it is extremely difficult to quantitatively validate an infinitely complex Earth system model and interpret the projection of future change obtained from such a model. To develop a reliable Earth system model, it is therefore desirable to identify the important processes of the Earth system that critically affect its behavior, discarding less important details. Clearly, this is a very difficult task, posing a huge challenge for the future modelers of the Earth system.

ROUGH, LONELY AND EXCITING

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Landowners' or bankers' children have an easy option when they "grow up:" they had been prepared over many years to follow the family tradition. My family owned neither land nor bank but did have a strong tradition: I was expected without saying to become a scholar of some sort. Any other activity would have required a specific reason. Did this molding make my choice any easier? It did not; in fact, I found growing up very complicated, first, because the effects of the Depression and World War II, but later because of sharply conflicting advice from my father and his youngest brother.

I was thirteen when my uncle became a professor at the famed Collège de France in Paris; hence I always knew that respected, comfortable, and altogether enjoyable professions included pure mathematics. Since I had a quick mind and good grades, the option of following after my uncle was widely open. But it encountered strong obstacles. First, Father was very strongly opposed; besides, his influence and what I kept reading over the years had instilled in me a powerful opposite interest, not in the simplicity of mathematics but in complex down-to-earth issues, for example, how complex machines work. Secondly, constant interruptions in my schooling had left me only a middling performer in analytic manipulations. On the other hand, when I was nineteen, a few crucial weeks revealed that I had a rare and powerful native gift: I could carry out in my head complicated mathematical arguments directly in terms of geometric shapes. Absent the war, more organized and exacting training in analysis might have diluted this almost freakish gift, or prevented it from manifesting itself, or even encouraged me away from science. As things stood when I turned twenty, none of the obvious paths open to me matched my tastes. I had also antagonized several well-meaning persons by not following their advice.

The resulting complexity of my professional life continually reflected the topic I went on to pioneer, namely, complexity in the real world. In 1950, this was a completely uncharted domain but I was lucky to be living in relatively free-wheeling times. This allowed greater risk taking than would be reasonable today. Entrepreneurs are more familiar than scientists with high risk and the paradox that a high-risk taker who wants to survive and continue must not be reckless but cautious. In a way, I was very conservative and began as a "misfit" but made myself over into a "maverick."

Actually, the word "complexity" did not enter scientific discourse until much later. Also, being a conservative high risk-taker deprived me of role models. Running against every trend, I did not even think of seeking principles that could be developed into a theory, from top down. Instead, I started by seemingly narrow problems and the steps I took began by being small and moved to being increasingly bold. Most significantly, I never underestimated the complexity of the real world and simplified it from the outset—without distorting it—by only paying attention to its most visible aspect, namely, roughness and fragmentation. Despite this limitation, much work was done. But much remains to be done because roughness is absolutely ubiquitous and often is the major obstacle to an understanding and control of reality.

For the nascent theory of roughness, browsing through a Latin dictionary made me coin a new term, "fractal geometry." Its development went beyond anyone's wildest hopes. It raised questions of pure mathematics that enchant, challenge, and often continue to frustrate the experts. It gave hope that a physics of roughness may soon arise next to the well-established studies of weight, motion, light, and sound. It tamed the complexity of physical clusters of all kinds. It came far closer to truth than any alternative model of financial price variation. It provided a major theme to the earth sciences. It yielded stunning graphics. It led to the discovery that examples of fractality that no one had previously recognized as such had been included in art and architecture since time immemorial.

My long ride through these activities has been rough and often lonely—but exciting. If given the same chance, would I want to repeat it? Yes, but no one is ever given the same chance. Should others try to repeat it? Certainly not, but two lessons I learned are worth sharing. Those who dare predict future manpower needs are biased, do not know enough, and are seldom right. To rush and narrow yourself down to fit their advice seems the safest path, but in fact is the most dangerous. To perfect the skills you have and love is by far the best beginning—as long as your skills keep broadening rather than narrowing. Once educated in the science you love, you may easily move to another or to a different path. But you could never catch up on science. Do you want your life to make a difference? More than ever, science—like all other aspects of society—needs young adherents who combine wide knowledge with a disciplined willingness to take high risks—both for their own personal satisfaction and the moral welfare of the human race.

RESEARCH IS ABOUT TOTAL FREEDOM

Mambillikalathil G.K. Menon
New Delhi, India

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Mambillikalathil G.K. Menon

When I was young, my choices on what I would like to do in life covered a range of possible careers: science, medicine, law, history, administration and business management. The commitment to go in for science essentially came from a chance interaction, when I was about 16 years, with Professor C.V. Raman who had won the Nobel Prize for his discovery of the Raman Effect. Raman was a very exuberant and enthusiastic individual. He told me that there was nothing as exciting as science to pursue in life. I was so taken up with that encounter that I decided to follow his advice, particularly since I was good in the various science subjects and found science to be fascinating—I was always asking "why". The memories of those days of early introduction to research are of total freedom to read and learn across a wide spectrum of sciences, and more particularly, a hard grinding in experimental physics and laboratory arts.

I was fortunate that, at an early stage, I was accepted by Cecil Powell to work in his laboratory in the Bristol University in England. I had sent him a handwritten letter saying how excited I was about his paper in "Nature" (which later won him the Nobel Prize); and requested to work with him. I had a prompt handwritten response agreeing to my request—a far cry from the process of applications now in vogue! Bristol was a very exciting place to be in. The group there had the enthusiasm of a successful young team in sports—that we are the leaders and on top.

Apart from being part of the Powell group, which was acknowledged as being a leading centre of research in the world in its field, there was also another outstanding group there, on condensed matter physics under Neville Mott and Charles Frank.

During my six years in the Powell group, there were, for me, several exciting moments in scientific discovery. These principally related to the first insights into the various ways in which the so-called K (or heavy) mesons decayed into secondary particles. The Powell group had already observed the decay of a tau-meson into three charged pi mesons in emulsion. My first involvement related to the discovery that the track of a particle, obviously coming to rest in nuclear emulsion, giving rise to a single very high energy secondary particle, was due to a heavy meson, whose measured mass was about four times the mass of the pi-meson. The second was the observation that when the masses were measured of the single secondary particles from such heavy meson decays, they turned out to be in some cases mu mesons and others pi mesons. However, when they were mu mesons they appeared to have varying energies; when they were identified as pi mesons, they all appeared to have the same energy; the latter clearly represented two body decay of the heavy meson, and if the neutral secondary was assumed to be a pi-zero meson, then the mass of the parent could be identical to that of the tau-meson. These were the opening shots in a fascinating field that later was characterized by the tau-theta puzzle, the need to have parity violation in weak interactions (of Lee and Yang), right handed and left handed neutrinos; it brought out deep issues relating to weak interactions, leading ultimately to the work of Salam (as also Weinberg and Glashow) on the electroweak unification. All these developments rapidly came about in the next three decades; and for me it was a great privilege to interact personally with many of the great scientists who played key roles.

We were also the first to show that there were electrons as secondaries in heavy meson decays thus establishing the many decay modes of heavy mesons to muons and pions in two body and three body decays, as also to electrons in three body decays. We also found that heavy mesons when brought to rest in nuclear emulsions never produced interactions, in contrast to pi mesons. Because of this, and their other properties, heavy mesons were referred to as “strange” particles; they are characterized by a strangeness quantum number (Gell Mann). Many of these initial discoveries were confirmed and elaborated through work done with what was then a huge nuclear emulsion stack of 15 liters in volume. From thin layers of emulsion of 400 microns thickness with which one had started this work, it was a spectacular jump to 15 liters. It became clear, however, that the nuclear emulsion technique by itself, and the low cosmic ray intensities posed limitations, so that further developments in this field would come only through accelerator-based studies.

In 1955, after six exciting years at Bristol I returned to India to the Tata Institute of Fundamental Research (TIFR) in Bombay. I had made up my mind that I had a responsibility to live and work in my country, and demonstrate that, provided opportunities were available, world class work could be done even in the many newly emerging fields.

At TIFR, research was pursued in two directions. The first was to master the technology of making and flying plastic balloons of enormous volume to do research relating to cosmic ray studies near the top of the earth’s atmosphere. The initial years, till we achieved success, was a saga of school boyish adventures. There are many anecdotes that would illustrate the fun in doing science. Since then, remarkable success has been achieved: balloons of volume 30 million cubic feet, carrying ton payloads of detectors can be floated near the top of the earth’s atmosphere for cosmic ray studies, and for X-ray, infra-red and gamma-ray astronomy.

The other area of research related to studies of phenomena with electronic detectors taken progressively to greater depths underground. This was done in the very deep gold mines, where depths were available up to 2 miles below the surface. I can recall the excitement when our detectors gave us no counts at the greatest depth of 8,400 meters water equivalent; and we could conclude that it would now be possible to detect interactions produced by natural neutrinos. We had pushed what was initially meant to be only a phenomenological study of cosmic ray intensity as a function of depth, to a new window to look at natural neutrino interactions, and perhaps the physics and astrophysics that could come out from studies of neutrinos from space. Indeed, with large detectors at great depths, a TIFR-Osaka-Durham (UK) collaboration was the first to detect the natural neutrino interaction.

I recall Abdus Salam and I discussing the possibility of detecting proton decay at a UN meeting in New York where we were both rather bored. If detected this would open up a new era of grand unification of the electroweak with strong interactions. Salam wanted to know whether we could detect proton decay if the lifetime was up to 10^{29} years as predicted by some simple theories. A simple calculation on the back of an envelope showed that it was indeed possible with a detector of around 100 tons of matter. At that stage, my colleagues at TIFR and from Japan were also thinking about these possibilities, and we embarked on what was the first dedicated experiment in the world to look for proton decay. While no proton decays have been detected in any experiment as yet, the new large detectors have recently provided wholly new insights in unexpected areas of neutrino physics and astrophysics.

For me personal research has meant a unique opportunity to make unexpected observations, and it has been a great experience to see that from many of these small beginnings have emerged such spectacular understanding of nature. A lot of this has been possible through the work of large numbers of scientists around the world—many of them of the highest class. It has also been wonderful to have had the joy of the many personal interactions with so many great intellects as these fields have advanced.

But for me science has also been important in terms of its culture of a rational and objective way of looking at the world and its spirit of transparency, openness and friendships. One has seen the many ways in which scientific discoveries have resulted in technologies and applications that have profoundly altered the world over the past half century. I was also fortunate in being able to provide opportunities in my country for many scientists to move into new areas of particle physics, astronomy, the new biology, electronics and information technologies, sustainable development and others. Some of these have related to "blue skies" research, but a great deal has also related to the possibility of benefits for huge numbers in the country for whom science is so important for development.

Science is truly exciting, and I am grateful for the opportunity to have been in science.

A LIFELONG AFFAIR WITH FLUID DYNAMICS

Keith Moffatt
Trinity College, Cambridge

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I always enjoyed mathematical puzzles as a child, stimulated by my father who loved them also. One that I remember from the age of about 12 was this: obviously $8+8+8=24$ and $(9\times\sqrt{9})-\sqrt{9}=24;$ the problem is to make up the number 24 in a similar way using each of the integers $1,2,\ldots,9,$ in turn exactly three times (no more, no less) and the standard symbols of arithmetic. Try it! Some are harder than others, but all are possible.

I enjoyed maths so much at school that there was no question about wanting to continue with the subject at University also; but I was attracted to physics also, and eventually settled at Edinburgh University in that no-man's-land of Applied Mathematics, which gave equal scope for both. I went on to Trinity College, Cambridge, and after a year in the mysteries of quantum mechanics, discovered that my real spiritual home lay in the classical field of fluid dynamics, a subject of more straightforward mathematical challenge, of immediate and wide-ranging practical applications, and with (for me) the great attraction that you can visualise the flow of a fluid, whether through waves on the seashore or a twig in a turbulent stream or a feather blown in the wind, a luxury that is not similarly available in the quantum realm.

I was fortunate to be supervised in my early research by George Batchelor, one of the great pioneers in the statistical theory of turbulent flow. He taught me the essential art of self-criticism in research—the need to be as critical of one's own ideas and arguments as of any others. I slowly developed what we know in our subject as 'physical intuition', i.e. an instinctive sense of how a complex fluid system will behave in given circumstances; this intuition provides an essential guide to the choice and refinement of appropriate mathematical models which can be subjected to rigorous analysis. The ultimate test lies in the confrontation of theory and experiment, and this usually leads to corresponding refinement of one's physical intuition!

One of the greatest practitioners of theoretical and experimental mechanics (both solid and fluid) of the twentieth century was Sir Geoffrey Taylor, Fellow of Trinity College, best known perhaps for his discovery in 1923 of 'Taylor vortices' in the flow between coaxial rotating cylinders. G.I. (as he was affectionately known to all who knew him) was still very active in research throughout the 1960's, and he provided inspiration to the vigorous young fluid dynamics group in Cambridge. His physical intuition was unsurpassed, and he had the marvellous skill of using mathematics at just the appropriate level for whatever phenomenon he happened to be investigating. I learnt from him that there is no merit in mathematical complexity for its own sake; the true art is in perceiving the simplicity of natural phenomena, and this can often be captured through relatively simple mathematics. But to recognise the key physical effects and to extract the relevant mathematics from the governing principles and equations of fluid dynamics: Ah, therein lies the problem!

My Ph.D. thesis in 1962 was on the subject "Magnetohydrodynamic Turbulence", i.e. turbulence in electrically conducting fluids (ionised gases at one extreme, liquid metals at the other), a subject with important applications in astrophysics, geophysics, thermonuclear fusion physics and engineering. Such fluids are generally permeated by a magnetic field whose lines of force are almost 'frozen' in the fluid, in the sense that they are transported by the fluid like elastic threads, a process that one may easily visualise in the 'mind's eye'. At the same time, they diffuse relative to the fluid, so that lines of force may in effect break and reconnect—their topology may change. These ideas are now very familiar, but they were not so in 1960, and their implications were by no means well understood. The challenges were great, and I was lucky to become involved in this exciting field of interaction between fluid dynamics and electromagnetism at this early stage of its development. I have returned to it regularly ever since.

From the beginning, I was heavily involved in teaching for the mathematics tripos in Cambridge, and this teaching has had an important bearing on my research. It was through posing an examination problem in 1962 that I was led to consider the flow of a viscous fluid in a corner bounded by two rigid planes. This led to the very surprising and counter-intuitive conclusion that an infinite sequence of eddies is in general present in such a corner flow. I published a paper on this subject in 1964; fifteen years later, the experimental evidence for the eddies was provided in Japan by the beautiful flow-visualisation experiments of Taneda (1979). There can be little more satisfying than having a purely theoretical prediction verified in this way after such passage of time.

Although not an experimentalist, I enjoy experimenting in an amateurish kitchen-sink sort of way. My first such published experiment (1977) concerned the complex dynamics of a thick film of viscous liquid (e.g. golden syrup) spread uniformly on the surface of a horizontal cylinder in rotation about its axis. This problem first came to my attention through a lecture given by V. Pukhnachev at an ICTP Summer School of Fluid Dynamics in 1974 (a school that contributed to the modern acceptance of fluid dynamics as a legitimate and reputable branch of theoretical physics!). There is a wonderful surprise awaiting anyone who tries this experiment: the fluid organises itself in a sequence of rings more or less equally spaced along the cylinder; each ring has a single depth discontinuity which rotates with the cylinder, but more slowly. The experiment encapsulates many aspects of viscous flow which are central to fluid dynamics—existence of steady flows, stability of these flows, steepening of waves, formation of discontinuities, and the associated formation of free-surface cusps, a phenomenon of great current interest through its relevance to the mixing (at the microscopic level) of fluids like oil and vinegar. Think of this the next time you prepare a salad dressing, or the next time you stir honey into yogurt; having visualised the mixing process, the product will taste all the better!

JOIN A GOOD GROUP

Marcos Moshinsky
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Mexico

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My interest in science started rather late in my life as in my primary and early high school I was just an average student.

Though my parents had a great admiration for knowledge they emigrated from Russia to Mexico in 1927 and were too busy making a living to be able to dedicate much time to continue their education beyond high school.

Arriving though at the third year of Secundaria (equivalent to the end of Junior High) I had a professor of mathematics who required an entrance examination—and to my astonishment I got the highest grade of the class.

This of course interested me in the field and I finished the course way ahead of the other students.

The following years of high school in Mexico required a definition of the interest of the student in subject matters with the view that they could enter the appropriate courses in college with some basic preparation. I chose mathematics and physics and was thinking of engineering as my future profession. It turned out that on finishing High School I had some psychosomatic illness that the doctors where not able to diagnose but suggested that I stop studying for a year to diminish the mental strain.

Thus I spent a year (1939) in New York as a worker in a knitting factory (on Broadway and 12th street) using part of the time to improve my English.

On my return to Mexico, on the eve of the Second World War, I learned that the National University of Mexico (UNAM) had established in 1938 a Faculty of Sciences among whose fields where mathematics and physics.

I entered the Faculty in the fields mentioned in 1940 and, still in my second year, I received an appointment as an assistant researcher at the Institute of Physics of UNAM, which was also founded in 1938.

My main job at the time was to attend a cosmic ray counter as that was the field of the Director of the Institute, a former student of Dr. Manuel Sandoval Vallarta, one of the pioneers in cosmic ray research.

My work did not interfere with my studies and in 1944 I received the equivalent of my Bachelor's degree.

At the time three distinguished American mathematicians spent long periods in Mexico as most of their students where in the armed forces during the Second World War. They were George Birkhoff, Norbeert Wiener and Solomon Lefschetz. It was Lefschetz, working in Princeton University, that suggested that I do my graduate studies in his University with Eugene Wigner then already an outstanding physicist and later a Nobel Prize Winner. He helped me get admitted in late 1945 and also obtain a fellowship, and I got my Ph.D. in 1949.

In those years Princeton was the Mecca of Theoretical Physics not only because of professors like Wigner, Wheeler and Bargman but also because of the fact that, in 1947, Robert Oppenheimer became the Director of the Institute of Advanced Study, a mile away from the University, and brought with him some of the best younger physicists and mathematicians of the time.

Thus my stay in Princeton was very fruitful and on my return to Mexico I was able to help the development of Theoretical Physics at UNAM.

During my stay in Princeton it became very clear to me that the future standing of nations in the international context would not be given so much by their industries or their arms, as by the knowledge and use of science by its inhabitants.

On my return to Mexico I wanted my country to be aware, of this situation and increase the scientific knowledge of its population as well as the use of the same.

Thus during more than 40 years I gave an undergraduate and a graduate course at my University, the first one usually on quantum mechanics that I considered the basic one in modern physics, and the second usually related to the research I was carrying out at the moment.

In that period I directed close to 30 thesis for Bachelor Degree in Physics, 15 for Ph.D. in the same subject and, as I became more well known internationally, more than 15 foreign postdocs came to Mexico for a year or more to collaborate on research with me.

My physics publications include more than 275 articles in refereed international journals and five books as well as many papers in newspapers mainly on the impact of science and education on society.

My permanent association has always been with UNAM though I have traveled widely to many other institutions for periods from a year to a few weeks. I never felt that working in Mexico was a hindrance to my scientific work. In fact it was beneficial as I did not have the pressure to work on subjects that were the rage in advanced countries, but could concentrate on the ones that were decided as important by myself.

In fact a few of the subjects that I considered worthwhile became popular after I had written a paper on them, such as the role of symmetries of canonical transformations in quantum mechanics; the simplifications of nuclear structure calculations with the help of harmonic oscillator transformation brackets; the relativistic many body problem and the Dirac oscillator. My advice to a young physicist and, also to a young scientist in any field, is not the example of Einstein to work in a light-house far from the pressures and distractions of the main institutions of learning, but rather choose a university or research group that is just beginning to be able to contribute to its transformation into a first-rate establishment.

MY SCIENTIFIC LIFE

David B. Mumford
Brown University, USA

Some of the decisions about your life seem to happen naturally, without any period of soul searching; others can be prolonged for years, maybe until the decision is made by circumstances rather than personal choice. I was lucky because for me, the choice of going into science was never an issue: I wanted to know how the world worked and science has seemed the 'magic' which is most powerful. There was another factor. When I was quite young, I interrupted once a painter who was an old family friend, at work on his canvas. I asked him whom he was working for—something I had just been told about—and he said 'myself'. Then it hit me. Why would anyone work for someone else if they could get paid for doing what they loved? This revelation has twists whose importance has only gradually dawned on me. One major aspect is that if you're working for someone else, there is always a deadline, a time when they want to see 'results'. If you work for yourself, a project can take 10 years—or a lifetime. It leads you in different directions if you think in those time scales. Andrew Wiles, who proved Fermat's conjecture, holed up in his attic for 10 years without publishing very much. This kind of freedom is rare in life and should be treasured. Funding agencies, pursued by accountants and politicians, are always trying to take this away from scientists. This is one reason why teaching is a good mix with research (especially in mathematics): you always have some useful work to do, even in the lean periods when new ideas are scarce.

In High School, I knew it was science that I loved the most, especially physics and astronomy. It was in college that I gradually switched to mathematics. One reason is that I tried to learn quantum field theory and failed. What I learned was how different physics and mathematics had become. From ancient times until 1930 or so, they were close partners and many people crossed the boundaries and worked in both fields. Then an odd thing happened: quantum field theory came up with a formalism which made perfect physical sense but for which no coherent mathematical framework could be discovered. Physicists found that by elaborating their physical intuitions (e.g. in Feynman diagrams), they could deepen their ideas and even come up with convincing heuristics for teasing out of their theories numerical predictions. Mathematicians occasionally found fragments of this theory which they could work out logically but never the whole. As a result, mathematicians regard physicists today as cowboys, who string together wild lines of reasoning with the faith that nature makes sense so they can't go wrong. Physicists, however, regard mathematicians as 'OCD' (Obsessive-

Compulsive Disease) patients, working forever on minor details to make an intuitive idea rigorous.

Having said this, my choice in the middle of college was mathematics. I had a Professor, Oscar Zariski, who had a remarkable skill. When he talked about his field, algebraic geometry, in which the major actors are geometric objects going by the name of 'varieties', he could make them seem alive! He would go to the blackboard and say, "Let V be a variety", writing V on the blackboard. Nothing else, but what you were convinced of from his bearing and his voice was that he was seeing into a world in which V was right there, living and breathing, with its qualities and characteristics just like people are. It was his secret garden and I knew I wanted to see into it too.

And so, after a while, I did. This is the quality of mathematics that I love the most: that it creates worlds which are not physically real but which are as colorful and idiosyncratic as anything on earth. Questions like "which moduli spaces of curves have positive geometric genus?" which are, on the surface, gibberish become as meaningful as "which countries in Asia are Muslim?" I fully believe that one of the most important characteristics of a successful mathematician is a kind of suspension of disbelief, the ability to think about these worlds having the same degree of reality as the usual tangible one. I found, anyway, in college that this is what happened to me and I wanted to be a Magellan and tour as much of this world as my mind and lifetime allowed. (Of course, it's nice when exploring this math leads you to colleagues in parts of the real world that you haven't explored: in my case, it took me to India).

So 'moduli spaces' and many sorts of allied questions in algebraic geometry became my thing and I spent 25 years or so working on ways to penetrate into their continent using any mathematical tool that might help. An interesting twist, which shows you the strange inter-relatedness of science, is that string theorists have taken up these moduli spaces and they have become major ingredients of this very speculative branch of theoretical physics.

I've also had a second interest. I was very interested in college in how the brain works. At that time, both the psychology and the neuroscience of the brain were still rudimentary. One had fragments of insights: Helmholtz's psychophysics, Freud's psychoanalysis, Cajal's amazing camera lucida drawings of networks of brain cells, Penfield's experiments on exposed cortices of awake epileptic surgery cases. But some of the most basic things about the physiology and anatomy of the brain, drugs that affect the brain, fMRI scans to correlate brain activity with different mental tasks were all unknown. And the transistor had not been invented so you can imagine how hard it was to build a computer. (I had built one from 100 relays as a science project in high school which burned up its paper tape trying to multiply 2 digit numbers).

In the early 80's, a colleague Jayant Shah and I, inspired by a brilliant British scientist David Marr who had died of Leukemia, convinced each other to take a chance and try working in the field of computer vision. The problem Marr had posed was this: what is the right mathematical/computer science model for understanding what the brain does when we parse a visual signal from the world and name the objects present in front of us? This seemed like a good way to take up the issue of 'how the brain works'. An interesting thing happens when you try to reproduce on a computer some seemingly simple skill like counting the faces present in a camera image. All the simple things you try fail! Somehow these 'simple' mental tasks have a subtle hidden layer which is not easy to pinpoint. It's easy to devise algorithms that work on, say 50%, of the faces but which don't take into account so many of the complications that human brains can handle effortlessly: changing illumination, pose, expression, facial hair, etc. And it finds 'faces' in completely weird places that are simply not there.

We worked on one mathematical approach to this, using a variational formalism to combine 'evidence'. But the breakthrough for me was discovering the statistical approach which Ulf Grenander had pioneered. I now have the firm conviction that what the brain does in both its smallest and biggest tasks has little to do with logic. Instead, it is engaged in statistical inference, especially the variety known as 'Bayesian statistical inference' in which long experience of the patterns of the world can be integrated into the analysis of the current percept. This approach is now making great strides and I have some confidence that Marr's problem may be solved in the foreseeable future. What is not so clear is such an approach is implemented in the brain, in what some call the 'wet-ware' of our cortex. Our brain is really constructed on utterly different lines from a computer, so nothing can be carried over by direct analogy. This is a frontier for biology beyond the genome.

I'd like to add a word on the split between pure and applied mathematics. Having worked in both areas, I am acutely conscious of the fact that these two fields do not communicate very well. As with physics and math, this split occurred in the mid 20th century when pure math began to develop intensely its most abstract branches (e.g. algebraic topology) and applied math began, with the advent of computers, to focus on numerical methods instead of classical analysis and explicit formulas. Talking to my colleagues today, I see the central issue as this: in pure mathematics, the gold standard is the proof of *theorems*. The success of a theory is measured directly by the depth and difficulty of the theorems to which it leads. In applied mathematics, the greatest achievements are the creation of new and effective *models*, finding the mathematics that simplifies the complexity of the real world just enough for the math to clarify what's happening but not so much that it cannot generate predictions that are relevant to the scientists. My hope is that this gulf will gradually close as a new generation of mathematicians takes over, who still love the formal beauty of the subject but are aware of how much calculations and applications can lead you to new and wonderful discoveries.

