EVENTS IN LIFE

STRUGGLE FOR SELF-RELIANCE IN AERONAUTICS AND ETHICAL PRACTICES IN SCIENCE

S R VALLURI

Foreword

Dr. Sitaram Rao Valluri narrates in this lucid, compact book his journey from Eluru, a small town in Andhra Pradesh, to Bangalore, the aerospace capital of India. He recounts how he directed the National Aeronautical Laboratory (NAL), presently known as National Aerospace Laboratories, from 1965 to 1984. The book gives the reader glimpses of his outstanding leadership in attracting and nurturing talent from varied disciplines and in creating an enabling environment for R&D support to the Indian aircraft industry. Indeed, Dr. Mashelkar, Director General of the Council of Scientific and Industrial Research (CSIR), called this world-class multidisciplinary engineering laboratory a jewel in the CSIR crown.

Dr. Valluri begins by recalling a few episodes from his childhood in Eluru. They show the first signs of fiercely independent thinking, fearless expression of views and bold actions based on deep convictions. His undergraduate education takes him to Rajahmundry and Varanasi and his postgraduate education to Bangalore and finally to Pasadena in the USA. He explains, with lively anecdotes, the lessons he learnt from his 14-year association with Caltech and interaction with the aircraft industry in the USA. They give an insight into the development of his values and his ideas on the management of research in science and technology.

His tenure at NAL coincided with stagnation in the aircraft design and development activities from the mid sixties to the early eighties following the unfortunate nonavailability of an appropriate engine for the HF24 aircraft. Indian aeronautical R&D organizations struggled to move beyond without much success till the proposal for building Light Combat Aircraft (LCA) was approved. Dr. Valluri gives the reader an intimate account of the events leading to the government clearance of the project and the establishment of the Aeronautical Development Agency (ADA) in Bangalore with Dr. Valluri as its first Director General to steer the multi-institutional LCA Project.

It is not easy, as the book reveals, to build excellent institutions or to restructure recruitment and promotional policies for scientists and technical staff of a vast national organization like CSIR, as bureaucratic impedances and institutional rivalries have to be overcome.

The book also contains a few incidents that bring out Dr. Valluri's passion for excellence and ethics in scientific research as well as in its management. His sharp criticisms of dubious practices in research and its management might appear rather harsh to some readers. But if we, in Research and Development, want to make India move closer to fulfilling its promise, we can only do so by honestly identifying our mistakes and by resolving to prevent their recurrence.

Finally, this book is an important contribution to the history of major scientific institutions of India in the form of a valuable record based on deep personal knowledge and intensive involvement in building them.

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PREFACE

Several friends of mine and in particular Mrs Chandra Jain over the years have suggested that I should put on record my experiences here and elsewhere in the world, during my professional career lasting over four decades. I thought a great deal about the wisdom of recording these experiences, as not all of them are pleasant. But I am reminded time and again of the statement of Theodore von Karman, the all-time great of the aeronautical scientists: "Good decisions come from experience and experience comes from bad decisions". The purpose of the events described in the following pages is to share them, if only to let others know how easy it is to commit mistakes, which may well have farreaching consequences, without realizing their implications. While writing, I kept in mind a statement made by Paul Hoffman, the first head of UNDP: "If you don't get facts, facts will get you".

In what follows, I mention dates extensively. To the best of my knowledge, they are correct. I am grateful to Kala Sunder for painstakingly going through the draft and making many corrections, primarily of grammar and spelling. The remaining errors are mine only. I note that in several places in what follows, there are repetitions in spite of the efforts made by Kala to delete them. I am of course responsible for these. I am grateful to my wife also for some suggestions and corrections. G N Vittal who worked with me ever since I returned home to India in 1964 has been a very valuable colleague over the years. He too went through the draft of what follows and made many valuable suggestions. I am thankful to him for his assistance.

The write-up is divided into five sections. The first section covers my life from childhood to college here, and in the US at Caltech. This section also contains some scientific details about my research in the US. The reader may skip this section, unless he wants to get a feeling for what I did in the US, and is also familiar with the field. The second section deals with my life in NAL and my efforts to build the institution, virtually from scratch, into an advanced research laboratory with long-term perspectives in its R&D programs. The third section deals primarily with aeronautics and the efforts to obtain cabinet approval for the Light Combat Aircraft program and the creation of the Aeronautical Development Agency. The fourth section deals with committee work to restructure CSIR, and my involvement in other committees. The fifth section deals with events that left unpleasant memories, which are not easy to forget. I felt that I should place them also on record, if only to relate how easy it is to take wrong decisions without realizing their implications, and which may well result in unhappy consequences, and set bad precedents. If there is one conclusion the reader can certainly draw after reading what follows, it is that I suffer from the virtue or vice (whichever you wish to call it), of transparency in my views, sometimes to the extent of being tactless.

Two objectives preoccupied all my professional life abroad and at home. The first was my effort to integrate the research and development activities and the industry in aeronautics in India along the lines of the Space and Atomic Energy Commissions. This is because of the conviction that without such integration, it would be impossible to obtain even a measure of self-reliance in this field, which is crucial for defending our skies in times of war. I regret to

state that I have not succeeded in this effort. The second objective was to argue for defining a uniform code of ethics for the practice and management of science. It is my conviction that without ethical practices in science, it would be impossible to establish a reasonably self-reliant S&T base in our country, and that without it, it would be impossible for us to join the cadre of developed nations, in spite of the tremendous capability we have in this country. It is not an argument to suggest that scientific misconduct does not take place in the developed world. But when instances of scientific misconduct are brought out, they tend to take salutary action, whereas more often than not, we tend to bury them. I deeply regret that I have not succeeded in evolving a code of honour that will be conscientiously obeyed by all. In what follows, I discuss extensively an instance that took place in NAL. I believe this sullied the image of NAL as well as CSIR because of the manner in which it was handled by the management. What follows has been written in the spirit of Edmund Burke's aphoristic statement, "All that is needed for evil to spread is for good people to do nothing". Put simply, I have always valued my ideals and principles more than my skin and sometimes I paid a heavy price for it.

I have always believed that when we spend public funds, we are trustees for public good and that we should not betray this trust implicitly imposed on us. Some of the events mentioned here are not the most pleasant to talk about. But they are discussed if only to let people know how easy it is to commit mistakes unknowingly. What follows is written in the spirit of the Right to Information Act (RTI).

Finally, if anybody wishes to seek clarifications on any aspect of the events described in what follows, I would be very glad to respond.

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Chapter 1

EVENTS IN LIFE

CHILDHOOD, ON TO COLLEGE AND THEN TO THE US

I was born on 25th June 1924 in a middle-class family, the first among 10 children. We lived in Eluru, a small town with a population then of 50,000, in West Godavari district of Andhra Pradesh. My father, Narasimha Rao, was a medical practitioner with a modest income. He was a strict disciplinarian, and inculcated in me the importance of living a disciplined life, upholding ethical values and always speaking the truth. Regrettably, I also inherited from him my short temper. I continue to feel embarrassed about it.

To be part of a 10-child family was not the easiest thing in the world. One day when I was about 10 or 12 years old, and we were already many, I remember asking my mother how they proposed to bring us up. It stung her to the quick, and she started crying, and said, "Will not the God that planted seeds pour water on them also?" I was not sure that God, if he existed, would be concerned with such things. I raised the same question with an uncle of mine around the same time. He was more rational in his answer. He said that it was the sin of a generation that felt the peace of the Victorian era would endure. It was not to be. A few years later, in September 1939, shortly after I joined college, the Second World War broke out, with disastrous consequences. Eight years later we obtained Independence, with Nehru taking us along the path of socialism and state-controlled capitalism, again with disastrous consequences. He embraced a god that had already failed elsewhere in the world. From a population of about 300 million at the time of Independence to currently more than a billion, we have many more poor people now. From childhood on, I strongly believed that there could be no solutions to our problems as a nation, if we did not control our population.

Another incident I remember from that time was my father doing his duty and performing the thread ceremony for me in 1935. He also retained a priest afterwards to teach me to learn by heart some scriptures in Sanskrit. While reciting them along with him, I asked him one day, what they meant. He did not know, and from then on I refused to learn them. The same uncle, in a casual way asked me, why then I was wearing the so-called sacred thread. Realizing the inherent contradiction, I tore it off. My mother complained to my father. He said that he had done his duty, and it was up to me to do what I believed was right. I was grateful to him for his very liberal attitude.

I graduated from high school in 1939. My academic record was average. After all these years, the thing I remember vividly from my childhood is running behind a light aeroplane making a forced landing. The only teacher from my school days who left a deep impression

on me, and many others, was Achanta Subbarayudu Garu, who taught us science and mathematics. I maintained contact with him over the years. The last time I saw him was in his son Gopalam's place in Bangalore, a few days before he died. As there was no college during those days in Eluru, I went to do my intermediate in science at the Government Arts College in Rajahmundry, about 60 miles away, completing it in 1941. There are no strong memories of my stay there. I got a first class. For the first time, a spirit of competitiveness got into my mind. I did not want to be left behind, but wanted to amount to something. I got admission into the medical college in Madras, but not into engineering, which I was anxious to study. As it turned out, even this medical seat was withdrawn, as I happened to have been born into a brahmin family, and introduction of caste-based seat allocation had just been introduced, Because of my marks, I was given a seat even after the caste-based allocation of seats, but my heart was in engineering. As a compromise, I joined BSc physics in Rajahmundry, with the expectation of trying again for admission into engineering the following year.

ENGINEERING IN BENARES.

At that time, Sir S Radhakrishnan (our future President) was the Vice-Chancellor of the Benares Hindu University (BHU). Having come to know of my situation through my uncle whose son was married to his youngest daughter, and after confirming that I had got a first class, he arranged for me to get a seat in the Engineering College in BHU in 1942, to do my mechanical and electrical engineering degree. In my third year, I remember an informal talk by Dr B D Kalelkar (son of Gandhian Kaka Kalelkar), who had just returned from MIT, after doing his ScD. He told us how those institutions of higher learning functioned, and how they stressed the importance of thinking on one's own and the lesser importance given to learning by rote. I remember making up my mind then, that no matter what happened, I would go to the US for further studies, and return to head a research institution here. I recall one of my sisters (who died in 1951) asking me who was going to give me money to do that, and my reply was that I would find a way. I was not a bad student, but my record in engineering was nothing to write home about. I was 22 when I completed my BSc (Engg) from Benares in 1946. As it turned out, I was one of the earliest from my hometown to go to the US for higher studies.

GRADUATE STUDIES IN AERONAUTICS – AT THE INDIAN INSTITUTE OF SCIENCE

I was at a loose end about what to do next. My love for aeronautics never left me. I came to know that in 1942, a department of aeronautics had been started at the Indian Institute of Science (IISc) in Bangalore. At that time, during the Second World War, Hindustan Aeronautics Limited (HAL) was functioning as a repair base for the South East Asia Command. William Douglas Pauley, the general manager, gave four years of his salary, amounting to $20,000, to the department for awarding four scholarships in his name for study in the US, to the student who stood first in a competitive exam, held annually for the graduating class. I felt confident that I would get it. My father told me that I was too young to take up a job, and wanted me to continue my studies at IISc. I was also given a

scholarship by the then Madras Government, a grand sum of Rs 56 monthly for my studies for two years.

During my studies in Bangalore, my cousin's wife, Shakuntala, daughter of Sir S Radhakrishnan, profoundly influenced me. I was staying with them. She studied in Oxford while her father was the Spalding Professor of Eastern Religions and Ethics there.
One day, shortly after I joined the IISc, she looked at my marks and said that I should be ashamed of myself. When I asked her why, she said that the fact that in some subjects I had got as much as 90 to 95% meant that I was not dumb, and the fact that I had got only 40 to 45% in some other subjects meant that I did not care enough to apply myself to serious studies. She asked, "What do you want to do with yourself?" It stung me to the quick. I told her that if not for anything else but to prove that I was not dumb, I would get the Pauley scholarship. She said, "Do that. I would be glad for you". This challenge, apart from my love for aeronautics, set the goal for my future. I never really looked back after that.

The IISc stay had a great influence on my learning process. Dr V M Ghatge, the wellknown fluid mechanics scientist, who was a student of Ludwig Prandtl in Gottingen, Germany, made us realize that there was more to science than learning by rote, and that there was science everywhere, that it was a way of looking at nature. He taught us the importance of inquisitiveness and of learning how to pose intelligent questions. He changed my entire outlook on what science was all about. Posing meaningful questions and seeking answers to them became an integral part of my life. It was also the first time that I was exposed to the problem of metal fatigue. I was fascinated by it, something that has not left me after all these years. I made up my mind that it would be my subject of research, if I were fortunate enough to go abroad.

Dr R G Harris from the Royal Aircraft Establishment (RAE) in Farnborough, UK initially headed the department, although Dr Ghatge started it and was equally capable of heading it. Dr Ghatge left IISc to join the Hindustan Aeronautics Limited as the chief designer. It was a serious loss to the cause of education in aeronautics in India. I stood first in the fourth and last of the Pauley Scholarship examinations in 1948. Shortly thereafter, Harris returned to UK and Dr O G Tietjens, another student of Prandtl, and a co-author with him of a book on fluid mechanics, took over. All these teachers taught the underlying principles of the science behind engineering. This made pursuit of knowledge a life-long passion with me.

GETTING MY MS AND PhD, AND LIFE AT CALTECH

I got admission in MIT, Stanford and Caltech for my master's degree. When I asked Dr Tietjens which place I should go to, he unhesitatingly said that I should go to Caltech, since Karman was there. Theodore von Karman was one of the most outstanding aeronautical scientists in the world at that time and the founder-director of the aero department in Caltech, called Guggenheim Aeronautical Laboratories, California Institute of Technology (GALCIT). I left for Caltech in September 1949, after spending one year as a research assistant in the Aeronautics Department after graduating. Satish Dhawan, who was doing

his PhD at Caltech at that time, took me under his wing, so to say. I have yet to find another person like him. Not only was he a brilliant scientist, he was also an extraordinary human being. He did path-breaking research in skin friction measurements in gas flows. He was an outstanding engineering scientist, and an excellent manager of engineering and science institutions. By the time I joined Caltech, von Karman had already left GALCIT to take up more important responsibilities in the US government.
His students went on to take up faculty positions in various academic institutions and research laboratories in the US. In a very real sense, von Karman laid the foundation for the unprecedented growth of aeronautics in the US. He was, so to say, its high priest. In wintertime he used to stay in Pasadena, where Caltech is located. It is my good fortune to have known him a little while I was at Caltech. Almost invariably he used to drop in at my lab, which was located near the GALCIT entrance, and is now converted to a lecture hall. He was proud of the Indian students he had at Caltech.

The teachers at GALCIT were brilliant. Notable among them was Hans Liepmann, who later on was awarded the President's Medal of Science. His book on gas dynamics with Anatol Roshko, who became a good friend of mine, was a classic. His researches in transonic and supersonic flows and turbulence, in particular, were path-breaking. To this day, I still remember the way he taught us. His lectures were mostly extempore with the equations argued from first principles, as he was deriving them. When he completed the derivations on the board, he would pull out a small piece of paper which he kept in his pocket, to check the correctness of the derivation. The subject really came alive in the manner he taught us. Equally thought provoking were the lectures by Dr Frank Marble, who taught us mathematical methods in engineering.

Perhaps as a student and subsequently as a junior staff member at Caltech, I was very much influenced by Prof Ernest E Sechler, who specialized in Elasticity and Aircraft Structures. During the 40's, he was well known for his book on aircraft structures with Louis G Dunn as a co-author and later for his book on elasticity in engineering. He had a decisive influence on the design practices for aircraft structures in the then nascent aircraft industry, when the semi-monocoque structures became the basis for design of aircraft. I was fortunate that he took a personal interest in me. When I exhausted the Pauley scholarship funds, I sought his help for continuation of my studies. A couple of weeks later, he told me that I was given a tuition scholarship, a graduate assistantship and a part-time job ($1.25 an hour!), and that they were mine as long as I was a student at Caltech. The job had to do with investigation of fatigue failure of a propeller blade in the Southern California Cooperative Wind Tunnel, managed by GALCIT, resulting in a $500,000 loss. To this day I feel profoundly grateful to him. I found that there were many other students also, whom he had looked after similarly as the executive officer of GALCIT.

Sechler in his classes and home assignments taught us the importance of accuracy in calculations. Once, in a home assignment given by him, in the last step of a problem, I made an arithmetical error. He gave me zero marks. When I asked him why, he said that such errors could not be tolerated in aircraft design, as they could lead to catastrophic failures involving loss of life; a lesson I never forgot. The importance and the relevance of

what was being taught to us were forcibly driven home through home assignments. It came as a surprise that although he contributed so much to the design of aircraft structures, he preferred to travel by train when he had to go to Washington DC for meetings. Perhaps he knew too much about the limitations of determining the stresses in aircraft structures. I recall seeing on the black board in his colleague Max William's office with some amusement,

Wrinkle wrinkle little spar
Stressed above the yield so far
Way up in the sky so high
Gee I am glad that I don't fly.

Many of the GALCIT faculty members used to go on Wednesdays for consultancy work to the local aircraft companies. We used to dread Thursdays, because the home assignments that day were frequently based on the problems they had faced the previous day. But we learned the relevance to the industry of what we were being taught in the class. The more complicated problems became thesis subjects for engineering degrees, and the even more complex ones formed the basis for PhD work. Thus, the cycle of knowledge generation and application became a closed loop, with benefit for all.

For my PhD thesis Dr Y C Fung from China was the thesis adviser. He got his PhD from Caltech in two years. He was a brilliant scientist, and subsequently he was honored with the President's Medal of Science for his studies in biomechanics. He wanted me to study the effect of fatigue on internal friction in metals, of which I knew nothing before. It is a property that is affected by many parameters, and isolating its response to fluctuating loads demanded that I first develop techniques for measurement of logarithmic decrements, which were sensitive to the fourth decimal place or better. It took me almost two years to understand the measuring techniques and to ensure that when I measured it, the friction coefficient (the log decrement) reflected only the effect of metal fatigue. It was a very messy experimental research problem. Nevertheless, I was able to demonstrate that the measurements did reflect in a reproducible manner the effect of metal fatigue on internal friction. Based on this work I got my PhD (cum laude) from Caltech and immediately joined as a research fellow in GALCIT on a salary of $5000 a year. Two years later, as a general policy, Dr Lee DuBridge, then President of Caltech, increased the salaries of all staff across the board by 20%.