THERE IS MUCH TO DO AFTER TWENTYFIVE

Yoichiro Nambu
Enrico Fermi Institute
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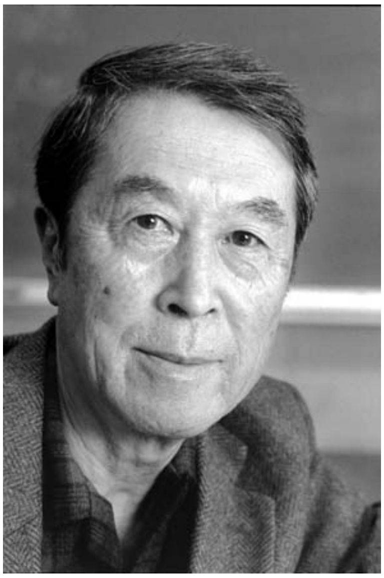

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I grew up in a conservative provincial town in the part of Japan dubbed the Buddhist kingdom. My father had run away from his parents, refusing to carry on their family trade of keeping a store of utensils for Buddhist rituals. He went to Tokyo, where I was born, to study literature and aspiring to become a writer, but soon he lost everything due to the great earthquake of 1923 that destroyed the capital. So he reluctantly came back to his home town and ended up as a high school teacher. He did not have training in science, but insisted that a writer must know something about all human activities. In my infancy he gave me books and magazines on science for children. Thomas Edison became my hero.

During my grade school days I used to visit my grandparents' home. An uncle of mine had died of tuberculosis there. I suppose he did not get higher education but apparently dabbled in science. I found a catalogue of scientific instruments made by a company in Kyoto, well known to this day. I enjoyed going over the pages, and was attracted by a slide rule. My father bought one for me as a New Year's present. I still have it. One day I also came upon some manuals and parts for radio circuits. The enigmatic diagrams and formulas captured my imagination. I tried hard to decipher them although I had of course no knowledge of electromagnetic theory. Nevertheless I got immense pleasure from guessing this way and that way what these formulas meant. Around that time a radio station opened in our town, but we did not have a radio set. So I spent a summer month building a crystal radio with my uncle's legacy. An annual nationwide high school baseball tournament was a big event then (it still is), which was broadcast over the radio. I cannot forget the thrill of listening to the final game, a duel between two great pitchers. The game went on for twenty-five innings without a score. (This number will show up again below). It still stands as the longest in the history of the tournament, as somebody told me recently after a Web search.

When I was in high school, I again discovered in my grand parents' home some notes on calculus which had been meticulously taken by another uncle of mine in his college days. I intensely studied them, and got a taste for higher mathematics. A prestigious publisher in Tokyo had started a series of booklets on specialized subjects in various disciplines. They were readily available at local bookstores. My curiosity was aroused by the title of one of them, which read "procession and procession formula" (matrix and determinant). I managed to buy a copy. Although it became too difficult for me towards the end, I learned, among other things, that Japanese mathematicians in the seventeenth century had developed the theory of determinants. High school physics and mathematics were not so interesting. But I was very pleased with myself when I figured out how the rainbow forms and then was able to calculate its angular size.

As the 1930's advanced and I grew older, life in Japan became more and more unpleasant because of the irrational and oppressive nationalism. Wars and assassinations came one after another. I have fond memories of my college days in Tokyo. The tide of intolerance had not quite reached it yet. It was my first experience in liberal and carefree life, and it was also an escape from threatening reality. The atmosphere of the self-governed dormitories exerted a great influence on the naïve country boy that I was. I acquired many interests. Regarding physics, I learned the physicist's way of reasoning by watching my more knowledgeable friends arguing with each other. Still I flunked thermodynamics. I thought it was the most profound and conceptually difficult subject in physics. A math professor gave me high stimulus, both sweet and bitter. An admirer of Poincaré, Galois and Abel, he loved to talk about them during his class again and again, but in the next breath he would scare us saying that you would be nobody unless you accomplished something by the age of twenty-five. There was also a motto going around among army cadets that man's life is twenty-five. What am I to do? I could not imagine myself five years into the future. In the end, however, I decided to go into physics anyway partly because I thought it was most suitable and challenging to me, and partly because of the recent fame of Yukawa.

I was fortunate to have lived through the war years past the age of twenty-five, even profited, in hind sight, by my service in the army radar research, and to have been able to come back to academic life and get initiated into particle physics by my office mates, at the very moment when they were working with Tomonaga to develop the renormalization theory. So I owe a great deal to my father, uncles, friends and others.

HOW I BECAME A SCIENTIST

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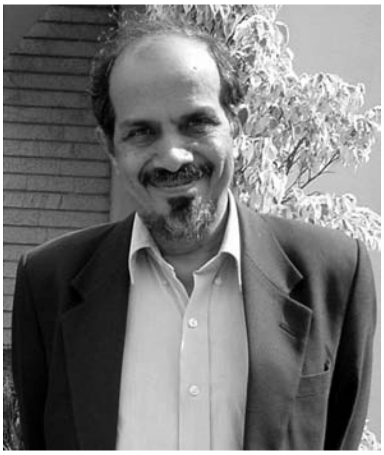

It actually started at home. My father was among the early graduates in science in the small town he came from, and went on to study with the great Indian physicist Meghanad Saha, who was then teaching at Allahabad (more than a thousand kilometres from home). Returning with a Master's degree, father taught at Central College in Bangalore, and, in his later years, wrote extensively about science in Kannada, the language of this part of India. He taught me very little physics or science directly, but his example and attitude—scientifically modern, socially liberal and culturally conservative—taught me something more basic, namely *pride*. My mother did not go to school beyond age ten, but she was an extremely well read and cultivated person— also proud. In the prayers she taught us to recite before going to bed the only things that we asked from God were intelligence and knowledge.

At school I was fortunate to have some great teachers. One of them managed to get the late C. V. Raman, Nobel Laureate, to visit us. Raman was then the biggest name in Indian science, and he spoke with such wit and verve about his work that he had the audience in the palm of his hand—both kids and teachers. Mr. KVR, the teacher who arranged the Raman visit, also taught me to value writing—briefly and honestly. In those numerous examinations we used to have, there were often questions of the kind 'How many planets are there in the solar system?' I would just answer: 9, and would be severely rebuked by my other teachers for not *naming* the planets as well. This I thought was unfair, for the question had not asked for the *names*. Mr. KVR stood by me on such occasions, and so grew a bond between us—never personally close, but full of affection on one side and regard on the other. But another teacher became personally close, and often took me and a few other students out—for coffee, snacks and long chats. He once casually presented two books to me. One was the *Lives of Great Scientists*, and it opened my eyes to the strange intellectual world of (western) science that I immediately found fascinating. The other book was a Kannada trans-creation of Lewis Carroll's *Alice in Wonderland*, which I fell in love with. How did Mr. BLA know so unerringly what books would make an extraordinary impression on me?

By the time I was to enter university in 1949, physics was one thing that I was seriously thinking of doing, but the vast (bloodless) political and social revolution that was then sweeping south India made this impossible. I eventually went to the Government Engineering College at Bangalore to study mechanical engineering. But the most inspiring event of those years was a visit I made to the Indian Institute of Science (IISc) when they had an Open Day.

In the quadrangle of the Department of Aeronautical Engineering (which had just recently been started) stood a lovely, World War II Spitfire, loaned for the occasion by the Indian Air Force. That was my first close encounter with an aircraft, and it opened another world for me. What struck me at that time was how smooth and graceful the exterior of the Spitfire looked (in particular its beautiful elliptic wings), but how complicated it was if I looked at the insides—which seemed like a jungle of cables, pipes, ducts, valves and so on. It seemed astonishing to me that beneath those graceful curves and surfaces (which I took to come from mathematics) lay hidden a bewilderingly complex technology—and I marvelled at those extraordinary people who had apparently mastered both.

So when I got my bachelor's degree I wanted to do aeronautics, but this was very unfashionable at the time. At my father's suggestion I consulted a family friend at the Institute. He brusquely told me not to be a fool: the right thing to do was to join the Indian Railway Service or Burmah Shell (the equivalents those days of a fat software job in today's Bangalore). I went back dejected to my father with our friend's advice, but he only asked me very simply, 'So what do you want to do?' I said I was bent on aeronautics. He just said, 'Go ahead, then.' So a matter that I had thought might have to be discussed over hours was settled in two minutes, and I ended up at the Indian Institute of Science—where I have stayed in some capacity or the other for most of the last fifty years.

At the Institute one more world was opened for me by Professor Satish Dhawan, who represented on the campus an exciting breath of fresh air from the New World, for he came with a Ph.D. from the California Institute of Technology (Caltech), and was informal in manner and serious about work (just the opposite of most other faculty at the Institute at the time). Somewhat like the Spitfire, his laboratory was a seamless mix of science and technology. He had big compressors running the supersonic tunnels he had built, but (in those early years of the electronic age) many of the measurements in those tunnels were still made with lenses and galvanometers. He went on to become a great scientific leader in India, building a space research organization that grew to be a spectacularly successful technology development enterprise. But Dhawan combined his nationalist commitment to big science with a deep love for little science. I learnt from him how to do research even when one did not have all the equipment needed. His laboratory was full of beautiful little 'gizmos', as he called them, and we jointly added some more to the collection, including a simple but effective one-dollar box camera for fast recording of oscilloscope traces, some of which ended up in a paper in the *Journal of Fluid Mechanics*.

At the end of two years Dhawan told me that to learn anything more I would have to go to Caltech. Strange as it may seem now, I was not at that time among those who were keen on going abroad; I was probably uneasy about doing the 'fashionable' thing, and my crazy circle of very intelligent friends in Bangalore had this extraordinary respect for those who stayed in India to do science—even if that meant not much science was done. Nevertheless I ended up at Caltech, and immediately realized that Dhawan was right to insist I go. I worked with Hans Liepmann, who had earlier been Dhawan's own adviser there. It took me a year to get used to my transplantation, for in the 1950's Bangalore and Pasadena were poles apart—geographically, scientifically, culturally and economically. But the following five years were some of the most intense in my life, because Liepmann and Caltech in their own friendly ways opened my eyes to one more world, that of international science done at the very frontiers of current knowledge. They generously accommodated an eccentric Asiatic, vegetarian fads and all, and later went so far as to make him feel Pasadena could be second home to him if not his first.

At any rate I returned to Bangalore at the end of 1962, and apart from visits now and then to other parts of the world rediscovered my (first) home here. Over the forty years since then I have tried to keep a balance between building and doing, and would not trade it for anything else—although there have been moments of frustration every now and then. I soon became a confirmed fluid dynamicist, and was looking all the time for the most interesting research I could do in Bangalore with the facilities we had or could build ourselves. This was often a slow process, but it was sure, and in a few years' time there was a group of outstanding students and assistants who quickly became experts at whatever they were doing, and it all became very exciting. Much of this research was concerned with what I now like to think of as *flow transitions*. It is a matter of common observation that a fluid flow can be laminar or turbulent—you can see that when you open a tap in your wash basin. For some reason the changes that take the flow from one state to the other began to fascinate me. Were those changes slam-bang, abrupt—as when the flow past a plate crumples into a turbulent spot—and if so how abrupt, and why? Or were they gradual—and if so when, and why? Can the flow go back from turbulent to laminar? This 'reverse' transition was a strange beast at the time, and I remember well-known fluid dynamicists who came to my laboratory in Bangalore in the 1960's and 70's and declared that they could not believe it—in spite of what I thought were the most elaborate measurements and the most convincing explanations that I and my students had fashioned by then. And I found shock waves—another kind of transition, this time from supersonic to subsonic flow—equally fascinating. And that kept me busy for many years.

But in the 1970's and 80's I became convinced that the most interesting fluid-dynamical problem for an Indian scientist to tackle was the monsoons. So after many years of effort we succeeded in setting up a new Centre for Atmospheric (and now also Oceanic) Sciences at the Institute in 1982. A dynamic group of scientists at this Centre have gone on to do very interesting things on different aspects of the monsoons since then. I myself have had a long interest in clouds, often wondering why the type called cumulus (so often seen in Bangalore and elsewhere in the tropics) does not spread out like a rocket plume, but instead occurs in cauliflower-like heaps or rises high in 'towers'. After several unsuccessful attempts we finally found an interesting way to study cloud-like plumes in (of all things) a water tank! This sounds surprising, but we could put heat into this 'water cloud' by passing electric currents (adding acid to the water to make it conducting), and so simulate the release of latent heat in the real cloud. (This, I thought, must be the special thing that makes a cloud different from an ordinary plume). And the experiments showed nicely why clouds look the way they do. (Unbelievable as it sounded at first, it seems as if this also has to do with a kind of 'sub-transition', involving loss of partial order—a rather soft transition this time, though). When I was watching clouds as a kid, a stern elder had (good-naturedly) rebuked me, 'Hey! You sky-gazer! Better look at the ground too some time!' But I continue to do sky-gazing, and love making those fake clouds in the laboratory as well.

Looking back I think I have been fortunate in my teachers; they opened so many new doors for me and showed me fascinating new worlds. And I found that doing the unfashionable thing became addictive—and a lot of fun!

THE EXCITEMENT OF DOING SCIENCE

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At a time when purely commercial attitude prevails in education it has become necessary to state the obvious: that pure science, motivated as it is by the thirst for knowledge, forms the foundation of the superstructure of science and technology that has become the mainstay of our present civilization. But even more than that it needs to be emphasized that pure science today is a natural extension of the age old and continuing efforts of intellectuals to understand the mysteries of nature. The ancient sages searching for enlightenment went through extended periods of agony which only made the attainment of goal a matter of great ecstasy. Scientists have experienced similar moments of agony and ecstasy in their search for truth. Agony that you go through when you are searching for the elusive solution to a problem—a solution that you feel it in your bones, must exist. Ecstasy that you experience when you find it. Let us begin with an episode from the life of Isaac Newton, the founder of physics as we know it today.

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Now more than 350 years have passed since the birth of Issac Newton. The following account illustrates how his contemporaries saw Newton. This was in 1696, at a time when Newton had ceased to be a professional scientist and had become the Master of the Mint. However a true scientist never ceases to be one. For, someone brought to his attention a mathematical problem posed as a challenge by a famous European mathematician, called Johann Bernoulli. The solution to this challenge problem had defied all scientists for over six months.

The problem may be briefly described thus. Imagine two points A and B in a vertical plane, with A at a greater height than B. Connect the points by a smooth wire of *any shape.* Let a bead slide in a frictionless way from A down to B. Bernoulli's question was *to find the shape of the wire so that the time taken by the bead is the least.* If you think that the straight line being the shortest path connecting A and B is the correct answer, you have to think again. Indeed the answer is not so straightforward (no pun intended!) and as mentioned above the best brains in Europe had failed to crack the question.

It is said that Newton saw the question on his return from work one evening. He was intrigued by it and tried to solve it. It took him several hours but by early morning he had solved it. The curve that the wire should be shaped as is called the *brachistochrone.* To obtain the answer, one needs to use variational methods in calculus, a branch of mathematics Newton himself had invented. Having solved the problem, Newton sent the solution to the Royal Society, asking the President to publish and forward it to Bernoulli without revealing his authorship. When Bernoulli saw the solution, he however immediately saw that it could not have come from anyone else but Newton. His remark reportedly was: “I know the lion from his paw”.

This episode illustrates the agony and ecstasy a problem can bring to a scientist. Even though he was out of touch with regular scientific pursuits, the scientist in Newton was aroused by the challenge posed and he did not rest till he had solved the problem. Let us look at an episode from the life of another great scientist.

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In his early life Lord Kelvin was known by his family name Thomson. This story refers to Thomson and another young man Parkinson both of whom were competing for the top rank in their Cambridge examination of the Mathematical Tripos. In the end Parkinson topped the list and Thomson stood second with the rest of the pack far behind.

There was one particularly difficult question which only the two had answered correctly. What struck the examiner most was the similarity of their answers so much so that he suspected malpractice. Did one boy copy the other’s answer? He called Parkinson for an interview.

“Tell me, how did you manage to solve such a difficult question?” he asked Parkinson.

“Sir, I occasionally read research journals. I had come across a paper wherein the author had solved this problem. So I knew the solution beforehand.” He gave the reference to the paper.

The examiner who himself had taken the problem from that very same paper was impressed. He complimented the boy for going beyond the teaching syllabus and reading new articles. Dismissing him with a pat on the back he called Thomson and asked him somewhat aggressively: “I would like to know how you solved this problem. Parkinson who solved it saw the solution in a research paper. Don’t tell me that you also saw it there.”

“No Sir!” replied the future Lord Kelvin. “I wrote that paper.”

In scientific research it is the originality that matters. Today we do not hear about Parkinson who topped the examination. But Thomson’s work is part of our textbooks.

Examples of great scientists like these inspire us in our own humble efforts. Let me come down to Earth and share with you my own experiences that led me to opt for a career in science. These are experiences of my formative years, which schoolchildren can very well appreciate.

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That I liked maths and science was noticed by my father who made me acquainted with the recreational aspects of mathematics, with its wealth of anecdotes, puzzles and paradoxes. He did this either directly or by giving me books of this nature. He also encouraged me and my brother to do experiments. Our house in the university campus was spacious enough for him to provide a chemistry lab for myself and my brother to play with.

In those days it was customary for visiting faculty from other universities to stay with their local host and so we had mathematicians like N.R. Sen, Ram Behari, A.C. Banerjee or Vaidyanathaswamy staying with us on such visits. Even if I did not understand what they were talking about, the overall ambience did help in creating an aura about mathematics.

However, a crucial development, which helped foster a competitive spirit in me took place when I was in the VIII standard. My maternal uncle Moreshwar Huzurbazar, or *Morumama* as I used to address him, came to live with us in order to do an M.Sc. in mathematics. He was a brilliant scholar, having done very well at the B.Sc. exam of Bombay University. [Later in his life he was a professor and finally became Director of the Institute of Science, Bombay.]

Morumama discovered that I enjoyed doing mathematics. He also noticed that my father had two blackboards built into the walls for myself and my brother to write or draw as we wished. He found a new use for the boards. Once in a while he would write a mathematical problem or puzzle, under the title "Challenge Problem for JVN". The problem would remain on the board till either I solved it or gave in and asked for the answer (which, I am glad to say, happened rather rarely).

Morumama's problems were certainly outside my school syllabus: they called for analytical reasoning and 'trick solutions' which would light up for me some hidden aspect of mathematics. My lasting regret has been that no record has been kept of those problems. But so far as I was concerned, I developed an attitude of taking on the challenge posed by a difficult question.

Perhaps I should also mention that books like 'Men of Mathematics', 'The World of Mathematics', and 'Living Biographies of Great Scientists' played a key role in bringing to my impressionable mind the excitement and frustrations of creative geniuses. The anecdotes given in the beginning of this article tell us that science is not a drab subject to be memorized, but an arena of adventures. It is revealing to know about the pride and prejudices of great scientists, and to learn that they too occasionally made mistakes. But science has a self–correcting tendency that leads ultimately to the right answer. This was one motivating influence in my opting for a career in science.

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I was fortunate in having as my Ph.D. guide at Cambridge, the distinguished scientist Fred Hoyle. Although I was a raw graduate student with no previous experience of research at the time I joined him, he always gave me the impression that he was discussing science with me as his equal. Thus he would throw out some idea and ask me “What do you think of it?” Or, if I had some suggestion he would listen to it carefully. Thus when in early 1961, barely within six months of my joining him as a student, the Hoyle-Ryle controversy erupted, I was drawn into the discussions as a coworker of Hoyle. In this controversy, Martin Ryle, the head of radio astronomy at Cambridge, had claimed that his radio data disproved the steady state cosmological theory proposed by Hoyle. The steady state theory stipulated that the universe is unchanging in its large scale properties and is without a beginning and without an end. Fred believed that there were several loopholes in Ryle’s data and asked me to work on a counter-example to Ryle’s claim.

I recall working against time to perfect our counterexample, so as to be ready in time for presentation at the Royal Astronomical Society, London, where Ryle was scheduled to announce his results. We did complete the work on time. However, Hoyle discovered that a conflicting prior commitment prevented him from attending the RAS meeting. So he asked me to present the work myself. I was flabbergasted. How can I, an inexperienced student take on a distinguished scientist like Martin Ryle in a public debate? However, Fred explained that in science it is not the prestige that counts but how firmly confident are you of the correctness of your work. He trained me in the way I should present my work in the short time allotted and wished me luck. I came through the ordeal with flying colours.

This experience gave me a lot of self confidence. I feel that a young student can benefit a lot if asked to share responsibility of defending his work. The experience also brought home to me the fun that scientific research can be. Unlike Morumama’s problems whose solutions were known, here one gropes for the unknown and relies on the facts and reasoning to decide who *may be right*. All through my scientific life it is this feeling that has sustained me through agonizing moments of searching for the answer, whether in my work in cosmology, gravity, electrodynamics or theoretical astrophysics. The agonizing efforts are sometimes, not always, rewarded by ecstatic moments of success. Not always, because science is a never-ending game you play with nature, in which sometimes you lose, sometimes you win.

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I should close this account with a brief reference to another career option which was open to me. When in 1957, before leaving for Cambridge for the Mathematical Tripos, I called on Mr R.P. Paranjpye, who had been Senior Wrangler at Cambridge of the 1899 vintage, he asked me: “After doing the Mathematical Tripos, will you go for the IAS?” For he was voicing a view common in those days, that a Cambridge degree was a good stepping stone for the Indian Administrative Service. When the great RPP distinguished himself at Cambridge he was expected to join the Indian Civil Service. But he opted for a teaching career.

My answer to Mr Paranjpye was likewise quite definitive: “No Sir, I wish to enter a career of teaching and research. I find it more exciting.” I have never regretted that decision.

COMBINING MATHEMATICS AND PHYSICS

Sergey P. Novikov
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Sergey Novikov is a Distinguished University Professor at the University of Maryland, College Park, U.S.A., and has worked in the Department of Mathematics and the Institute for Physical Science and Technology since 1996, and actively interacts with several Russian Institutions. He was elected a Member of the Russian Academy of Sciences; Foreign Associate of the National Academy of Sciences, U.S.A.; Foreign Member of the Accademia dei Lincei, Italy; Member of the Pontifical Academy of Sciences; Academia Europea; Fellow of the European Academy of Sciences in Brussels; Honorary Member of the London Math Society; Serbian Academy of Arts and Sciences; and Doctor Honoris Causa of the University of Athens and the University of Tel Aviv. He was awarded the Fields Medal of the International Mathematical Union, Lenin Prize of the USSR, and the Lobachevski Prize of the Academy of Sciences of the USSR. Novikov succeeded Kolmogorov as President of the Moscow Mathematical Society in 1985 and served 11 years, leaving this position to Arnold in 1996. This society played a fundamental role as a central forum for Moscow mathematicians in the XXth Century. He was advisor to over 35 Ph.D. students, many whom became outstanding scientists.

Novikov grew up in the prominent Novikov-Keldysh scientific family. His father was Petr Novikov (1901-1975), famous for his classical works in group theory, mathematical logic, descriptive set theory, and the inverse problems of Newtonian gravity. Sergey's mother Liudmila Keldysh (1904-1976) was a full professor in mathematics and a well-known expert in descriptive set theory and geometric topology. She was an outstanding woman whose enormous energy was needed in order to raise and educate five children, to help her extremely talented, but physically unwell husband, and to combine this hard job with active scientific work. It was not without her contribution that her husband, brother, and two sons became leading scientists in the different areas of mathematics and physics. In the late 1920's she helped her younger brother Mstislav Keldysh, who had brilliant mathematical talent, to become a mathematician—against the will of their father Vsevolod Keldysh (1878-1965) who badly wanted to educate his son as an engineer (he was a prominent building engineer himself, mentioned in the Nikita Khrushev book). Finally, M. Keldysh (1911-1977) became a leading applied mathematician in the USSR and was responsible for the creation of the first "sputniks" as the Main Theoretician, in the newspaper terminology of the late 1950's and 60's. The

Bolsheviks hid the real names of these people from the world community, but many mathematicians and physicists in Moscow knew who was who. After the sputniks he served as head of the Academy of Sciences in the USSR during the years 1961-1975. Sergey's older brother Leonid Keldysh (b.1931) is a famous solid state physicist. There were no musicians, artists and actors in the family; nobody wanted a party career as a communist, so science was a very natural, simple choice. Sergey realized in Middle School and High School that he could easily learn mathematics and effectively solve mathematical problems. However, he hesitated, thinking that there were already too many mathematicians in the family. He finally decided to choose mathematics for a professional career in 1955, entering the Math Department of the Math/Mech College at Moscow State University, whose heads were the famous mathematicians A. Kolmogorov (1903-1987) and I. Petrovski (1901-1973). It was the best mathematical university in the world at that period and all mathematics was unified there. There was an important tradition and skill at the Moscow school—how to attract students very early to creative science. Especially successful was Kolmogorov: some of his students of that generation, i.e. V. Arnold (b.1937) and Ya. Sinai (b.1935), became famous very early working with him.

After taking many different courses in the 1955/56 years, S. Novikov decided to choose Algebraic Topology: there were rumors about the great, complete breakthrough in this area made in the West but missed by the Russian school. This was far enough removed from the other mathematical fields of members of his family (as he wished). Clever topologists of the younger generation, such as professor M. Postnikov (b.1927) and A. Schwarz (b.1934), who was only a postgraduate student, started to teach the new topology. S. Novikov was impressed by this area and joined their courses and seminars in 1956. However, only after a very hard period of learning the recent achievements made by the best western topologists, was he able to start his own research in topology. Besides that, the Soviet Iron Curtain became more transparent in the late 1950's. Some leading western scientists (such as J. Milnor, F. Hirzebruch, S. Smale, H. Cartan, M. Atiyah, and I. Singer) visited the USSR. They enormously helped the young Russian scientists, especially Novikov, in the early 1960's.

Novikov was awarded the Lenin Prize of the USSR in 1967 and the Fields Medal of the International Mathematical Union in 1970, which were the highest scientific awards (he was not allowed to attend the corresponding ceremony in Nice as punishment for a letter defending some dissident who was arrested and sent to the mental hospital). His deepest topological theorem (1965) established topological invariance of certain analytical expressions made out of Riemannian Curvature (the so-called Pontryagin classes). These quantities enter practically all important formulas of the analysis and geometry on manifolds. He constructed the effective method of classification of multidimensional manifolds (the Browder-Novikov Theory, 1961-64) and developed a new algebraic technique on how to calculate crucial topological quantities—such as homotopy groups and cobordisms. Under the influence of his friends who started the new theory of "hyperbolic" dynamical systems (such as Smale, Anosov and Arnold), Novikov created a qualitative theory of foliations of the three-dimensional spaces by two-surfaces (1964/65). This theory became very well known in the math community. Starting as an undergraduate student in the Algebra Chair of the University, Novikov became a graduate student (aspirant) in the Department of ODE in the Steklov Math Institute in Moscow. In 1964 he was invited to work permanently in this institute—in the

Algebra Department where everybody worked in Algebraic Number Theory and/or Algebraic Geometry but wanted to learn algebraic pieces of topology. As a consequence, Novikov developed a broad interaction with people working in all of these areas. In the early 1960's he started to pay attention to the activity of his friends in dynamical systems and classical mechanics, attending the Arnold Seminar. Since 1963 he developed very active contacts with people working in Functional Analysis and PDE, participating in the famous Gelfand Seminar. Algebraic Topology became more and more popular in the math community in the 1960's. Several areas of mathematics started to use the various ideas and results of topology.

However, dissatisfaction started to grow in his mind in the late 1960's: why modern communities of pure and applied mathematicians lost contact with modern theoretical physics—the greatest theoretical science of the XX Century? Certainly this area used (and sometimes produced) many new mathematical ideas. Its language was especially designed for the effective use of mathematics, for the formulation and rational understanding of the unbelievable quantum or relativistic laws of Nature. It demonstrated a great engineering skill and changed our society forever. In the late 1960's many physicists started to believe that in extreme conditions (like high energy, large magnetic field, strong gravity, low temperature) we cannot understand what is going on without topology. Novikov spent several years learning theoretical physics. In 1971 he began to work at the Landau Institute for Theoretical Physics, helping them to learn and apply new mathematics like topology and dynamical systems. Several important works were done by well-known physicists in the 1970's, where consultations with Novikov and his group were essential. He also looked for new areas of his own research: special attention was paid to concrete problems whose solutions required use of modern mathematics (like topology, dynamical systems, algebraic and Riemannian geometry), unusual for the community of mathematical and theoretical physicists. One of Novikov's best discoveries was done solving the periodic boundary problem for the famous Soliton Systems (such as KdV). The unusual behavior of the localized "soliton-type solutions" had already led to the famous Inverse Scattering Transform in the late 1960's, but nobody knew what to do with the periodic problem. The famous "finite-gap solutions" invented by Novikov in 1974 led to the first great combined use of dynamical systems, algebraic geometry, and analysis on Riemann surfaces in modern PDE problems. All of his group and many other scientists participated in the development of these ideas important for Nonlinear PDEs, and the Spectral Theory of Periodic Operators and Quantum Mechanics. A large program of analytical and numerical investigations was realized by him and his group in this area. Another series of Novikov's achievements was the discovery of new topological phenomena in the Calculus of Variations and Field Theory in 1981 (the Morse-Novikov theory): he observed that in several classical and modern physical systems, such as the top in the constant gravity field, the charged particle in the field of the Direct magnetic monopole and its field-theoretical analogy, the action functional is, in fact, multi-valued (i.e. it is a closed but nonexact one-form on the space of fields, like the angle coordinate on the plane). It led to the "topological quantization of coupling constants" for the Wess-Zumino-Novikov-Witten Lagrangians, generated new ideas in the topology of manifolds, and in the calculus of variations for periodic trajectories. Recently Novikov has actively worked with his group constructing some remarkable discretization of several fundamental systems like 2D Schrödinger equations and the famous Cauchy-Riemann equation, revealing a deep connection of the Quantum Scattering Theory on Graphs with Symplectic Geometry, studying electrons moving in 3D single crystal normal metals with complicated Fermi Surfaces (like gold) in the strong magnetic field (like 10^2t). This theory predicts some nontrivial topological phenomena in electrical conductivity.

IT IS ALL CURIOSITY

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What first stimulated my interest in science was an over-whelming curiosity about how the world worked. I first remember being aware of this whilst walking to school, maybe at 9-10 years of age and noticing that leaves on the same plant seemed bigger when they were growing in the shade compared with when they were growing in sunlight. This got me thinking, and without really understanding what was going on, I thought it might be something to do with the fact that leaves in the shade got less sunlight and so needed to be bigger. After that it seemed that the world was full of questions which I assumed someone knew the answer to, but certainly I did not. Questions like how does a fly's wings move to keep it in the air, how could the same animal make both a caterpillar and a butterfly, why did different chemicals differ in colours, how was it known that all the stars were at different distances from the earth, and so on. I am still asking questions in science although they are more complex now, or at least the language I use to ask the questions is more complex. Which raises the question of what I think is key for keeping an interest in science. For me, two points are important. The first is keeping a real curiosity about the world and the second is a determination to find explanations for what we see. Without that curiosity and a wish to know answers, the passion for science is soon lost.

So where did my curiosity end up? It ended up wanting to know what controls the division of cells from one to two. All living organisms, including ourselves, are made up of cells—the basic unit of life. Cells reproduce by binary fission, and it is this division process which forms the basis for the growth and reproduction of all living things. When it goes wrong then disease can result, especially cancer. I studied this problem using a simple single celled organism yeast similar to the yeast used for making bread, beer and wine. Yeast is very simple and grows fast so the work can be quick. My colleagues and myself found a small sub-set of genes which controlled the division of yeast cells. We then worked out how the genes worked, that is what they did 'to divide' a cell. Having worked this out, we asked: did the same process work in other cells including those which make up our own body? So we then went on to show that the same genes working in more or less in the same way control the division of human cells as well. Given yeasts are so simple and humans so complex it is likely that the same genes control the division of cells in most living organisms.

Throughout all this work I was driven by curiosity, and the need to know the answer of what controls the division of cells. Not really very different to how I was as a boy thinking about leaf size walking to school.

PHYSICS: EXPLORING OUR UNIVERSE

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I was one of five children, and grew up in Aberdeen, Washington; a logging community in the Pacific Northwest. My siblings and I would often take walks along deserted logging roads that wound their way into the hemlock and fir forests surrounding Aberdeen. We would pretend that we were explorers, perhaps the first to ever walk through these mighty forests! Thanks to physics, this sense of adventure has never left me.

I suppose my fascination with 'physics' began at age six, when I tore apart my electric train in order to play with the electric motor inside. The non-event that was then crucial to my continued movement toward a career in science was that my parents did not scold me for this, but rather my father took the time to show me how the motor worked. He seemed fascinated with my fascination. As time went on, he would bring me other objects that he felt might interest me, such as a box of magnets from the electric power company, and boxes of parts from the telephone company. At age eight, he gave me the camera he had used as a child. Within an hour, it too, had succumbed to my curiosity. After this, my father brought home a mechanical watch with a set of jeweler's screwdrivers, suggesting that I see if I could take it apart and put it back together. Such was the sort of gentle nurturing that sustained my interest in science.

In grade school I learned almost no science, save what was provided in our 'Weekly Reader' magazine. I do recall learning about phonons there. However, middle school was different. Here we took two years of health and science, followed by one full year of science. My teacher for health and science, Mr. Miller was rumored to beat children who disturbed his classes, and I was quite frightened of him. He gave weekly quizzes in class based on film strips he had shown that week. During the first test, I was so scared I could barely write my name on the exam sheet. However, when Mr. Miller turned back our exam he said he was certain I could do much better than this, and asked if there was anything he could do to help. He had done enough, though this simple act of kindness and concern. For the next two years no one in any of Mr. Miller's five classes ever scored higher than I did on a single one of these tests.

In high school it was my chemistry teacher who had the most impact on how I thought about science. Mr. Hock had spent time as a graduate student in chemistry, and explained to his classes how one could learn about nature by asking the right questions and then finding the answers through experimentation. It was very different from my tinkering with high voltage electricity and gunpowder, but just as exciting. I should add, however, that few of my classmates appreciated Mr. Hock's efforts in this regard.

I attended Caltech for college, and was fortunate to be there when Professor Richard Feynman was teaching entry-level physics. Imagine, a brilliant man spending his time on freshman and sophomores! Many years later, when Caltech was offering me a faculty position, I thanked him for his educational gift. But in my junior year I began to lose sight of my goal as a physics major. Physics seemed like endless problem sets, which I didn't much enjoy solving. I began to find excuses for not doing these problems, and my grades began to slip. Then, just as I teetered between the career I was to have and the failure I might become, Professor Gerry Neugebauer invited me to work in his astrophysics research group. I soon found that research was very different from problem sets. You do have to do calculations, but they are calculations you *want* to do.

In the spring of 1967 I graduated from Caltech with a bachelors degree in physics. Perhaps the most important thing I had learned at Caltech was not any of the physics I had been taught, but that astrophysicists don't do experiments. They only build instruments and make observations. They can ask the same questions Mr. Hock had described to his high school chemistry classes, but they would not answer them by doing experiments, but by making observations. I wanted to do experiments. I wanted to control the systems I studied, and by doing so force them to give up their secrets! I thus switched my field of interest from astrophysics to condensed matter physics.

I went to Cornell for graduate study in the fall of 1967. This was an amazing time, as so much was happening around me. I took quantum mechanics from Ken Wilson, who was then learning all he could about systems with second order phase transitions, so that he could apply the ideas of re-normalization that had been developed by particle theorists to solid state systems. He was later to win the Nobel Prize for this contribution. But it was two solid state seminars that most affected me. They both new cooling mechanisms that I felt gave promise of allowing scientists to study nature in a strange new realm. Still in my first year of graduate study, I began building one of these devices, a ^{3}He-^{4}He dilution refrigerator, ultimately capable of cooling objects to within 0.015 degrees (Kelvin) of absolute zero. The other device, called a Pomeranchuk refrigerator after the Russian theorist who had first suggested the device, was expected to reach temperatures as low as 0.002 Kelvin. I designed my Pomeranchuk refrigerator during the winter of my second year of graduate study, in a hospital bed while recovering from knee surgery following a serious skiing accident. In less than three years I was to use these two devices to discover three superfluid phases of liquid ^{3}He. These phases are neutral analogs to superconducitivity, but in these cases the Cooper pairs that form the macroscopic superfluid condensate have internal degrees of freedom, spin and angular momentum, making their behavior much more complex. These were the first known examples of 'unconventional' BCS states, in this sense similar to the high temperature superconductors to be discovered 15 years later. It was for this discovery that I shared the Nobel Prize for Physics in 1996 with my two professors.

My career has involved discovering and understanding rare and exotic forms of order in nature. These include the three superfluid phases of liquid ^{3}He, two nuclear spin ordered phases of solid ^3He, which serve as model magnetic systems because of the simplicity of interactions leading to the order; and a phenomenon called 'weak localization' in which weak disorder leads to an ever increasing electrical resistance in conductors as the temperature is lowered. I also study the properties of glasses near absolute zero, some of the most highly *disordered* systems in nature. Even these exhibit a form of order at very low temperatures. To me, physics is not a body of knowledge, nor just a way of thinking about things. It is a lifelong pursuit of knowledge of how the universe behaves and how it evolves. It is the exploration of our universe. As long as I can ask questions to which there are no answers, and then find the answers through experimentation, I will always be an explorer.

THE JOY OF BEING A SCIENTIST

Jacob Palis
IMPA, Brazil

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I was probably attracted to Science at an early age without really being aware of it, since in those days one would hear about doctors, engineers and lawyers, not as scientists but as practitioners, all of them much present in my family. Indeed, since my early days in school, I loved to play with numbers and questions involving elementary mathematical reasoning. I also remember being very happy to talk to other kids and explain whatever I could about lectures, textbooks, homeworks and tests.

Later on, still in secondary school, sometimes a few classmates and I were even invited by the teachers to discuss some topics at the blackboard. It was a very stimulating atmosphere, made noteworthy by the presence of some foreign teachers that had emigrated at war or post-war time from Europe to Brazil, including mid-size towns like mine. Clearly, a stimulating atmosphere and competent teachers, either nationals or foreigners, were and still are crucial to the nurturing of students at all ages.

When I went to Rio de Janeiro at age sixteen to prepare my entrance to the university, again I was much stimulated and I recall that a classmate and a couple of teachers mentioned that I could consider getting a degree at the School of Mathematics or Physics. My horizon in terms of knowledge had increased considerably in Rio, but still I found the idea strange, for I believed that frontier mathematics, physics and chemistry were performed at the Engineering School, "certainly" at a much higher level than at the other schools just mentioned. During my course at the Engineering School, I did ask many questions to my professors and slowly but steadily the idea of learning in parallel more mathematics and physics grew in my mind. And so I did learn especially more mathematics. I obtained my degree in engineering in very good standing, but somewhat contrary to the wishes of my family, decided to pursue a Ph.D. in mathematics and to "experiment" the idea of becoming a scientist. Like that …

I just wish to proclaim to the young talented people: I became a much happier person! I felt since then much more engaged in helping to foster a better society, more equitable both socially and economically and not only in my own country but in others as well. More than ever the world is changing at a rapid pace driven by Science and Technology, moving rapidly toward the knowledge-based economies of the new century. So, in the present environment, scientific training allows much easier adaptation to innovative technologies, not to mention the excitement in taking part in their creation. This is particularly relevant to young people in an era where jobs appear and disappear in whole fields of activity. Actually, some frontier industries nowadays increasingly prefer to hire mathematicians, physicists, chemists, biologists and engineering scientists, no matter their specific final production of goods. So, you talented young people need not fear for the future and well being of your family by treading audaciously the intriguing path of Science.

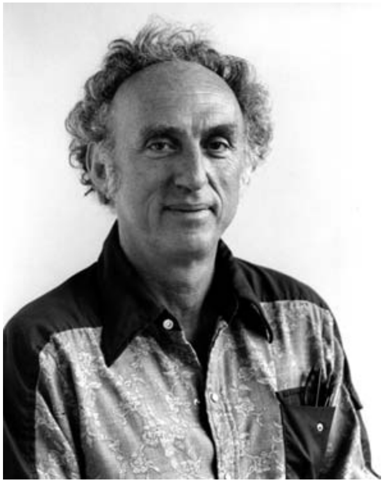
My account and views expressed above lead me to talk about a major world institution such as The Abdus Salam International Centre for Theoretical Physics—ICTP. Fulfilling a great vision and dream of its founder Abdus Salam, a Nobel laureate, ICTP has been promoting high-level research and training activities for four decades on a broad range of topics in theoretical physics and mathematics. The activities also encompass other fields in which physics and mathematics play a fundamental analytical role. It serves, and it was designed to do so, mostly the scientists from the developing world, but not only them. In fact, there are fields to which ICTP has contributed much by advancing them on a worldwide basis.

ICTP has become, through the decades, a crucial reference for the promotion of Science in the developing countries, especially mathematics and physics. More than ever, in the era of knowledge, we must engage ourselves on the one hand in contributing to strengthen it and on the other to spread its beautiful image in our nations to attract much needed young talents for the well being of our societies.

At the same time, ICTP is a symbol for the young talented people of how joyful it is to be a scientist. It was built as and continues to be a dream-place for the scientists of the whole world without divides, inclusive of the least developed countries from all continents. With charming and generous support from the Trieste community and the Italian government, UNESCO and the International Atomic Energy Commission as well as several Agencies and institutions, it turned out to be a tall palace for Science, with wonderful premises facing the Adriatic and a most stimulating scientific atmosphere. It embraces on a global scale my little world mentioned at the beginning of this article. And what can I say about the presence of the few foreign teachers in my former little world? I do sense that we are now all together in a community without borders, sharing the endeavor of strengthening Science for the betterment of humanity and enlightenment of our spirits.

DOING EXPERIMENTAL SCIENCE

Martin M. Perl
Stanford Linear Accelerator Centre, USA



The popular image of a scientist and how one does science is very distorted and that is what drives many young people away from careers in science. And so I want to tell you what I have learned in the course of fifty years of doing experiments in physics. I will summarize in 14 maxims what I have learned and it is these maxims that make doing experimental science enjoyable and exciting. I will use examples from my own life.

You must take account of your personality and temperament in choosing your science and your interests in that field.

I have a mechanical view of the universe, I am competent in mathematics but I don't excel in mathematics and so I have been an experimenter. I speculate about experiments that might be interesting but I don't work in physics theory. I like to work on equipment because I am mechanically handy. But don't try to fit yourself into any particular image of what a scientist should be. You don't have to be a mathematical genius, you don't have to be mechanically handy. You just have to want to find out new things about nature and you just have to have the strength to keep working on an experiment when no one knows the answer. The great joy will then occur when you are the first one to know the answer.

It is best to use your own ideas for experiments.

You can't always use your own ideas because you may be part of a larger science group with defined goals, but it is always more fun to work on your own ideas.

You don't have to be a fast thinker or a fast talker. In fact, it is best to avoid such people.

When you begin to get a new idea it may be badly formulated or even wrong. Beware of fast thinkers and fast talkers who delight in showing that your idea is wrong. This is because by working on a somewhat wrong idea you often can get a good idea. But this takes time and you need sympathetic and helpful colleagues, not fast-talking critics.

You don't have to know everything. You can learn a subject or a technology when you need it.

Science moves very fast these days and if you try to get into a new area you may think you will have to first spend all your time studying the subject before you get into it. It is best to jump fairly fast and then learn what you need from colleagues or books or courses or from experience.

For every good idea, expect to have ten or twenty bad ideas.

But expect that most of your own ideas will not work out, but when you get a good idea that works it is marvelous.

It is often impossible to predict the future of a technology used in engineering or science.

I was a chemical engineer before I was a physicist and in late 1940's I worked for the General Electric Company. I worked on an R&D project to make very small electron vacuum tubes so that radios could be made smaller and use less power. Meanwhile the transistor was invented at Bell Laboratories.

You must be interested in, even enchanted by, some of the technology or mathematics you use. Then the bad days are not so bad.

There will always be bad days when you do experimental science when nothing works or you discover that designs have to be changed. It is crucial that you be enchanted with some parts of the experiment so that you can get through these bad times.

Another advantage of being enchanted by the technology or the mathematics is that you will be more likely to think of improvements and variations.

This is obvious.

You may dislike, even dread, some of the technology or mathematics used in a large experimental or engineering projects, and you may be happy to leave these areas to colleagues. But don't be surprised if you have to get into one of these areas yourself.

Although I started my career as a chemical engineer these are many areas of chemistry that I don't like. But our present searches for fractional electric charge particles in meteoritic material uses much colloidal chemistry. I have had to learn it.

You should be fond of the technology or mathematics that you use, but not too much in love with the technology or mathematics. There may be a better way.

This is obvious.

You must learn the art of obsession in science and technology.

When working on an experiment it is important to be obsessed with it. When you wake up in the middle of the night you should be thinking about the experiment. But with all experiments there will come a time when you cannot improve it substantially or when someone else has devised a more fruitful experiment in the same area. Then you should end the present experiment and move on. This is the art of obsession in science.

In many areas of science it is getting harder and harder to have the time to do both experimental work and original theory. In some areas, such as particle physics and astrophysics it is usually impossible.

I believe that in many parts of science the design and building of modern experimental apparatus has become a full time job, as has doing original theoretical work. It is sad, but there is usually not enough time in the day and the night to do both.

Theory should be a good companion to the experimenter, inventor and engineer. sometimes leading, sometimes following. The experimenter or engineer should not let theory set the fashion or dictate what is important.

Theory, even very speculative theory has come to dominate the thinking and presentation of science inside and outside the science community. These days, experimenters do experiments because a theory, often a very speculative theory, suggests the experiment. If you are doing the experiment anyway you will not waste much time in also testing the speculation, but you will be happier and find out more about nature if you do the experiments in which you believe. In the end the validity of science depends upon experimental results and measurements.

RESEARCH GETS MORE EXCITING WITH TIME

William D. Phillips
National Institute of Standards and Technology, USA

Almost as far back as I can remember, I was interested in science. By the time I was about 5 years old, I had assembled a collection of bottles of household substances as my "chemistry set." I was examining almost anything I could find with the microscope my parents gave me. Science was only one of the passions of my childhood, along with fishing, baseball, bike riding and tree climbing. But as time went on, Erector sets, microscopes, and chemistry sets captured more of my attention than baseball bats, fishing rods, and football helmets. I remember that sometime before I was 10 years old, I decided that I wanted science to be my life work, and that I was beginning to appreciate, in a very incomplete and naive way, the simplicity and beauty of physics.

I had a laboratory in the basement of our family home where, ignorant and heedless of the dangers of asbestos, electricity, and ultraviolet light, I spent many hours experimenting with fire, explosives, rockets and carbon arcs. And while my parents were not directly involved in my scientific interests, they tolerated my experiments, even when the circuit breakers all tripped because of my overloads. They were always encouraging, giving me the freedom to explore, to learn, and to have fun.

In high school, I enjoyed and profited from well-taught science and math classes, but in retrospect, I can see that the classes that emphasized language and writing skills were just as important for the development of my scientific career as were science and math. I certainly feel that my high school involvement in debating competitions helped me later to give better scientific talks, that the classes in writing style helped me to write better papers, and the study of French greatly enhanced the tremendously fruitful collaboration I was to have with Claude Cohen-Tannoudji's research group.

The summer after my junior year in high school, I worked at the University of Delaware doing sputtering experiments. It was a great experience and I learned an important truth from the graduate student who supervised me. "An experimental physicist," he told me, "is someone who gets paid for working at his hobby."

At Juniata College, I truly began to appreciate the connection between mathematics and physics. Calculus as the context of physics was a challenge, but also a true joy. I began to see a beauty and a unity in physics and mathematics that, until then, I had lacked the tools to appreciate.

At Juniata I began doing research on electron spin resonance, and deepened my appreciation of the subject, spending one semester at Argonne National Lab. This experience helped me find my way into Dan Kleppner's group at MIT for my graduate studies, where I worked on the precision measurement of magnetic moments. But while I was enjoying the thrill of measuring things more accurately than anyone had ever done before, I was also struck by the thought that the tuneable dye lasers just beginning to make their way into our lab represented an important new feature in experimental physics.

With Dan Kleppner's encouragement I started a new experiment using lasers to excite sodium atoms and studying their collision properties. I finished my Ph.D. thesis with two very different kinds of research experiences, both of which were valuable in the years to come. I had Dan to thank for that, but also for the more important lesson he taught by example: that one can do physics at the frontiers, competing with the best in the world, and do it with openness, humanity and cooperation.

After two exciting years of postdoctoral studies at MIT, I went to the National Bureau of Standards (now the National Institute of Standards and Technology) in Gaithersburg. I got the position on the strength of my experience in precision measurement, but with the promise that I could spend some of my time experimenting with lasers. So, while I was working on precision electrical measurements I also began to design an experiment to cool and trap atoms with lasers.

While I was still at MIT in 1978 I had read some papers about the laser cooling of trapped ions and about the possibility of laser cooling and trapping sodium atoms. With my experience in using lasers to excite sodium in an atomic beam, I believed I was in a good position to accomplish the first laser cooling of neutral atoms. It turned out to be a lot harder than I had thought, but over a period of years, with colleagues like Hal Metcalf from SUNY-Stony Brook, and a succession of brilliant and skilful postdocs, we slowly learned the tricks needed to first slow the atoms in an atomic beam, trap them and cool them to incredibly low temperatures. We discovered, much to our surprise, that we could laser cool an atomic gas well below the limit that everyone had presumed to be the lowest possible temperature. Eventually we laser cooled cesium atoms below a microkelvin!

The development of atom trapping and laser cooling techniques in our laboratory and in other laboratories around the world has opened a new avenue of research in atomic physics. It has led to advanced ranging from atomic clocks of unprecedented precision to Bose-Einstein condensation. I find the research to be getting more and more exciting all the time. The satisfactions of being a research physicist have come not only from the joy of learning new and unexpected things, but also from doing it in a community of similarly motivated scientists in laboratories around the world—men and women from whom I have learned so much, and whom I am happy to count as my friends.

THE STARTING POINT

Alexander M. Polyakov
Princeton University, USA

When I was in high school, I came across a popular article, written by the famous mathematician Andrey Nikolaevich Kolmogorov. He suggested some litmus tests for mathematical talent. Something like this: you must be able to imagine a section of a cube by a plane passing through its center perpendicularly to the diagonal. I tried and failed. Next, he said, there are algebraic abilities—he gave some expressions and the problem was to expand it in simple factors. Again—no luck. So I decided that I didn't have the abilities to be a mathematician and attempted to study physics. I tried to read some popular books and also some freshmen physics textbooks. The result was not good—I was bored by the slow pace there and the more detailed their explanations were getting, the less I understood.

And then one day I bought in a second-hand book store a book "Mechanics" by Landau and Lifshitz. That was the moment of epiphany. When I started reading it, I got very excited. The beauty of the "least action principle", the arguments based on symmetry, the unifying view of the world, the fantastic intellectual intensity of this book left permanent impression on me. I realized that whether or not I have abilities—this is my science and I will study it no matter what.