Dr DuBridge earlier headed the Radiation Lab in MIT. I recall an incident when my very good friend, Gregory Loew, was riding with him to a picnic on the beach. Gregory apparently asked him why he gave up that position. Apparently Dr DuBridge said that he felt he was past his prime in research and should try his hand in the management of an academic institution, when the offer was made to him. He succeeded Dr R A Millikan, the Nobel laureate, as the President of Caltech. Millikan set the standards, and DuBridge built

up on them, without compromising the ideal of pursuit of excellence. Caltech has so far produced about 30 Nobel laureates. I feel privileged to have studied there.

When you are a student in Caltech, you are surrounded by brilliance. With its utterly informal culture, it is not uncommon to have somebody like Feynman (future Nobel Laureate) sit at the same table with you during lunch in the coffee shop, or see a professor and a mechanic working under him having the time of their lives across the lunch table. I recall one day soon after joining Caltech, when I was having my lunch in the coffee shop, with my head down, I heard somebody saying, "May I join you?" It was Millikan. I stood up. He asked me to sit down and asked where I was from. I said, "Sir, I am from India". He said he knew that but wanted to know which part of India. When I told him that I was from Bangalore, he asked me about various people from there including Dr R Narasimha's (my successor in NAL) father. Apparently he had met them when he was visiting Bangalore for some cosmic ray research. He wanted me to accompany him to his office, and invited me to visit him from time to time, which I used to do.

It has been a privilege to know such people. Coming from a hierarchy-ridden feudal system, it was a revelation to me to find how informal that society was, and how easy it was to get to know people there. I was at that time very much of an introvert, and felt stupid that I was not making any effort to know people. There were at that time students from about 40 different countries at Caltech, and we used to get to know each other through the so-called Inter Nations Association (INA). To this day my closest friends in the world are the ones I got to know there. I functioned as the President of INA for some time. I invited Dr Millikan to give a talk at an INA meeting. I thought he would speak about science. Instead, he spoke about the place of religion in one's life. Perhaps in the minds of many scientists there is no incompatibility in believing in God and in science. Perhaps for them the concept of so-called "intelligent design" is more satisfying than Darwin's theory of evolution.

WAITING FOR THE GREEN CARD IN INDIA

In mid-1956 Senator Eugene Knowland from California, getting to know about me through a mutual friend, sponsored me for an immigration visa. There was only a nominal quota of 100 visas for Indians during those days. I returned home in October 1956 and waited for my green card. It took longer than expected. Noting that there were a fairly large number of people like me, President Eisenhower sent a bill to Congress for approval for issuance of green cards to such people. I got my card in November 1957. It was a difficult period, not knowing what was going to happen.

During this period I spent some time at the IISc, mostly in the metallurgy department, learning experimental techniques in metallography. At that time, Dr DuBridge came to visit IISc, and I was happy to accompany him during his visit. When he met Dr H J Bhabha, Director of Tata Institute of Fundamental Research and Chairman of the Atomic Energy Commission, in Bombay the following day, Dr DuBridge apparently told Bhabha that he would not mind releasing me from Caltech, provided a suitable job could be found for me. It seems that Dr Bhabha, as a Council member of the IISc, took up the matter at a Council

meeting. The Council advised Dr Bhagavantam, the then Director, to offer me a job as an assistant professor in the aeronautics department, then headed by Prof Satish Dhawan. While extending the offer, Dr Bhagavantam desired that I should join immediately, something I could not do, as I was then working on a research topic sponsored by the National Advisory Committee on Aeronautics (NACA, subsequently rechristened NASA), and felt morally compelled to complete it before returning. So I asked for a year's time to join. This he refused, and I returned to Caltech to continue with my NACA-sponsored project. I got the feeling that Dr Bhagavantam resented the Council virtually ordering him to offer me a job. Incidentally, I was asked to indicate what research programs I proposed to undertake at the IISc. I provided a list of the programs. Apparently the head of the metallurgy department stated that they could handle them, and I was not needed to carry them out. I completed them in Caltech.

RESEARCH LIFE IN CALTECH

The NACA project concerned the use of internal friction technique to study the effect of metal fatigue on solution treated Al, 4% Cu alloys in their solid solution phase. These alloys are extensively used by the aircraft industry. I felt it was important to understand what was going on long before a dominant crack took over and resulted in ultimate fatigue crack failure. I was able to demonstrate that the accelerated formation of the socalled GP (Gunier-Preston) zones was directly due to metal fatigue in solution treated aluminum alloys. I was happy. But the metallurgy department in Caltech was unhappy. As it turned out, they thought I was encroaching into their field, and opposed my appointment to a regular faculty position. It did not matter, as by that time I had offers of tenure track appointment from University of Washington in Seattle, apart from offers from Douglas, North American, Northrup aircraft companies, Wright-Patterson Air Force Base (WPAFB), and other places.

Shortly after the internal friction studies, I wanted to study crack propagation at the very early stages due to impact loads. This required the development of a high-speed camera capable of taking photos at the rate of 10 million frames per second. The fabrication was a successful venture, but it took some time. It called for the image of the ongoing event to be swept across the stationary film by a mirror rotating at about 100,000 rpm, with a Kerr Cell functioning as a shutter timed at 10 million frames per second. The research was supported by the Aeronautical Research Laboratories (ARL) at WPAFB. Incidentally, making the camera did not set a precedent. A similar camera was being used in the engineering division at Caltech. After I left Caltech, my son, who was also a student in GALCIT 35 years later, told me that this camera was still being used for research. I was glad.

In 1959 I attended an international conference on fatigue and fracture held in Swampscot, near Boston. It was an impressive conference with the "who's who" in the field from all over the world presenting their latest findings. To me it was memorable for another reason also. I met one Dr Poncelet, who was then working at the Stanford Research Institute in Palo Alto, Ca. Shortly thereafter, when I was visiting Palo Alto, Dr Poncelet invited me for dinner at his place. He did not tell me that he had also invited other people. I saw an elderly and distinguished person coming in. Dr Poncelet introduced me to him. He was Kerensky,

who held power in Russia shortly before Lenin took over, and fled later to France and came to the United States in 1940. He was then associated with the Hoover Institution in the Stanford campus. I was naturally thrilled, and wanted to talk to him about Russia and the Soviet Union. Instead, he was more interested in talking about India and in particular about Calcutta. When I asked him why he was interested so much in India, he told me that his son was then living there! It was a nice evening and became a memorable one because of meeting Kerensky. This elderly man was most gentle and it was hard for me to believe that he headed the Russian government before the communists took over under Lenin.

The delay in the fabrication of the camera led me to the most satisfactory research period in my life. The delay gave me time to look into other aspects of metal fatigue. It demanded answers to some basic questions: how does the fatigue crack propagate and what are the parameters that influence it? And what precipitates its eventual catastrophic failure? Finally, is it possible to evolve an engineering theory of metal fatigue that can explain quantitatively various aspects of the problem, without invoking different hypotheses to explain different aspects, and yet be supported by established facts and a physically plausible hypothesis? The answers came to me suddenly when my attention drifted towards this problem while sitting in a seminar.

I became familiar with the dislocation theory of metals, a powerful concept that explains various aspects of plastic deformation of metals. I was also giving a course on inelastic aspects of engineering to students registered for PhD, when Dr Fung, who was handling the course, left for Germany on a two-year assignment. While teaching this course, A H Cottrell's classic book on dislocation theory of metals had a very important effect on me. I could visualize the inelastic behavior through dislocation motions and postulate an equation for crack propagation based on it. But it did not explain how long the crack could propagate before a catastrophic failure occurred. It suddenly occurred to me (at the seminar) that if I invoked the Griffith theory of cracks, I could establish the critical crack length and the associated number of cycles to failure for any stress at which failure occurs. The experimental data generated and reported by NACA and this theory correlated remarkably well. Furthermore, it could handle the problems of variable loads also, and showed where the Miner's cumulative damage theory, extensively used in those days, was wrong. When I visited AVRO in Manchester in 1962, the designers of AVRO 748 told me that they had used these equations to estimate its fatigue life. I had to tell them that I had not anticipated its application to full-scale structures. The theory could also predict high temperature effects below the "recovery temperatures", extrapolating from the room temperature data. To me it was an immensely satisfying period. It was still a linear theory, in the sense that it did not take into account the residual stress fields which form at the crack front, and the manner in which they were affected by the varying loads. It is quantitatively a very complex problem. I wrote a series of reports on the subject covering various aspects of it, and they were published as ARL reports. I told Harry Lipsitt from Aeronautical Research Laboratories, who funded the camera project, that I had wasted their grant money. He remarked that if everybody wasted money the way I had, it would be good. In fact he wanted me to join him in the Aircraft Research Laboratories of WPAFB.

BACK IN INDIA AGAIN

Around that time Chandiramani, the then Secretary, Department of Education, Government of India and Dr Kelkar, the first Director of IIT Kanpur, were visiting the US to recruit staff. While visiting my lab in Caltech, they extended an invitation to me to join as a professor, either at Madras or Kanpur. I became eligible for American citizenship in November 1962, having obtained my green card in 1957. I felt that before I made up my mind to take US citizenship, I should go home to see how things were. On the way home I visited various research labs in the UK and Europe. I visited IIT Madras and had discussions with the Director, Prof Sengupto, and felt that I should give it a try. It so happened that I was working very hard, virtually 12 to 14 hours a day, my pulse rate was extremely high, and I was on the verge of a nervous breakdown. The Caltech medical center doctors could not do much to control it. I felt that I should stop all work, take a break, go home, and relax. It helped tremendously.

During this visit I also met Dr P Nilakantan, who became the first Director in July 1959, of the newly created National Aeronautical Laboratories. I was in correspondence with him earlier, while still in Caltech. He wanted me to prepare a proposal for building the structures and materials divisions in NAL. I prepared a fairly detailed proposal outlining the scope of research to be taken up, with details of the areas of R&D. He was willing to offer me the position of an Assistant Director, whereas I felt I should be given the position of Deputy Director. By that time I already had the offer of senior professorship and the headship of the Department of Aeronautics and Applied Mechanics from IIT Madras. The idea was not followed up. I was not particularly surprised when I heard that Mr S P Venkateswaran, whom Nilakantan recruited from the Meteorology Department to assist him in windmill development, apparently commented that I was not really needed, as they had my proposals, and that they could handle them. Sounded very familiar. I went back to the US in October 1962 to continue my research and complete it, so that I could return home around the end of 1963. Loyalties to family and a strong feeling that I should be available to my parents in their old age, and a strong compulsion that I should try to do something for the country that nurtured me, made me decide to accept the job offer from IIT. As it turned out, my parents preferred to stay with my brother, who is a doctor, and visit us now and then. My father passed away in April 1983 and my mother in November 1990.

STAYING WITH SATISH DHAWAN AND MEETING MY FUTURE WIFE

During my visit in 1962 to India I stayed with Satish Dhawan, who was then heading the Department of Aeronautics in the IISc. We had stayed in the same dormitory in Caltech. He too felt that I should return to build aeronautics in the country on more solid foundations to obtain a measure of self-reliance, instead of depending entirely on licensed production. We used to discuss this problem for hours on end. His idea was that if Karamcheti Krishnamurthy, also a Caltech graduate, who was then at Stanford, could also return, together we could build a strong base for aeronautics.

While staying with Satish Dhawan I met Shyamala Manel, a cousin of Mrs Dhawan. I got to know that she was going to Mills College in Oakland, California for her master's degree. We met several times there. We were married in July 1963 at Prof Karamcheti Krishnamurthy's residence. She stayed on, while I returned home in November 1963. I joined IIT end of 1963. We now have two children; Monica was born in March 1965 and Siddhartha in December 1966. Monica took her master's degree in Physics from Pilani in 1987 and her PhD in astrophysics from IISc in 1993. She is now married to her childhood classmate, Puneet Manchanda, who also took his degree from Pilani and an MBA from Ahmedabad, followed by a PhD in Business Administration from Columbia. He is now an associate professor in the Graduate School of Business in the University of Chicago. Monica is in the Kavli Institute for Cosmological Physics in the same university, and has been managing research programs funded by the National Science Foundation, apart from her own research in astrophysics. They have a son, Akhil Mohan, born in September 2000. Our son, Siddhartha, took his bachelor's degree in aeronautics from IIT Kanpur in 1990 and a master's and PhD in aeronautics in 1996 from Caltech. He married Andrea Higgins in 1998. They have a daughter named Chaya Isabel, born in 2005. Both our children have settled down in the US.

I made sure before finally returning home, that I had complied with the minimum requirements for receiving social security benefits from the United States Government after retirement. These, plus my contributions to retirement funds through Caltech assured me of financial independence after retirement, and enable us to indulge in things such as visits to the US to see our children and grandchildren whenever we desire.

I returned to Douglas in March 1964 to complete some work that I was handling earlier as a consultant. I was associated with Douglas for a number of years as a consultant in their design department. I learned about the importance of weight control in design. It was a fruitful association. I understood the significance of forward-looking research and the need for forward technology development in anticipation of future requirements in the design of aircraft. This understanding was of tremendous help later on, when I joined NAL. During this period Douglas assigned George Bockrath and Jim Glasgow from their design group to work with me on problems of crack propagation under various conditions. I also had working with me a young graduate, Phil Francis. He subsequently left to pursue higher studies. He specialized in aspects related to management of R&D, and years later wrote a book about it. It was nice of him to have dedicated it to me and one other person, who had also apparently influenced him.

In Douglas, I came across jealousy even in scientific research, something that was unknown to me at Caltech. I found that normalized critical crack correlates better with net section stress than remote stress. Further, this relationship was found to be sensitive to strain hardening coefficients. These observations were supported by voluminous test data for sheet specimens, available in Douglas in 1962. George Irwin, who was responsible for extending the Griffith theory of cracks for materials that have a yield point, came to know about these findings. He was unhappy. He preferred to use the gross section stress, which remains constant through the test procedures and is much easier for designers to handle,

rather than the varying net section stress, even if it was not really an accurate parameter to describe what was going on. He was correct in this sense, but it did not represent the state of stress in the plane of the crack. I met him in his office in Washington, DC in April 1962 after I found this correlation. He reiterated his view. He was well known in the field, and he carried the day, never mind even if his equations did not represent the reality. Anyway, I presented a paper before the Society of Automotive Engineers (SAE) shortly thereafter, in 1963, resulting in the award of the Wright Brothers Medal for meritorious contributions to me, Bockrath and Glasgow. I felt vindicated.

Incidentally, it was found through enormous test data from the Douglas aircraft company that there was a strong correlation between the normalized net section stress (compared with ultimate tensile stress) in the plane of the crack and the normalized crack length (compared with the specimen width). It was found that there is a critical crack length below which failure occurs only when the net section stress in the plane of the crack is the same as the ultimate tensile strength of the material. The data also identified the critical crack length above which rocket motor cases lose their strength rapidly. This correlation enabled the designers to determine the minimum critical crack length to look out for while proof testing the rocket motor cases, in the Sky Bolt missile program designed for launching nuclear warheads. This way, the designers knew the minimum crack length that could be tolerated without resulting in catastrophic failures in service.

LIFE AT IIT MADRAS

In May 1964, while I was working at Douglas on leave from IIT, I received a telegram from Dr Husain Zaheer, the then DGSIR, that Dr P Nilakantan had died suddenly in April, and that I should immediately meet M C Chagla, Cabinet Minister, who was going to New York for a Security Council meeting. Chagla was the Vice President of CSIR, and was looking for a successor to Nilkantan. Karamcheti also received the invitation. When Chagla asked me how soon I could join, I told him that I was already in IIT, and it would not take any time to shift, if he wanted me. I did not hear from CSIR and mentally wrote off any possibility of joining NAL, and decided to return and continue to work in IIT. I thought that I should work there for some time without losing my right of return to the US, as by then I had already become eligible for US citizenship, and could stay in India without losing this benefit for some more time. I returned to IIT in August 1964. I found to my surprise that the culture in which I grew in Caltech was just not there in the IIT. Except for a few, there were no faculty members who were seriously interested in research. It was not uncommon to see the faculty members leaving their offices to go home or to play tennis early in the afternoon. I recall Prof Sengupto, the then Director, asking me if that was how academic institutions in the US functioned, and academic freedom was interpreted. I was miserable and used to return home very unhappy. The student body was superb. My wife said that if I was so miserable working in the IIT, we should go back to the US. The decision was not easy.

ACCIDENTS DO HAPPEN - THE START OF LIFE IN NAL

Karamcheti was offered the position of Director, NAL. I told him that he should accept it so that we could all work together to build a strong base for self-reliant aeronautics in India. But he put a condition to Chagla that he would come only as a US citizen, and that he would like to have the right to go to the US as frequently as he wished. Apparently Chagla rejected it, stating that he would have to face serious questions in Parliament, and instead offered the appointment to Dr J P Chawla, then in DRDO. It seems he put the condition, that whatever was agreed to for Karamcheti should be agreed to for him also.
Chagla was disappointed and called for a meeting of the selection committee to review the situation, on July 19th 1965. He thought that scientists were idealists, and did not demand such things; he was apparently disappointed by these developments. The committee consisted of Chagla, Dr Zaheer, DGSIR, Dr Ghatge, then heading HAL Design Bureau, Dr D S Kothari, former Scientific Adviser (SA) and at that time Chairman of UGC, Dr S Bhagavantam, SA, and Prof Satish Dhawan, Director, IISc. At the earlier meeting at which Chawla was appointed, apparently Ghatge, Kothari and Bhagavantam had supported Chawla, with Zaheer and Dhawan supporting me. It appears that Dhawan told the committee that we were related through our wives, and he could be prejudiced, so his view should be taken with a pinch of salt, but that he felt that I was the right person to head NAL. Dr Zaheer who had met me earlier at a meeting convened by him for the creation of a Society for the Cultivation of Scientific Temper, and was aware of my views on building scientific institutions, apparently reiterated his preference for me.