It was clear, however, that to pass the entering exams at the university I must be able to solve the standard problems like the ones I mentioned above. I started reading the books which contained the problems and solutions. After a while I started to notice some patterns in them and eventually became quite good at finding solutions. An important lesson for me was that special abilities can be developed if you have a goal and curiosity. At the university my advisor, Arkady Migdal, a remarkable man and a brilliant physicist, suggested to me to look into phenomena which are common to condensed matter physics and particle physics. I started doing just that, although it was highly unusual approach at that time. For a while other people thought that it was a crackpot business. But gradually the situation changed. Today this connection between two fields has become commonplace. I saw an analogy between the so-called critical phenomena and interaction of elementary particles at very small distances. The former is something very familiar. It occurs in the piece of iron when it becomes a magnet or in boiling water at a pressure when it has the same energy as vapor. At that time ('66-'67) the theory of such things were beginning to develop. I worked on that, trying to understand the appearance of the so-called "anomalous dimensions" and "conformal symmetry". And then I realized that there is a mathematical analogy with particle physics and I can apply the same ideas to analyze the experiments in which electrons with huge energy hit protons and produce a complete mess. Moreover, to my great pleasure, I saw an analogy with yet another branch of physics—the theory of turbulence. Today, almost 40 years later, I am still enchanted by these three fields. The unity of physical ideas may be the most beautiful, strange and poorly understood message which we are receiving from Nature.

A little later I once again benefited from the analogies. In the condensed matter physics there are interesting objects which define the properties of matter. In crystals there exist certain defects of structure called "dislocations". At a certain temperature they start multiplying exponentially and lead to melting of the crystal. In superconductors there are vortices, playing the same role in destroying superconductivity. I looked for the similar objects which, I thought, must exist in the vacuum at very short distances and indeed found them. They are called now "monopoles" and "instantons". Among other things, their abundance in the vacuum prevents quarks from propagation, leading to what is called "quark confinement". Incidentally, these objects turned out to be important in mathematics, they helped to solve some long-standing problems of topology—something I didn't expect. Perhaps this indicates that there is even more unity of ideas than we thought.

Later, like many others, I began to explore even smaller scales at which space-time becomes quantum. Again I am trying to use the analogy with things which we know, in the hope that the unity of ideas continues to these scales. There are no guarantees that this is the case. But after long experience one develops a certain intuition which is hard to define. Perhaps, just as the spatial imagination (which I lacked at school) helps to place things in space, there is also a temporal imagination which helps to foresee the development. This temporal imagination is what we call "intuition" and it improves with experience.

The ultimate goal of these efforts is to answer very simple questions. Today we can answer the question "What is heat?" in a precise and non-trivial way—it is the motion of molecules. Perhaps we will be able to answer with the same precision the question "What is Time?" Presently we can't do it, but as a result of the fascinating research of the last decade we can formulate this question in scientific terms.

The problems facing us are getting deeper and require higher level of abstraction. I am somewhat worried that there may be a level of complexity beyond which the human brain can't operate—just as it is impossible to teach algebra to a three years old kid. Be that as it may, we are still very very far from this boundary.

YOU COULD BE A MATHEMATICIAN

Helen R. Quinn
Stanford Linear Accelerator Centre, USA

© Courtesy of Dan Quinn

My education

I grew up in Australia, in a time and place where young girls were not encouraged to think in terms of lifelong careers. We might work for a few years before settling down to marriage and raising a family. Not only my teachers, but also my parents viewed that as the expected pattern of my life. It never occurred to me to question it. So I never dreamed of the life I would actually lead, as a scientist with world-wide connections to colleagues and an international reputation.

Fortunately, my parents valued imagination and curiosity. They treated me no differently from my brothers in the way they encouraged these skills, while ensuring that I also developed the skills I would need as a wife and mother. The school that I moved to in second grade was also a very fortunate choice for me, as its elementary school program had a progressive educational style, modelled on the thinking of John Dewey, that fostered individual motivation and intellectual development. Its high school program, while more formal and structured, was fortunate in the quality of the women who were its teachers. These were intelligent women, almost all of them unmarried, teaching was one of the few careers open to them. They valued and supported my eagerness to learn and challenged me to think.

I remember my excellent high school math teacher once strayed from the party line on careers. She said to me “Helen, you could be a mathematician” but added after a short pause “because you are so lazy. You refuse to do a problem the hard way, you always think until you find a clever way to solve the problem.” I was not sure whether I had been scolded or praised, but I was surprised by the suggestion that mathematics could be a career.

I never consciously decided that science was my path. The first time that I had a choice about what subjects I would study was in 10th grade. When I suggested a plan of study that did not include the most challenging level of science offered both my teachers and my parents insisted that was not the right choice for me. I took the courses they suggested. In the next two years the number of subjects one could study continued to narrow and the content of the courses deepened. I always took as many courses as I could, including all the science and mathematics courses. I think this was partly because those were the courses I excelled in, and partly because I was steadily encouraged in my interest in this direction particularly by my father, himself an engineer.

I matriculated from high school at age 16 and started at Melbourne University. At my parents' urging I applied for a number of cadetships. This is a system whereby a company or government agency supports a student through University, in return the student is required to work for that organization for five years after graduation. I accepted a cadetship offered by the Australian Weather Bureau to become a meteorologist.

One experience I remember vividly was that I was working at the Australian Weather Bureau in the summer of 1959 or 60, when satellite data of cloud cover first became available to Australian meteorologists. Since the weather for Melbourne comes mostly from a direction where there is nothing between the nearby coast and Antartica, Melbourne weather forecasting was until then based on weather maps that were an extrapolation of very limited data. The maps improved greatly with the satellite data. The difference between data and the customary extrapolations was dramatic! My skeptical approach to theoretical predictions today goes back to those pre-satellite weather maps for Melbourne.

In my second year at Melbourne University my father was invited to transfer to the US to work for the parent company of the small engineering firm he had led in Australia. The US company offered to move the entire family to the US for three years, or longer if we decided to stay. We all agreed that a three year stay in the US would be an interesting experience. We all stayed much longer than that!

I was released from my bond to the Weather Bureau; no one in that time and place would expect a young woman, not yet 18, to live so far from her parents for three years. I knew nothing about the US education system. I looked up and applied to two Universities that were close to where my family would be; Stanford and the University of California at Berkeley. Stanford was more generous about giving me credit for the work I had done in Australia, so I chose to go there. A physics major turned out to be the easiest one to complete. I could do it in one year and one quarter of study. Here I must thank Jerry Pine, the physics professor to whom I was sent to evaluate my placement. In effect, he let me place myself. So I became a physics major.

By the time I had completed my undergraduate degree I had become truly interested in physics, and Stanford faculty were actively encouraging me to go on to graduate school. I applied for Ph.D. programs, even though I doubted I would complete one. I did this simply because the schools that looked most interesting to me did not accept students for a Masters degree program. Secretly, I planned to complete a one year masters degree and then become a high school physics teacher. I simply did not yet have confidence that I could have a career in physics. But by the end of that first year I had become fascinated by the physics I was learning. I stayed and become a physicist. My specialty is particle physics.

A very brief outline of the rest of my life

I married a fellow physics student and we began our careers with postdoctoral appointments at DESY, a high energy physics laboratory in Hamburg, Germany. We then moved to the Boston area, where my husband taught at Tufts and I eventually became a faculty member at Harvard. We lived there for seven years and our two children were born during this time. (So I was, and am, a wife and mother, as well as being a physicist; indeed this year I am looking forward to becoming a grandmother). We returned to California in 1976, when my husband began a new career in decision analysis. I have been at SLAC (Stanford Linear Accelerator Center) since 1977.

My major scientific contributions

I am asked to tell you about my major scientific contributions. To do so I must first explain a little about the understanding we now have of the fundamental interactions or forces of nature. In our everyday world we recognize four very different types of interactions: gravity, which you know about because you feel it every day; electric and magnetic interactions, which again you have probably experienced in electric motors and magnets, and which at the more basic level are responsible for binding the electrons to the nucleus to form an atom; and two more types of interactions that operate inside the nucleus: the strong interaction, which binds the quarks which form the neutrons and protons, and also is responsible for the fact that protons and neutrons are bound together in the nucleus; and the weak nuclear interaction in which one quark type turns into another, thereby turning a proton into a neutron (or vice versa in certain circumstances) with the emission of some very light particles that escape the nucleus.

The first of my famous papers was at a time when particle theories had recognized that the strong, electromagnetic and weak interactions all have very similar mathematical properties, aside from their very different strengths. The similarity could be evidence that these three interactions might all be different aspects of a single or unified interaction, this idea is called a “grand unified theory”. But if the interactions are unified, then why do they have such different strengths? Steven Weinberg, Howard Georgi, and I figured out how this could be.

We recognized that the interaction strength depends on the energy of the interacting particles, and that the different interactions change strength at different rates. We found that there is a very high energy scale where the three interactions that look so different at everyday energies, or even in the highest energy accelerator experiments, would actually look the same. We could explain also how the symmetry of the unified theory, a symmetry that relates these different interactions, could be broken in such a way that their strengths would differ at lower energies. The idea of grand unified theories is still very much part of particle physics thinking today, even though the energy scale involved is so high that we have no direct evidence for the additional particles or processes that such a theory predicts.

My second major contribution is even more technical to explain, and also is yet to be confirmed by experiment. However it is also part of many theories today, and certainly has not been excluded as the answer to the puzzle we, that is Roberto Peccei and I, were trying to solve. The strong interactions have a property, called CP symmetry, which the weak interactions do not. This property means that the laws of physic for matter and those for antimatter are exact mirror images of each other. (Antimatter has been observed in the laboratory so we know it exists, it is very like matter, except with a reversal of charges, so antiprotons have negative charge, while anti-electrons, which are also called positrons, have positive charge). The puzzle is that, in our standard particle physics theory, if you do not have matter-antimatter mirror symmetry for the weak interactions, then that lack of symmetry would more or less automatically infect the strong interactions too.

We found a class of theories, extensions of the standard theory, that maintain all the good properties but avoid this infection. As an added bonus, it turns out that these theories predict a new type of particle that interacts very little with ordinary matter and so is a possible candidate for the mysterious dark matter that pervades the universe. This particle is called the axion. It is not the only possible dark matter particle, but it is an interesting one. Very clever experiments are underway that can possibly detect the axions if they are the constituents of the dark matter in our galaxy. So far they have not yet been detected, but neither has this possibility been excluded yet.

Perhaps I will live to see one or other of these ideas confirmed by direct experimental evidence, perhaps not. One of the challenges of this type of theoretical work is that much of it is very difficult to test. Most of my research is closer to experiment, but that work is more a matter of examining the details of the theory and does not get the kind of recognition that the more daring new ideas do. These ideas are evaluated first by their impact on the thinking of others, only much later do we learn whether they are correct ideas about how nature works.

JOY OF A LIMITLESS PURSUIT

Chintamani N.R. Rao
Jawaharlal Nehru Centre for
Advanced Scientific Research, India

I was fortunate in having parents who were academically inclined. Learning and scholarship were paramount to my parents. Then, there were a few fine science teachers in high school. I was enamoured by laboratory demonstrations in chemistry. I loved the colours, the smoke and the smells. I had encountered people who were doing research in the nearby Indian Institute of Science when I was in school, and was in awe of them. I was greatly attracted to the idea of research—discovering a new substance or a new phenomenon—something new, even when I was in school. I thought that it was wonderful that people could discover things not known till then. I had heard of famous scientists and was aware of the aura they carried with them. Seeing and hearing a great scientist in person in the form of Nobel laureate C.V. Raman during my high school years made a great impact. C.V. Raman once took me around his laboratory with two other students when I was 11 years old. I was truly amazed by the manner in which scientists like Raman could raise questions about Nature and the material world. I developed a desire to do research when I was an undergraduate student, but had no opportunity to do so. Neither was there much research in the college where I studied. I was, however, encouraged by a teacher in my college who showed me a research paper that he had published from his Master's degree thesis. He showed me a way of furthering my studies after the Bachelor's degree by going for a Master's degree programme in a University where research was an integral part.

When I completed my Bachelor's degree in science in Bangalore at a relatively young age of 17, the only serious option that I had was to go for higher studies in science. Fortunately, the Master's degree programme in chemistry that I pursued in Banaras had a research thesis component, and a professor who was most encouraging. I published a paper or two from my Master's thesis, but more importantly, wrote a little note on a piece of work that I did on my own and got it published in Science. During this period, I read a book by Linus Pauling which influenced my way of looking at chemistry and enhanced my desire to pursue research in certain areas of fundamental importance. This desire made me go to the US in 1954 to do a Ph.D. degree in chemical physics at Purdue University. At Purdue, I found opportunities to carry out research in a variety of areas with a number of professors, besides my own research supervisor. This freedom enabled me to publish several little notes and papers, some based on my own problems and ideas. These little successes boosted my morale. A few of my teachers also provided me great support at that time. Nobel Laureate H.C. Brown who taught me graduate courses constantly encouraged me to publish my little findings based on my spare-time research. He is a truly dedicated chemist and still publishes at the age of 93.

In my career, there have been a few great souls who have inspired me and have been my role models. I owe much to them. Nevill Mott at Cambridge, the doyen of solid state physics, was one of them. Nevill Mott was intensely involved in research till he passed away at the age of 91. Michael Faraday, the greatest experimentalist of all time, has been my hero since boyhood. He published 450 research papers single handedly, without any interest in administration or positions of influence. He would have received at least five Nobel prizes if he were in the 20^{th} Century. Faraday said that science is nothing but, “work, finish and publish”. It is not only picking a good problem, but also having the strategy for completing it properly so as to publish it, that defines scientific pursuit.

Over the years, doing science has become a way of life for me. The limitless ladder of excellence that science offers, and the passion for science that gives unparalleled joy, have brought a meaning to my life. Nothing gives me more satisfaction than accomplishing something new in my research. Wherever I am, and whatever I am doing, I keep thinking of my research and this undercurrent dominates my being. The thrill of accomplishing research is probably like the thrill experienced by an alpine skier, which can be only felt through direct experience.

I have benefitted much from my students. The questions they have raised have occasionally resulted in new explorations. One such questions was “How many atoms are required to make a metal?” It took me some years to do the necessary experiments to answer the question. I find it most wonderful to work with young people. It has also helped me to remain young.

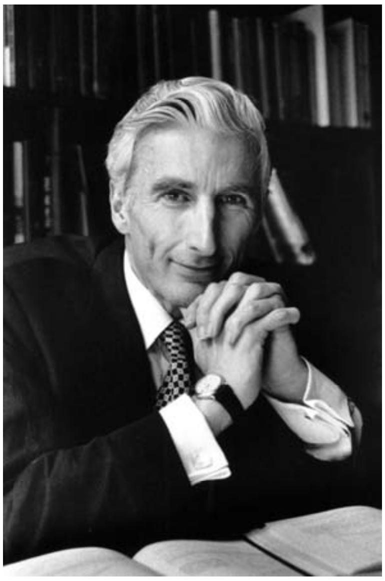
While encouragement of parents, useful advice of teachers, and occasional meeting with scientists have all contributed to my becoming a scientist, it is the successful research experience that I had at a relatively young age as well as the opportunities of working closely with role models that have truly motivated me to pursue science. The freedom to try out a few ideas when I was young and to publish the results enhanced my appetite. Once the healthy virus of doing science caught hold of me, it started feeding on itself. It became a passion to see my name in print and the thirst became inexhaustible.

Many people that I know who have excelled in business, banking and industry do not seem to be as happy as one would expect. They give the impression of suffering from some unsatisfied desire. This is specially so of the scientists who go astray and become administrators and managers. They appear to be apologetic about something and put on a brave face, often mixed with some superficial arrogance. I am truly grateful that I have no complaints. I have had a most satisfying life and would not change places with any one else. I feel that I have been treated handsomely to do what I like most. I cannot think of any other profession that would give me such all-round happiness as science has.

What is also wonderful about science is that there is no limit to the pursuit. It is this opportunity to keep climbing a limitless ladder that makes science specially attractive to me. I marvel the way research areas develop as one pursues ideas: "Great Oaks from little acorns". What starts off as a mere grain of pollen develops into an acorn. The acorn then grows into an Oak tree. The Oak tree develops into a forest. We then begin to see the outlines of a whole new continent. There are clearly many continents to be created and explored. Much of the life of a scientist may be spent in the search of the grain of pollen. Happy are those who partake in the growth of a pollen grain in to a continent. This can happen by chance, but chance favours the prepared mind. I feel that I am most fortunate to have been able to seek happiness in my search for the pollens of grain and in my efforts to develop continents in association with young people.

SCIENCE IS AN UNENDING QUEST

Martin Rees
Cambridge University, UK



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Each branch of science sometimes stagnates and sometimes surges forward. Back in the 1960's when I was a student, black holes were a novel idea, and we'd just had the first firm clues that our entire cosmos began in a big bang.

It was a good time for beginners in cosmology—when everything is newly discovered, inexperience is less of a handicap. Thirty years later the pace of discovery is just as high.

I am, myself, an armchair theorist, trying to make sense of what is discovered, but the main credit should not go to people like me but those who design and build instruments, on the ground and up in space.

Most people have heard of the Hubble Space telescope, but we're now learning as much—and far more cheaply—from giant telescopes on the ground. The most impressive is the European Southern Observatory's Very Large Telescope (VLT), on a mountain top in northern Chile, one of the driest and clearest places in the world, which consists of an array of four mirrors, each eight metres across.

Big telescopes are in some ways like time machines. They reveal galaxies so far away that their light has taken billions of years journeying towards us.

We view these galaxies as they were in the remote past—early in their evolution—when they consisted mainly of pristine hydrogen gas.

Cosmologists have an advantage over, say, geologists who try to study the Earth's past. We can actually see how cosmic history unfolded, rather than simply inferring it from fossil evidence.

If I were asked to describe the goal of my research in just one sentence, I would say it was to map out how our universe has evolved from simple beginnings to its present state; to the cosmic panorama of billions of galaxies, each containing billions of stars.

On one planet around one of these stars, our Sun, something has happened that is far more marvellous than anything in the inanimate world: the emergence of a complex biosphere, containing conscious beings like us, able to ponder the origins of the atoms we're made of.

We all wonder, of course, whether this has happened anywhere else. Is life widespread? Or are we unique?

Prospects for advanced life elsewhere in our solar system seem unpromising, but in the next decade we'll know more about whether there may be "bugs" on Mars; and spaceprobes may even find some exotic life-forms under the frozen oceans of Europa, one of Jupiter's moons.

Our sun is just one star among billions, but ten years ago we didn't know whether any other stars had planets orbiting around them. We now know that other planetary systems are common.

I'd bet high odds that there are millions of Earth-like planets orbiting other stars in our galaxy. But I wouldn't bet on whether any of them harbour life: we don't know enough to say whether life's emergence is likely or unlikely.

But now we understand far more about how our Earth was formed and its place in the wider cosmos. When we only had a unique example of a solar system, it was hard to make sense of it; it would be like trying to infer biological evolution from just one rat.

It may seem arrogant to claim to understand anything about the cosmos. But it really isn't. What makes things hard to understand is how complicated they are, not how big they are.

A star is much simpler than an insect. There's no complicated chemistry inside stars; they're so hot that everything is broken down to its simplest atoms. On the other hand, in even the smallest living organism atoms are linked together into layer upon layer of intricate, complicated structure. Biology is in some ways a far harder subject than astronomy.

We are ourselves, in size, midway between atoms and stars. It would take as many human bodies to make a star, as they are atoms in each of us. And we owe our existence to stars, because they made these atoms.

Stars are kept shining by nuclear fusion, a controlled version of what happens in an H-bomb. This process transmutes hydrogen, the simplest atom, into carbon, oxygen and the other atoms of which we are made. When stars end their lives, exploding as supernovae, debris is flung back into space and then condenses into new stars and planets.

If you're less romantic, you can say we're nuclear waste from the fuel that makes stars shine.

Nearly everyone subscribes to the big bang concept; the idea that everything started off in a hot dense state. We can be fairly confident about what conditions were like a few seconds after the big bang. The temperature was a billion degrees. But what about still earlier times, the first tiny fraction of a second when everything was hotter and denser still?

To understand the very beginning, we'll need some breakthrough in our concepts of space and time; a new theory of all the forces of nature that combines gravity and the quantum world.

Probably the idea that space has three dimensions and time just ticks away will be transcended. We'll need to envision a world with 10 dimensions rather than just the three that we're aware of.

I'm often asked about the impact of all this on religion and philosophy. I'm afraid my answer is a dull one. I don't think the interface with these subjects is any different now from the way it was three hundred years ago, in Sir Isaac Newton's day.

Newton could explain some features of the cosmos—why the planets move in orbits of a particular shape, for example—but he couldn't explain how the solar system, with its Sun, planets and comets, was set up.

We do now understand this; the planets formed from a swirling dusty disc that encircled the newly formed Sun. We can trace the casual chain right back to an era before the galaxies formed, back even to the initial seconds of the big bang. But we still at some stage have to say 'things are as they are because things were as they were'.

And science will never tell us why there was a universe, what breathes life into our equations so that they are actualized in a real cosmos. We still confront a barrier, just as Newton did.

Cosmologists react in various ways to this, some being religious believers, others not—just as in the 17^{th} century.

But cosmologists have learnt one thing that does, I think, affect the way we see ourselves, and humanity's place in nature; that the future lying ahead is more prolonged than the past.

Our biosphere has taken four and a half billion years to evolve, but the Sun won't run out of fuel for another five billion years; it's less than half way through its life. The entire universe may even continue expanding forever. In this perspective, humans are far from the culmination of evolution; maybe we're still near its beginning.

It's intrinsic to any science—and part of its appeal—that it's an unending quest. Each advance brings into focus a new set of questions.

We'll surely extend our knowledge in the new millennium, but some aspects of reality may always lie beyond what human brains can cope with. They'll remain as mysteries, awaiting more powerful intelligences, natural or artificial.

But what really amazes me is that we've made any advances at all: that we discern so many patterns in nature, and can at least make some sense of our cosmic habitat.

A few centuries ago, the pioneer navigators learnt the size and shape of our Earth and the rough layout of the continents. We are now mapping our entire cosmos and learning what it is made of. There is far more data than before. Moreover, it's far more accessible. You can access or download it anywhere in the world.

This is an enviable time for young people to embark on a scientific career.

Even being a middle-aged scientist isn't too bad.

WE MUST IMPROVE OUR IMAGE

Tullio E. Regge
Institute for Scientific Interchange at Turin, Italy

The image of science is tarnished, a sizable and growing fraction of the public distrusts scientists and thinks that we are all Frankensteins: we must seek a remedy for this lamentous state of affairs. Even worse, some of our most bitter critics are scientists themselves; if old fellow Freud could come back he would have something interesting to say about them.

Quite recently I was invited to speak as a member of a round table centered on the image of science. The public was mainly composed of high school science teachers active in Piedmont. I talked about the hottest world wide issues on the matter of science and environment, among them nuclear energy, GMO's, electrosmog and intervention on human genoma, this last still in the experimental stage.

All Italian nuclear power plants have been shut down or demolished and we keep on paying salaries to people who have nothing to do. These plants have been replaced by others based on imported fossil fuels, i.e. coal, oil and natural gas. We have a sizable amount of "nuclear ashes" stored in the peninsula in conditions which are far from optimal. All attempts to treat then and store them in a national site in a state of optimal safety have been thwarted. The very people who most energetically complain about the dangers of nuclear energy are actively engaged in keeping nuclear waste in a state of maximal unsafety.

At the round table we also discussed the issue of "pollution" of conventional crops by GMO's. This unwanted intrusion can be avoided by newly developed GMO's which are either sterile or do not contain the unwanted gene in the pollen. When I talked about this possibility, the reaction of the greens present in the hall was a fit of uncontrolled rage. I was accused of spreading unreliable information. In fact I got them directly from a worldwide expert on GMO's.

I was member of a governmental committee where the problems of ES, the so-called "electrosmog", was discussed; one of the members was a high ranking expert in this matter within the WHO. It turned out that the impact of ES had been vastly overestimated in the press and that in some countries, Italy among them, the law on ES was leading to heavy and useless waste of public funds.

Mankind is afflicted by countless genetic diseases, some of them are lethal and cause untold suffering. Believe it or not, there are dogooders who oppose research on genetic diseases but, of course, have nothing to offer in exchange with the exception perhaps of ignorance.

I quoted four issues where science is under attack either directly or through spin off of research. A direct approach with our critics with the aim of reaching some minimal agreement is “mission impossibile”. They are a strange mixture of zealots and political demagogs who reject any solutions to the problem for the fear of losing votes. All this reminds me of a Robert Mencken's quotation:

“Puritanism: the haunting fear that someone, somewhere, may be happy.”

I’ve never met a puritan from the time of Mencken but I’ve met many in recent days. We must improve our image.

WE NEED YOU

Vera C. Rubin
Carnegie Institution of Washington, USA

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As a teenager in Washington, DC, I had my bed under a window that faced north. I found it more interesting to watch the sky than to sleep. I waited to see the stars move in arcs about the North Star; I saw an occasional meteor. The mystery and the majesty of the night sky captivated me, and I could not imagine living on earth without trying to understand what I was seeing. I knew that there were continents and oceans and what a map of earth looks like; I now wanted to learn about galaxies and stars and planets, and what the map of our corner of the universe looks like.

The local library supplied books, my father helped me build a telescope, and friends drove us to the Virginia countryside for better views of the sky. Even now, I find the view of bright stars against the dark sky from a remote mountain top observatory the most remarkable view on earth. At a Southern Hemisphere observatory, where the sky is dark and the central region of the Milky Way shines bright overhead (and even casts shadows), I can look at the broad expanse of the Milky Way and understand that I am standing on a tiny planet in space, viewing the principal plane of my galaxy. What a wonderful thing to know.

I am a scientist because I fell in love with a way of life that would permit me to be a perpetual student, to learn what is known about the cosmos. The beauty, the unlimited scope, and the cumulative structure of science made astronomy my career choice.

I entered Vassar College, a woman's college, on a scholarship to study astronomy. Maria Mitchell taught astronomy there from its opening in 1865 until 1888. But in 1945, astronomy in the United States was taught mostly in private colleges which did not admit women. Following my next degree (from Cornell), my husband and I moved to Washington, DC, where I completed my Ph.D. degree at Georgetown University, and wrote my thesis with George Gamow, the renowned physicist/cosmologist who was teaching at George Washington University. Hence my entry into the world of research astronomy was unconventional, because I did not attend a college which "traditionally" trained astronomers. Consequently, my earliest studies were unorthodox.