At the meeting Chagla apparently said that he thought scientists were idealists, and was disappointed to see the conditions demanded by Karamcheti and Chawla, and wondered if appointing Chawla was the correct thing to do. He desired the issue to be reexamined. It seems Bhagavantam, under whom Chawla was working, changed his view, and stated that it was important for DRDO to have access to the 4 ft trisonic wind tunnel being built in NAL, and that he was not sure of having easy access to it, if Chawla was appointed. The tables were turned. While apparently Ghatge and Kothari demurred, they did not oppose this view. Dr Zaheer apparently told the committee that I continued to be his first choice. Dr Dhawan reminded the committee of our relationship, but that in his view, I would be the right choice. So the committee that was convened to consider Chawla's conditions ended up selecting me as the successor to Nilakantan!

I came to know of this development the following morning, July 20th 1965, from Dr Nayudamma, who was at that time Director of CLRI. Till then I knew nothing about it. I was mentally preparing myself to return to the US, with my wife telling me that there was no point in staying in India, if I was so miserable in the IIT. According to Dr Nayudamma, Dr Zaheer wanted me not to put any conditions, and to take over immediately. While sitting next to him at an evening tea at the Governor's residence when I hosted the Directors Conference in Bangalore, in July 1966, Dr Zaheer told me that he had prevented the appointment of Chawla by the "skin of his teeth". So I became the Director of NAL by accident! One of the first things I did as soon as I joined NAL was to give an open-dated letter of resignation to Dr Zaheer, informing him that he was hiring me sight unseen, so to

say, and that I might not be the person he was seeking as the Director, NAL, and so he should not feel obliged to have me, if he had any reservation. He tore it up in my presence, and said that the question would never arise.

But I did put a couple of minor conditions. One was that I would like to bring with me to NAL my junior stenographer from IIT, who I found was very good, and the second was that I did not want the CSIR architect to be involved in designing the NAL buildings. I had very definite ideas about how to design the NAL buildings, architecture being a hobby for me. Dr Zaheer readily agreed to the first one, but expressed difficulty in agreeing to the second. G N Vittal, who came along with me, turned out, over the years, to be an excellent personal assistant, and eventually retired as Controller of Administration in another CSIR lab. The problem of the CSIR architect resolved itself. Once, while coming from the Belur campus to Kodihalli along with Dr Zaheer, J R D Tata, Chairman of the NAL Executive Council, saw the guest house building designed by the CSIR architect, and asked Dr Zaheer who had designed that building. Dr Zaheer replied that it was done by the CSIR architect. JRD's comment was succinct. He said, "Fire him". I handled from then on, planning and execution of all buildings in NAL. I took personal interest in the design and construction of all NAL buildings and the layout of the gardens, with much help from some other staff. There was an interesting story about the landscaping of the NAL campus.

The IISc campus, and how pleasant it looked with all its greenery, always impressed me. I felt it was a good example to emulate, with the belief that a pleasant environment contributes to good working conditions. The Kodihalli campus buildings are located about 300 ft from the airport road. I was certain it was going to become a very noisy road, and that one way of reducing the noise was to plant plenty of trees. I found people like Dr Shivaraj Desai and Dr M V Subbaiah, who were very much interested in gardening, and gave them a free hand to develop the NAL gardens. I am proud of them and the landscaping they have done in NAL both in the Kodihalli and Belur campuses. Many people from within the country and abroad complimented us on the NAL landscape. The horticultural overseers used to take particular pleasure in decorating the front of the administration building at the time of Independence and Republic days. Mr C Subramanyam, Cabinet Minister, during one such occasion remarked how beautiful these decorations were. Full credit goes to the successive heads of the horticultural section of NAL. Dr Ramaseshan and I used to have our lunch together in my office. One day, with a smile on his face, Dr Ramaseshan remarked that as a Director, I was a good gardener. I did not know quite what to make of it. I told him that I was proud of the NAL landscape, and I would take his comment as a compliment.

Figuratively, I tore up my green card, and accepted the challenge of building an institution virtually from scratch. NAL then had a budget of Rs 29 lakhs (Rs 2.9 million) and about 350 staff. About 60 were scientific staff. We have come a long way since then. The years in the US and in particular life at Caltech, exposure to the aircraft industry, NASA and WPAFB, and their profound influence on me prepared me to face this challenge of virtually building an R&D institution from scratch. At the time I retired there were about 1250 staff, with about 350 staff in the scientific cadre, of which about 120 had doctorate degrees. We

had as much income from sponsored research as we were receiving from CSIR. Currently NAL apparently has a budget of about Rs 90 crore (Rs 900 million), of which roughly half is for contract/sponsored research.

LESSONS FROM CALTECH

The 14 years or so at Caltech and my interaction with the aircraft industry taught me some basic facts and principles. Early on, while I was still a student, I was invited by the Society for Experimental Stress Analysis to present a paper on crack detection at its very early stages. Dr Sechler assigned the task to me when the Southern California Cooperative Wind Tunnel suffered catastrophic damage due to the fatigue failure of a propeller blade. After I prepared the paper, I took it to Dr Sechler for his corrections. He made a few changes and instructed me to fair it. In the faired version I included his name also as a co-author, and took it to him for final clearance. He deleted his name. When I said that since he had helped me in the preparation of the paper, his name should be there, he said that as a senior, it was his duty to advise me, but he had no right to take credit for my work. It was an important lesson, which I never forgot in my professional life. On another occasion, Anatol Roshko, who subsequently became a professor at GALCIT, and I were attending a party shortly after I was admitted to do my PhD. In a pensive mood Anatol remarked, "To be honest when everybody is looking at you, that is nothing. But to be honest when nobody is looking at you, that is some thing". A profound statement, even if it was made in a casual manner. Shortly after I got my PhD, I found an error in my thesis due to carelessness. I had recorded the room temperature wrongly. Although it did not affect the results, I felt ashamed and told Dr Sechler that Caltech should take my degree back for my carelessness. He said, "Get back to your work. If you did not come and tell me, I would have thought less of you. When we award the degree, not only do we examine the contents of your thesis, but also try to assess whether you have inculcated the culture of Caltech and its ethics". I never forgot this lesson, and it became a guiding principle of my professional life.

Caltech also had an "Honor Code", which is the statement of an article of faith about the manner in which you conduct yourself. It simply stated: "No member may take unfair advantage of any other member of the Caltech community". The undergraduate and graduate student bodies elected the Honor Code monitoring committees to ensure that any deviations were examined carefully and to take suitable action. I understand that faculty members who indulged in unethical practices were made to leave. Scientific misconduct was taken very seriously to ensure that no bad precedents were set. This was a lesson that I did not forget during my years at NAL.

Noticing that scientific misconduct was not uncommon even in the US, President Clinton appointed a high-level committee to examine it. He issued a "presidential finding" and authorized his Office of Science and Technology to monitor it in institutions funded by the federal government. He further stipulated that where proven beyond reasonable doubt by preponderance of evidence, such institutions and the persons indulging in misconduct shall be denied federal funding. More about this, later.

In this context it is worth noting that scientific misconduct is defined by his committee as "fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results. Fabrication is making up results and recording or reporting them. Falsification is manipulating research materials, equipment, or processes, or changing or omitting data or research results such that the result is not accurately represented in the research record. Plagiarism is appropriation of another person's ideas, processes, results or words without giving appropriate credit ... Research misconduct does not include honest errors or differences of opinion". "Findings of research misconduct require that there be a significant departure from the accepted practices of the relevant research community and the misconduct be committed intentionally, knowingly or in reckless disregard of accepted practices … And the allegations must be proven by the preponderance of evidence".

It is hard to improve upon this definition. I became aware of this explicit "presidential finding" only after I retired from service. However, this definition of misconduct has been the implicit foundation of my whole attitude to scientific research, and has been a guiding principle for the manner in which I functioned in NAL. When instances of scientific misconduct were brought to my attention, I took definite salutary action. I deeply regret that I did not introduce a formal honor code among the staff in NAL while I was in service. After I retired, some instances of scientific misconduct by some senior scientists and heads of divisions were brought to my attention. I was helpless, as I had retired by then. However, even after retirement I took a stand when an NAL divisional head indulged in scientific misconduct. He presented a paper at an international conference with falsified data. The then Director of NAL, Dr T S Prahlad, was reluctant to examine the issue and take suitable action. More about it, later.

Some of my successors were unwilling to take any action even to establish facts when instances of scientific misconduct were brought to their attention. It is a pity. I found that there is an iron curtain in front of the office of the Director, and he would get to know only what the senior staff that have access to him would wish him to know, unless he made direct efforts to penetrate this curtain. To be true to this principle, in my professional life here and in the US, I made sure that no scientific paper carried my name as an author or co-author unless the first draft of the paper or report was prepared by me. Clearly, I could not expect others to follow this principle, if I myself did not live up to this code of ethics in the practice and management of science in NAL. I made it a point to clear the files the first thing in the morning in a couple of hours or so, and then used to walk around the lab talking to people to understand what they were doing and getting to know them personally, and not just be familiar with their faces. It made hundreds of the senior staff happy that I knew them by their names.

DIFFICULT DECISION TO GIVE UP RESEARCH

For some time after joining NAL I tried to do some work based on some ideas I outlined at an international conference on fracture held at the Orlando Air Force Base in Florida in 1962. It was about a method for handling fatigue due to random loads. This demanded

familiarity with handling statistical techniques, with which I was not familiar. After joining NAL, I still tried to see if could tackle it. I sought the help of Dr Narasimha, then at the IISc, to learn how to handle such problems. It was a hopeless situation, with the wretched files on my desk demanding urgent attention. I gave up all ideas about doing personal research from then on. I felt that I was being paid to build and run a young research lab then virtually in its infancy, and not for sitting in the office of the Director, and doing my own research. I never attempted it again. But I made it a point to know broadly the progress of the various R&D programs in NAL. I also learned in Caltech that you couldn't build institutions for pursuit of excellence by knowingly appointing secondrate people. If you appoint a second-rate person today, you would inevitably attract a third-rate person tomorrow, and the first-rate person would tend to leave. You cannot compromise these principles, which demand that when you spend public money, you should be a trustee for public good, and you cannot compromise the basic foundations on which such institutions are built. These are the principles that I ardently believed in, and they formed the basis and foundation on which I built NAL during my eighteen and a half years as its Director. However, for scientists who are deeply committed to research it is an intellectually deadening experience to have the responsibility for running an R&D institution. Scientists occupying such positions will certainly get public exposure and opportunities to participate in high-level committees, but such work inevitably takes away time from research. If they try to do research work while occupying such positions, they will inadvertently tend to destroy the institutions that they have the responsibility of leading. Research demands analysis of problems in finer and finer detail, whereas management of R&D institutions requires a global perception and the ability to formulate policies and take steps to implement research programs in the short term to achieve such objectives. Not all scientists are gifted with both these qualities.

I felt that a PhD background, which essentially teaches how to pose intelligent questions, would help NAL go a long way in its ability to build a first-rate applied R&D institution. In order to achieve this objective, I recruited people with a PhD, wherever possible. Also, when I found any staff members with such potential, I encouraged them to do their PhD. Many of the staff took advantage of this, and some of them who went abroad decide to stay back, and did extremely well. The most satisfying instance of this approach was when I found a bright staff member at a very junior level. I encouraged him to pursue further studies while working, and gave him study leave when necessary. He got his PhD shortly after I retired, and is now working at the deputy director level. Such things were possible because of my close interaction with staff.

At the time I retired, out of about 1250 total staff strength, approximately 350 were scientific staff, of which about 120 had a PhD. At that time, NAL had more PhDs than the whole of DRDO and the Space Department put together. It did not happen by accident. I planned it that way. The nicest compliment NAL received was an audit paragraph placed before Parliament asking why the other CSIR labs were not being run along the lines of NAL. But this did not necessarily make it technologically more productive, for the reason that it was not vertically integrated with any hardware development organization to ensure that its output would form a desirable input downstream for some hardware development.

Dr Dhawan succinctly commented upon this situation one evening in the early ’70s. He remarked, “NAL is like a beautiful bride, all decked up, and nowhere to go”. There was much truth in that statement. This situation haunted NAL all the time. It made some efforts in recent times to overcome this situation by generating major hardware development programs of its own, but so far without unqualified success. To achieve this object, it had to shift emphasis from research to technology and hardware development, and obtain knowledge in a multiplicity of areas, so that the several systems make the hardware work smoothly. In this, there is no substitute for experience, something that NAL lacked, and had to learn on the job.

NAL’s main problem is that it is not vertically integrated with any hardware development organization, nor is its headquarters seriously concerned with it, so that its output could naturally become a deliberate input downstream for such activity. In fact, the then chief of planning in CSIR in his performance assessment gave low marks to NAL in spite of his awareness of its high quality research base, for it did not contribute to industrial growth directly. Its activities were too highly specialized for that. In the absence of viable administrative linkages, NAL was not able to mount any major and specific R&D programs, which would help it make direct inputs to the aeronautical industry, till the Light Combat Aircraft (LCA) program came along. It has been at the mercy of others to put its contributions to good use. Even in the LCA program, its contributions were possible only because of long-term planning and lack of competition from any DRDO labs, which were by and large jealous of NAL for the reputation it had built over the years. Its efforts to stake out a claim in the civil sector ran into trouble because of inadequate understanding of the design and development of commercially viable civil aircraft. More about this and my life in NAL, in the next section.

Chapter 2

YEARS IN NAL

Although I started taking an interest in NAL activities from the time my appointment was announced end of July 1965, I actually joined NAL only on 23rd November 1965. There was an interesting incident in this connection. My father wrote to me to say that I should not take charge on that day, as it was Amavasya and very inauspicious. I wrote back to him that not taking charge on that day would deny everything I stood for, and that I intended joining on 23rd as planned. And I did. From the time Dr Nilakantan passed away early 1964, till I took over, Mr K G Krishnamurthy, who subsequently became Secretary, CSIR, functioned as an Officer on Special Duty. Not being familiar with the field of aeronautics, he kept the organization going, with substantial freedom to the scientific staff to do as they pleased. There was no overall sense of direction, but he was popular, as he told me, for leaving the scientific staff alone to do what they pleased. I recall telling him that I was not there to win a popularity contest, or get along with people, but to build the best engineering lab in the country. The research programs taken up during that period were ad hoc, with no long-term plans to achieve any specific end objectives, in striking contrast to my experience in the US.

THE NEED FOR FORWARD-LOOKING RESEARCH AND TECHNOLOGY DEVELOPMENT

The fact of the matter is that the aircraft industry is a high science, high-technology industry. It is driven by long-term perspectives of future requirements, and tends to have long gestation periods for developing successful R&D and specific technologies in anticipation of potential future requirements. If today the Air Force conceives a threat scenario that would develop ten years from now, the technologies that are required to develop a suitable aircraft must be already available, when the Air Force issues its Air Staff Requirements (ASR). It takes about that much time or more to develop the relevant know-how and technologies needed to respond to the ASRs. The fact is that basic research leads to applied research, which in turn leads to preliminary forward-looking technology development, which again leads to specific technologies to respond to potential ASRs. There are no short cuts, and each step is time consuming and cannot be short-circuited. The cost escalation is typically two to five times from one step to the next. The last phase of this cycle is normally the responsibility of corporate R&D in the industry. Unfortunately, till Dr Krishnadas Nair took over as Chairman of HAL, the corporate R&D in HAL was not significant. This again posed problems of linkages for the R&D establishments, since there was nobody in the industry with whom they could interact. Without such forward-looking research and technology development, there would be interminable and avoidable delays, as has been amply demonstrated in the development of the Light Combat Aircraft

(LCA). Thus, the aeronautical R&D organizations have to have a 20-year perspective for their R&D programs, if the ASRs of the Air Force have to have a fair chance of being complied with, in these time scales. By and large, this has not been appreciated in our aircraft industry and R&D organizations, not to speak of the administrators in Delhi, who have to give the formal sanction and financial support. In this connection, I recall attending a Douglas/Dupont meeting late 1962 to examine the future of composite technologies in the aircraft industry. Earlier, while visiting RAE in Farnborough, UK in early 1962, I saw the development of carbon fibres and their use in experimental automobile bodies as a prelude to understanding their application in aircraft structures. I said at that meeting, "In our life time we will routinely see the use of composite structures in aircraft". The agency heads at the secretary level in India, who have the ultimate responsibility for financial approvals, were ignorant of the importance of such forward-looking technology development programs.

One of the important early decisions I took in NAL was to give priority to development of composites technology. Some time before I joined, a glass fibre composite structure was used in the 1 ft supersonic tunnel in NAL. Over the years, we probably spent about Rs 50 lakhs (Rs 5 million) to come to grips with composites technology, particularly for fabricating load-carrying composite structures. As carbon fibre was very expensive, costing about Rs 6000 per kg, we developed the technology using glass fibres. The Structures Division successfully developed the analytical techniques to handle composite structures. Development of this capability in anticipation of future needs prepared NAL to meet the challenge of developing major carbon fibre composite structures for the LCA, when the time came more than 10 years later. Another important decision I took was to develop a first-class library. It has been said that a "true university is a collection of books". I have been fortunate in obtaining, over the years, first-class people to run the library. It is now generally recognized as one of the best specialist libraries in the country.

Early on in NAL, I realized that if one is honest and conscious of the responsibilities that running a research laboratory entails, one cannot afford the luxury of sitting in the office of the Director and doing one's own research, or getting too deeply involved in any one particular R&D program in the lab. Dr Ramaseshan, my distinguished colleague and Head of the Materials Science Division in NAL, once told me that I was destroying myself by not taking up any research on my own. Several years later, after he succeeded Prof Satish Dhawan as Director of the IISc, I asked him how his research there was coming along. With folded hands, he said that I was correct about the responsibilities to be handled by the heads of multi-disciplinary institutions, and they would leave no time for any personal research. It entails sacrifice of one's personal desires. A lab would be in a shambles, as was mentioned to me by a distinguished scientist who was in NAL, and had hands-on experience in dealing with such an issue while handling his own research. The fact of the matter is that it is difficult to build a first-rate R&D institution but easy to destroy it, without realizing it.

The creation of NAL was the result of the awareness of the lacunae in R&D capability in aeronautics in India, when Kurt Tank and his team of German designers were given the

responsibility in early 1956, for developing the supersonic HF24 fighter aircraft. Prof Dhawan and his colleagues at the Aeronautics Department at IISc were burning midnight oil to generate low-speed wind tunnel data, but there were no supersonic wind tunnels in India at that time. For generating such data, the designers had to go abroad. If such wind tunnel data were to be available to our adversaries, it would not be difficult to determine the performance characteristics of the aircraft we were developing.