While a young assistant professor at Georgetown University, I studied the outer, mostly unknown regions of galaxies. Unlike centers of galaxies, which were subjected to active observation and study, outer parts of galaxies were neglected. I chose a research program where I could work at my own pace, without competition from other astronomers. As a wife and mother of four active youngsters, I knew that juggling a family and a career could work, but it required special considerations and special attention.

Combining a life in science with an active family has been possible and fun because of encouragement from my mathematician/biologist husband, Bob, and because of the opportunities offered me by the Carnegie Institution of Washington. Allan, our youngest child, recently recalled that as a young child, he occasionally would ask where Mother was. And the reply, “She’s observing”, reassured him, for everyone seemed content even though Allan did not know what “observing” meant.

The combination of family, teaching. and research was complicated, so in 1965 I moved to the Department of Terrestrial Magnetism (DTM), a research laboratory of the Carnegie Institution of Washington. At DTM, Dr. Kent Ford had just built a state-of-the-art image tube spectrograph which made it possible to determine orbital velocities of stars located at the faint outer limits of their galaxies.

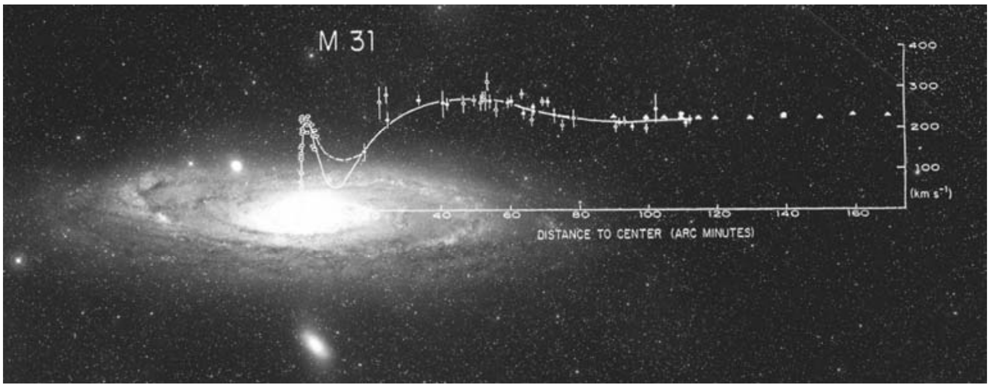
During the next 15 years, Kent and I studied orbital velocities of stars and gas in over 100 galaxies. In each galaxy, the orbital velocities in the outer regions were much more rapid than velocities expected from the light distribution in the galaxy. We were forced to conclude that most of the matter in a galaxy is dark. It is the gravitational acceleration from this “dark matter” that causes the stars to move at unexpectedly high velocities, and keeps the stars from flying out into space. Thus, the distribution of matter in a galaxy is VERY different from the distribution of light.

Studying something that you cannot see is difficult, but not impossible. Not surprisingly, dark matter is detected by its effect on bright matter that we do see. We can deduce a few characteristics of dark matter: it is less concentrated to a galaxy center than is the bright matter, it extends far beyond the optical boundaries of a galaxy, its form is less flattened than that of a disk, it is not radiating at any wavelength. In a spiral galaxy, at least 90% of the matter is dark. Thus, the atoms and molecules which compose our bodies and compose the luminous universe are not the major constituent of the universe. Clumps of dark matter that formed in the very early universe may have been regions into which infalling matter would later form galaxies.

In the 1930’s Fritz Zwicky had concluded that dark matter existed in clusters of galaxies, but this result was not then widely embraced. With the new evidence from high rotation velocities in galaxies, arguments for the existence of dark matter were persuasive.

There is a caveat to this evidence. Early in the 20th century, physicists learned that in domains as small as atoms and nuclei, conventional physical laws were not valid. We have only now tested Newton's laws on scales as large as galaxies. Here too, they fail, but we attribute the failure to the existence of dark matter. Until we identify what the dark matter is, we cannot rule out the possibility that Newtonian gravitational theory must be modified for distances as great as galaxies. Best of all would be a new cosmology that solves several major questions at once: what is dark matter, what is dark energy, do we correctly understand the evolution of the universe.

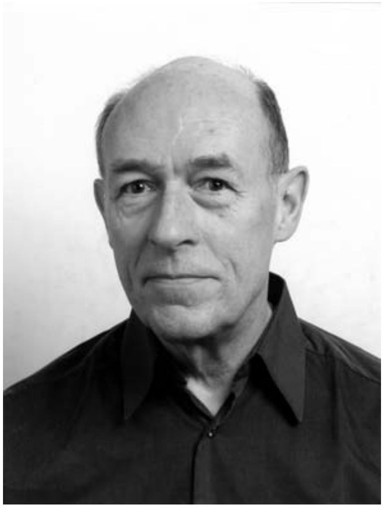
Virtually everything we know about the universe has been learned in the 400 years since Galileo used his new telescope to see that the Milky Way consists of innumerable stars grouped together in clusters. In time, you may learn that there are hidden dimensions, hidden time, perhaps that ours is not the only universe. For those of you interested in science, I offer the following advice. Don't give up. We need you. There is not one of you who cannot add major contributions to our knowledge. You too can be a scientist, and along the way also interest others in science. My hope is that you will enjoy doing science as much as I have enjoyed learning about the universe.



The Andromeda Nebula (M31), our near neighbor in space, copied from the Palomar Sky Survey. The velocities from optical gas clouds are shown as open and filled circles. Velocities from neutral hydrogen radio observations are filled triangles. Note that the stellar orbital velocities remain high, even where the luminosity from the galaxy is very very low. (Montage by Rubin and Janice Dunlap, after Roberts.)

THE FASCINATION OF KNOWLEDGE

David Ruelle
Institut des Hautes Etudes Scientifiques, France



© Courtesy of David Ruelle

It was after the end of World War II that, as a young Belgian teenager, I made my intellectual discovery of the world. And I found many wonderful things that I wanted to know and understand: faraway countries, strange languages, the traces of ancient human cultures, prehistoric animals, the dangerous beauty of medicinal plants, the mysteries of the microscopic universe, and the immensity of the cosmos. After some time my curiosity became more radical, abstract, and conceptual. I was fascinated by what I read about mathematics and modern physics: Gödel's theorem, relativity, quantum theory, ... I also devoured some books on chemistry, psychoanalysis, botany, and philosophy (Auguste Conte and Baruch Spinoza). But soon I was more interested by formulas like $e^{i\pi} = -1$ or $E = h\nu$ than by the ontological proof of the existence of God.

Those were good days if you had some gift for science: you could concentrate on an intellectual quest rather than on finding a job. Still I had to make professional choices. After some aborted engineering training, I studied physics and mathematics, and made it my job to understand the physical nature of things by using mathematics. Mathematics is a purely intellectual kind of magic, and quite satisfying as such. But mathematical physics is powerful magic that applies to the real world. The control over the world that ancient men tried to obtain through divination and spells was achieved by Newton, Planck, Einstein, Heisenberg and Schrödinger through mathematical physics. Although Einstein described himself as a mathematical physicist, a later generation of physicists (like Feynman) played down the role of mathematics in understanding the physical world. Then more recently mathematics made a spectacular comeback with *string theory*, which has unfortunately not yet made contact with experiments.

I had the great luck that, in the course of more theoretical and abstract studies, I hit upon one topic with concrete applications: *chaos*. In a nutshell, the study of *low dimensional chaos* is the analysis of how some physical systems evolve with time in ways that appear complicated, erratic, noisy, and can nevertheless be understood and predicted (or more precisely, their predictability can be analyzed). While chaos had important forerunners (Poincaré, Edward Lorenz), it became an important subject of research in the 1970's and 1980's because the mathematical, computational and experimental techniques were then ready for seriously analyzing a variety of complex phenomena. It turned out that some phenomena could be understood as low dimensional chaos (weak fluid turbulence, meteorology on some scale, chemical turbulence, part of the astronomy of the solar system, ...), while others (like financial time series) could not. The golden age of the theory of chaos was a great period for me because I could go all the way from pure, often difficult, mathematics to discussions with experimentalists in their labs, meteorologists, chemists, economists, and generally speaking a great variety of extremely competent, intelligent and interesting people. Let me say that I didn't shy away from the intellectual experience of *chaos* and its applications, unlike some colleagues who preferred to stay in their offices and continue the routine of proving rigorous theorems about a reality that they were not eager to see more closely.

The golden period of chaos theory is over, and those who played a role in it mostly do other things now, if they are still active. My own interests have largely shifted to nonequilibrium statistical mechanics (a part of theoretical physics that uses dynamical systems and ideas of chaos). I also keep a less professional interest in a number of topics that I found intellectually exciting as a teenager. As it turns out, many scientists I know also remain fascinated by topics like fungi or linguistics (e.g. Murray Gell-Mann) far from their professional interests in mathematics and physics. This fascinating power of knowledge remains present for all those of us who accept it, whether it is in a professional capacity, or simply as an essential enrichment of life.

WHY PHYSICS?

Myriam P. Sarachik
City College of New York, USA

© Courtesy of David Price

Owing to historical circumstances, my early years were eventful and quite unusual. I was born in 1933, the year Hitler came to power, and my life's trajectory was irrevocably altered by subsequent events which caused my family to flee Belgium. We found refuge in late 1941 in Cuba, where I grew up from age 8 to 13 and a half, and then immigrated to the United States, the land of promise, freedom and limitless possibilities. This is where I attended high school, then College and graduate school, where I obtained my advanced degrees.

My family and my community were quite traditional. Women were not expected to work outside the home. Women were responsible for raising and caring for children, which is certainly very hard work. But they did not embark on their own career. If a woman worked outside the home, it was a sign that her husband was an inadequate provider.

I remember my very early years as moderately boring and depressing. My life changed on the day I entered school. It opened an entirely new and exciting world for me. Reading was a pleasure. Numbers were a pleasure. I truly loved it! Before I started school, I was considered sweet, a good child, but a bit of a sad sack. Now I was considered worthy and special.

After first grade, my education was interrupted for more than a year as we fled from country to country. Although my father disapproved spending money on books (we were in terrible danger and there was no time or money for such things), my mother bought books for me to read. And I read each book over and over and over again until I got another book to read. I was eight years old when my family arrived in Havana, and I could then go to school again.

I was interested in many things. I liked grammar, languages, anatomy, geography, almost everything. I read voraciously, I played the piano and loved music passionately, and I took special pleasure in arithmetic, algebra, patterns and quantitative things generally.

What made me finally settle on physics when I had to make the choice? I'm not sure I know the answer. I found physics the hardest subject I had ever encountered, and I did quite badly at it in the beginning. It was a real challenge. Other things had been relatively easy for me. Physics was TOUGH and physics was held in very high regard. It was considered the epitome of intellectual achievement by my father, whom I loved dearly, and respected and admired very highly. My father was curiously conflicted about my scholastic pursuits. On the one hand, he placed great value on intellectual excellence and encouraged me to excel; he surely would have chosen to be a physicist had life offered him that chance. On the other hand, I was a girl, and girls should marry and have children.

I earned my Bachelor's Degree in 1954 with a major in physics (at Barnard College, although all the courses were given across the street at Columbia because there weren't enough of us girls interested in the subject). Philip Sarachik and I were married that summer, and it was time to move on to the life I was expected to live. But there was no harm in waiting a bit, and I took a job at IBM Watson Laboratories very near Columbia University. By this time, I had been quite captured and captivated by physics. It was still tough but I could do it, and it was fascinating. I very much wanted to go on for a Ph.D., but I felt I must not. But, there was no harm in taking one or two graduate courses down the street at Columbia, was there? My husband then decided to work toward a Ph.D. in Electrical Engineering. I then allowed myself to do the same.

There were many challenges waiting for me along my chosen path. There were very few women candidates for Ph.D.'s in physics in those days. I received no encouragement from the faculty; on the other hand, except for many faculty members' negative perceptions of women's ability and role, there were no overt blocks. We all had to pass the same exams, we all had to do a thesis, and we all had to defend that thesis.

The next step on the road was quite different: I had an incredibly tough time landing my first job. I believe that part, perhaps a large part, of the problem was that I had become a mother, having given birth to my first daughter Karen. But I persisted mightily, and I managed to meet that challenge too.

I will not dwell on my middle years. In brief, after doing a postdoc at IBM Watson Laboratories, and another at Bell Telephone Laboratories, I joined the faculty at City College of New York, where I have spent essentially my entire professional life. I have enjoyed every aspect of my work at CCNY. I began as a middling teacher and grew to love teaching, learning to do it better and better with time. I've taken on the usual responsibilities in my department and university over the years.

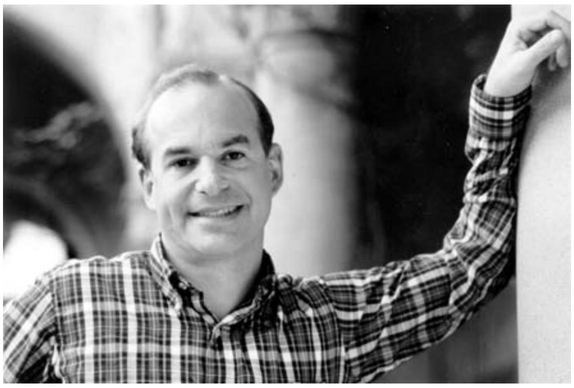
But I've derived the greatest joy and satisfaction from my research, where I believe I've made a few significant contributions. While a postdoc at Bell Labs, I did experimental measurements that established a one-to-one correspondence between the presence of a localized moment and the occurrence of a minimum in the resistance versus temperature in certain alloys; contemporaneously, Jun Kondo performed a now-famous calculation that demonstrated that the minimum is indeed due to a local moment. This solved a long-standing puzzle that had existed since the 1930's. More recently, I have done work (with Sergey Kravchenko) that raises the possibility that an unexpected metallic state can exist in two dimensions. And in 1996, a Ph.D. student in my group, Jonathan Friedman, and I discovered tunneling of a large magnetic moment through the observation of steps in the magnetization curve of a molecular nanomagnet, Mn12-acetate, a discovery that has stimulated an enormous amount of activity in what's now referred to as "single molecule magnets".

My interests have taken me to issues and places outside City College. I have participated in many efforts to defend the human rights of scientists. I have served the physics community in a number of ways: through advisory assignments, service to the American Physical Society, organizing conferences, and so on. These activities culminated in my election to the position of President of the American Physical Society in 2003, a year that was unique for its intensity, involvement, and rewards.

My life as a physicist has been enormously satisfying and great fun. That doesn't mean that every moment has been fun. There have been problems and challenges along the way, and there have been setbacks, small and large. One of the most exhilarating aspects of being a physicist (or a mathematician, chemist, biologist, ...) is that one continues to learn, stretch and expand. I'm referring not only to finding truly new facts and phenomena through doing research. I'm referring also to the joy of learning things that are known and understood by others, but that you now understand for the first time. It's a wonderful challenge.

SUPERSTRINGS

John H. Schwarz
California Institute of Technology, USA



My parents, both of whom were scientists, were very supportive of my early interest in mathematics and science. During my undergraduate years at Harvard I majored in mathematics, which I thoroughly enjoyed. However, when it came time to apply for graduate school, I decided to switch to theoretical particle physics. I wanted to study mathematical formulas that arise from attempts to describe the real world. At Berkeley, where I was a graduate student, my tastes in physics were strongly influenced by my advisor Geoffrey Chew. After receiving the Ph.D. in 1966, I had a six-year junior faculty stint in Princeton. Following that I moved to Caltech, which is where I still remain.

Almost all of my career as a theoretical physicist has been devoted to the study of string theory. This is a type of relativistic quantum theory based on fundamental objects that are tiny loops (called strings) rather than points, as is the case in more traditional relativistic quantum field theory. Let me describe how that came about, since this history is rather unusual. As we will see, superstring theory, which is currently the leading approach for constructing a unified theory of all fundamental forces and particles, was initially developed in an attempt to solve a different problem.

The strong nuclear force, which is responsible for binding quarks together inside protons, neutrons and other hadrons, was not yet understood in the 1960's. During that decade, theorists faced the challenge of finding a simple explanation for the zoo of new particles that the experimentalists were producing with their large accelerators. At UC Berkeley, Professors Chew, Mandelstam, and others were developing influential ideas, such as the "bootstrap hypothesis" and "Regge pole theory," as part of their "S-matrix theory" program. This program did not give a complete understanding of the strong nuclear force, but by a remarkable sequence of events it led to superstring theory.

During 1968-70, Veneziano, Nambu and others developed the "dual resonance model," which was soon interpreted as the theory of a relativistic string. This model was motivated by the bootstrap and Regge ideas, which it incorporates, and thus it is able to describe many qualitative features of hadron physics. In 1971 a second dual resonance model (or string theory) was introduced by Ramond, Neveu and me. It contains a new type of symmetry called supersymmetry, and thus (after some further refinements) this string theory came to be known as superstring theory.

Both string theories shared certain shortcomings, however. Two of the most striking were that mathematical consistency requires extra spatial dimensions and the existence of massless particles. Both of these features were clearly wrong for a theory of hadrons. The final nail was driven into the coffin of string theory in 1973, when a quantum field theory, called quantum chromodynamics (QCD), emerged as the correct theory of the strong nuclear force. Its successes were immediate and convincing. String theory, a very active area of research for almost five years, dried up practically overnight.

At Caltech, I arranged a half-year visit in 1974 by Joël Scherk, a French physicist with whom I had worked earlier in Princeton. We felt strongly that string theory was too beautiful a mathematical structure to be completely irrelevant to nature. Even though we were convinced that QCD is correct, we believed that string theory deserved one more chance before being abandoned. Soon we realized that its defects could be turned into virtues if it was used for a completely different purpose from that for which it had been developed originally.

Massless particles do exist in nature: The particles that constitute light (photons) and gravity (gravitons) are examples. These particles are not hadrons, however. We showed that the string theory graviton interacts at low energy in precise agreement with the predictions of Einstein's general theory of relativity. (This result also was obtained by Yoneya). In this context extra dimensions were no longer a problem. In the 1920's Kaluza and Klein had showed that extra dimensions of space can play a useful role in gravitation theories.

Prior to the 1980's, almost all particle physicists ignored the gravitational force, which is entirely negligible under ordinary circumstances. For example, the gravitational attraction between the electron and proton in a hydrogen atom is about 10^{38} times weaker than the electric attraction. On the other hand, relativists, physicists who specialize in the study of gravity, had no use for particle physics. They study the largest things in the universe (even the universe itself), not tiny particles. Relativists attended different meetings, read different journals, and (until the 1980's) had no need for serious communication with particle physicists, just as particle physicists felt that they had no need to think about black holes and the early universe in their quest to understand elementary particles.

For these reasons, even when Scherk and I realized that string theory had mathematical features suggestive of gravity, we were not predisposed at first to interpret it as a physical theory of gravity. Fortunately, after a few weeks of intense deliberations, we were ready to take the plunge. Thus, Scherk and I proposed reinterpreting string theory as a candidate for a unified theory of gravity and the other fundamental forces. This was a radical change in viewpoint that required, among other things, supposing that the typical size of a string is approximately the Planck length (10^{-33} cm) in order for the gravitational force to have the correct strength. This is 20 orders of magnitude smaller than what was envisioned when strings were being used to describe hadrons, whose typical size is about 10^{-13} cm.

In addition to incorporating gravity in a unified theory, superstring theory provided another bonus. All previous proposals for a quantum theory of gravity gave meaningless infinite answers for quantum corrections to classical formulas. String theories have a much better short-distance behavior than point-particle quantum field theories, and therefore they do not have this problem.

Scherk and I were very excited by the possibility that string theory could be the Holy Grail of unified field theory, overcoming the problems that had stymied other approaches. In addition to publishing our work in scholarly journals, we gave numerous lectures at conferences and physics departments all over the world. For the most part our work was politely received—as far as I know, no one accused us of being crackpots. Yet, for a full ten years, very few other experts took the proposal seriously. It required considerable strength of conviction to keep working on a subject in which others were not interested. Tragically, Joël Scherk passed away in 1980 at age 35. It was very unfortunate that he could not witness the acceptance of superstring theory and participate in its modern development.

In 1979, Michael Green and I began an intense collaboration developing superstring theory. Each year we made discoveries that we felt would convince other physicists of the virtues of this theory, but it took five years for that to happen. In the summer of 1984, while working at the Aspen Center for Physics, we figured out how superstring theory circumvents certain apparent inconsistencies, called anomalies. Within months of that discovery, superstring theory became one of the most active areas of research in theoretical physics, which it has remained ever since.

PERSONAL FREEDOM FOR SCIENCE CAN EXIST IN ANY SYSTEM

Yakov G. Sinai
Princeton University, USA

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When I was a high school student I didn't show any brilliance, I was not a winner of any serious mathematical competition or Olympiad and was interested in sports, mainly volleyball. My grandfather was a very prominent mathematician, a professor at Moscow State University. His name was Kagan and even now one can encounter references to his books on differential geometry. My half-brother, G.I. Barenblatt, and I were under his and his wife's (my grandmother's) influence. After my graduation from high school it was not clear at all whether I should become a mathematician. My grandfather told me at that time: If you decide to become a mathematician you should know that mathematicians differ from other people by thinking about their problems 24-hours a day. I remember this phrase even today, and have tried to follow this rule until now.

I was a student of Kolmogorov, who was a great mathematician of the 20th century. Kolmogorov was very much interested in applications of mathematics and wanted his graduate students to be involved in various researches of this type. There were several years when Kolmogorov worked on statistical problems related to the motion of the axis of rotation of Earth. Young people around Kolmogorov wanted to do only mathematics. This gave Kolmogorov a reason to complain: My graduate students prefer to write a paper for Doklady[20] than to do something useful. Certainly, we had enough freedom to do what we wanted, i.e., mathematics. This personal freedom can exist in any political system. In remembering those years I do not put much pressure on my students when they do something I don't like.

[20] One of the most popular mathematical journals in Russia at that time.

DOING SCIENCE IS DEMANDING BUT NEVER BORING

Maxine F. Singer
Carnegie Institution of Washington, USA

Each scientist discovers a passion for science in a unique way. For many, it begins with inspiring teachers. I had my own share of amazing teachers in the New York City public schools. Circumstances in the 1930's and 1940's made school teaching attractive to people who might otherwise have been scientists and scholars; the Great Depression made jobs scarce and U.S. universities rarely appointed either women or Jewish scholars to their faculties. Now, more than 55 years later, I remember the names and faces of several of my outstanding science teachers. My high school chemistry teacher made chemical processes seem more alive and interesting than the biology we learned. For a city child whose experience with nature itself was limited, biology seemed mainly descriptive. I decided to concentrate on chemistry in college.

Aside from the choice of a mate, probably the most important decision I made in my life was to enter Swarthmore College, a small, coeducational school, after high school graduation in 1948. My parents had not attended college and left the choice to me. Swarthmore is a peaceful, green, country place, very different from the busy city streets where I grew up. It seemed like a perfect place to live and study, and it was. A scholarship from the College made my matriculation possible. Besides a lot of chemistry, I studied a good deal of mathematics and biology as well as some history, philosophy, English literature, and foreign languages. By the time I was graduated in 1952, I realized that biochemistry was beginning to reveal the processes underlying the bewildering if dazzling diversity of the natural world.

Besides the formal studies, the Swarthmore experience prepared me to join the scientific community and sustained me as a scientist. It may seem surprising that in the 1950 Swarthmore environment, with its 19th century American middle-class roots, its deep ties to the religious traditions of the Society of Friends, and its commitment to a liberal education—in the classical meaning of that term—a young woman could acquire an outlook suitable to a life in science. But it was at Swarthmore that I began to acquire an iconoclastic skepticism, the will and skill to think independently and challenge received wisdom, the experience of criticism by peers, and, consequently, a degree of self-confidence. Importantly, Swarthmore shielded us from the outside world where there was little space for women as scientists.

In the fall of 1952, 21 years old and newly married, I entered graduate school in the Department of Biochemistry at Yale University. This was another very lucky decision. The department chairman and my thesis advisor, Professor Joseph S. Fruton, valued female and male students alike. My thesis topic was one of the first investigations of a protein phosphatase. Professor Fruton gave me an exceptionally good piece of advice when he recommended that I do postdoctoral research with Dr. Leon A. Heppel at the National Institutes of Health (NIH) outside of Washington, DC. At the time, 1956, Heppel was one of the very few biochemists working with RNA or polyribonucleotides. It was only three years since the structure of the DNA double helix was described, and nucleic acid chemistry and enzymology were poised to develop into a major field.

Heppel was deeply involved in analyzing the polyribonucleotides formed from nucleoside-5' diphosphates by action of polynucleotide phosphorylase (PNPase) which was discovered in bacteria two years earlier by Marianne Grunberg-Manago and Severo Ochoa. This enzyme, together with DNA polymerase I, discovered by Arthur Kornberg and his colleagues about the same time, were the first enzymes known to catalyze the synthesis of polynucleotides. These discoveries opened a new era for biology by bringing biochemists and geneticists together although it turned out that PNPase was not responsible for RNA synthesis and DNA polymerase I was not the key enzyme involved in DNA replication.

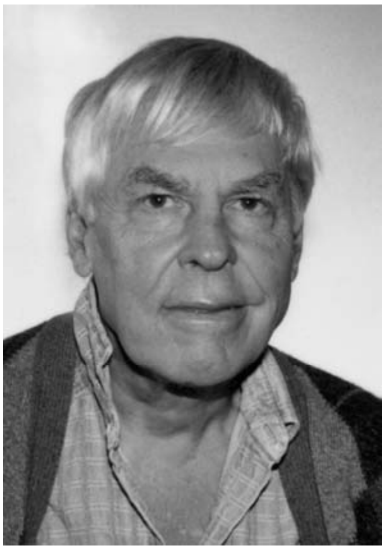
My work in Heppel's lab and for a decade afterward when I had an independent lab at NIH, was on the mechanisms of PNPase and ribonuclease catalyzed reactions. Together with several postdoctoral fellows we described processivity in enzyme-catalyzed polymerization and depolymerization reactions, a mechanism that has turned out to be quite common. In a processive reaction, the enzyme remains bound to the polynucleotide through successive additions or removals of nucleotides rather than releasing the polymer after each step. Then came the exciting time when our cache of polymers was used by Marshall Nirenberg and Heinrich Matthei in the dramatic experiment that defined the genetic code word for phenylalanine. By the spring of 1961 several of us were hard at work synthesizing additional polyribonucleotides and then trinucleotides for determining the other codons.