This was the genesis of the National Aeronautical Laboratory, with the 4 ft tunnel as its core facility in the Wind Tunnel Center, now named the Nilakantan Memorial Wind Tunnel Center in honor of the first director. Apparently recognizing the importance of such a facility, Dr Dhawan and Dr Ghatge brought the matter to the attention of Prof M S Thacker, the then DGSIR, and stressed the importance of setting up such a facility in India. Apparently, Dr Homi J Bhabha, who was heading the Department of Atomic Energy at that time, told Prime Minister Nehru that it would be desirable to locate it in a civilian organization to enable it to interact with its counterparts elsewhere in the world. UNDP came forward and gave $1.4 million to build the infrastructure. Recognizing that the 4 ft tunnel could be used to design fighter aircraft also, Pakistan complained. The result was that NAL had to surrender the $400,000 that had been used to build the air compressor and storage facility.

I had known Paul Hoffman, then heading UNDP, when he was heading the Ford Foundation in Pasadena. I wrote a letter to him requesting him to reinstate the funds and saying that I would guarantee the use of these funds purely for open-ended research in materials. He was gracious enough to reply and stated that it was too late to do anything in the matter. While I was at Caltech, I was the president of the Inter Nations Association (INA). In that capacity, I invited Hoffman to give a lecture at an INA meeting and introduced him with a brief speech. He grabbed my draft as a keepsake and remarked that he had never been introduced more eloquently. I was glad. It was this contact that made it possible for me to write to him. It was only later on that I realized I had violated protocol in writing to him directly. Incidentally, in his lecture at Caltech Hoffman remarked that if you don't get facts, facts will get you - an important lesson that I did not forget. I wanted to be sure of my facts before I took a stand, ever since then.

The aeronautical industry in India has been, and continues to be, largely dependent on licensed production, and the licensors are not keen to tell us how their aircraft were designed. After all, they are more concerned about their health, not ours. For example, Sir Richard Smeeton, the then secretary of the British aerospace industry, came to visit NAL with his colleagues and offered an opportunity for collaboration in the Jaguar program. I told him that I was amused, as the Jaguar was already under production in the UK, and that I would move earth and heaven, if the UK had offered us an opportunity for collaboration in the MRCA (Tornado) program. He kept quiet, and said while leaving, that in my position, he would take the same stand. The country, of course, went into the licensed production of the Jaguar. The civil servants and the Air Force were happy with this

situation, as they have no accountability for any failures in such a scenario, and the Air Force got what they wanted.

It is a pity, because there is so much capability in the country. In moments of crisis, our suppliers can hold us to ransom, because our policies have not encouraged self-reliance, till the LCA program was approved. It has happened. For example, when one of the aircraft HAL was building under license for the Air Force was giving trouble, the then Chief of Air Staff wanted NAL to look into the matter. When we sought from the original developers some data, they said that to manufacture that aircraft here, we did not need that information and so they were unwilling to provide it. Since we had the capability, we went ahead and generated it. When they came to know about it, they said that they would be glad to provide the information. I was happy to say "No thanks". We should not hesitate to buy aircraft from abroad or produce them under license, not because we are incapable, but because the cost and time scales may not permit us to design and develop them ourselves.

While planning the future of NAL, I was very particular to avoid this pitfall. I felt that accountability and ability to respond to potential future needs in our assigned area of responsibility must be built into the system from the very beginning, while planning for R&D. I took the initiative to create scientific sub-committees for each scientific division, consisting of representatives from our users and experts in the field. Only after finalizing our annual research programs in extensive consultation with our users, experts and the heads of the Divisions, would we place them before the Executive Council, then presided over by J R D Tata.

JRD had a profound influence on the growth of NAL. The Council used to meet twice a year and his queries in the meetings were incisive. Virtually every page of the agenda sent to him was flagged for questions. This was so unlike some of his successors. He took immense personal interest in what was going on in the lab through his visits. Two instances readily come to my mind. While visiting the Kodihalli campus workshop tool crib, he rubbed his hand against the glazed white tiles below the crib counter, and showed it to me saying how dirty the tiles were, and said, "Dr Valluri, our lab should not look like this". I did not know where to hide my head. In another instance, while looking at the throat of the 4 ft tunnel, he asked me what the throat opening would be at Mach 4. I did not know, but guessed it. Dr B Satyanarayana, who was then heading the 4 ft tunnel group, corrected me, and JRD stared at me. Once again, I did not know where to hide my head.

JRD never said "NAL". He always said "our lab". It was a serious loss to NAL that following the Sarkar Committee recommendations, executive councils were abolished and his guiding hand in shaping the destinies of NAL was not available. I recall him telling me once, that he went abroad quite frequently, and that he would like somebody telling him about some nice paper from NAL, that he had come across. I believe we had not failed him. It so happened that during a CSIR Directors' conference held in Mysore, I told Dr A Ramachandran, DGSIR, that such councils have an important role to play in the CSIR labs. Following this, the Research Councils were established over and above the essentially internal management councils. Such bodies can only be as useful as the directors desire,

and the depth of understanding of the chairmen of the issues involved. No more and no less.

I really missed JRD, his unstinted advice and support, and felt lost without him. Dr Ramaseshan and I went to receive him at the airport for the last Executive Council meeting he was going to preside over. He was in a pensive mood, and said, "Of all the institutions with which I was associated, I feel proud about three of them". I was wondering which those three were, and then he went on to state, "No, only two, as Air India is a commercial organization. The other two are TIFR and NAL". No greater compliment could have been received by NAL from this visionary. The nicest compliment that I personally received from him was that when Dr R Damania flew the Light Canard Research Aircaft (LCRA) built under his leadership in NAL to Mumbai, JRD came to see it, and apparently asked Damania how I was. I felt deeply touched that after all those years, he still remembered me. I now believe that I made a serious mistake in not seeking the benefit of his advice afterwards in an informal manner.

When Dr Atma Ram took over as the DGSIR after Dr Zaheer resigned on a matter of principle, many of the CSIR labs faced serious problems. There was an interesting development during this time. In fact, about nine of us appointed by Dr Zaheer, were invited by Mrs Gandhi, the then PM, who wanted to know what was going wrong. Dr V K R V Rao was the Vice-President of CSIR. It turned out that Dr Atma Ram was suspicious of all the directors hired by Dr Zaheer. JRD told Dr Atma Ram to leave me alone and was kind enough to send me in confidence a copy of his letter to him, a measure of his confidence in me. In fact, I told Dr Atma Ram at that time that I had only one interest, and it was to build a very good lab, and I could not do it without his help, and that if he would not support me, I would be glad to leave. He then gave instructions on record to his administration that I should be given whatever I needed. Subsequently, the planning group in CSIR, which was concerned with budget allocations, told me that I did not have to defend our budget, as others told them how good NAL was, and they wished they could give us more funds.

Early on in 1969, I had an unnerving experience while interacting with Dr V K R V Rao. Mr K G Krishnamurthy, then in CSIR, wanted me to host the CSIR Bhatnagar Memorial tournament. I readily agreed, as the Systems building, which had just been completed and was yet to be occupied, provided enough place to accommodate the participants. Dr V K R V Rao was requested to inaugurate the event with Veerendra Patil, then the Chief Minister of Karnataka, as the chief guest. Patil told me that he would be in NAL exactly on time, at 6 pm. It was therefore important that the Vice-President was there in time to welcome him. I met the Vice-President at the airport and in a casual conversation told him that the chief guest would be on time. While he was talking with some politicians and getting delayed, I reminded him of the intent of the chief guest to be on time. He was visibly annoyed and remarked, "Let us go. This man is treating me like a school teacher". I did not know what to say. I was dumbstruck and related the event to Dr Atma Ram. He kept quiet and I had to handle the situation as best I could.

When I went to the Raj Bhavan to escort the Vice-President to NAL, Patil was already there, and they traveled together to the venue of the meeting in NAL. As soon as the car stopped, the Vice-President quickly got out of the car, opened the door for the Chief Minister to get out and formally welcomed him! In my welcome address, I remarked that when the Vice-President sneezes, people below him, like me, may well get pneumonia. He took it as a theme for his lecture! The following day, before he left for Pune on a visit to NCL, he spent the whole morning visiting the various divisions of the lab. He was visibly impressed by what he saw. He did not say much, as he left for Pune that afternoon.

More surprises were to come. A couple of days later, I got a telephone call from Dr B D Tilak, Director of NCL. He wanted to know how I was running NAL. When I asked him why he was asking that question, he said that the Vice-President had asked him why he was not running NCL along the lines of NAL! I was amused and felt quite happy. That was not the end of it. Shortly thereafter, I got a telephone call from the Vice-President's office with a proposal that I should take over as the Chairman of the Governing Council of IIT Kanpur, and that he wanted my answer in 24 hours. Prof P N Murthy, then in IIT, was visiting me. I asked him for his advice. He said that it would be a gain for IIT, but a loss for NAL and me, as it would involve considerable additional work. I declined the offer. But from then on V K R V Rao was kind to me. So to say, all's well that ends well.

Incidentally, I requested Dr Atma Ram to make my position permanent so that I could seek a CSIR loan for building my house. He agreed and I then started construction. He delayed confirmation. It was an embarrassing and difficult situation for me. I brought the matter to the attention of Dr Nayudamma, who subsequently took over as DGSIR. He immediately brought the matter to the attention of Mr C Subramanyam, then VicePresident of CSIR, who immediately confirmed my appointment. Incidentally, Dr Nayudamma advised me not to hire any contractors who were working for NAL. I did not, and kept full account of the expenditure in case any questions were to be raised at a later date. The construction of my house, which I completed in June 1972, cost about Rs 125,000, at the rate of about Rs 34 per sq ft of construction and including about Rs 14,000 for the plot.

In the US there is freedom for research staff to do their own research, as long as it is relevant to the objectives of the institution and does not demand additional funds to support it. But if such funds were needed, proposals had to be submitted to various funding agencies of the US government. Typically, no more than 15 to 20% of federal R&D funds are given for open-ended basic research, with the balance going to respond to specific needs as conceived by the Pentagon or the NASA in aeronautics. For example, when the Pentagon wanted the turbine entry temperatures of aircraft jet engines to be increased by 50 degrees centigrade, it funded several programs worth millions of dollars. Once the cooled turbine blade technology was successfully developed, it closed all the ongoing programs it was funding to achieve this objective. In other words, the funding was project-specific, with clear end objectives. This is something that we rarely do here except, possibly, in project-specific agencies such as DOS and DAE.

As chairman of the Technology Advisory Board for the engineering group of laboratories in CSIR during Dr Joshi's time as DGSIR, I proposed that the annual plan allocations to the labs must be project-specific and must be screened at a higher level. This was rejected. It entailed responsibility and accountability, something that the staff in CSIR did not want to take on. The general trend in India is for scientists to submit a proposal, with the concerned department constituting a committee to examine it. More often than not, the S&T departments (except those vertically integrated, such as Atomic Energy and Space) have no clear vision of their own for funding or supporting R&D, with specific end objectives. These programs are in no way related to the objectives set by the Planning Commission except, possibly, in some technology missions. Mission-oriented development of high science and high technology is not very common. The result, of course, is that the benefits from R&D to build a viable and coordinated applied research and technology base, which is a prerequisite for establishing high-technology industries, are largely missing in India. Only these will enable us to join the cadre of developed nations.

My first task in restructuring NAL and making it a purposeful research laboratory demanded answers to some basic questions: Where are we? Where do we want to go? Why do we want to go there? And finally, how do we want to get there? These in turn, raised two specific objectives to be met, which were, of course, planning the bulk of the research programs, both short- and long-term, so that their output formed either a desirable input downstream to some other program, or led to desirable applied research and/or preliminary technology development to help establish a reasonably self-reliant aircraft industry. In order to achieve these objectives through planning the R&D programs, the divisional heads had to answer the question: "If the program succeeds, who is going to be benefited downstream? If nobody is going to be benefited, why should we take up such a program?" The budgeting was zero-based. Every ongoing program was reviewed every year as a prelude to planning the following year's R&D programs, in order to decide whether it should be continued or closed. Typically, about 20 to 30% of the programs were closed annually, either because they were completed, or because they did not attract a sponsor. I told my colleagues that if a lab-sponsored program did not obtain a sponsor at the end of three years, we should close such a program. If a scientist succeeded in obtaining a sponsored program, his lab-funded research program was held in abeyance till he completed the sponsored program.

An interesting development followed. I wrote a long letter to Dr Nayadamma shortly after he took over as the DGSIR from Dr Atma Ram, about this restructuring of R&D planning in NAL, outlining the scope of our short-term and long-term research programs and the philosophy behind it. I told him that sooner or later, India would have no alternative but to mount an indigenous fighter aircraft development program, and outlined the potential areas of our responsibilities. I showed this letter to Dr Zaheer during one of his visits to NAL. He said that it was the first time a Director had written such a letter to the DGSIR and that I should send it, and complimented me for my long-term planning of R&D in NAL. Dr Nayudumma, of course, approved it.

FRAMING THE RESEARCH PROGRAMS – DIVISION-WISE ACTIVITIES

Planning the research programs in the Aerodynamics, Structures, Materials and Propulsion Divisions did not pose any problems to me. But it was not clear why Dr Nilakantan created the instrumentation and electronics activities, which were subsequently integrated to form the Systems Division. Perhaps they had some justification while the 4 ft tunnel was being built. I had the feeling that we were taking up activities to justify their existence. These divisions also had many bright people, who were searching for worthwhile tasks. For example, the Electronics Division tried to build a calculator based on transistors then available in India. If I recall correctly, they had to use about 350 transistors to make a simple calculator! I wrote a letter to Prof M G K Menon, then Secretary to the Department of Electronics, that this was an impossible situation, and that he should relax the import regulations for integrated circuit chips. A short while later, Japan flooded the Indian market with pocket calculators. The company that bought the technology from NAL for this unit had to bury it. This division was so good that when the Light Combat Aircraft program was launched, they developed the control laws for its fly-by-wire control systems.

THE AERODYNAMICS AND FLUID MECHANICS DIVISIONS GENESIS OF AIRCRAFT DEVELOPMENT PROGRAMS

The Aerodynamics Division occupied a pivotal position in the functioning of NAL. It was its activities and its initiatives that would ultimately determine the conceptual design of aircraft and the fine-tuning of the design through wind tunnel tests. This division played a crucial role in studying virtually every Air Staff Requirement of the Air Force, and in the Space Launch Vehicle development, through wind tunnel tests in the 4 ft tunnel. Its researches were largely molded by the needs that arose from aircraft design. Initially, Dr D M Rao headed this division. When he decided to migrate to the US, Dr M A Ramaswamy succeeded him. Temperamentally, he was a scholar and somewhat out of his depth in administration. Later on, he left for a faculty appointment at IISc. In ’69-’70 we hired several people from the HAL Design Bureau. Their contribution to obtaining a design perspective in the division’s operations was decisive. While the Canadian Vickers built the 4 ft trisonic tunnel under contract, the division built the transonic 2 ft tunnel with the major responsibility for it resting with Venkatesh Murthy, Seshadri, Desikan, and the late K S Raman. It was funded by DST and was a commendable achievement. It is being used extensively for R&D.

It was realized that the mother science of aerodynamics is fluid mechanics, which needed to have a separate identity of its own, to explore future horizons in this area. Consequently, the activities in fluid mechanics evolved into a separate division. This division too did path-breaking work under the leadership of Dr K S Yajnik.

One evening in 1971, when Satish Dhawan and I were chatting, he suddenly asked why we were not building an aircraft of our own. I had no reply. I called for a meeting of the senior staff immediately, and related what transpired. Dr D M Rao was then heading the Aerodynamics Division. He was supposed to be very much interested in such things. I was surprised by his response. He said that it was not the business of NAL to build planes, but to assist HAL in every manner possible in designing planes. So to say, the Aerodynamics Division was the tail that wags the dog. Without the support of this division, the idea was a non-starter. I had to wait for somebody who was keen to take up such a task and had the ability to handle it. This opportunity came up when Dr Rustom Damania from the IISc wanted to join NAL. He already had an offer from the Aeronautical Development Establishment, but preferred to join NAL. He wanted $9000 to buy a kit for building the fibreglass composite Light Canard Research Aircraft (LCRA) from Rutan. I agreed, and he joined NAL and built the LCRA. But the design was not ours. Shortly after Dr Narasimha succeeded me, he supported the development of Hansa trainer, which was also made of fibreglass composites, and eventually certified by the DGCA. NAL owes much to Dr Damania for launching these programs. The project was subsequently taken over by Dr Shiva Kumar Swamy and Dr Sivaraj Desai with RMVGK Rao and his group having the responsibility for its fabrication. They all made significant contributions to the program. Dr Dhawan's dream of making NAL become acquainted with the problems associated with aircraft design and development were realized in a small way. But it posed some difficult problems later.

After his resignation from the LCA program as its principal architect, Raj Mahindra, a self-effacing person and an outstanding designer, who was earlier responsible for the development of the HJT16 trainer jet aircraft in HAL and for launching the ALH program, was studying the design of a small passenger aircraft suitable for feeder-level operations. While visiting his home one day, Dr Dhawan, who was the Chairman of the NAL Research Council at that time, saw what Raj was doing and brought it to the attention of Dr Narasimha, and suggested that NAL should take it up. After many trials and tribulations lasting several years, Cabinet approval (apparently the first time it was needed for any CSIR project) was obtained to launch the ambitious program of designing a commercially viable 9- to 14-passenger aircraft. The program incidentally showed up NAL's inadequacies in understanding the complex problem of aircraft design. The main problem is that in research we analyze a problem in finer and finer detail, whereas the design of aircraft demands the ability to synthesize knowledge to build a complex structure, while assuring safety of the people who are in it. More about it, later.

STRUCTURES DIVISION

The Structures Division's activities were primarily concerned with developing analytical techniques to handle aircraft structures, including aero-elasticity and responses to dynamic loads through vibrations. It is to be particularly noted that the division became fully familiar with handling composite structures, including the analytical techniques, in anticipation of future requirements. These developments came in handy when it was recognized that to satisfy the ASR of the Light Combat Aircraft, carbon fibre composite structures were

essential for the wing and some other components. NAL could respond to this challenge because of its prior preparation in anticipation of future needs. NAL later took on the responsibility for building the large autoclave for HAL at a cost of about Rs 5.5 crores (Rs 55 million), for curing large composite structures under high pressure and high temperatures. Ningaiah from this division had the overall responsibility for this project. In this connection, the contributions of the advanced composites group under Subba Rao's leadership were notable. It has played a significant role, including the development of co-curing technology for building complex composite structures.

MATERIALS SCIENCE DIVISION

For all practical purposes, the Materials Science Division did not exist when I joined NAL. My experience in the US made me realize two important aspects of materials science. One is, of course, development of new materials and the second understanding the behavior of materials. Every historical milestone in aeronautics was preceded by development of new materials and seeking an answer to the basic question: "Why do materials behave the way they do, in so far as their mechanical properties are concerned?" I had no desire to start a metallurgy division in NAL, but needed an answer to the above basic question. Otherwise it would have distorted the scope of activities in NAL. Furthermore, I wanted the understanding of the mechanical behavior of materials to be science based and not knowledge obtained by trial and error. I needed a first-rate scientist and in Dr S Ramaseshan, the well-known physicist, I felt I had found such a person. He was at that time heading the Physics Department at IIT Madras. When I told him what I had in mind, he readily agreed, but put the condition that he would like to continue with his work in X-ray crystallography, his first love. We too needed that knowledge for better understanding of problems related to internal stresses, etc.

Dr Ramaseshan built what was at that time considered an outstanding materials science group in the country. It tackled problems all the way from development of new materials such as super purity chromium at lab level, semiconductor materials and new equipment such as a spark erosion machine, an electrochemical milling machine, equipment for fabricating composite structures, equipment for making titanium powders for powder metallurgy applications, to failure analysis of structures and full-scale aircraft fatigue test facilities, etc. JRD used to take particular pleasure in visiting this division. It is regrettable that after Dr Ramaseshan left, many of these ideas were not actively pursued.

The Surface Engineering Group in this division played a significant role in developing practical technologies. I suggested to this group of scientists that I was not looking for publication of papers but that they should develop technologically sophisticated hardware. They did not let me down. A V Ramani developed an electrochemical milling machine as was proposed by Dr Ghatge in one of the executive council meetings. But it turned out far more difficult to develop the electrodes to manufacture components such as turbine blades. The technology for developing the electrodes was successfully tackled by Dr S R Rajagopalan and, in fact, the railways were supplied an electrochemical milling machine for manufacture of turbine blades.

Dr Ramaseshan desired that I should create for him a separate research lab out of this division. I could not agree to it, as I could not conceive NAL without a strong Materials Science Division. He soon left NAL, never to return, and took up the Nehru Fellowship, and functioned from Raman Research Institute. When it was over, he joined as the Joint Director of IISc to help Satish Dhawan, who was concurrently handling the position of Secretary, Department of Space. When Dhawan retired, there were some doubts whether Dr Ramaseshan would be selected to succeed him. In fact, he called me on the phone to ascertain if he could return to NAL if he was not selected. I, of course, told him unhesitatingly that he would be welcome. As it turned out, he did succeed Dr Dhawan as the Director of IISc and retired in1984. After he left NAL, the Materials Science Division somehow lost its bearings. Dr Rajagopalan did not want to succeed Dr Ramaseshan. He said he was not willing to give up research, his first love. After trying other people from the division, I asked Dr A K Singh to handle the responsibilities. His perception of how to run the division turned out to be very different from the manner in which Dr Ramaseshan ran it. Furthermore, my successor, Dr Narasimha, apparently did not want to make project-wise allocations and instead, reverted to lump sum allocations to the divisional heads and asked them to handle the funds. He ran NAL more as an academic institution with (academic) freedom to the staff to do virtually what they pleased. The very basis on which NAL was built was changed with this policy. Individual accountability of scientists made way for the division heads to do as they wished. For example, Dr Singh decided that scientists who were getting sponsored projects should not be given funds from the divisional allocations. While on the face of it, this does not seem unreasonable, it removes all incentive for the scientists to explore new avenues or to seek any sponsored projects. As a division, it lost its sense of purpose and vision, although it still had many outstanding scientists. It is a pity.

PROPULSION DIVISION

The Propulsion Division, from the very beginning, was headed by Dr Pramod Paranjpe, a very competent engineering scientist and extremely knowledgeable person, and a nononsense administrator. I picked him up from IIT Kanpur and he did a very good job of building the division and its facilities. I showed a visiting UNDP officer its activities when we sought funds from them to support its activities. I told him that even if our request for funding were not agreed to, we would somehow build the division. He immediately said that that comment alone was sufficient for him to recommend UNDP funding. We received substantial funds from UNDP to build the infrastructure of this division. Among various other facilities, the division had built the sophisticated transonic cascade tunnel under the direction of Mr M R Narasimha Swamy. After seeing this facility, a visiting MIT professor commented that it would have cost at least ten million dollars to build it in the US. It was built for a fraction of it. The scientists in this division built virtually all the test facilities. But it faced a basic problem. Under DRDO, there already existed the Gas Turbine Research Establishment (GTRE) headed by AVM Roy Choudhuri, with fairly extensive facilities. A very competent engineering scientist, Roy Choudhuri had earlier built a centrifugal gas turbine. It did not make sense to compete with GTRE. I proposed instead that we should

take up forward-looking R&D, which could hopefully become an input to GTRE. Institutional rivalries in this country being what they are, we could not make more decisive contributions. Dr Paranjpe was disappointed that he did not succeed me. When Dr Narasimha was willing to be considered as my potential successor, Paranjpe did not stand a chance. He subsequently became the Director of the Automotive Research Association in Pune, his hometown.

SYSTEMS DIVISION

The Systems Division, in a sense, continued to look for meaningful problems they could tackle. It had very good scientists on its staff, but not much scope for interaction with the aircraft industry, which was based primarily on production under license. It came into its own when the LCA program was launched. It contributed to the development of fly-bywire control technology by developing the control laws.

RESTRUCTURING THE NAL ADMINISTRATION

Some important administrative aspects came up while restructuring the NAL administration. It was clear from the very beginning that the reputation of NAL would rest on the scientific research and technology development being carried out in the lab, and that the responsibility of the administration was to be an enabler and nothing else. I made it very clear to successive administrative and accounts officers that the junior-most junior scientific assistant was more important to me than all of them put together, and that they did not owe me an explanation if they said "yes" to any scientist, but if they said "no", they had to have a very good reason for saying so. I also told them that I would always support a scientist when the issue came to me to take a decision, as it was on them that I had to depend for successful growth of NAL. As a matter of fact, some people in the administration were taking pleasure in delaying file movement, sometimes even by locking up the files in their cabinets. In such cases, I required them to render account of the files they had cleared each day and the files still under consideration. File movement became very rapid as a result of my interest in the matter. I recall the incident of a Mr Kabilan who was selected for the Indian Administrative Service (IAS) while in NAL. The administration was delaying the release for no good reason. He came and reported it to me. Apparently the section officer who was to clear the file told Kabilan that he should not grow horns just because he was selected for the IAS, and that he (the section officer) would decide when to clear the file and issue the relieving order. On instructions from me, Kabilan was released the same day. He became Chief Secretary to the Assam government. It would seem that administrative staff take pleasure in exercising their power by saying 'no' rather than 'yes'. I was surprised to hear that when an instance of corruption in administration was brought to the attention of one of my successors, he remarked that looking into such matters was not his responsibility, but that of the head of administration in NAL! No doubt the Controller of Administration has a responsibility to control corruption, but the Director is the ultimate authority for all that happens in NAL, and he obviously cannot abdicate his responsibility when such matters are brought to his attention. It is the surest way of making

such practices acceptable. If the Director does not take action in such matters, everybody gets to know about it, and it is implicitly assumed that he approves such practices.

NAL was fortunate in having some gifted scientists like Drs P N Shankar, S R Rajagopalan, R Sunder, K S Yajnik, Gangan Prathap, Pramod Paranjpe, and D M Rao, who were virtually left alone to pursue knowledge as they saw fit. I am glad that they were all elected as Fellows of the Indian Academy of Sciences. Dr R Narasimha, who succeeded me in NAL in 1984, is also a Fellow of the Academy and was awarded the Fellowship of the Royal Society for the work he did earlier and in NAL. These and many other gifted scientists like Dr Balakrishna, Subba Rao, Dr RMVGK Rao, Dr Srinath Kumar, Dr R Srinivasan, and so many others had to be treated as treasures, and NAL had to protect them and leave them alone. Narayana Iyer, who did not complete high school, worked in the Central Machine Shop and was in a class by himself. As far as I recall, he was the first person without a degree to be promoted to the rank of Assistant Director, when I brought to the attention of Prof M G K Menon, the then DGSIR, his contributions to the cause of scientific research in NAL. Incidentally, after my retirement, I was shocked to hear that in internal assessment, Dr Shankar was not promoted! This says something about the assessment system.

There was an amusing incident relating to Prathap joining NAL. He was the President's medallist in BTech at IIT Chennai, as high an honor as a student can hope to receive from his school. Shortly before I received a letter from him seeking an appointment in NAL, a staff officer in Dr Dhawan's office in the Department of Space told me that they had interviewed a President's medallist from IIT, and that he could not answer a single question put to him. It was Prathap. The fact of the matter is that anybody can be made to look ignorant in such meetings. When you appoint a person, you want to appoint him for what he knows, and not for what he does not know. Selection committee members tend to ask questions in fields with which they are familiar, but not necessarily in the fields with which the candidate is familiar. I invited Prathap to join us. I was happy that he accepted my invitation. He is an outstanding scientist.

Selecting candidates for scientific cadres is a very tricky affair. As I have already said, we should recruit candidates for what they know, and not for what they do not know. I recall an incident when a selection committee member was asking the candidate a question for which there was really no answer. After the candidate left, I asked the member for the answer to that question. He said that he did not know. I then asked him on what basis he posed such a question to a candidate. He had no answer. In another instance, selection of a candidate demanded ability to use the hands to produce a product. In such instances trade tests were allotted 70% of the marks and the interview 30%. The chairman of the committee, preferring a particular candidate, decided to allot 300 marks for the interview while retaining 70% for the trade test. In a personal talk I had with him, he could not defend his decision. I ordered him to change his recommendation and told him that he had no business tampering with well laid out procedures.

I laid down broad guidelines for selection. I proposed that the candidate must be first asked questions in his area of professional competence to ascertain the depth of knowledge in his

chosen field of specialization. He should then be asked questions in a related area to ascertain how broad his understanding was in his field of research endeavor. Finally he should be asked questions in a field with which he was not familiar. The purpose was to ascertain whether he could pose some intelligent questions. Most times his answers to these questions would be wrong, but they would give an idea about his ability to pose intelligent questions, for that was what research was all about. These guidelines were by and large successful in finding talented staff. I also stated that we should recruit people who could thumb their nose at the Director, because such people knew that they could get jobs elsewhere, and that we should not hire scientists who did not have the intrinsic ability to get a job elsewhere. We did commit mistakes in recruitment, but did not deliberately appoint second-rate people. If we hire a second-rate person today, the first-rate person will tend to leave, and we will end up attracting a thirdrate person tomorrow. Once, a member of a selection committee complained to the DGSIR that I was instructing the committee members about how to ask questions. I explained to the DG the basis of my guidelines. He rejected the complaint. Ultimately it was my responsibility to deliver the goods, and not that of the selection committee member.

In early 1966, I completely changed the confidential report (CR) format to better represent a staff member's performance during the year. In March 1966, when I went to pay a courtesy call on JRD shortly after I joined NAL, he put me in contact with Prof M G K Menon, who had taken over as the Director of TIFR shortly after Homi J Bhabha died in an air crash in January. The NAL confidential reports are now based upon the TIFR format, with some small variations. The format was subsequently adopted by CSIR for compliance by all its labs. With the help of Dr R Srinivasan of the Systems Division, we used to do a thorough statistical analysis of the CRs annually, to obtain a better appreciation of the performance in the divisions and the lab as a whole. I used to personally compliment the scientists who consistently scored high in the annual CRs and personally advised the poor performers to do better. It helped a lot.

I learned the hard way that it is easy to destroy a first-rate research laboratory, but difficult to build a good one. Running a first-rate research laboratory demands disciplined conduct from all the staff. I found that this was not always easy. Compliance with good traditions also requires a sense of discipline, which does not come easily. Furthermore, successive Directors also should accept such traditions and consciously try to uphold them, unless there is something fundamentally wrong with the traditions. I also found that bad traditions will certainly endure, as they do not require any discipline from the staff, particularly if the senior staff and the Director are unwilling to take tough decisions, even after knowing improper things are taking place. For example, it was apparently known that a staff member was holding concurrent appointments both in NAL and the Karnataka government over a 20-year period! This matter was not brought to the attention of the Director till CBI came to know about it, although many in NAL knew it!

In recruitment, we made no distinction between men and women candidates. This raised some problems. I felt it was important for the women staff to feel absolutely secure in NAL. They had free access to my office to bring to my attention instances of harassment. In two

cases, I had the unpleasant responsibility of terminating the services of two newly recruited male staff for indulging in improper acts. Once, Dr S Varadarajan, the then DGSIR, brought a White House team from the US to see the lab. Coming across a number of women scientists, one of the ladies asked me how it happened that there were so many lady scientists. I told her that in recruitment, we made no distinction, and tried to appoint the best candidate. She said, "How wonderful! We have problems in the US in hiring women, because of the deliberate preference for hiring men". I was happy to hear that.

I felt that the problem of maintaining discipline in NAL was of importance. All matters of indiscipline were carefully looked into. If an instance were to be overlooked and not curbed, it would set a precedent and give license for others to emulate. The lower level staff desired to start an association to highlight their grievances. I readily agreed and told them that if their grievances were genuine, they would not find a better friend than me, but if they indulged in indiscipline in the name of clearly unreasonable demands, I would take them to task. I was the honorary president of the association and the administrative officer (now called the COA), a permanent invitee, with the responsibility to implement the decisions taken in these meetings. The elected secretary of the association had the right and the responsibility to ensure that the decisions taken were implemented. Once, the Deputy Commissioner for traffic brought to my attention that some NAL drivers were taking money and illegally carrying passengers. At a meeting of the drivers, I drew their attention to this matter and told them that I would excuse them twice, but if a third instance were to be brought to my attention, I would charge-sheet them and hold an inquiry, and if the charges were established beyond reasonable doubt, I would terminate their services. The services of many drivers were terminated. We never lost a case, when some took disciplinary action cases went to the courts.

As a matter of policy, I encouraged the really good scientific staff to take up higher studies leading to PhD programs within the country and abroad, for reasons I mentioned earlier. I delegated to the divisional heads the responsibility for approving such applications, up to five candidates from each Division, with a proviso that no more than 15 people would be given study leave at a time. CSIR administration brought the matter to the attention of Dr Nayudamma, stating that a large number of scientists from NAL were abroad. He called me for a discussion. I told him that aeronautics is a field of high science and high technology with rapid changes, and that our staff should know what was happening abroad. He readily approved what I was doing. Some of those who went abroad did not come back, but I felt that was a price we had to pay to get our scientists exposed to the R&D environment abroad. By and large, those who did not return were occupying very responsible positions in academic and research institutions abroad. Some people from the US aircraft industry personally approached me to spare some of our scientists. It was as nice a compliment as we could get about our standards in recruitment and the culture prevailing in NAL at that time.

The policy of sending people on deputation also gave rise to a very unpleasant situation. It even led to the involvement of two successive Prime Ministers, before the issue was resolved. Dr Basavaraj, a scientist from the Structures Division, applied for the Humboldt

fellowship without the knowledge of the Head of the Division. The fellowship was awarded to him. It was an honour. But he put me in an embarrassing situation, as he had clearly violated the guidelines framed by the senior staff, to which he was an explicit party. Nevertheless, I decided to forward the application to the DGSIR for approval, explaining the situation and recommending the leave as an exception. It was sanctioned. The senior staff decision in such matters called for approval of leave for no more than one year, along the lines of the sabbatical, subject to the proviso that a further four months of leave could be granted to complete the work, if necessary. When his leave was expiring, Basavaraj wrote to say that he would like to go to NASA and wanted the leave to be extended by one more year. I wrote to him that he was a party to the decision of the senior staff and should return. He wrote back to say that it was his right to get the leave requested by him. Incidentally, Dr Zaheer, DGSIR, told me when I joined NAL, that leave was a privilege given to staff, but not a right, and that I should ensure that it was not misused. Basavaraj requested a four-month extension to complete his work. Just before the sanctioned additional leave expired, he wrote to say that he had got the NSF grant tenable in NASA, and sought further extension of the leave. I wrote to him that it was not possible, as I did not wish to make such exceptions, as the first loyalty of the staff was to serve NAL, not spend time abroad.

I issued a show cause notice to him. His brother, a senior engineer in Karnataka government service, came to see me. I explained the situation to him. He said his brother was wrong, and that he would advise him to return. I told him to advise his brother to resign, with a guarantee that I would arrange to have him hired back at one level higher, as I had nothing against him, and to me it was a matter of upholding policies unanimously laid down by the senior staff, to which he too was a party. Instead, he brought the matter to the attention of a Member of Parliament from Karnataka, Mr Dasappa, and asked him to talk to me. Mr Dasappa called on me. I explained the situation to him. He too said that I had done the right thing, and that he would have Basavaraj informed accordingly.

Instead, to my utter surprise, I received a letter from Dr Nayudamma, the then DGSIR, enclosing a letter from Mr P N Haksar, then Principal Secretary to Mrs Gandhi, about the matter. Basavaraj had apparently appealed to the PM. I wrote a detailed letter to DGSIR explaining that I had no intention of running the lab in an arbitrary manner, particularly when we framed specific rules to handle such matters, with the consent of senior staff, of which Basavaraj was also a member. I further told DGSIR that if by any chance, my decision was reversed, my resignation would be in his hands in 24 hours. Haksar rejected Basavaraj's request and defended my decision.