By 1970, I was ready to do something different. Animal viruses seemed a challenging field in which experience with polynucleotides would be helpful. In the summer of 1971, my husband and I and our four children traveled to the Weizmann Institute of Science in Israel, where I spent a year's sabbatical learning about simian virus 40 (SV40) from the very accomplished virologist, Professor Ernest Winocour and his students. Restriction endonucleases had just recently been characterized and we used them to study defective SV40 genomes containing repeated DNA sequences from the host monkey cells. When I returned to the NIH, the repeated sequences themselves, captured most of my attention. We studied and sequenced repeated DNA segment at the centromeres of primate chromosomes—the so-called α-satellite. By the late 1970's, cloning and DNA sequencing allowed us to learn more about these repeated sequences than we could have imagined even a few years earlier. Most exciting was the discovery that a highly repeated DNA segment in the human genome was a transposable element. When I closed my lab in the mid 1990's, we were studying the mechanisms by which this element, called LINE-1, makes copies of itself and inserts into new sites in the genome.

Doing scientific research is demanding, hard work. The sometimes frustrating experiences are more than made up for by curiosity to understand nature and those extraordinary moments when an experiment reveals something new and unexpected. It is sixty years since I entered high school and I don’t remember ever being bored.

INITIALLY MARGINAL, SUPERB LATER

Stephen Smale
City University of Hong Kong



© Courtesy of Stephen Smale

Stephen Smale was not always an inspired student. As an undergraduate student in mathematics in Michigan, his time was often consumed by other interests, especially campus politics and travel. Even as late as the second semester of his graduate work, Smale dropped two courses and received a C in another. The department chairman, T.H. Hildebrandt, felt it necessary to notify Smale that to remain a graduate student he would have to improve his grades. Later, for Smale's first job search, Hildebrandt wrote that Smale was a "marginal, underachieving graduate student."

As a Freshman, however, Smale had the luck to be put, after a placement examination, into a small class with an exceptional young instructor, Bob Thrall. Smale thinks of him as his "first memorable teacher." Smale's thesis advisor, Raoul Bott, was not yet well known, but he did provide Smale with an excellent problem. Smale was Bott's very first graduate student. Bott suggested an accessible problem that laid the foundation for the creative work that flowed. A little later, Smale proved the counter-intuitively famous theorem that one can turn a 2-sphere in R3 inside out. The whole area of immersion theory flourished. Topology was front and center stage in mathematics.

Rarely in mathematics, science or engineering does one find open doors when migrating to other subdisciplines, so Smale opened his own doors. From topology, he moved to dynamical systems where he discovered his now famous and ground-breaking "horseshoe", possessing stability amidst chaos. A bit later, he attacked the classic Poincaré conjecture, where he "broke the dimension barrier" by announcing his proof for dimensions greater than 4.

In short, in his mathematics, Smale found a few good teachers and accessible problems as a young man, and this permitted him to make the giant leaps that he has been able to make in the subject.

SCIENCE OFFERS AN IMPORTANT INPUT

Susan Solomon
National Oceanic and Atmosphere Administration, USA

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I began life in Chicago, and first got hooked on science watching the undersea adventures of Jacques Cousteau on TV. In high school, my confidence that science was the right choice for me was boosted when I was lucky enough to take third place in a nationwide science fair with a project that measured the amount of oxygen in gas mixtures. While in undergraduate school studying chemistry at the Illinois Institute of Technology in Chicago, I was fascinated to learn of work being done regarding the chemistry of the atmosphere of the planet Jupiter. That's what started me on the path to doing chemistry on a planet instead of in a test tube. After graduating from IIT I went to graduate school in chemistry at the University of California at Berkeley. My doctoral dissertation was about chemistry on a planet—not Jupiter but on Earth. I earned my doctorate in chemistry in 1981.

Prior to the discovery of the ozone hole, my work focused on what you might call some esoteric aspects of understanding the atmosphere. I was looking at things like the impact of natural factors including the aurora on the chemistry of the mesosphere, thermosphere, and stratosphere. Then the ozone hole was discovered, and that changed everything. I was intrigued by the observation, and one of the first things I thought about, coming from this mesosphere/thermosphere kind of work, was whether reactive nitrogen from phenomena like solar protons could be responsible. I convinced myself, however, that that hypothesis didn't fit the data. So then I thought about what could be producing this kind of effect, and I began to wonder whether surface reactions on polar stratospheric cloud surfaces could be the cause. There had already been satellite measurements of polar stratospheric clouds, but they were considered a curiosity rather than a topic of any importance. But the ozone changes of the Antarctic weren't being seen in the Arctic, and Antarctica really is the coldest place on Earth, which leads to many more clouds there than in the warmer Arctic. No one had imagined that surface chemistry involving chlorine could seriously impact the stratosphere, but I starting thinking about ways in which ozone might be depleted through reactions involving chlorine that might happen on those clouds. I ended up proposing in my Nature paper[21] that a reaction between hydrochloric acid and chlorine nitrate could be occurring on the surfaces of polar stratospheric clouds. That reaction doesn't happen in the gas phase, but it happens quite readily on surfaces due to the incorporation of the acid onto the surface. Then the chlorine

[21] Solomon, S., R.R. Garcia, F.S. Rowland, and D.J. Wuebbles, On the depletion of Antarctic ozone, *Nature, 321,* 755-758, 1986.

nitrate comes along and you get a reaction occurring in a completely different phase that is quite amazingly fast. So that was my proposal and it turned out to be the right answer.

I also got involved in the observational side of the issue because I strongly argued that we ought to go to the Antarctic and make some measurements to figure out what's going on. It's one thing to see the ozone drop. It's another thing to measure the chemicals that actually influence ozone and to be able to make a science-based statement about why it's changing, whatever that would be. I argued that we ought to have a ground-based expedition to Antarctica, and I was fortunate enough to lead one in 1986 and again in 1987. We worked from McMurdo Station, Antarctica, where among other things we measured how the air above the frozen south absorbed the moonlight of the long polar night. I was incredibly fortunate in that two of my colleagues in the Aeronomy Laboratory had designed and built a very high-quality, extremely sensitive instrument for measuring the intensity of incoming light in the visible region of the spectrum. We could not only measure ozone with it—but also nitrogen dioxide and chlorine dioxide, two very important chemicals. And chlorine dioxide turned out to be there in huge amounts—without reactions on those clouds it couldn't have been present. So we made some of the first measurements that showed what caused the ozone hole.

Being in the Antarctic for three months was one of the most exciting, challenging, fantastic experiences of my life. When the door of the airplane opened and that incredibly cold (-40° C) air rushed in and hit my face, it felt like being on another planet. I have never seen such unspoiled, natural beauty. The intense purple and blue colors of Antarctic twilight are incredible. The polar stratospheric clouds—those very clouds that facilitate the depletion of the ozone—are wonderful to see. They resemble tiny suspended rainbows. I was really honored in 1994, when the Solomon Glacier (78°23'S, 162°30'E) and Solomon Saddle (78°23'S, 162°39'E) were named in honor of my leadership in Antarctic research. I also was stunned when I received the United States National Medal of Science, which is the highest honor an American scientist can receive, for insights in explaining the Antarctic ozone hole.

Other topics I have enjoyed studying include how volcanoes, though not damaging to the ozone layer by themselves, can speed up CFC-induced ozone destruction. After explosive volcanic eruptions, liquid sulfate aerosols can form, and those surfaces can cause major ozone loss at mid-latitudes via processes analogous to that in the Antarctic. I have also done some work on the issue of gases other than carbon dioxide that could contribute to global warming. Among other things, one question my colleagues and I have probed is the role of perfluorinated chemicals like CF_4, SF_6, and others. There's not a lot of that stuff in the atmosphere today, so I'm not saying they are significant contributors to today's global warming. But we've shown that these molecules live for literally thousands of years—they may as well be immortal—and they are potent absorbers of infrared light, hence greenhouse gases. A molecule that can outlast the pyramids of Egypt might be one to think about venting to the atmosphere especially carefully. In fact, maybe one of the most interesting overlaps between ozone depletion and climate change is that the ozone-depletion issue shows the need for science to help understand not just impacts but also time scales in environmental problems. It's all part of using science to help inform society about the questions that have to be asked—not just what is or may be happening now, but if something does happen tomorrow, how long will we have to live with it?

Recently, I've taken a change of direction doing work more related to understanding how various molecules in the lower atmosphere actually absorb incoming sunlight. It's relevant to the climate problem, but it really addresses the fundamentals of how the system works. So I'm moving into an area that is related to the climate problem, but a little more esoteric. I'm coming back to something like my early work on auroras and things like that, helping to lay the basis for understanding how radiation propagates—how sunlight really gets in through the atmosphere.

I think of myself as a very privileged person. I have the satisfaction of doing things that are relevant. Having a goal and the idea of public service are absolutely consistent with what I do as a scientist. This planet supports many people and will have many more who will be putting all kinds of chemicals and substances in the atmosphere, so the 21st century will have enormous opportunities for people in many areas of environmental chemistry, including climate change. Science has a very important role to play in serving society, helping to understand what is happening and why. But in my opinion, that is where my job as a scientist stops and those of others—the economists and the politicians—begins. Science is an important input to many societal choices, but it is only one input. And when we keep our science focussed on the historic standards of scientific values of objectivity, leaving politics and personal opinion to others, I think we contribute the most we can to the world.

TECHNOLOGY OFFERS IMPROVEMENT

Robert M. Solow
Massachusetts Institute of Technology, USA

Unlike most of the people in this book, I am an economist and not a biologist or chemist or physicist. So my story is likely to be rather different from theirs.

I was a high school student during the Great Depression of the 1930's and graduated just at the beginning of the Second World War. Indeed I was for three years a soldier in that war. It was pretty clear to my generation of students that our societies were functioning badly. Unemployment, dictatorship and war were all very destructive events; and they seemed to be connected with each other. Unemployment and inflation in Germany contributed to the rise of Hitler and the Nazis, and war was then inevitable. This I had seen at first hand.

When the time came to choose a subject to study at my university, I think I was attracted to economics because it promised a chance to understand one of the great catastrophes of the 1930's. More than that, economics was quantitative, precise and logical rather than merely ideological or rhetorical. I don't think that I ever believed that economics could be an exact science, like physics or even biology, but I still think that it is very important to observe carefully and think clearly and quantitatively about economics, and to give in as little as possible to prejudice and self-interest. It may be even more important in economics than in science because there is not much possibility of doing experiments, and because self-interest can be very strong.

Later on, when I had finished my studies and was beginning to teach and do research, another set of economic problems came into view. The poorer nations of the world—including even some in Europe—were beginning to think about their long-run economic development, about narrowing the wide gap in income between themselves and the rich industrial countries. And the rich nations, which had poor people of their own, were also thinking about economic growth, about ways to achieve a steady increase in their income. So I began to try to improve our understanding of the economics of the long run, of the process of increasing human productivity and improving the standard of living. The problem of unemployment has not gone away, but at least the world economy has avoided deep depressions in the 60 years since the end of the Second World War.

It was an excellent choice of a research topic, or at least a lucky one. In 1956 and 1957 I found a good way to make sense of what happens in the long-run growth of a national economy. The basic elements—labour, capital, natural resources, technology—could be put together in one framework, with a place for most of the facts we had learned from the histories of modern economies. Fifty years later, that basic framework is still in use by economists all over the world, though of course it has been improved by many people.

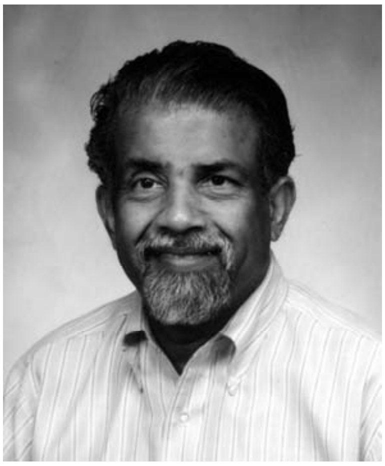
The framework led to a way of analysing the historical record to see how much of a country's growth came from increasing employment, how much from mechanisation, and how much from other sources. One very surprising conclusion soon emerged. Most of the observed improvement in productivity and income per person has come not so much from material capital, like machinery and buildings, but from better technology and from the education and training of the working people who have to use new technology. This has been true even in agriculture. In fact it is probably true that the good thing about up-to-date machinery is that it allows new technology to move from the drawing-board (or the computer-screen) into actual production. New technology has to be understood to include improvements in the way production is organised. All this applies most clearly to nations that have already taken some steps toward industrialisation. For even poorer countries, the replacement of hard physical labour by the use of machinery remains an important task.

The study of long-run economic growth is not finished, and may never be finished. The reason has to do with an important difference between economics (along with other social science) and most natural science. As our societies and our technological possibilities evolve, in directions that no one can predict, many of the underlying cause-and-effect relations change too. So economics may have to rethink their basic ideas from time to time.

We live in the short run, and, in the short run, questions about employment, recession and inflation are still with us and still important. Unemployment rates in Europe used to be two or three points lower than in the U.S. Now they are two or three points higher. There are also big differences within Europe. Why is that? Many economists, including me, are trying to answer these questions. There is general agreement about some basic facts and their meaning, though not about all. But every year brings new evidence and some years bring new ideas. Some years even bring good new ideas.

AN ACCOUNT OF MY THEORETICAL PHYSICS CONTRIBUTIONS

Ennackal C.G. Sudarshan
University of Texas at Austin, USA



The beauty of classical geometry captivated me while in middle school, and in the high-school years I came across analytic geometry. But then came the pleasant surprise that mathematical schemes could be used to describe physics. First came the computation of the period of the simple pendulum and the focal length of a lens. The undergraduate year showed that mathematical models could be used to understand various aspects of physics.

In research this became a useful method; my first piece of research was to use models of tracks of cosmic rays in photographic emulsions and the random noise, that affected these measurements; we could (and did) determine the masses of cosmic-ray particles. Statistical models also were used to determine the number of mesons which came from antiproton annihilation.

My first major contribution to particle physics was the determination of the precise form of beta radioactivity. This is a process in which a nucleus increases its charge by one unit and at the same time an electron along with a neutrino is emitted. Even though a, b, c radioactivity was known from the beginning of the twentieth century. It was my doctoral research that established the precise law (vector-axial vector) in l957, easily the most important discovery in that decade. I was not yet 25 years old then.

Quantum mechanics is usually formulated for isolated systems. When the system is open to disturbing influences from outside, we have the analog of stochastic processes. In 1961 I gave a formulation of a stochastic quantum mechanics. This is now very relevant for quantum computing. Amusingly, these results have been rediscovered, many times since 1961.

When particles can be created or destroyed, the proper mathematical model is that of a quantum field. It is particularly relevant for the emission and absorption of light. But most often it was the custom to use ordinary wave optics. In 1963 I showed that for most applications this Optical Equivalence Theory is an exact correspondence and is now standard in Quantum Optics. Along with John Klauder, I have published a book on Quantum Optics.

The (classical) law of radioactivity is strictly exponential. But in quantum theory at very short times the decay rate itself vanishes; and so if one keeps observing the unstable object very frequently, it would not decay at all! This effect discovered by Baidyanath Misra and myself is now known as the Quantum Zeno effect. Since then this effect has been experimentally verified.

Quantum states can be superposed like ordinary waves. But when we consider two-particle states such a superposition can lead to nonlocal correlations called quantum entanglement, discovered by Schrödinger and rediscovered by Einstein, Podolsky and Rosen. These correlations appear as if information can be instantaneously transmitted and leads to what is nowadays called quantum teleportation. We have been developing methods to detect such entanglement for various kinds of states. We make use of the method of quantum tomography, which is an adaptation of medical tomography. This work is done together with one Russian and two Italian physicists (Manko, Marmo and Zaccaria).

The great miracle is that the physical world can be understood in terms of mathematical models which can make predictions that could be verified by laboratory experiments. From the beginning of my research career I have been fascinated and even mystified by this.

NATURE'S SUPERB LOGIC

Gerardus 't Hooft
Utrecht University, The Netherlands

© Courtesy of Gerardus 't Hooft

Taming the Laws of Physics, this is what caught my imagination, when I was a child. Automobiles, bicycles, radios had been invented by people who had understood how the forces of Nature work. How did they do that? I figured that these inventions must have been preceded by an insight that, perhaps, I *could* understand: the wheel. Wheels are marvelous; they allow us to move heavy weights over smooth surfaces with very little resistance. The world of animals and plants does not know about wheels. Wheels must have been discovered by someone. I envied this person. How has this discovery come about? Would I have been smart enough to invent the wheel if it had not yet existed? Probably not.

Questions like these were keeping my mind busy. Maybe not everything has been discovered yet. Certainly not all diseases could be cured, the gravitational force could not be cancelled out, nobody had yet flown to the Moon. Immense forces were thought to exist inside atoms, but it was said that these "smallest particles" were not yet well understood. Huge monsters called dinosaurs lived on Earth millions of years ago. Surely, many more things had to be out there that still had to be discovered, and, if so, I decided to discover all of them.

The nice thing about Nature's Laws is that they are fair. They are the same for everybody, and nobody has the power to change them, unlike the Laws that humans have invented themselves: you should speak politely, use your knife and fork when you eat, go to school and brush your teeth. Those rules could be changed by someone overnight without advance warning, but they can't do that with Nature's Laws. Also, these Laws do not contain contradictions. They can't.

I was extremely fortunate to have good teachers. They told me about mathematics. Mathematics is a superior language if you want to describe Nature's Laws, better than the languages normally used by humans. Mathematics describes things that are true: $\frac{1}{2}+\frac{1}{3}=\frac{5}{6}$ Nothing, nobody can change that. There are many properties of numbers and geometrical figures that you can figure out using mathematics. If you make a triangle whose sides have the ratio $3:4:5$ in length, then one of its angles is perfectly straight. This is something that one can understand. I figured that, since I planned to make discoveries about Nature, I had to master mathematics.

My friends at school did not care much about these things. That did not bother me. I became very good at mathematics. When I went to the University I already knew a lot about physics, or so I thought. Actually, there still was a lot more to be learned. Simple mechanical laws of motion could be handled much more elegantly by mathematics than I had realized before. There were many more things known about the atom. There existed a fantastic scheme that controls the tiny particles called “Quantum Mechanics”, and it is based on even more extravagant mathematics. Superb laws of logic underlie all of this. Nature turned out to be even more beautiful than I had ever imagined.

And indeed, there was a lot that still had to be discovered. There is so much that still has to be discovered today that *every* scientist, not only the smartest ones, is making discoveries. Making a discovery gives you a kick, it is wonderful. So, indeed, I made discoveries. Small ones and big ones. Sometimes, you don’t believe your own eyes: an equation that I had worked on for months turned out to describe *exactly* some basic forces of Nature, but I hadn’t dared to believe it; I hadn’t fathomed all logical consequences of this equation, and I had always thought that this simple identity would be too perfect to apply to the real world. Now, we know that it indeed does.

Paul Dirac had the same experience. He had derived a new equation for the electron, using pure logic. But then he noticed that his equation implies the existence of anti-particles, electrons whose electric charge is positive instead of negative. Dirac did not have the courage to predict new particles from a simple equation, so he suggested that these must be the protons. Several properties of the proton do not agree with this, but, Dirac thought, maybe I haven’t understood it all yet. Well, the anti-particle of the electron was soon discovered experimentally, exactly in agreement with Dirac’s own equation. “This equation is smarter than its inventor!”, Dirac was quoted as saying.

Of course, I realized how lucky I have been. I was lucky to have been born with a curious mind, wanting to investigate the world that we live in, but I was also very lucky with the teachers around me, and the opportunities given to me.

In the mean time, the world has changed. Most of these changes are due to scientific discoveries. People have been on the Moon now. We now understand the particles out of which atoms are made, and the forces by which they are controlled. But many more discoveries are *still* waiting ahead of us. We haven’t traveled to Mars yet, and the other planets and moons. The particles in the atom in turn must be controlled by even tinier objects that we do not understand, strings, or sheets, or something else. We do not understand how the gravitational force squares with what we know about these particles. Biologists are unraveling the DNA codes of life, but they cannot yet cure all diseases, and they do not know how to improve DNA codes by hand, or how to write entirely new DNA codes for organisms that do not yet exist. The memory chips in our computers are still stone-age compared to what should be possible theoretically.

Future generations of smart kids should be able to figure these things out. Perhaps we are all dinosaurs compared to the generations of the distant future, if today’s children decide to exploit the tremendous opportunities science is likely to offer, by making their own new discoveries. It is like playing chess with Nature, but then better: the rules haven’t been made by man. The rules are fair. The ones with the smartest ideas get there first. Only a few decades ago, people from poor countries, or, countries separated from the West by an Iron Curtain, were in a severely disadvantaged position to make any discovery at all. Today, the situation is much better: all you need is an Internet connection, and you will have access to the most up-to-date knowledge of the most reputable centers of science in the world.

The History of Lasers

Charles H. Townes
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From the time when man first saw the sunlight until recently, the light which he has used has come dominantly from spontaneous emission, like the random emission of incandescent sources. Lasers operate on the principle of stimulated emission. That is, the energy delivered by the molecular or atomic system has the same field distribution and frequency as the stimulating radiation and hence is in phase with it. This type of electromagnetic radiation possesses several features. The narrow light beam produced by most lasers maintains its direction and small size over very large distances. Because of that, this narrow beam of coherent light is suitable for a wide variety of applications. Because light is amplified by stimulated emission, it can be made almost indefinitely powerful. Lasers have been used in industry for cutting and boring metals and other materials, and for high precision optical equipment. In medicine, lasers are used for surgery, and can perform operations which are not practical by other methods. Holography is based on the fact that actual wave-front patterns, captured in a photographic image of an object illuminated with laser light, can be reconstructed to produce a three-dimensional image of the object. Lasers have also opened up new and diverse avenues of scientific research, such as new instrumentation for a wide variety of precision measurements, the study of crystals, chemical reactions and combustion, plasma physics, extremely low temperatures, and communication. A single laser beam can, over a short period of time, have more power than all the electrical power now used on earth; a laser beam can also be so delicate and precise that it can pick up and move a single biological cell without injuring it. I will take this occasion to recount my role in the invention of lasers, and, using this as example, briefly reflect on the character of science itself.

As a student, I was interested in a variety of things: natural history and biology, swimming, newspaper editing, football band—to name a few. My interest in physics won over eventually. I was fascinated by physics since my first course in the subject because of its beautifully logical structure. My formal education was thus completed with a Ph.D. from Caltech for work on isotope separation and nuclear spins.

I then worked extensively during World War II, as a member of the technical staff of Bell Telephone Laboratories, on the design of radar navigation and bombing systems, and related technology. From this, I turned my attention to applying microwave techniques of radar research to spectroscopy, which I foresaw as providing a powerful new tool for the study of the structure of atoms and molecules and as a potential new basis for controlling electromagnetic waves. This evolution and connection was natural.

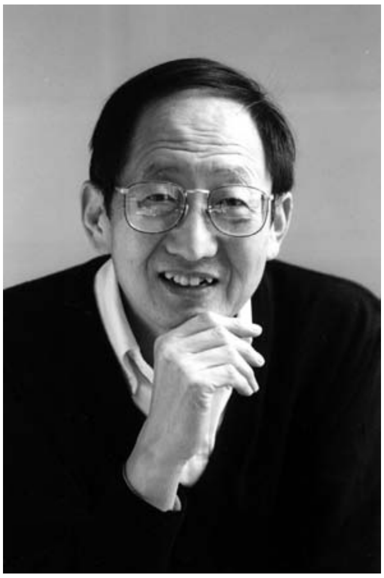
I carried on my research as a professor of physics at Columbia University, particularly focusing on the interactions between microwaves and molecules, and using microwave spectra for the study of the structure of molecules, atoms, and nuclei. In 1951, I conceived the idea of the amplification and generation of electromagnetic waves by stimulated emission, and a few months later my associates and I began working on a device using ammonia gas as the active medium. The first successful device was completed about three years later. My students and I named it “maser”, which is an acronym for Microwave Amplification by Stimulated Emission of Radiation. In 1958, my brother-in-law, Arthur Schawlow, and I showed theoretically that masers could be made to operate in the optical and infrared region and proposed how this could be accomplished in particular systems. This work resulted in our joint paper on optical and infrared masers, or lasers (Light Amplification by Stimulated Emission of Radiation). The rest, as they say, is history.

My interest in the fields of quantum electronics and astronomy was continued at MIT and later at the University of California in Berkeley, which still serves as the hub for my activities. There is some truth to the idea that in science individual contributions of great significance are possible. Indeed, the invention of lasers illustrates this idea very well. But today’s range of laser applications could not have been foreseen by anyone at the time of its invention, and could not have occurred through the efforts of any single individual. In general, the large-scale development of science is basically a social phenomenon, dependent on the hard work and mutual support of many scientists and on the societies in which they live. Scientists do stand on the shoulders of giants from the past. No one can be an intimate part of a scientific effort without being appreciative of the exciting and cumulative effects of this broad cooperation and the many individual contributions to science. Nor can any scientist be an intimate part of his world without wondering if, somehow, aspects of human affairs can benefit still more from these cumulative effects if a larger portion of human efforts, as in science, can be harnessed to be additive and mutually supporting. Our nature is, of course, more severely taxed in nonscientific fields to clearly recognize overriding human goals and to work towards them with objectivity. In such fields we rarely have clear-cut experimental results to straighten us out when we err. But the imposing edifice of science provides a challenging view of what can be achieved by the accumulation of many small efforts in a steady, objective and dedicated search for truth.