I advised Basavaraj again that he would do well to resign, and that I would take him back at one level higher as I had already promised, and that I had nothing personally against him, and that it was a matter of running an institution without setting bad precedents. He refused to resign. Mrs Gandhi's government fell. Basavaraj appealed to Morarji Desai, her successor. Morarji ordered his dismissal. If I dismissed him, the chances of his getting a job in India were nil. He accepted my advice and resigned. It turned out that the work he

was doing in NASA was trivial and, in fact, he admitted at a job interview in HAL subsequently, that a BSc graduate could have handled what he was doing!

It was a mistake on my part to have agreed in the first instance to forward Basavaraj's leave application to the DGSIR, when he had clearly violated the policy laid out by senior staff meeting, to which he himself was a party. This had its repercussions. Another scientist also applied for a NASA fellowship without the knowledge of the Head of the Division, taking it for granted that I would approve it, as she knew me well. After getting the fellowship, the scientist approached me for sanction of leave. I declined to forward it to CSIR for approval, as that would have given license to all scientists to thumb their noses at their divisional heads. Many a time, I had to tell my colleagues that I was their friend, but in the Director's office, I had to discharge the duties of the Director, and my friendship would have no role to play in taking a decision. I felt it was important that my decisions should be predictable and consistent. I did not have the luxury of taking arbitrary decisions merely to get along with the staff. Incidentally, I understand that one of my successors had got the leave abroad for many staff extended beyond that agreed to at the senior staff meeting. It was not clear to me who was going to be benefited more by such a policy. I believe that scientists are hired by NAL to respond to its needs, and not to spend time abroad to satisfy their desires. Even the sabbatical leave given by academic institutions is only one year once in seven years. I believe such arbitrary decisions set bad precedents.

I heard recently that a senior staff member staying in the campus quarters was ordered to vacate the quarters, since she had her own residence in Bangalore. As far as I know, rules do not permit allocation of staff quarters to such persons. The decision was perfectly in order. But in an identical case, another staff member who had his own flat within a few hundred yards from the NAL campus was permitted to occupy staff quarters! This was a highly discriminatory application of rules. In another instance, a scientist who apparently did some good work was authorized by the Director to use his contract funds to hire a taxi for official purposes. The scientist apparently felt it would be all right for him to use the taxi for personal purposes also, and the taxi was apparently used more for personal purposes than for any official purposes. This apparently set a pattern for others to emulate. Following this precedent, many scientists who were also doing contract research felt that they too had the right to hire vehicles from contract research funds to serve their "official needs". The NAL finance officer in a casual conversation told me that at one time there were more than forty taxis on hire, paid for by contract research funds, and he was paying close to Rs 50 lakhs (Rs 5 million) a year for this purpose! I suggested to the Director, Dr Prahlad, that it would be far less expensive to hire a few taxis and pool them under the transport section to serve the official needs of all staff. As could be expected, he appointed a one-man committee under Dr Ramachandra Pai, who subsequently became Director. He recommended that it would be all right to continue the practice, as the taxis were hired from contract funds and not from the NAL budget! To me it was a strange argument, as virtually all contract research funds of NAL come from government in one form or the other. I always believed that when we spend public funds, we are trustees for public good, and that we should be more careful with public funds than with our own money. Such decisions attract from the staff contempt of the Director and his advisers. The Director is vested with

tremendous authority and responsibility, and he is obliged to exercise his authority judiciously. For good or for bad, the ultimate responsibility for such decisions rests squarely on his shoulders and not on any of the advisers or any committees he appoints. He cannot abdicate his official and moral responsibilities in such matters to get along with staff.

I noted shortly after joining NAL, an interesting feature in running the administration. When I was a student in Caltech, if the library did not have a book or report I wanted, the librarian called the Pacific Coast Library Association, and borrowed it from some other institution. It greatly impressed me. When I proposed to the then registrar of IISc that we too should initiate a similar practice in India, he said that they did not share their resources with others. I told him, "Fine, don't give your books to us, we will be glad to share ours with you". He then said that I was putting him to shame. I told him that books were not bought to decorate the library shelves, but to be used. We then started a scheme whereby institutions could share books with each other, and would be held responsible for any loss of books loaned by one institution to the other. Since then several other institutions adopted the idea in my time.

Equally important was the Health Center set up by Dr K S Nanjunda Swamy, the Chief Medical Officer of NAL. I felt that if a staff member were sick, the lab would lose his services. If any of his family members were sick, he would be worried and would not be able to bring out the best in him. Therefore I felt that it was essential to set up a good Health Center. I asked Dr Swamy to build one, and told him that if he had any problems, he could come to me. He did not let down NAL. Many retired staff from other CSIR labs preferred to settle down in Bangalore because of access to these excellent health facilities. He brought together a dedicated medical staff to work with him in NAL. NAL staff owes much to him and his colleagues.

REVISING THE SYSTEM OF BUDGET PREPARATION AND ACCOUNT KEEPING

Another important task I faced was revising the accounting procedures, so that I would know precisely how much each project was costing. This demanded, among other things, determining the overheads for the lab as a whole for supporting the infrastructure, such as salaries of administration, accounts, stores and purchasing staff, etc. In addition, the divisional overheads were also determined. This was the system in which I grew up in Caltech, with monthly statements of expenditure for my project sent to me. In this connection, shortly after I joined NAL, I asked the accounts officer how much money was being spent on the construction of the 4 ft tunnel. He said that he did not know, as they did not keep that kind of accounts. I then asked him who prepared the budget. He proudly said that he prepared the budget. I asked him how he determined the needs of each Division. He said that the budget preparation was based on the previous year's actual expenditure, and percentage increases thereon were proposed for each budget head. The budget proposals had no relationship to what the lab should be doing.

Incidentally, one year, the CSIR staff came to NAL for budget negotiations. They followed the same procedure of percentage increase of each budget head as compared to the previous year. During lunch, they asked me if I was satisfied with the budget allocations. I told them that I was, and in turn asked them how they knew whether the budget allocations were appropriate. They said that they went by the previous year's actual expenditures. I then asked them whether on that basis, if I got large allocations for the previous year, I could get equally large allocations for the following year also. They kept quiet. I decided to revise the whole budget preparation procedure and make the budget preparation project specific to build accountability, and make it a direct responsibility of the Project Monitoring and Evaluation (PME) section in the Director's office.

Not surprisingly, the Aerodynamics Division turned out to be the most expensive Division. Incidentally, this fact made me approach the then Secretary, Defence Production, Mr Harish Sareen and prevail upon him to consider the 4 ft tunnel as a national facility, and share the cost of running it. When I showed how I was going to do it, he told me that I was pushing on a door that was already open, and he readily agreed to it, seeing the manner in which our accounting procedures reflected the actual costs of operation. The primary users of the tunnel shared the fixed costs equally and the variable costs were shared as per actual extent of usage. A committee of user representatives was responsible for approving the budget for the facility and laying down policies for its operation.

I introduced the new system of account keeping and associated budget preparation in April 1966 itself. It was an instant success. In each division, every project that was proposed was examined in detail, including the expected end results, which projects were going to be closed, either because they were completed, or because no useful purpose would be served by continuing them. The scientists were particularly happy, because they could spend the money at their discretion, within the limits of budget allocation to their project. Monthly statements of their expenditures were being sent to them. They became very careful in spending, since any expenditure they incurred was debited to their project budget head. They were no longer spending NAL money. In essence, they were spending their own money! An example will illustrate this. One day, a senior scientist came to my office and said that he saw no point in asking the Graphic Arts section to develop some films for him, as he could get it done outside for far less. When I looked into the matter, I found that the charge for development was being based on the number of films developed each day: the less the number, the more the cost, as it was considered an earning center. Of course it did not make sense. It was subsequently changed and brought under general overheads.

These procedures formed the basis for recasting the project-based budget to the format required by the CSIR. Budget preparation became the responsibility of the Director's office, and not that of the accounts section. After the allocation of the funds, the division heads were made accountable for delivering the research results from their division's sanctioned projects, and the accounts section had the responsibility of keeping accounts project-wise. PME in the Director's office kept track of the expenditure on each project and kept the concerned scientist, the Head of the Division and the Director informed of

these matters on a monthly basis. In addition, my office was receiving half-yearly and annual scientific progress reports for each project. Incidentally, these reports formed the basis of the annual audit by the staff of the Controller and Auditor General (CAG).

When I hosted the Directors' conference in July 1966, I explained to Dr Zaheer, the DGSIR, the restructuring of the research programs and the accounting to build accountability in the functioning of NAL. He was very impressed and desired that copies of the NAL budget document should be sent to the Directors of all the CSIR labs. The Directors were not at all keen to implement this procedure, as it demands accountability, something that they did not want. About 20 years later, following an audit note by the Controller and Auditor General (CAG) to the Parliament, CSIR tried to introduce such a system in all the CSIR labs. Apparently CAG asked the CSIR why its other laboratories were not being run along the lines of NAL. Around1968-1969, the Public Accounts Committee of Parliament under the chairmanship of Mr A B Vajpayee visited NAL. It too was impressed and apparently informed the Secretary General of the Lok Sabha. He too made it a point to visit NAL shortly thereafter to understand how NAL administration was structured. The basic principle used in restructuring the administration was that when you spend public money, you are a trustee for public good, and that you should not betray this trust, implicitly or explicitly, while running the administration. In fact, when the annual audit party visits were taking place, I used to ask them to tell me what was wrong in the functioning of the lab and not what was right. They used to tell me that I was the only person who made such a request. I needed to know what was wrong in the manner in which the lab was functioning. They were always helpful.

In this system, the budget document provided information under various budget heads as desired by CSIR, and specifically indicated how it was proposed to spend the funds on the various projects. Instead of sending the accounts officer to submit and defend the budget, I made it a point to go to the CSIR every year to defend it, specifically indicating why various projects were being taken up. I never had any trouble in defending the budget. Furthermore, after the budget was approved, I used to instruct the purchase section to order capital equipment approved in the budget, and keep the bills ready for payment by 31st March of the year. As many of the labs did not follow this practice at that time, and CSIR was reluctant to surrender funds at the end of the year, we invariably got more money from CSIR to pay the bills before 31st March of every year. Forward planning helps! In this connection, I must place on record the tremendous and unstinted support of Mr Rahman, the then Chief of Planning in the CSIR headquarters, and the implicit trust he reposed in NAL while determining the annual budget allocations of NAL. Without his support, much of what was achieved in NAL during its formative years would not have been possible.

THE SPECIAL POSITION OF THE PME - DISTINCTION BETWEEN LINE AND STAFF RESPONSIBILITIES

In running institutions, there are two distinct types of delineation of responsibilities, the so-called line and staff responsibilities. For example, the day-to-day running of the lab through

administration, accounts, stores and purchase, etc follows strictly defined procedures, and the persons in charge of such responsibilities are obliged to follow specifically laid down rules and regulations. They have no discretionary authority. It is a staff responsibility. However, the divisional heads, who are responsible for running their divisions and keeping track of the R&D in their divisions, have some leeway, with a specific general budget allocation also, to be spent in their divisions at their discretion. But they are responsible for monitoring the R&D programs assigned to their divisions, each project with its own specific budget allocations. They are responsible for monitoring the progress of research in their divisions, which requires some expertise, and are held responsible for delivering results. This is apart from carrying on the research assigned to them. This is line responsibility.

In this system, PME has only a staff responsibility for keeping track of the expenditure details of each project, division and for the whole lab, and reconciling them with the traditional CSIR monthly account statements. The staff officer managing the PME section had no authority whatsoever to allocate funds at his discretion, without the specific knowledge and approval of the Director. Mr S C Narayana Murthy as Head of the PME section at that time, with his background of the Institute of Cost and Works Accountants (ICWA), always took my specific approval for transferring funds from one head to the other, or from one project to another. This self-effacing person did a brilliant job of organizing the PME section in the Director's office. The result of this restructuring was delegation of authority to the scientists in charge of projects with inbuilt accountability. Mr L Ramanathan, who was earlier in the CSIR headquarters, handled the general administration of NAL for most of the time I was Director. He was also an excellent and self-effacing administrator, on whom I could depend implicitly to run a clean and efficient administration.

There is a very important reason for making the PME section a part of the Director's office. The Director is ultimately responsible for all expenditure, and this authority cannot be delegated, except under specific written orders, or as per strictly laid down rules. The PME used to send the statement of monthly accounts to each project scientist, itemizing in detail the expenditure on salaries of staff deployed on the project (or percentage thereof, if they were working on more than one project), overheads, cost of consumables, capital expenditure, commitments for acquiring capital equipment, expenditure till the preceding month, proportionate expenditure, etc. It was a comprehensive monthly statement. Copies were sent to the division heads also. As stated already, the project scientists, in turn, were obliged to submit half-yearly and yearly research progress reports. Annual meetings were held and the project scientists were obliged to make presentations to the invited scientists. Accountability was built into the system. Once, the Head of a Division complained that he did not know how much money the scientists in his division were spending. He did not realize that this information was being routinely sent to him every month by the PME! His secretary did not brief him.

I was informed that in recent times, PME is a Division with authority implicitly vested with it to reallocate funds at its discretion, and not necessarily with the approval of the Director. The designation of Division implicitly provides authority to transfer funds from one head to another, or from one project to another, or from one Division to another, and not necessarily with the knowledge of the Director. In principle, whoever controls the budget and authorizes spending really runs the lab. This is a matter of serious concern in administering a lab. The authority vested in the administration for expenditure of funds is explicitly determined by rules and regulations, which cannot be violated without the Director's knowledge. Even he has to obtain approval on occasion from CSIR, if he wishes to exceed the authority explicitly vested with him. It is to be noted that the discretionary authority presently being exercised by the PME Division rests squarely with the Director, and it cannot be exercised by PME if the Director wishes to exercise control over what goes on in the lab. He will be responsible for all decisions taken by PME with or without his knowledge. Assigning such responsibility to PME amounts to one Division sitting in judgment on another, which is administratively inappropriate. There seems to be some confusion in the designations currently being used, without a clear distinction between line responsibilities and staff responsibilities and between divisional responsibilities and secretarial staff responsibilities. This needs to be looked into, if the Director wishes to have the last word in such matters, for these decisions decisively influence the course of R&D. Designation of groups of staff rendering purely staff responsibilities as Divisions raises basic questions.

BASIC ISSUES FACED BY NAL

I learned an important lesson in running the lab. You cannot win if the policy-makers are not aware of the implications of their decisions. Time and again, I faced this situation. It is not that the Indian scientific community is incompetent. It is that many a time, the policy-makers sitting in Delhi are unaware of the implications of their decisions. By and large, they were comfortable with licensed production in the public sector. It did not demand any accountability from them. Limping from licensed production to licensed production became a way of life for the Indian aircraft industry and many other public sector industries, too.

Early on, I realized that to build the credibility of NAL among its potential users, we should not let our customers lose their shirts. If need be, we should be willing to lose ours. A situation soon developed to test this principle. The Instrumentation Division supplied a transducer amplifier to IIT Mumbai. Shortly thereafter, I got a letter from its Director, that our instrument was no good. I sent a person from this division by air to find out what was wrong. Apparently a soldered terminal became disconnected in shipment, and was soon corrected. This expenditure paid back several fold subsequently. We established the reputation that we would not let our customers down. A similar situation developed with a radome supplied by us to the Air Force to protect the Patnitop radar, with a similar comment by the then Air Chief.

Once, hearing about our capability in developing transducers, the Maruti Company newly founded by Sanjay Gandhi wrote to us before it went into collaboration with Suzuki of Japan, that we should supply them transducers. I wrote back to say that transducers were made to respond to specific requirements, and that we would be glad to consider their development if they could give us the specifications. I never heard from them again. When I related this incident to the then Vice President of CSIR, I was told to be careful in responding to such requests.

We also learned another important lesson in dealing with our potential customers. The Instrumentation Division developed foil strain gages. The Head of the Division was anxious to transfer the technology. I suggested that he should make about 5000 of them and get a feedback from our customers, before we transferred the technology. We did get feedback which helped to make improvements. But the company to whom we transferred the technology did not maintain the standards of quality in manufacture, and folded. There were other instances also where we were successful in development of technologies. But what works successfully in the laboratory does not necessarily work on the production line without considerable additional effort.

All decisions setting precedents or having policy implications were discussed in depth at frequent senior staff committee meetings to which not only the Heads of Divisions but S&T staff at E1 level, who were not many at that time, were also invited. It became the unwritten responsibility of the divisions to implement the decisions taken at these meetings. The decisions were not always wise. When wrong decisions were brought to the attention of the committee, it reversed them.

Sometimes the arguments tended to be acrimonious. But that was the price to be paid. I recall the discussions preceding the acquisition of the Sperry Univac computer costing about Rs 2 crores (Rs 20 million). The alternative offer was for a CDC computer, which was apparently more powerful. Dr Shankar and Dr Yajnik strongly argued for it. The Structures Division argued that we should order the Sperry unit, as the offer also included the AD 380, the Interactive Graphic Terminal. Based on the majority opinion, we ordered the Sperry unit. The AD 380 unit was never used. We should have obtained the CDC computer to better serve our requirements. Majority opinions were not always correct. Drs Shankar and Yajnik were correct. I found it unwise to ignore the advice of experienced persons who were familiar with the implications. I became weary of staff members who always agreed with me. Nobody is ever so infallible. I have learned not to take their views seriously, but I respect those who had the courage to differ with me. When the Director takes a decision, it may well have far-reaching implications, and it is essential for him to be surrounded by people who will not hesitate to express their views. The final responsibility for the success of the laboratory rests with the Director, and ultimately, he cannot blame anybody else for his wrong decisions.

I made it a point to cut across the hierarchical structure and interact with all staff, getting to know them by their names, more often than not. They were happy that they were not just

faces, but that the Director knew them by name. In such an environment, they felt free to talk to me about their problems. So to say, the iron curtain in front of my office parted a little. But all administrative decisions were handled through the Heads of Divisions and senior administrative staff only. This way, only these people directly reported to me and it was only these people who handled all administrative matters. There was nobody between them and me in running the lab. At the time I was about to retire, I wanted to completely computerize all administration and accounting procedures, including the stores and purchase sections. I do not know if it was done after my retirement. I was benefited in taking these decisions by a course I took in Caltech on industrial management and business administration, while doing my master's degree and by study of a book titled *Organization of Scientific Research* by Sir John Cockroft. I understand that after I retired, my successors subdivided the divisions with independent responsibilities assigned to them and by implication having direct access for discussing policy matters. If they did not wish to take responsibility, they would inevitably seek access to the Director and would wait for his decision. My experience has been that it is not wise to have too many people (no more than 8 to 10 people) directly reporting to the chief executive.