Lasers illustrate another aspect of scientific discovery and invention. Many of laser’s uses have been extraordinarily beneficial to humans, a few not so. The fear that science can sometimes be used for destructive purposes can be no reason to shun it. Shunning it also means shunning its vast potential for the enrichment of human life and for the alleviation of human suffering. As long as the human mind remains curious, it will discover new vistas and invent new devices. Our progress depends on our ability to cope well with this characteristic of our species.

CURIOSITY HAS BEEN MY BENT

Daniel C. Tsui
Princeton University, USA



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My life has had many journeys. My childhood years were in a remote village in Henan, China, where my main activity was helping my father tend and harvest crops. My parents, who never had the opportunity to learn how to read and write, recognized that adequate education was not to be possible during that time in the village, and so seized the first and perhaps only opportunity for education, when I left with a relative, ultimately ending up, at age 11, in faraway Hong Kong.

The school I attended in Hong Kong, Pui Ching School, had some outstanding teachers, especially in the natural sciences, which inspired me. Normally they would have been university professors in China, but the upheaval of the war forced them to leave, with high school teaching then their only opportunity. In unconscious ways, the teachers challenged us, living in a most commercialized city, to look beyond the dollar sign, and see the exploration of new frontiers in human knowledge as an intellectually rewarding and challenging pursuit.

My preference was to be a doctor, and I was admitted to the medical school of National Taiwan University, however my need for a full scholarship led me to my church pastor's alma mater, Augustana College, in the United States, where I graduated with a major in mathematics.

I was inspired by C. N. Yang and T. D. Lee who were awarded the Nobel Prize for Physics in 1957, and who both went to the University of Chicago. And so I chose University of Chicago for graduate school, majoring in experimental physics. I had great satisfaction working under and with Professor Royal Stark, an energetic, young solid state experimentalist. With him I had full opportunity of tinkering and of learning from the bottom up: from engineer drawing, soldering, machining, to construction and building the laboratory apparatus.

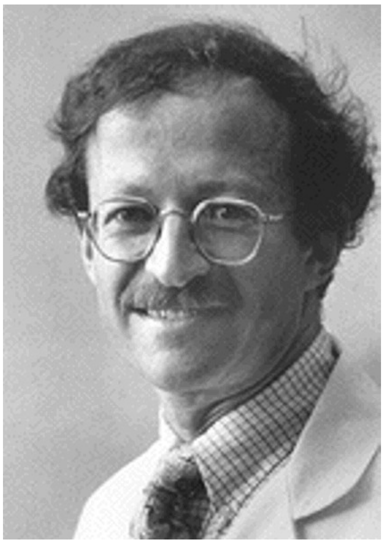
I left Chicago in 1968 and took a position in Bell Laboratories in New Jersey to do research in solid state physics. It was there that I took another journey, leaving behind the more familiar landscape of metal physics and entering a new, strange terrain of what we nowadays call low dimension electron physics in semiconductors. I have elsewhere described this early research shift as "wandering into a new frontier." For me, at least, "discoveries" came from doing (and redoing) varying experiments, talking with other scientists at the then provocative Bell Labs, thinking over related theoretical ideas, taking advantage of various technical advances, and working together with others. It was only in time, as experiments confirmed some of my hunches, and yielded more information, that what might be found became clearer.

The discovery that the 1998 Nobel Prize in Physics was given for was done by myself and Horst Stormer in 1982 for “discovering that electrons acting together in strong magnetic fields can form new types of ‘particles,’ with charges that are fractions of electron charges,” and also described by the Nobel Committee as “their discovery of a new form of quantum fluid with fractionally charged excitations.” In 1982, we shared the unexpected and exciting results with a theorist, Robert Laughlin, who only a year later came up with equations that offered a theoretical understanding. He shared the Nobel Prize with us.

So I have no fascinating story about childhood science experiments, or moments of grand insight. But curiosity has been my bent, and it led me to the edges of scientific knowledge where it was my fortune to determine, by experiment, phenomena that continue to generate new findings and understandings. I’ve been teaching at Princeton University for over two decades, and advising students in their research. It is clear to me that there are many intriguing insights and productive technical advancements in science that are yet to be discovered!

OBSERVATIONS GIVE RISE TO APPLICATIONS AND IMPROVEMENT

Harold Varmus
Memorial Sloan-Kettering Cancer Center, USA



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Unlike most people who have had successful careers in the sciences, I was not strongly drawn to either laboratory life or the wonders of nature in what are generally thought to be the "formative years"—when I was a student in grade school and high school, growing up in a suburban town outside New York City. Throughout most of those years, my major goals were rather typical for my age group: doing well in school, regardless of the subject, and having fun in sports and at parties. I wasn't an especially good athlete and was probably considered a bit more "intellectual" than others in my high school class, because I had strong interests in literature and politics. Because my father was a physician and enjoyed his work, I entered college with the idea that I would prepare for medical school. But the most exciting events in my undergraduate life occurred during my studies of English literature and through my work as editor of the college newspaper. So, after college, I set off for graduate studies in literature.

Early in my graduate career, I began to feel the absence of those aspects of a scientist's approach to life that I hadn't appreciated very much before—the power of drawing conclusions from observations of natural phenomena and the potential to apply new learning to solve practical problems and improve people's lives. Since I had completed a pre-medical curriculum, the possibility of putting some science back into my life seemed most easily achieved by going to medical school. So I did. But along the way, at medical school in New York City and during my training thereafter, I became increasingly interested in the newly created opportunities to understand diseases—cancers, infectious diseases, blood disorders, and others—at the level of cells, molecules, and genes, thanks to the emergence of the field of molecular biology. As a result, I began doing serious work in a laboratory for the first time—at the National Institutes of Heath in Bethesda, Maryland—at the relatively advanced age of twenty-eight. (I do not recommend such a protracted pathway into science for others, but it may be useful to remember that slow starts do sometimes occur and can still lead to satisfying results).

In my experimental work at the NIH, I learned how small chemicals, like glucose or cyclic AMP, alter the expression of genes that control cell metabolism in bacteria. My rapid transition from a physician taking care of patients to a scientist trying to understand biological systems at a molecular level was very exciting, and it encouraged me to believe that I could make another transition—to use similar methods to understand what happens when a normal cell becomes a cancer cell or when a virus grows in animal cells.

So I found a place in a laboratory at the University of California Medical School in San Francisco to learn about working with viruses and cancer. I began what proved to be an over twenty-year series of experiments that asked how a class of cancer-causing viruses (the retroviruses) multiplies and how some retroviruses alter the behavior of cells so that infected cells form a tumor. Along the way, my colleagues and I discovered that a single viral gene with cancer-causing properties was derived from a gene present in normal animal cells; moreover, the progenitor gene performed functions essential to normal development of the animal. This finding set a precedent that we and others subsequently showed to be true for many other so-called “oncogenes” (cancer genes). Many of these oncogenes are now known to be involved in the causation of human cancers, even when (as is usual) no viruses are present.

Such discoveries about genes are now beginning to change the way in which cancers are diagnosed, classified, and treated; several new therapies have been designed to inhibit the products of oncogenes; and the public’s current optimism about controlling cancer in the foreseeable future is based largely on this fundamental change in our conception of the origins of cancers. As someone who now runs a large center that performs cancer research and takes care of cancer patients, I find these changes immensely gratifying and hopeful. They have fully justified my decision to leave my studies of literature—although I still get some of my greatest pleasures from reading novels and watching the plays of Shakespeare!

A REWARDING LIFE

Rafael Vicuña
Pontificia Univ. Católica de Chile, Santiago

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I was born in Santiago, Chile, during the spring of 1949. My family could be considered traditional in my country, with ancestors that have played leading roles in politics, business and also in the catholic church. Prior to me, there had been no scientists in my family. Therefore, I can say with certainty that I was not influenced by any relative in my decision to devote my life to research. For this reason, I have always considered myself a "black sheep" in my family. My incentive did not stem from my high school science teachers, as by that stage my vocation had already been shaped. All I can say is, that since I can recall, I have felt fascinated with the *understanding* of nature. I still carry with me, my childhood science books that my father would give me for my birthday or for Christmas.

I first studied Biochemistry at the University of Chile, where I received my first laboratory training under Professor Osvaldo Cori. In these early years, I was strongly influenced by Dr. Cori's human values and by his highly developed scientific rigor. In 1974, I joined the Ph.D. program at the Albert Einstein College of Medicine in New York, where I received a Masters degree in 1976 and my Ph.D. degree in 1978. My Ph.D. thesis was conducted under the guidance of Dr. Jerard Hurwitz, who steered me onto the path of critical reasoning required for scientific maturity. In August of 1978 I was hired by the Faculty of Biological Sciences at the Catholic University of Chile, where I continue to work. Initially, I collaborated with other professors who were working on DNA replication, the same subject matter of my Ph.D. thesis. My main contribution in this period was the identification and characterization of two protein factors that selectively inhibit viral DNA from the fX174 phage from using the replication machinery of other single stranded DNA phages such as fd or M13. I later elaborated my own research project in the field of highly thermophilic bacteria. Together with colleagues within my group we characterized the enzymes involved in the restriction-modification system of bacteria within the genus *Thermus,* along with characterizing plasmids from these bacteria. Several of these plasmids were later used by other colleagues as vectors in DNA transformation systems of thermophilic bacteria. In these early years, we further isolated and characterized the DNA polymerase from *Thermus thermophilus*. Due to the subsequent development of PCR techniques, this paper received many citations.

In 1986 I was awarded a Guggenheim Fellowship, which took me for four months to the group of Dr. Kent Kirk at the Forest Products Laboratory located at the University campus in

Madison, WI. Being one of the world leaders in the field of lignin biodegradation, Dr. Kisk's laboratory ws my choice for the initiation of a long term research project in a relevant, although relatively unexplored field. Once back in Santiago, I started to study the bacterial metabolism of lignin-related aromatic compounds. Our main accomplishments in this area were the isolation, identification and characterization of natural bacterial strains capable of metabolizing lignin model compounds, the elucidation of metabolic pathways involving novel catabolic intermediates and the discovery, cloning and sequencing of the enzyme benzaldehyde lyase. In the early 90's, I decided to switch fields to study fungi, since these are the most efficient ligninolytic microorganisms in nature. Since most groups in the field had been studying the fungus *Phanerochaete chrysosporium*, I chose as a model the basidiomycete *Ceriporiopsis subvermispora* as, when growing on wood, this microorganism is highly selective towards lignin. Since then, we have elucidated that the ligninolytic system of *C. subvermispora* is composed of a manganese-dependent peroxidase (MnP) and a copper containing phenol oxidase called laccase. We also found correlations between lignin decay by *C. subvermispora* and the production of MnP and laccase. Both enzymes are translated as a family of isoforms, with isoelectrofocusing patterns that differ according to the growth conditions of the fungus. Based on the N-terminal sequences of various MnP isoenzymes, we proposed that there might be more than one gene encoding this enzyme in *C. subvermispora*. This hypothesis proved to be correct. In fact, we have already sequenced four genes coding for MnP and also one gene coding for laccase. The expression of these genes is regulated by metals. Other significant contributions have been the demonstration that MnP from *C. subvermispora* does not have a strict requirement for Mn(II), as does the enzyme from *P. chrysosporium*. Furthermore, we demonstrated the presence of LiP-like genes in *C. subvermispora* in spite of the fact that this fungus does not produce lignin peroxidase (LiP). Finally, we have described in this system a novel mechanism for the production of the extracellular hydrogen peroxide required as a substrate by MnP. Whereas other fungi secrete various kinds of oxidases for this purpose, in cultures of *C. subvermispora* it is MnP itself which generates peroxide by oxidizing organic acids produced by the fungus. The mechanism employed by MnP in this reaction involves the production of both oxygen and carbon centered radicals, as well as superoxide anion.

My scientific life has been highly rewarding. It has allowed me to develop without restriction my vocation for inquiry and to interact with outstanding colleagues along with creative young students. I have been given the opportunity to travel frequently to North America, Europe and Asia. At present, I am honored to be Vice President of the Chilean Academy of Sciences and a member of the Pontifical Academy of Sciences, the Latin American Academy of Sciences and the Third World Academy of Sciences (TWAS). In my leisure time I play golf and indulge my passion for literature relating to the history and philosophy of science.

ONE'S SCIENCE SURVIVES BEYOND ONESELF

Klaus von Klitzing
Max-Planck-Institut Für Festkorperforschung at Stuttgart, Germany

© Courtesy of Klaus von Klitzing

Born in 1943 at the end of World War II, I do not remember the terrible time of the war. I belong to the generation that grew up in a time of continuously improved living standards. The political barriers between countries became more and more transparent and international organizations like the United Nations were developed as a guarantor for human rights. With the Internet we have today a worldwide network for communication and the dissemination of knowledge. As a scientist, I am optimistic for our future if we are able to avoid unacceptable gradients in living standards and improve worldwide the educational system in such a way that a manipulation of the public opinion can be avoided. Especially scientists will have an important responsibility since the fundamental laws in science are universal and the logical consequences leave no room for ideological interpretations. The Abdus Salam International Centre for Theoretical Physics is a wonderful example for the internationality of science; as long as a worldwide network of scientists exists, I am sure that this network will stabilize the co-existence between individuals and countries all over the world.

How did I start with science? Perhaps when I earned some money at the age of 7 by doing some simple calculation for my father—mainly adding many numbers. At school, at the beginning, I was in a classroom where four different age-groups were taught together so that I had the chance to learn something from the advanced classes. The teacher identified my interest in mathematics so that I had to explain and solve mathematical problems for children who were much older.

My dream was to study mathematics at the university but, very soon, I saw that physics is the ideal subject in which to apply mathematics and to solve problems in real life. I was never disappointed that I became a physicist. During my university time, the first lasers were developed (today everyone uses a semiconductor laser to hear music from a compact disk) and semiconductor physics was an exciting new field. During the free time between terms, I took jobs at the Physikalisch-Technische Bundesanstalt (PTB) in Braunschweig, the German national institute of metrology. This was really an ideal combination for learning new physics on the one hand and earning some money for my study at the university on the other. High precision measurements are one of the characteristics of metrology, and I learned how to measure accurately the melting point of gold, to measure the exact distance between atoms in a crystal and to calibrate electrical resistors with high precision. At that time, I did not know that this experience would be very valuable for the discovery of the Quantum Hall effect, which led finally to the Nobel Prize in Physics 1985. From my work at the PTB, I knew that electrical resistors are not very stable and change the resistance value due to variations in temperature and microscopic changes in the wire. It was a dream to produce a resistor which has a value which is the same everywhere in the world, and stable with time so that one has a universal reference standard. I was lucky that I saw during an experiment in the night of 4/5. February 1980 by accident a phenomenon which resulted in a new type of electrical resistor whose resistance depends exclusively on fundamental constants like the charge of an electron and the Planck constant. The value of this resistance is always 25812.807 Ohm, today known as the von Klitzing constant. This discovery was a result of basic research on a device which can be found in any computer—the silicon field effect transistor. As a scientist one has the chance to contribute to an extension of knowledge, which survives beyond one's life.

"THE RED CAMARO"[22]

Steven Weinberg
University of Texas at Austin, USA
Austin, USA

On October 15, 1764, Edward Gibbon conceived the idea of writing the history of the decline and fall of the Roman Empire while he was listening to barefoot monks singing vespers in the ruins of the Roman Capitol. I wish I could say I worked in settings that glamorous. I got the idea for my best-known work while I was driving my red Camaro in Cambridge, Massachusetts, on the way to my office in the physics department at Massachusetts Institute of Technology.

I was feeling strung out. I had taken a leave of absence from my regular professorship at Berkeley a year earlier so that my wife could study at Harvard Law School. We had just gone through the trauma of moving from one rented house in Cambridge to another, and I had taken over the responsibility of getting our daughter to nursery school, playgrounds, and all that. More to the point, I was also stuck in my work as a theoretical physicist.

Like other theorists, I work with just pencil and paper, trying to make simple explanations of complicated phenomena. We leave it to the experimental physicists to decide whether our theories actually describe the real world. It was this opportunity to explain something about nature by noodling around with mathematical ideas that drew me into theoretical physics in the first place. For the previous two years, I had made progress in understanding what physicists call the strong interactions—the forces that hold particles together inside atomic nuclei. Some of my calculations had even been confirmed by experiment. But now these ideas seemed to be leading to nonsense. The new theories of the strong interactions I had been playing with that autumn implied that one of the particles of high-energy nuclear physics should have no mass at all, but this particle was known to be actually quite heavy. Making predictions that are already known to be wrong is no way to get ahead in the physics game.

Often, when you're faced with a contradiction like this, it does no good to sit at your desk doing calculations—you just go round and round in circles. What does sometimes help is to let the problem cook on your brain's back burner while you sit on a park bench and watch your daughter play in a sandbox.

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[22] First published in *George*, October 1997. Reprinted in *Facing Up: Science and its Cultural Adversaries*, by Steven Weinberg (Harvard University Press, Cambridge, MA, 2001).

After this problem had been cooking in my mind for a few weeks, suddenly on my way to MIT (on October 2, 1967, as near as I can remember), I realized there was nothing wrong with the sort of theory on which I had been working. I had the right answer, but I had been working on the wrong problem. The mathematics I had been playing with had nothing to do with the strong interactions, but it gave a beautiful description of a different kind of force, known as the weak interaction. This is the force that is responsible, among other things, for the first step in the chain of nuclear reactions that produces the heat of the sun. There were inconsistencies in all previous theories of this force, but suddenly I saw how they could be solved. And I realized the massless particle in this theory that had given me so much trouble had nothing to do with the heavy particles that feel the strong interaction; it was the photon, the particle of which light is composed, that is responsible for electric and magnetic forces and that indeed has zero mass. I realized that what I had cooked up was an approach not just to understanding the weak interactions but to unifying the theories of the weak and electromagnetic forces into what has since come to be called the electroweak theory. This is just the sort of thing physicists love—to see several things that appear different as various aspects of one underlying phenomenon. Unifying the weak and electromagnetic forces might not have applications in medicine or technology, but if successful, it would be one more step in a centuries-old process of showing that nature is governed by simple, rational laws.

Somehow, I got safely to my office and started to work out the details of the theory. Where before I had been going around in circles, now everything was easy. Two weeks later, I mailed a short article on the electroweak theory to *Physical Review Letters,* a journal widely read by physicists.

The theory was proved to be consistent in 1971. Some new effects predicted by the theory were detected experimentally in 1973. By 1978, it was dear that measurements of these effects agreed precisely with the theory. And in 1979, I received the Nobel Prize in physics, along with Sheldon Glashow and Abdus Salam, who had done independent work on the electroweak theory. I have since learned that the paper I wrote in October 1967 has become the most cited article in the history of elementary-particle physics.

I kept my red Camaro until it was totaled by one too many Massachusetts winters, but it never again took me so far.

MEMORIES OF A LATIN AMERICAN WOMAN PHYSICIST

Mariana Weissmann
Comisión Nacional de Energia Atomica, Argentina

The first thing I would like to express is that it takes a good deal of luck to be able to earn your living doing the kind of work that you enjoy most. My luck started when I was born in an educated middle class family in Argentina, a country where women are not discriminated at all in higher education. It continued when I went to college, as the University of Buenos Aires was then starting the best period of its history. In fact, in the 1960's there was a lot of enthusiasm in the hope that education, and in particular science, would be of help for the people in underdeveloped countries. This worldwide mood probably also inspired Professor Abdus Salam to create ICTP at that time, as a meeting place for physicists from both the developed and the underdeveloped world.

I chose to study sciences mainly because I like abstract thinking, was good at it in school, and was curious. I had no idea how long that interest would last and never thought of it as a career. Only many years later I realized that physics had become a very important part of my life. At present, when I am close to retirement, I find new novels, movies or plays all remind me of previous ones, a 'déjà vu' effect, while physics journals usually surprise me with some new idea, something that I had never thought about before.

The life of scientists in Latin America has not been at all tedious; most of us have been forced to move from one country to another. Some decided to stay in the USA or Western Europe and have permanent jobs there, but many of us remained in the region and have lived in several places, sometimes due to political persecution but mostly just due to administrative nonsense. Very few administrators in the last 30 years have understood the value of research and therefore not taken into account that creating a research group requires a long time while destroying it is a very quick process. Professor Salam lectured about this subject at different times and places but was not always understood.

My personal wandering includes a scholarship (paid by the University of Buenos Aires) for graduate studies at California Institute of Technology. After my return I presented a doctoral thesis in Buenos Aires and was appointed Assistant Professor in the newly created Meteorology Department. I taught atmospheric physics and collaborated with an experimental group interested in cloud seeding, to prevent hail falling in a wine producing region. My research work was theoretical, studying the properties of water, ice and IAg solutions. For it I used a Mercury computer, the first one installed in Latin America. It was a British born monster, full of valves, locally called Clementine, that needed all morning to heat up. Afternoons and nights it was shared by graduate students and professors, who felt very lucky to have access to such a modern facility.

Those happy times ended in 1966, with a military coup that intervened in the University and produced the resignation of about 1000 teachers, most of them scientists. Our Latin American leadership in computer science was lost, and never recovered. I was then invited for a postdoctoral stay in the USA and later returned to this southern part of the world as a research fellow at the University of Chile. For the following four years I lived in Santiago, which I left in 1972, a year before the military coup by Pinochet. With the Chilean colleagues I have kept close contact and real friendship ever since. During that period we profited from the visit of Professor Vladimir Tolmachev, from Moscow, who taught us how to use Feynmann diagrams for atomic and solid state physics. Our students of those years are among the best Chilean physicists of today.

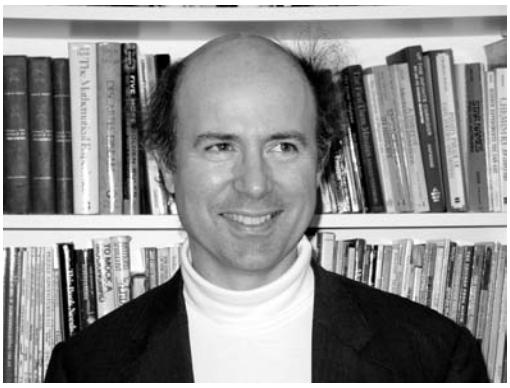
Back in Buenos Aires I was appointed at the National Research Council as a researcher, and have worked in the laboratories of the Atomic Energy Commission in Buenos Aires since then. Only during the years 1979-81 I was on leave at Universidad Simon Bolivar, Caracas, Venezuela. My research work of the last 30 years has been related to the effects of disorder, or non-periodicity, in the properties of materials. The subjects studied have been: amorphous and incommensurate solids, surfaces, clusters and large molecules. Semiconductors, high temperature superconductors, magnetic multilayers, fullerenes, are some of the particular materials studied. At first I collaborated with my former thesis advisor, Professor Norah Cohan, but since 1985 graduate students from the University of Buenos Aires come to do their research work with me. Each one of them studied a different system, either developing his (or her) own computer code or more recently using standard available ones. I am very proud because all of them are now active physicists, in Argentina or in Europe, and we are very close.

ICTP played an important role in my scientific life, as an Associate member first and as Senior Associate later. It was the place to discuss my difficulties with more experienced colleagues, the place where I realized which problems would be relevant in the near future and what would be helpful to guide the work of my graduate students. Professor Norman March's ability to organize the solid state group, listening patiently to each one of us when we presented our physics problems, must certainly be remembered. ICTP was also the place to make friends, and learn from the similarities and differences of the problems of our profession in different countries. Generosity is the quality I have always admired most of Professor Salam's original ideas. The Centre never imposed any activity on its members, each one took what he was able to use. I remember in the beginning some people could only photocopy books for their libraries but after some time they performed interesting research work and sent their own students back to ICTP.

It is a real pleasure to celebrate the 40th anniversary of this generous institution, that is even more necessary now than when it was founded. Globalization has only increased the gap between developed and underdeveloped places, so I sincerely hope it will keep the spirit of its foundation for many years to come.

DOING SCIENCE GAVE ME FREEDOM

Frank Wilczek
Massachusetts Institute of Technology, USA



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The most deeply formative events of my scientific career long preceded my first contact with the research community; indeed, some of them preceded my birth.

My grandparents emigrated from Europe in the aftermath of World War I, as young teenagers; on my father's side they came from Poland, near Warsaw, and on my mother's side from Italy, near Naples. My grandparents arrived with nothing, and no knowledge of English. My grandfathers were a carpenter and a mason, respectively. Both my parents were born on Long Island, in 1926 (and have lived there ever since). I was born in 1951, and grew up in a place called Glen Oaks, which is in the northeast corner of Queens, barely within the city limits of New York City.

I've always loved all kinds of puzzles, games, and mysteries. Some of my earliest memories are about the questions I "worked on" even before I went to school. When I was learning about money, I spent a lot of time trying out various schemes of exchanging different kinds of money (e.g., pennies, nickels, and dimes) in complicated ways back and forth, hoping to discover a way to come out ahead. Another project was to find ways of getting very big numbers in a few steps. I discovered simple forms of repeated exponentiation and recursion for myself. Generating large numbers made me feel powerful.

With these inclinations, I suspect I would have gone into some kind of intellectual work no matter what. A few special circumstances led me to science, and eventually to theoretical physics.

My parents were children during the time of the Great Depression, and their families struggled to get by. This experience shaped many of their attitudes, and especially their aspirations for me. They put very great stock in education, and in the security that technical skill could bring. When I did well in school they were very pleased, and I was encouraged to think about becoming a doctor or an engineer. As I was growing up my father, who worked in electronics, was taking night classes. Our little apartment was full of old radios and early-model televisions, and with the books he was studying. It was the time of the Cold War. Space exploration was a new and exciting prospect, nuclear war a frightening one; both were ever-present in newspapers, TV, and movies. At school, we had regular air raid drills. All this made a big impression on me. I got the idea that there was secret knowledge that, when mastered, would allow Mind to control Matter in seemingly magical ways.

Another thing that shaped my thinking was religious training. I was brought up as a Roman Catholic. I loved the idea that there was a great drama and a great plan behind existence. Later, under the influence of Bertrand Russell's writings and my increasing awareness of scientific knowledge, I lost faith in conventional religion. A big part of my later quest has been to regain some of the sense of purpose and meaning that was lost.

I went to public schools in Queens, and was fortunate to have excellent teachers. Because the schools were big, they could support specialized and advanced classes. In high school there was a group of twenty or so of us who went to many such classes together, and both supported and competed with one another. At least half of us went on to successful scientific or medical careers.

I arrived at the University of Chicago with great but amorphous ambitions. I flirted with brain science, but soon decided that the central questions were not ready for mathematical treatment (and that I lacked the patience for laboratory work). I read voraciously in many subjects, but wound up majoring in mathematics, largely because doing that gave me the most freedom. During my last term at Chicago, I took a course about the use of symmetry and group theory in physics from Peter Freund. He was an extremely enthusiastic and inspiring teacher, and I felt an instinctive resonance with the material. I went to Princeton University as a graduate student in the math department, but kept a close eye on what was going on in physics. I became aware that deep ideas involving mathematical symmetry were turning up at the frontiers of physics; specifically, the gauge theory of electroweak interactions, and the scaling symmetry in Wilson's theory of phase transitions. I started to talk with a young professor named David Gross, and my proper career as a physicist began.

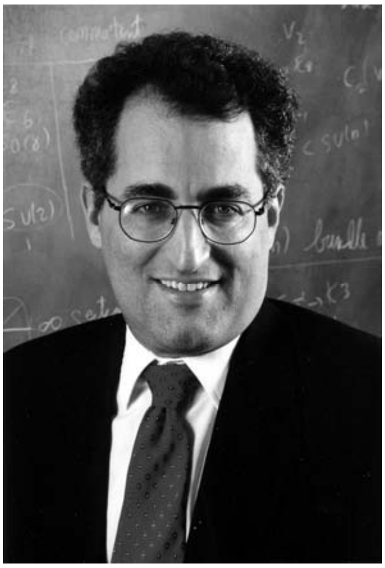
The great event of my early career was to discover the basic equation for the strong force, that is the force that holds atomic nuclei together. These equations, which define a theory called Quantum ChromoDynamics or QCD, predicted the existence of new kinds of particles, gluons, which were later discovered. The equations of QCD are based on gauge symmetry principles, and we solve them using scaling symmetry. It was very gratifying to find that the ideas I admired as a student could be used to get a powerful and accurate theory for an important part of fundamental physics. I continue to apply these ideas in new ways, and I am certain that they have a great future.

Recently I described some of my most important work and where it's led in a short and simple way, in a paper you can find at http://arxiv.org/hep-ph/0401034. There's a lot more information on my web page,

http://web.mit.edu/physics/facultyandstaff/faculty/frank_wilczek.html.

LOOKING BACK

Edward Witten
Institute for Advanced Study at Princeton, USA



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I was fascinated by arithmetic at the earliest age I can remember. A little later, I became interested in astronomy. I was about seven years old when the first satellite was launched, the Soviet Sputnik. In those days, everyone around me was excited about space, and certainly I was. I thought I would be an astronomer when I grew up. I vividly remember, however, a fear that by the time I was an adult, astronomers would have to do their work in space. I was not sure that would be safe enough. Looking back, I can see that my concerns were exaggerated. Forty some years afterwards, astronomical satellites play an important role in scientific research, but nearly all astronomers still do their work safely on the ground.

My parents gave me a telescope (a three inch reflector) when I was about nine. Once, but only once, I saw the rings of Saturn through this scope. Looking back, I am puzzled at why, as a child, I thought it was difficult to view Saturn through my telescope. It seems obvious to me now that, even through the light-polluted skies of most urban and suburban areas, Saturn is one object that, when it is high in the sky (as it is for several months every year), stands out at a glance.

At the age of eleven, I was introduced to calculus, which was the most exciting thing I had heard of, and for some years I thought I would be a mathematician. The prevailing philosophy at the time was that children should not be encouraged to go too far, too fast, with their studies, and it was quite a few years before I was exposed to more advanced mathematics. The result of this was that for a long time, I thought that mathematics consisted merely of more complicated versions of what I had already learned. This is probably one of the reasons that my interest in math waned for a while.

Ultimately, there were a number of zigzags in my interests. As a teenager, I considered a variety of fields (including history, which I majored in as an undergraduate, linguistics, and economics) before deciding at the age of twenty-one to concentrate on physics. I ultimately decided that physics and mathematics were the only fields that I was talented in and would find challenging enough.

It was a little bit of an accident that made me chose physics rather than mathematics. I went to the library to select some physics and math books. I liked the physics books better. In hindsight, I can see that I chose the physics books well, while this was less so for the math books.

I have often wondered what would have happened if I had grown up in other circumstances. Of course, I had the good fortune to have excellent opportunities to study math and science, and I ultimately took advantage of these opportunities, though only after some meandering. Many people in our world do not have the same opportunities I did. But the other aspect of my good fortune was that the educational system in the U.S. is flexible enough so that when I did decide at the age of twenty-one to become a scientist, it was still possible to do so. In many countries that do offer excellent opportunities for math and science education, the educational system is more rigid and it would be very hard to decide at age twenty-one to go into physics, without having followed the proper track up until that point. On the other hand, perhaps if I had grown up in one of those countries, I might have been channeled into math and physics from the start, without the meanders that I actually followed.

I began graduate school at Princeton in the fall of 1973 in the applied math program, which was flexible enough that I could have concentrated on almost any area of math or science. I decided to study particle physics and after a year transferred into the physics department. This was the period when the Standard Model of known particle interactions was emerging in its present form. I was barely in time with my studies to understand what was going on when the J/ψ resonance was discovered in the fall of 1974. This was one of the major milestones in establishing the Standard Model. If the Standard Model had not emerged, I believe I would have become a particle phenomenologist, trying to understand the clues offered by experiment. To a large extent, that is how I started; my Ph.D. thesis was devoted to questions such as deep inelastic photon-photon scattering (which is measured, in practice, in the reaction $e^+e^- \rightarrow e^+e^-$ + hadrons).

But I also, as a student, became preoccupied with a different kind of question that could only be asked because the Standard Model had emerged. This problem was to understand QCD and explain some of its surprising properties, such as quark confinement. Unfortunately, though fascinating insights have been obtained—and I have had a lot of fun with whatever I've been able to contribute—to a large extent this problem remains too difficult. But thinking about it led to a lot of other questions about the behavior of gauge theory in general, and I gradually got interested in those. Gauge theory turned out to have many relations to differential geometry—little appreciated at first by me or other physicists—and in time I became interested in those questions, some of which are applicable to QCD and other possible physical theories, and some of which have a surprising mathematical depth in their own right. One of the many turning points here occurred when I was a postdoc at Harvard, and Sidney Coleman explained to me the work of Albert Schwarz applying the Atiyah-Singer index theorem (which none of us had ever heard of previously) to explain properties of the Dirac operator that had been important in Gerard 't Hooft's work on the U(1) problem of QCD.

The first time I heard about string theory was also in relation to my interest in QCD. David Gross, my advisor, recommended to me around the beginning of 1975 to study 't Hooft's paper on the planar diagram expansion of strong interactions. In a modern formulation, 't Hooft suggested that QCD with N colors is equivalent to a string theory with 1/N as the string coupling constant. (The evidence that has accumulated since then indicates that this idea is on the right track, though it still has not been properly developed). This began my lifelong, though not yet fully requited, interest in the 1/N expansion of gauge theory. It didn't quite begin my interest in string theory, because at that time I started working on the 1/N expansion without learning much about string theory. Later on, when John Schwarz and Michael Green revived string theory and obtained many spectacular results, it became clear to me that unified string theory of particle interactions was the most ambitious thing I could try to work on and a suitable arena for my efforts.

About twenty years ago, one of the senior physicists whom I admire most told me that in his estimation, the key to remaining active as one grows older is that one must not be bashful about working on things that are invented by other people. He went on to give examples of physicists who in his estimation had or had not followed this rule. Now that I am my fifties, I suppose it is up to me to try to take my friend's advice.

MY EXPERIENCE IN LEARNING MATHEMATICS

Shing Tung Yau
Harvard University, USA

I was not so good in mathematics when I was in primary school. I was expected to do some routine exercises which were not interesting. However, things changed when I was 13 years old. I was shown the beauty of plane geometry. The idea of using simple axioms to derive many beautiful and complicated theorems in plane geometry really excited me. I started to explore the subject myself. I attempted to construct some interesting statements and tried to prove them by these simple axioms. It was great fun.

In fact, I read a lot of books sitting in the bookstore. (The library was very poor in those days). I soon managed to get information which was not known to my classmates or my teacher. I was proud of my "secret weapon": knowledge more advanced than that of my friends.

I still remember that there was a construction that I tried to accomplish using ruler and compass only. I tried for more than half a year and could not accomplish it. I was rather frustrated as I thought I was getting good with this kind of arguments. Finally, I found a theorem in a reference book published by a Japanese mathematician, which said that such construction was not possible. Since this proposal was not the standard statement of the impossibility of trisecting an angle, my teacher did not know it. At the same time, I was impressed by the power of algebra for understanding difficult questions in classical plane geometry.

This incident also taught me the rewards of reading reference books that were not taught in school. My high school was in fact one of the best high schools in mathematics. However, a strong interest to read and to understand materials beyond those required in class were important for my future development. Despite the great difficulty in finding books in the library, I was very much benefited by reading them. It was not easy to read them without any help. In many cases, I read them more than three times. Even then, I still could not understand them well. However, I found them very useful.

Gradually I started to digest what I was learning. When I needed to use concepts that I may not have understood at the beginning, they suddenly came back to me and I understood them very quickly. Over all these years of working in research, this kind of experience has happened again and again.

My own experience taught me that it is very important to be exposed to subjects of interest, even if the study is not immediately rewarded. I started to think about other parts of mathematics that did not seem as axiomatically formulated as plane geometry. I was rather upset by this as I believed mathematics must be completely rigorous. When I went to college, I learnt the concept of Dedinkind cut and other related constructions. I was able to realize the beauty of the system of mathematics.

Although I was never excited about working on mathematical logic, the beauty of simplicity and rigor of mathematics certainly impressed me deeply. I had no reservation about devoting my life to research in mathematics. The idea of seeking beauty of a subject has been a real driving force for many scientists. I believe any good student should see the charm of science.

When I was in Hong Kong, I was not exposed to great mathematicians. This all become different when I arrived in Berkeley in 1969. It was a radical change. My taste in mathematics became completely different from what I had before. Our taste of science really depends on the scientists that we contact. It is like fish in the water. Depending on whether a fish is in a small pond or in the ocean, the fish can see a completely different scope to their life. Meeting great scientists is certainly a very important step in a scientist's efforts to become first class themselves. I think that only in some very rare situations can a mathematician be great without such contact. Therefore I always like to go to listen to lectures given by first class scientists when I have the chance.

These are some of my personal experiences, and though I do not think I am more smart than most students, it is a fortunate road that I have taken to be able to accomplish the things I have.

MY ENCOUNTERS WITH MATHEMATICS AND SCIENCE BOOKS

James A. Yorke
University of Maryland, USA

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It is a bit mysterious what ideas will grab our attentions and stick with us throughout our lives. It is a common experience that small events can lead to big changes in our lives, and I think the same is true of our intellectual lives. I begin by describing such an event.

When I was a boy, our family would visit New York City once a year or so. To me the dominant color was the grey of the soot covered buildings. One day when I was 9, I visited the American Museum of Natural History in New York City with my family. The immense structure houses great displays running from full-sized dinosaurs to magnificent gems and displays of human culture of the past. I am sure I found much of this interesting, but only the planetarium really captured my mind with its displays of the other worlds of the solar system. In one wonderful display a huge asteroid sat immobile displaying its glassy pitted surface that had burned as it tore through the air. At another display you could weigh yourself on scales of the various planets. The scales would reveal that a 100 lb. boy would weigh 220 lbs. on Jupiter or 38 on Mars or 16 on the Moon. A scale model of the solar system showed the outer planets rotating about the sun far more slowly than the inner planets, the satellites spinning rapidly around their planets.

The biggest impact came from the planetarium bookstore. I came away with astronomy books that became my regular companions, and when I had absorbed what they had to offer me, I began spending all my afternoons after school in the library reading and learning about the solar system, learning the facts of the planets. For Mars, I learned how long its year is; it is cold, especially at night; it is nearly airless; the day is 37 minutes longer than Earth's and it has two tiny moons; the tilt of its axis is 24 degrees, nearly the same as the earth's. Jupiter spins so fast (its day is 10 hours) that its diameter through the equator is 10% larger than through the poles. Its massive atmosphere is mostly hydrogen and helium, etc etc etc. I learned these facts as many other children would learn the statistics of their favorite sports teams. Many of these facts stay with me today. A nine-year old can go only so far. For example I did not learn the Kepler law that the year of a planet is proportional to its distance from the sun to the 3/2 power.

I generally understood how these facts were obtained, how the great photographs in my books required long observations by telescopes such as the 200-inch reflector at Mount

Palomar. I had absolutely no interest in looking through tiny telescopes at the stars and planets. Many years later in high school and physics labs, I had little interest in doing experiments which I felt really only revealed the limitations of my equipment. In contrast many of the students feel empowered by such experiments. Perhaps these experiences are saying I was not born to be a laboratory experimentalist!

There were only so many facts about the solar system available in 1951 so eventually my interests shifted. When I was ten, I discovered science fiction. It gave me a tour of the universe and its space-time continuum. Captain Bullard and his crew fought vicious space pirates. Characters in other books included exploding super novas and colonized planets. Many of the stories I read had some scientific basis for the setting of its stories, while at the same time often violating other rules of the universe like the ban on faster than light travel. They provided great adventure and fun and greater insights into the universe and into possible civilizations.

My chance encounters with astronomy books was followed over the years by many other chance encounters with non-technical books, many of which had a great deal of impact on my understanding of science and mathematics. Looking back, it seems that books probably always had a bigger impact on my thinking than the courses I took and this became truer as I passed through high school and college and into graduate school. When reading I would proceed at my own pace, while in class if I was lost, the lecture proceeded leaving me further behind. Taking detailed notes of what was written on the board in class often precluded understanding what was said. I still prefer a well-written book.

As long as I can remember, my mentors were not my teachers or other people I met; they were the authors of those wonderful books or the people the books were about. *Men of Mathematics* by E.T. Bell tells about many outstanding men and women mathematicians and their ideas. (Books on relativity told about the thoughts of Albert Einstein, my personal favorite mentor. I was amazed that in a step of considerable courage, he could first overthrow Newtonian physics by declaring the wave nature of light to be the fundamental axiom on which he based his revolution. Then in the same year (1905), he wrote that light should sometimes be thought of as discrete particles—in his work on the photoelectric effect. For that work he later received the Nobel Prize. It became a foundation of quantum mechanics. His ability to work in several areas at the same time was a model I have followed (of course without any hope of matching his level!!!).

In high school my favorite scientist author was probably Norbert Wiener. I read all his popular books including his two-volume autobiography and his non-technical descriptions of cybernetics and control and thermodynamics. I was particularly struck by his description of Maxwell's imaginary demon who could reverse entropy. In the late 1800's Maxwell described his idea of a demon standing at a tiny door between two air-filled compartments watching the approaching air molecules. When a fast molecule approached from the left, he opened his door and let it pass into the right compartment and when a slow molecule approached from the right, he let it into the left compartment. Slowly the right compartment became hotter than the left. If such a process could be realized, it would generate usable energy apparently without expending significant energy. In fact, the demon must expend as much energy in observing the atoms as he gains by segregating them. Trying to understand why the demon cannot create usable energy seems was a deep problem before the development of quantum mechanics.

My introduction to reading technical books independently, cover to cover, came in the summer before college when a professor told me I could get into an advanced course if I read two mathematics books he recommended, including *Finite Dimensional Vector Spaces* by Paul Halmos, which I really liked. I read it throughout the summer on long bus trips commuting to and from my summer job in a laundry of a tuberculosis hospital. I found that reading technical material is an acquired skill that develops over time. It has seemed strange to me that we put such complete emphasis on learning science through lectures when in fact after graduation people must learn through reading without lectures, if they are to continue to grow intellectually. Books can always be with us.

IT IS POSSIBLE

Ahmed H. Zewail
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On the banks of the Nile, the Rosetta branch, I was born in Damanhur, the "City of Horus", only 60 km from Alexandria. In retrospect, it is remarkable that my childhood origins were flanked by two great places—Rosetta, the city where the famous Stone was discovered, and Alexandria, the home of ancient learning. I am the only son in a family of three sisters and two loving parents. My father was liked and respected by the city community—he was helpful, cheerful and very much enjoyed his life. He worked for the government and also had his own business. My mother, a good-natured, contented person, devoted all her life to her children and, in particular, to me. She was central to my life with her kindness, total devotion and native intelligence. Although our immediate family is small, the Zewails are well known in Damanhur.

The family's dream was to see me receive a high degree abroad and to return to become a university professor—on the door to my study room, a sign was placed reading, "Dr. Ahmed," even though I was still far from becoming a doctor. My father did live to see that day, but a dear uncle did not. Uncle Rizk was special in my boyhood years and I learned much from him—an appreciation for critical analyses, an enjoyment of music, and of intermingling with the masses and intellectuals alike; he was respected for his wisdom, financially well-to-do, and self-educated. Culturally, my interests were focused—reading, music, some sports and playing backgammon. The great singer, Um Kulthum (actually named Kawkab Elsharq—a superstar of the East) had a major influence on my appreciation of music. Reading was and still is my real joy.

As a boy it was clear that my inclinations were toward the physical sciences. Mathematics, mechanics, and chemistry were among the fields that gave me a special satisfaction. Social sciences were not as attractive because in those days much emphasis was placed on memorization of subjects, names and the like, and for reasons unknown (to me), my mind kept asking "how" and "why". This characteristic has persisted from the beginning of my life. In my teens, I recall feeling a thrill when I solved a difficult problem in mechanics, for instance, considering all of the tricky operational forces of a car going uphill or downhill. Even though chemistry required some memorization, I was intrigued by the "mathematics of chemistry". It provides laboratory phenomena which, as a boy, I wanted to reproduce and understand. In my bedroom I constructed a small apparatus out of my mother's oil burner (for making Arabic coffee) and a few glass tubes, in order to see how wood is transformed into a burning gas and a liquid substance. I still remember this vividly, not only for the science, but also for the danger of burning down our home! It is not clear why I developed this attraction to science at such an early stage.

After finishing high school, I applied to universities. In Egypt, you send your application to a central Bureau, and according to your grades, you are assigned a university, hopefully on your list of choices. In the sixties, Engineering, Medicine, Pharmacy, and Science were tops. I was admitted to Alexandria University and to the faculty of science. Here, luck played a crucial role because I had little to do with the decision, which gave me the career I still love most: science. At the time, I did not know the depth of this feeling, and, if accepted to another faculty, I probably would not have insisted on the faculty of science. The passion for science became apparent on the first day I went to the campus in Maharem Bek with my uncle—I had tears in my eyes as I felt the greatness of the university and the sacredness of its atmosphere. My grades throughout the next four years reflected this special passion. I graduated with the highest honors—"Distinction with First Class Honor". With these scores, I was awarded, as a student, a stipend every month of approximately £13, which was close to that of a university graduate who made £17 at the time!

After graduating with the degree of Bachelor of Science, I was appointed to a university position as a demonstrator ("Moeid"), to carry on research toward a Master's and then a Ph.D. degree, and to teach undergraduates at the University of Alexandria. This was a tenured position, guaranteeing a faculty appointment at the University. In teaching, I was successful to the point that, although not yet a professor, I gave "professorial lectures" to help students after the Professor had given his lecture. Through this experience I discovered an affinity and enjoyment of explaining science and natural phenomena in the clearest and simplest way. The students (500 or more) enriched this sense with the appreciation they expressed. At the age of 21, as a Moeid, I believed that behind every universal phenomenon there must be beauty and simplicity in its description. This belief remains true today. On the research side, I finished the requirements for a Master's in Science in about eight months, and was ready to begin research for a Ph.D. degree. All the odds were against my going to America. First, I did not have the connections abroad. Second, the 1967 war had just ended and American stocks in Egypt were at their lowest value, so study missions were only sent to the USSR or Eastern European countries. I had to obtain a scholarship directly from an American University. After corresponding with a dozen universities, the University of Pennsylvania and a few others offered me scholarships, providing the tuition and paying a monthly stipend (some $300). There were still further obstacles against travel to America. It took enormous energy to pass the regulatory and bureaucratic barriers.

Arriving in the States, I had the feeling of being thrown into an ocean. The ocean was full of knowledge, culture, and opportunities, and the choice was clear: I could either learn to swim or sink. The culture was foreign, the language was difficult, but my hopes were high. I did not speak or write English fluently, and I did not know much about western culture in general, or American culture in particular. My presence—as the Egyptian at Penn—was starting to be felt by the professors and students as my scores were high, and I also began a successful course of research. My publication list was increasing, but just as importantly, I was learning new things literally every day—in chemistry, in physics and in other fields. I was working almost "day and night," and doing several projects at the same time. Now, thinking about it, I cannot imagine doing all of this again, but of course then I was “young and innocent”. The research for my Ph.D. and the requirements for a degree were essentially completed by 1973, when another war erupted in the Middle East.

I had strong feelings about returning to Egypt to be a University Professor, even though at the beginning of my years in America my memories of the frustrating bureaucracy encountered at the time of my departure were still vivid. With time, things changed, and I recollected all the wonderful years of my childhood and the opportunities Egypt had provided to me. Returning was important to me, but I also knew that Egypt would not be able to provide the scientific atmosphere I had enjoyed in the U.S. A few more years in America would give me and my family two opportunities: first, I could think about another area of research in a different place (while learning to be professorial!). Second, my salary would be higher than that of a graduate student, and we could then buy a big American car that would be so impressive for the new Professor at Alexandria University! I applied for five positions, three in the US, one in Germany and one in Holland, and all of them with world-renowned professors. I received five offers and decided on Berkeley.

Early in 1974 we went to Berkeley, excited by the new opportunities. Culturally, moving from Philadelphia to Berkeley was almost as much of a shock as the transition from Alexandria to Philadelphia—Berkeley was a new world! I saw Telegraph Avenue for the first time, and this was sufficient to indicate the difference. I also met many graduate students whose language and behavior I had never seen before, neither in Alexandria, nor in Philadelphia. The obstacles did not seem as high as they had when I came to the University of Pennsylvania because culturally and scientifically I was better equipped. Berkeley was a great place for science—the BIG science. My general research direction was established, and I immediately saw the importance of the concept of coherence. I decided to tackle the problem, and, in a rather short time, acquired a rigorous theoretical foundation which was new to me. I believe that this transition proved vital in subsequent years of my research. We wrote two papers, one theoretical and the other experimental which were published in Physical Review. These papers were followed by other work, and I extended the concept of coherence to multidimensional systems, publishing my first independently authored paper while at Berkeley. In collaboration with other graduate students, I also published several papers.

During this period, many of the top universities announced new positions, and I was encouraged to apply. I decided to send applications to nearly a dozen places and, at the end, after interviews and enjoyable visits, I was offered an Assistant Professorship at many, including Harvard, Caltech, Chicago, Rice, and Northwestern. My interview at Caltech had gone well, despite the experience of an exhausting two days, visiting each half hour with a different faculty member in chemistry and chemical engineering. The visit was exciting, surprising and memorable. The talks went well and I even received some undeserved praise for style. At one point, I was speaking about what is known as the FVH picture of coherence, where F stands for Feynman, the famous Caltech physicist and Nobel Laureate. I went to the board to write the name and all of a sudden I was stuck on the spelling. Half way through, I turned to the audience and said, “you know how to spell Feynman”. A big laugh erupted, and the audience thought I was joking—I wasn't! After accepting the Caltech offer, I was granted tenure in two years and the research group was well established. I never regretted the decision of accepting the Caltech offer.

At Caltech over the years, my science family came from all over the world, and members were of varied backgrounds, cultures, and abilities. The diversity in this “small world” I worked in daily provided the most stimulating environment, with many challenges and much optimism. My research group has had close to 200 graduate students, postdoctoral fellows, and visiting associates. Many of them are now in leading academic, industrial, educational, and governmental positions. Working with such minds in a village of science has been the most rewarding experience—Caltech was the right place for me.

My biological children were all “made in America”. I have two daughters whom I am very proud of, Maha, a graduate of Caltech (B.S.) and the University of Texas, Austin (Ph.D.), and Amani, a graduate of Berkeley (B.S.) and currently an M.D. student at the University of Chicago. Dema, my wife, has her M.D. from Damascus University, and completed a Master’s degree in Public Health at UCLA. We have two young sons, Nabeel and Hani, and both bring joy and excitement to our life.

The journey from Egypt to America has been full of surprises. As a Moeid, I was unaware of the Nobel Prize in the way I now see its impact in the West. We used to gather around the TV or read in the newspaper about the recognition of famous Egyptian scientists and writers by the President, and these moments gave me and my friends a real thrill—maybe one day we would be in this position ourselves for achievements in science or literature. Some decades later, when President Mubarak bestowed on me the Order of Merit, first class, and the Grand Collar of the Nile (“Kiladate El Niel”), the highest State honor, it brought these emotional boyhood days back to my memory. I never expected that my portrait, next to the pyramids, would be on a postage stamp or that the school I went to as a boy and the road to Rosetta would be named after me. Certainly, I never dreamed that one day I would be honored with the Nobel Prize. But with passion and sincerity, *It Is Possible*, as human achievements should be limited neither by race nor by origin.

In this collection of about 100 highly readable essays, some of the most eminent physicists and mathematicians with some connection to ICTP tell us about what attracted them to science as youngsters and kept it alive, and what main piece of knowledge they have added to the extraordinary lore of science. High school students and young college students, for whom the collection is primarily meant, will benefit from spending some time with the book. Even the most seasoned researcher will find it interesting.

- from the preface

1964
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2004