In spite of its excellent infrastructure and first-rate scientific staff, NAL faced some serious problems. Essentially its clients were only two, and if they did not seek its help, NAL was lost. In a sense, its excellence became its own enemy. While the then nascent Department of Space was unhesitatingly taking assistance from NAL in its areas of competence, the same could not be said of Defence, NAL's principal client. The Aeronautical Development Establishment (ADE) resented NAL's popularity and its equation with the Air Force. For example, NAL made a 500-channel strain data-logging recorder, which could be of use in DRDO labs also. NAL's offer to supply it was rejected, with the argument that they could develop one by themselves, never mind even if it was available off the shelf, if it was an Indian shelf.

Another example was the responsibility given to NAL for determining fatigue life of Gnat fighter aircraft by Air Marshal Malse, then heading the Maintenance Command. During a visit to NAL, he saw the programmed loading fatigue testing machine built by Dr K N Raju for fatigue testing of sheet specimens. Air Marshal Malse said that they were having problems with the Gnat aircraft and did not know how much useful life it had, and asked whether we could be of any help. I was waiting for such an opportunity to establish an equation with the Air Force. ADE proposed that they would monitor the program on behalf of the Air Force. In good faith we readily agreed. Instead of handling this task, the ADE representative wanted to know how the facility was designed in NAL! ADE was more concerned about learning how to build such a facility. The quotation we received from abroad to supply it was about Rs 25 lakhs (Rs 2.5 million), whereas Dr Raju built it for less than Rs 10 lakhs (Rs 1 million). Put plainly, ADE was keen to build a facility of its own and squeeze NAL out. Subsequently, Dr Raju took on the responsibility for fatigue testing of the MIG 21 aircraft, at the request of the then Chief of Air Staff and built a much more sophisticated computer-controlled fatigue test facility which was later substantially modified by Dr Sunder.

Recognizing the importance of establishing an equation with Defence at the highest level, I approached Dr Nayudamma, the then DGSIR, and proposed to him that while NAL would continue to be a part of CSIR and receive its budgetary allocations through it, we should involve the SA to the Minister of Defence, Dr Nag Choudhuri, to approve the coordinated R&D programs of ADE, GTRE and NAL and the associated plan allocations. He readily agreed and called for a meeting. Nothing came out of it, with the Director, ADE opposing the suggestion. Incidentally, shortly after this incident, Dr Nag Choudhuri wanted to propose my name as his potential successor. I declined, with the conviction that others could handle the position of SA and that I should concentrate on obtaining a measure of self-reliance in aeronautics.

Prof M G K Menon succeeded Dr Nag Choudhuri as the SA. I took up the matter again with him and told him that aeronautics was too important for squabbles and proposed that he should clearly delineate responsibilities among NAL, ADE and GTRE, and that I would strictly comply with his guidelines. I took prior approval of Dr Nayudamma, before I made this proposal. As mentioned already, I explained in a comprehensive letter to Dr Nayudamma my vision for aeronautics and the role to be played by NAL. Prof Menon called for a meeting in his office on 11th May 1976. Present at the meeting apart from him were Dr Satish Dhawan, Mr Vivek Sinha, the then Director of Aeronautics in DRDO, Air Cmdre. H N Krishnamurthy, then Director of ADE, and myself. Director, ADE spoke first. I was astounded by his views. He said that NAL scientists were a bunch of theoreticians and had no business to poke their noses into defence matters, and they should be restricted to doing some purely theoretical work and writing papers. Prof Menon promptly rejected his suggestion and delineated the responsibilities. He proposed that NAL should primarily concentrate on R&D related to aircraft structures, ie aerodynamics, structures and materials, play a supporting role in propulsion to GTRE, and give up its work on simulators while ADE should take responsibility for developing simulators and aeronautical systems. NAL religiously followed this delineation of responsibilities during my time as Director. Shortly thereafter, Air Chief Marshal Latif personally wanted NAL to take over the responsibility for development of simulators. I declined stating that I would not like to violate the guidelines laid down by the SA. I was pleasantly surprised when he said aeronautics was safe in my hands. It was an uneasy truce between NAL and ADE, although with GTRE there were no problems. But the basic issue was to make the functioning of NAL, ADE and GTRE purposeful. I realized that this has to be done through persuasion and not by order. Easier said than done.

In 1971, Dr Dhawan left to spend a year on sabbatical at Caltech, with the expectation, as he told me, of giving up the directorship of IISc, and returning to active academic research in the aero department. But shortly after Dr Sarabhai died, Mrs Gandhi asked him, while he was still in Caltech, to take over the responsibility for space research and the design and development of the Space Launch Vehicle SLV3 in the Department of Space, shortly to be created. Till then, it was being supported by DAE. I asked him if he had given up the fight for building self-reliance in aeronautics. He threw up his hands and said that the powers that be, sitting in Delhi, were unwilling to support this cause. Like it or not, as the only person at that time who commanded a measure of confidence among the academic and

research community, the industry, and the Air Force as well, it became my lot to strive for it. Being the Director of NAL made this objective my first cause in my professional career. It was mainly for this purpose that I returned home. I regret to say that I did not succeed in my efforts to integrate R&D and the industry in aeronautics by creating an Aeronautics Commission served by the Department of Aeronautics in the Ministry of Defence. More about this, later.

Incidentally, shortly after taking over as the Secretary, Department of Space, Dr Dhawan invited Dr Narasimha, Mr Raj Mahindra and me one evening for an informal chat and proposed that one of us might wish to consider heading the Vikram Sarabhai Space Center. Each of us had his own reasons for declining the offer. My relationship with Dr Dhawan through our wives and its implication in handling the VSSC administration haunted me.

RAJA RAMANNA AND NAL

The problems between ADE and NAL did not really end after this broad delineation of responsibilities by Dr Menon. After Dr Raja Ramanna succeeded Dr Menon, apparently the issue was taken up with him again, with the suggestion that DRDO should create a separate laboratory for handling the tasks NAL was undertaking. When Dr Ramanna related this in casual conversation to a professor from the aero department of IIT Kanpur, he apparently suggested to Dr Ramanna that he should visit NAL before he made up his mind. While I was on vacation end of May 1979, I got a call that Dr Ramanna was proposing to visit on 31st May. I immediately returned to receive him and show him around NAL. Earlier, he had visited NAL along with Sethna, then Secretary, Department of Atomic Energy, shortly after Dr Sarabhai died, to close a research program NAL was handling for DAE. Dr Ramanna spent the whole day in NAL and he even desired that the program that was closed could perhaps be revived. I declined. He subsequently told an academic, “All these days, I used to think BARC is the best engineering laboratory in the country. I changed my mind after I visited NAL”. But what BARC has and NAL did not have is the assurance that the R&D programs taken up by it would necessarily make an input downstream for some other programs. This is a luxury that NAL did not have. Dr Ramanna proposed my name to the PM as his successor when he went back to BARC. But my priorities were different. It did not mean much to me to be called the Scientific Adviser to the Minister of Defence. As mentioned already, I wanted to try to integrate all aeronautical activities under one authority along the lines of Space and Atomic Energy. To my regret I found that it was easier to create a new department of the government than restructure existing ones. Looking back, I think I made a mistake in rejecting the offer.

HONOURS

In the last Indian Academy of Sciences meeting presided over by Prof C V Raman, I was invited to speak on crack propagation. While I was speaking, Prof Raman suddenly asked me how to define a crack. I went back to fundamentals and explained it, starting from the pile up dislocations leading to crack formation at lattice structure level and its subsequent growth. Dr Ramaseshan told me that following this, Prof Raman felt that I should be made

a Fellow of the Academy. The actual election came after he passed away. I did not do any scientific work in India that really qualified me for the fellowship of the Academy. But I was happy that Prof Raman thought that I was good enough to be elected.

Election as Distinguished Alumnus of Benares and IISc I took in my stride. Early 1974, I got a telephone call from Dr Nayudamma, DGSIR, asking me whether I would be willing to accept the Padma Shri**.** It came as a complete surprise. I did not know the basis on which these awards were determined. I agreed to it, but like so many others who received such awards, I built NAL as a matter of duty and never sought any honors.

I was given the Vasvik award in 1978 and was subsequently invited to be a member of the awards committee. I was surprised when I received for approval, the nomination of Dr Manju Sharma for her contributions to mechanical sciences. She was at that time in the Department of Science and Technology and had not been in the corridors of a research laboratory for the previous two decades or so. She was essentially working as a staff officer for Dr M G K Menon, then Secretary, DST. I used to see her regularly in the meetings of the Science Advisory Committee to the Cabinet. When I asked the Vasvik committee on what basis they had put up her name for approval, they wrote back to say that they had no choice in the matter as the committee Chairman, Dr Menon, had nominated her. Seeing the absurdity of the situation, I resigned from the awards committee.

It occurred to me that such awards are frequently determined by whom you know and their perceptions. I was embarrassed to know that some persons actively seek and canvass for such awards. For example, as the founder-president of the IISc Alumni Association, I created the Distinguished Alumnus Award to honor former students of the IISc who have made distinguished contributions. I became aware of an instance when a scientist who was not a student of the institute apparently wanted to know why he was not given the award. The Association Council apparently recommended him for the award and the then Director approved it! When I asked him why he had approved it knowing full well that the proposed person was not a student, he had no real answer! Then I found that this was not an isolated instance and that many people who were never students of the IISc had been given the awards, thus distorting the very purpose of these awards. I am given to understand that the policy has been changed since then, as a result of my letter to the then Director of the IISc. Ethics in the practice and management of science have been a lifelong concern for me. But I regret to say that these are not taken seriously in our country and are violated with impunity even by reputed scientists and scientific institutions. The names of Directors and senior scientists are routinely included as coauthors of publications of which they knew little or nothing. Is it any wonder that science does not thrive and science-based technologies have not taken a solid foothold in our country?

NALTECH

Incidentally, experience elsewhere in the world indicates that being a high-science, hightechnology industry, aeronautical activity has resulted in many technological spin-offs. Robert Solow, the Nobel Laureate in economics, specifically drew attention to this fact.

The Charter of NAL, following the Sarkar Committee recommendations, specifically stated that NAL shall not forget such potential spin-off benefits and should make efforts to put them to use. Based on this, Dr Narasimha, my successor, created NALTECH, a registered society, primarily as an agency to put to use knowledge gained by NAL in nonaerospace applications. Except for Surface Engineering Group activities, NALTECH was not very successful in transferring any significant technologies developed by it to nonaerospace applications. To be effective, the scientists must be actively conscious of the potential non-aerospace implications of the technologies they are developing in the first instance to serve the cause of aeronautics. It would seem that they are not. This would also imply encouraging patent culture, which was not actively looked into. NALTECH over the years distorted its perspective and is now being used routinely to get around government restrictions on recruitment of staff. With the Light Transport Aircraft development demanding additional staff and restriction on additional recruitment by NAL, NALTECH became an agency to hire staff and deploy them on LTA work. One would wish that NAL/NALTECH explored more actively the inherent potential of NAL in helping non-aerospace areas.

NEED FOR INTEGRATION

About 14 years of experience in the US, mostly at Caltech, association with the aircraft industry and about 18.5 years at NAL have been, by and large, a fulfilling experience. I got an opportunity to virtually build from scratch what was generally felt to be a first-rate applied research laboratory in the country, on the excellent foundations laid by my predecessor, Dr Nilakantan. But I did not really succeed in my first cause, to integrate R&D and the industry in aeronautics under one authority. This is essential, if ever we want to obtain a measure of self-reliance in this all-important field, which is so crucial for our national defence. It has been suggested that the 1971 war between India and Pakistan was determined more by Russia and the US, as they knew to the last bolt and nut our spares, and that they could turn off the tap of supplies any time. It may not be true, but there is considerable substance to this conjecture. For example, we could not achieve the design potential of HF24, as it did not have a sufficiently powerful engine. Efforts by HAL to obtain the Rolls Royce RB199 engine as a power plant for it were not fruitful, as by that time Jaguar was in production and the UK wanted to sell it to us. Studies indicated that we would not need the Jaguar, if we got the RB 199 engine to power the aerodynamically clean HF24. It is in the larger interests of the country to set aside parochial considerations and integrate R&D and the industry in this field crucial to national defence.

Years of involvement in NAL and committee work towards shaping science and technology policies in the country made me realize that good scientists are not necessarily good administrators of science and technology institutions. But it is essential to have the relevant science and technology background to successfully run such institutions. Even the best of general administrators cannot run S&T institutions, unless they have an intrinsic appreciation of the S&T aspects of the institutions they will be running. With due apologies, IAS officers, excellent as general administrators, are out of their depth in running such institutions, as they just do not have the background to appreciate the implications of the

decisions they make. Even good scientists who have no background in the particular field are no exception. More about this, later.

It would be instructive to recall in this connection, a perceptive comment made by Praxi Fernandes, an IAS officer and the then Finance Secretary, at a meeting of the Governing Body of CSIR in 1978. He said that before Independence, the ICS had four major responsibilities, and they did very well in administering them. They were defence, finance, external affairs and home. Come Independence, the nation faced additional responsibilities. It required economists to do economic planning and scientists and technologists to implement them. These are specialized fields, requiring understanding in depth the scope of their activities and the implications of any decisions that may be taken. General administrators just do not have such experience. Fernandes went on to conclude that the IAS, which succeeded the ICS, was not willing to share this responsibility with the specialists at the highest levels of government. They felt that they were the only people who had the right of direct access to the ministers. Even those departments of the government which were headed by scientists when they were first created, were taken over by the IAS. Many of the problems now faced by the country in its transition from a developing nation to a developed nation could be attributed to this, as our ministers are not knowledgeable or professionally competent in these complex areas, and depend upon the people who advise them. The fact of the matter is that if you do not have the background, you do not necessarily understand the implications of your decisions. I have seen this happening time and again. In the following pages I would like to relate my experiences in this regard through involvement in committees.

This section would be incomplete, if I did not relate an event that took place while I was doing my master's degree in Caltech. It made a deep impression on me. In the course I took on Industrial Management and Business Administration, we were required to study case histories from the Harvard Business School. One of them was concerned with the functioning of the CEO of a company. He was most helpful to his staff and was everywhere, giving a helping hand to all. But the company was losing business. The assignment was to analyze what was going wrong. I had put in considerable effort to study the problem but the teacher gave me a C- for my case study. My teacher told me that my recommendation was worth only that much to the company. He said that the CEO was incompetent to run the company and to cover his incompetence he was getting involved in matters that were not his business, and so he should be sacked. The CEO's responsibility was to plan the overall programs, delegate authority with accountability, and keep track of the overall progress. In principle, if an employee was not living up to his sphere of responsibility and accountability, he should be sacked. Since in a government organization one cannot sack anybody for professional incompetence, one has to be very careful in recruiting and grooming the staff, and not hesitate to shift or ease out people who are not doing well. This I did many times. This also meant keeping close track of the R&D contributions of the scientific/technical staff, through the PME office, which reported directly to me, with no scientist in between. Narayana Murthy, who was heading PME at that time, did an excellent job. The fact is that this responsibility could not be delegated to any other scientist, if I wanted to have a grip on what was going on. I delegated authority with accountability and

responsibility to the S&T staff, while keeping a close tab on the R&D progress through the PME, with its monthly financial statements on each project, detailed review through the annual and half-yearly progress reports and frequent annual presentations by the staff to experts, and of course, zero-base budgeting. As a result, no project scientist or division Head could take anything for granted. It was this approach to management that gave me ample time to take up groundbreaking work that led to the eventual launching of the LCA program and Cabinet approval for it. It is a pity that my successors did away with this system, which could have helped them tremendously in shaping the destinies of NAL in a purposeful manner.

Chapter 3

THE LCA PROGRAM
&
THE ROLE OF THE AERONAUTICAL DEVELOPMENT AGENCY

It is said that those who can't remember their past will be condemned to repeat it. This is true in the field of aeronautics during the last sixty years or so in our country. India's involvement in aeronautics started in a modest manner when what is now Hindustan Aeronautics Limited (HAL) was transformed from being a repair base for the South-East Asia Command to an aircraft manufacturing center after the war. It was to respond to the needs of the Indian Air Force and functioned mainly as a production agency under license. Its work in converting the old DC3 into civilian aircraft gave an initial impetus to starting a number of commercial airlines, which were soon integrated into what is now the Indian Airlines. The earliest attempt to develop indigenously an aircraft in HAL was the HT2 designed by Dr V M Ghatge, then Chief Designer. No serious attempt was made to indigenously develop any military aircraft, till the government decided to invite Kurt Tank and his German team. The result was the HF24, an aerodynamically clean aircraft designed for supersonic performance. It never attained its design performance due to the inadequate power of its two Orpheus 703 engines. The aircraft was designed around one Bristol Siddeley BOR12 engine, yet to be developed. It was a reheat variant of the Orpheus 703 engine being produced under license to power the Gnat and later the derated version to power the HJT16 developed by Raj Mahindra. This was a program initiated by Dr V M Ghatge. Raj did his studies in Imperial College in London and worked for some years at De Havilland and Napier Aircraft companies before returning to India.

Bristol Siddeley hoped to sell this reheat variant to the NATO countries also. Apparently they were not successful. They approached Mr V K Krishna Menon, then Defence Minister, and apparently offered to develop it for use in HF24 and asked for Rs 5 crores (Rs 50 million) for development. The Minister rejected it. Looking back, it was a fatal error in judgment, for the HF24 never achieved its designed supersonic performance. It was a classic example of decision making by people who did not understand the implications of their decisions. This was repeated time and again by people in the South Block.

GTRE, then a fledgling research laboratory set up by DRDO under AVM Roy Choudhuri as its Director, took on the responsibility for developing the afterburner to fit it to the Orpheus 703. He demonstrated successfully its design performance of 20% increase in thrust on the test bed when operating in the reheat mode. HAL had the responsibility to suitably modify the HF24 to accommodate the afterburner. Instead of redesigning the fuselage to accommodate the afterburner to reduce any potential increase in drag to a minimum, S C Das, then in charge of the program, decided to fit the afterburner with a sudden increase in the aft end area of the fuselage. This resulted in an increase in drag, which substantially washed out the increase in thrust obtained by the afterburner. HF24

reheat version was closed shortly thereafter when Suranjan Das, the Chief Test Pilot of HAL, died in a test flight of the HF24 with the afterburner. Apparently, its cockpit canopy suddenly opened as he was taking off. Licensed production of other military aircraft continued in HAL.

This was the situation when Mrs Gandhi constituted the Aeronautics Committee in 19691970, under the chairmanship of Mr C Subramanyam, former Cabinet Minister at the Center, to determine what had gone wrong in the aeronautical industry, and sought recommendations for changes, with a view to achieving some self-reliance. The Committee constituted a number of sub-committees to study various aspects of the problem. I was nominated as the chairman of Study Group No. 5, to look into the issues pertaining to research and development. Thus started my involvement in matters relating to problems of policy that affected the cause of R&D in general in the country, and aeronautics in particular.

I sought an appointment with Mr C Subramanyam, and asked him how I should proceed. He told me to bring out facts. I told him that facts might hurt some people. He said that it did not matter, and he needed the facts to make appropriate recommendations. As could be expected, most of the time of the committee was taken up with the problems related to the development of HF24. Then started my problems.

I got a telephone call from AVM P C Lal (subsequently Chief of Air Staff), then heading HAL. He desired that I should not pay too much attention to this problem. I told him about the instructions from the chairman. I got a similar call from Dr Ghatge with a similar answer from me. Then the secretary to the Committee, Mr S Krishnaswamy, at that time a joint secretary in the Department of Defence Production, attended one of the meetings, and asked me halfway through, whether we should examine the problems in such detail. I asked how else we could establish facts. He never came back again. I requested Dr R Narasimha and Raj Mahindra to determine the increase in drag due to changes in the design of the aft end. They studied the problem in some depth. In essence, they established that the sudden change in the aft end increased the frontal area of the fuselage, which essentially washed out the 20% increase in thrust from reheat.

Mr Subramanyam then called me to make a presentation of the findings to his committee. I reported that our studies established that the increase in thrust due to the afterburner demonstrated on the test bed was washed out due to the mutilation of the aft end of the fuselage. AVM Lal was unhappy, and stated that his staff gave a different estimate. The chairman then asked him how else facts could be established, if not from the findings of an independent committee.

The Aeronautics Committee in its report recommended that HAL should take up the development of a combat aircraft, a short take-off and landing (STOL) aircraft and a helicopter. It took ten years for the government to sanction the development of a combat aircraft. The STOL never saw the light of day. The helicopter development was sanctioned soon after, with Mahindra handling the program in its initial phases. The Air Force released

a number of Air Staff Requirements over the years. Feasibility studies were conducted by HAL on all of them, with extensive wind tunnel tests in NAL to optimize the aerodynamic configurations. Nothing came out of them, and licensed production of aircraft developed and manufactured abroad became more a rule than an exception. The Air Force was comfortable with the situation. They lacked confidence in the ability of HAL to successfully see through the design and development of a fighter aircraft within the country. The bureaucrats too were satisfied, because then they did not have to answer any Parliament questions, as they merely approved what the Air Force wanted. Meanwhile, the research labs were busy conducting "forward-looking research" and they did not have to respond to any problems generated by the industry. The industry did not have any, as they were using "proven technology", even when it was obsolete in the country of its origin. The academic institutions were taking up research inspired by a study of scientific papers in foreign journals. As stated earlier, the loop of knowledge was not closed, each going its own way, and none interacting with the other. In essence the situation was pathetic as far as the cause of self-reliance in aeronautics was concerned.

During the time of Dr Nag Choudhuri as SA and Dr Satish Dhawan as the executive delegate from India for the Commonwealth Advisory Aeronautical Research Council (CAARC), I was nominated as the coordinator for research in materials among the CAARC countries, and subsequently took over from Dr Dhawan as the executive delegate from India. It is to be noted that among the member countries, UK and Canada were members of the Advisory Group for Aeronautical Research and Development (AGARD), an active body under NATO to coordinate R&D among NATO countries, and their involvement in CAARC was less than enthusiastic. Research programs were being conceived, obeying Parkinson's Law to justify the existence of CAARC. It provided opportunities for travels abroad for the delegates, and not much else. The programs conceived were by and large trivial. It became clear that CAARC was not serving any useful purpose. It was wound up.

I continued to feel that unless there was coordination of research efforts with a view to responding to the long-term projected requirements of the aircraft industry, there would be no change in the then prevailing situation. As mentioned already, with the permission of the then DGSIR Dr Nayudamma, I approached the then SA to the Minister of Defence, Dr B D Nag Choudhuri, who succeeded Dr S Bhagavantam as the SA, and proposed that NAL, ADE and GTRE should coordinate their efforts to establish a strong R&D base in aeronautics in the country. As stated already, the idea was that in our respective fields, we needed to ask some fundamental questions: Where are we now? Where do we want to go? Why do we want to get there? And finally, how do we want to get there?

Answers to these questions were essential, if the R&D institutions were to contribute in a meaningful manner to aeronautics, with its long gestation periods for the development of successful technologies. For example, if the Air Force today issues an ASR for the development of a combat aircraft required ten years from now, to respond to the threat scenarios likely to be faced at that time, the R&D institutions should have already developed the technologies needed to build such an aircraft. In other words, these institutions need to have at least a twenty-year perspective to plan their research programs.

The output of these institutions would then become a desirable input downstream to the corporate R&D, whose output in turn would have to become an input to the design bureau of HAL. ADE rejected the idea of coordination of effort. Not only that, the governing councils that existed then to oversee the functioning of the DRDO labs were subsequently abolished. DRDO itself had no clue to the kind of forwardlooking R&D that its labs working in the field of aeronautics should be taking up. Put plainly, there was jealousy among the research labs, and no coordination among them for the common good of aeronautics. Instances were known where research labs were duplicating their efforts, instead of reaping the benefit of mutual interaction.

When Dr Nag Choudhuri laid down office as the SA, Prof M G K Menon, the distinguished physicist from TIFR, succeeded him. The situation in the field of aeronautics did not change. What it required was a deep understanding about what was needed, and steering these R&D institutions and the corporate R&D towards that objective. Good as the scientific advisers were, they did not understand what needed to be done to build a strong aeronautical base. They continued to fund the institutions based on previous years' actual expenditures, and merely supported the research programs proposed by these institutions. They did not have any long-term goals of their own to direct the activities of these institutions. The situation in NAL was basically no different. To the extent that it was not vertically integrated with a specific user agency, its R&D programs also lacked potential user focus. Its headquarters knew even less about its research programs. But it was generally acclaimed as the best engineering laboratory in the country for the quality of its research. As Dr Dhawan put it, "NAL is a beautiful bride, all decked up, and nowhere to go". It was not a master of its destiny.

This meant helping HAL wherever we could in the feasibility studies of the ASRs and taking up forward-looking research and technology development, in the fields related to aircraft structures. As far as aerodynamics was concerned, we were quite well up. I felt that we needed to concentrate on composite technology development, and in particular how to handle it analytically, and how to design and build composite structures. As carbon fibre was expensive, we learned these principles and techniques using glass fibre. As stated already, NAL later was given contracts for developing many structural components based on carbon fibre composite technology after the LCA program was launched, to make us less dependent on others for them.

GTRE continued to study the design and development of gas turbines. It did not get as much support as it required for making any significant progress, although AVM Roy Choudhuri, the Director, was highly committed to developing gas turbines. As a last resort, he apparently subdivided his research programs for engine development, and sought separate funds for R&D related to the compressor, combustion chamber and turbine, etc. Having been a member of the GTRE governing council for some time, I appreciated the problems he was facing. What he had hoped to do was some day to integrate knowledge obtained by a study of these various subsystems to develop the experimental gas turbine GTX35. Had the office of the SA been aware of the complexities in engine development and the importance of such engines for fighter aircraft development, it would certainly have

given full support to GTRE. Tragically, that was not the case. In fact, as it turned out later on, Dr Raja Ramanna as the SA eased him out of his position instead of giving him an extension. It was a tragic error of judgment. The fact is that no matter how good your aircraft designers are, if you do not have a suitable engine, you cannot develop your own aircraft. Our potential foreign suppliers held us to ransom by denying us engines that would have improved the performance of HF24. They wanted to sell their aircraft to us, and they did so, subsequently.

The Aeronautical Development Establishment (ADE) was in an entirely different situation. Its activities were spread out, with no clear focal point. They were even designing passenger seats for commercial aircraft. Although the SA specifically assigned the responsibility for aeronautical systems to them, they did not seem to have paid much attention to studying the problems of relaxed static stability and the fly-by-wire control system development. Neither did the office of the SA take the initiative to ask ADE to take up R&D in FBW control systems, its assigned area of responsibility. To me, it was clear that when the Air Force issued its next ASRs, they would demand a thorough understanding of this field as well as carbon fibre composite (CFC) technology for making airframe components. The result was that when LCA was sanctioned, we did not have the FBW technology within the country, while NAL was prepared to handle CFC technology. More later about this problem.

Shortly after this assignment of responsibilities, it so happened that Mr P N Haksar, Principal Secretary to Prime Minister Mrs Gandhi, was visiting Bangalore early 1976. Air Marshal S J Dastur, the then Chairman of HAL, and I called on him and told him that the progress in aeronautics was not satisfactory, and that something should be done. Perhaps some others also might have conveyed to him the same impression. The result was that Mrs Gandhi constituted the Aerospace Group to look into the matter, with wide-ranging terms of reference, including the possibility of restructuring the organizational structure of aeronautics in the country. The committee consisted of Prof M G K Menon, SA, as the convenor, with the other members being Secretary, Defence, Secretary, Defence Production, the three Chiefs of Staff of the armed forces, Prof Satish Dhawan, Secretary, Department of Space, Dr A Ramachandran, Secretary, Department of Science and Technology, Air Marshal S J Dastur, Chairman, HAL, and me. The committee held discussions from 1976 to 1978. Also present was Air Marshal Narasimhan, Secretary to the committee.

At its first meeting, Air Marshal Narasimhan presented a proposed organization chart for restructuring the various aeronautical institutions to obtain a measure of coherence in their activities. There were reservations from the Secretaries in the Defence Ministry from the very beginning. The Secretary, Defence Production stated that if the annual budget in aeronautics was above Rs 1000 crores (Rs 10 billion), integration should take place. I pointed out that it was already in excess of that. There was silence. Then he said that he had been negotiating with some foreign companies, and so it was not opportune to do it then. I pointed out to him that the information needed to hold such negotiations would not be with him, but would be with the staff down the line in the department, and that if he

were to be transferred, these staff would brief his successors. There was again silence, and the meeting adjourned shortly thereafter. This set the trend for all the discussions that followed. The scientific members in the committee were clear that integration should take place, and Air Chief Marshal Moolgaonkar and Air Marshal Dastur supported it. The Air Chief proposed that an Air Force officer should head the proposed Agency. Secretary, Defence stated there could be differences of opinion about that. After further discussions, the Secretary, Defence reluctantly agreed that integration would be needed. Immediately, Air Marshal Dastur requested him to set a deadline for integration. He was not willing. For all practical purposes, the very concept of integration was stillborn. Some time later, Dr Raja Ramanna succeeded Prof Menon as SA.

Shortly before Prof Menon left some time in 1978, an important development took place. It could have adversely affected the destinies of NAL. Morarj Desai, the then Prime Minister, wanted to dismantle the CSIR and distribute the bulk of its laboratories to other government departments. This was apparently inspired by the IAS officers. Dr A Ramachandran was the DGSIR when this proposal was mooted, and I happened to be a member of the Governing Body as well as of the Society of CSIR, presided over by the PM. It was proposed to transfer NAL to Defence. I asked Vivek Sinha, then Director, Aeronautics in DRDO, what the probability was that an Air Force officer who would not necessarily have relevant experience would succeed me. Vivek replied that such a possibility could not be ruled out at all. NAL required a specialist who had a good understanding of the relevant R&D and aspects of design of aircraft to direct its destinies. I then asked Dr Dhawan whether Space would be interested in taking over NAL, if it was to be transferred, as the culture in Space was very similar to that of NAL. He stated that he did not need NAL and the only thing for which Space would have to depend upon NAL was access to the 4 ft tunnel, and that NAL was in no position to deny it. He further said he was not going to ask the PM for NAL and that the PM did not offer NAL to Space. He was correct in his assessment. NAL needed Space more than Space needed NAL. I then approached Dr Ramachandran and said that he had to fight against NAL being transferred to Defence. He told me to keep quiet at the Society meeting when the item came up for discussion. There had apparently been a change in the PM's view. It was decided to transfer the museums but retain almost all other labs within the fold of CSIR. NAL escaped being transferred to Defence by the skin of its teeth, so to say. It would have been an unmitigated disaster, considering how the DRDO laboratories were being run at that time. Had there been an integration of R&D and the industry under an aeronautics commission in the Defence Ministry, along the lines of the Space Commission, it would have been an altogether different situation. Sadly, it was not to be.

In an informal chat after he took over as SA, Dr Ramanna said that perhaps all was not lost, and if we did not succeed in formal integration, we could still try to do it through a project, and suggested that I should approach Air Chief Marshal Latif, then Chief of Air Staff. I had interacted with the Air Chief on several occasions earlier, while handling problems for the Air Force, and NAL established good credentials for delivering results. I told the Air Chief that if it was the policy of the Air Force to defend our skies with imported aircraft or aircraft built under license in the country, the research laboratories would do well to take up some

other work. I stated that if the Air Force were to support the development of a fighter aircraft in the country, their ability to defend our skies would not be compromised, because the country was already producing MIG 21 aircraft under license, and the chances were very high that we would soon be producing under license the Jaguar, and even the Mirage 2000 might be added to the Air Force fleet. So the time was opportune to take up the design and development of a combat aircraft.

The response of the Air Chief was that if I were to be behind the program, he would support it. I told him that I would be fully involved in it. I requested him not to give a specific Air Staff Requirement (ASR) but give an Air Staff Target (AST), and said I would constitute an integrated team cutting across artificial barriers and involve the industry, the R&D laboratories, academic institutions, and of course the Air Force. He was fully supportive. The aeronautical community owes a debt of gratitude to him for his vision and support. I reported to the SA what had transpired. He said that we should invite foreign consultants also to take up feasibility studies of the AST. We invited BAE, Marcel Dassault, Dornier, MBB, and the Italians. The Russians said that they would be "disinterestedly interested" and answer any questions we might wish to pose. They were a party who would be directly affected if the LCA program were to succeed. We found that the Italians did not have the crucial technologies needed to build the LCA. In fact, during the briefing, Mr Rajiv Gandhi, who was then a Member of Parliament, asked me what the possibilities were of the Italians providing the necessary know-how. I told him that we had invited them to assess their capability and found that they did not have the necessary technologies needed for the development of the LCA. Yugoslavia was keenly interested in collaboration but it was a country that was about to break up. Mr Rao Sahib, then Cabinet Secretary, advised against any collaboration. His advice was correct. Some time thereafter, it started breaking up. In our discussions with these companies, we found that while BAE, MBB and Dornier were open and fully answered our questions, Marcel Dassault were less than honest. They were making factually incorrect statements. In an aside, one of their members told a senior staff member from NAL sitting next to him, "You can discuss here all you wish. But we are going to get the contract". And they did initially get the contract. It would seem political considerations weigh heavily in such matters.

There are a couple of interesting anecdotes worth relating here. When the LCA integrated design team was in Germany for discussions with Dornier, in a casual conversation I asked Johnny Green, who subsequently retired as an Air Marshal, whether a person like me with my qualifications and experience in aeronautics, could ever become a Chief of Air Staff. There was a smile, which covered the dismay on his face, that I could ever ask a question like that. I told him that I would not be fit to occupy such a post, as it required a lifetime of direct and specialized experience in that field of professional endeavour, which I did not have. I then asked him the rhetorical question, how an equally demanding and specialized field like aircraft research, design, development, and manufacture could be handled by an Air Force officer, or for that matter by any administrator who did not have a deep understanding of the field. Of course, there was no answer, and there could not be any. Each of these specialized fields would require a lifetime of relevant experience, which even the general administrators, good, as they are in general administration, do not have. This

has been a problem that has haunted the aeronautical scene for half a century and prevented us from obtaining even a measure of self-reliance. Shortly after a meeting called by the then Secretary, Defence Production adjourned, in an informal chat, Air Marshal Zaheer asked me how I succeeded in making people from different aeronautical organizations work together. I told him that everybody would be benefited when a program such as the LCA was launched. He shook his head with a sense of surprise and said, "All these days we were able to get what we wanted from abroad by dividing the R&D and the industry. It is amazing that you were able to bring these institutions together". I succeeded because nobody's interests were sacrificed by launching such a program.

The feasibility studies were interesting. We found that as far as the conceptual design of the aircraft was concerned, we were in no way behind these companies. But what they had, and we did not have, were the technologies that were needed to build the LCA. These were, primarily, aspects of relaxed static stability and FBW systems. We did not need any know-how from abroad to build composite structures, but we lacked at that time a sufficiently big autoclave to produce CFC wing skins for the LCA. These were imported from Italy, but NAL subsequently designed an autoclave to cure such large components and supplied it to HAL.

While these studies were going on, some political developments took place. The elections were on. Mrs Gandhi was fighting to get back to power and wanted to make the acquisition of Jaguar aircraft an election issue, stating that it was a bad choice. I got a telephone call from the office of the then Defence Minister, Mr C Subramanyam, asking me to come immediately to Delhi. I was not told the reason. I met Dr Ramanna, the SA, also at the airport. He too had been called back. We were directly taken to the Air Chief's residence. Air Chief Marshal Latif then told us that the Air Force acquiring the Jaguar was based on a detailed study of the offers and that there were no underhand dealings in its acquisition. He wanted us to study all the relevant files and draw our own conclusions. The following morning, we met the Defence Minister. He too said the same thing. He asked Air Marshal Gole, then in Shillong, to come back and assist us, and opened up all relevant files. Dr Ramanna told me that he knew nothing about aeronautics and therefore, did not wish to participate in the investigation, and wanted me to handle the issue. This was revealing. He was an outstanding nuclear physicist and the prime mover behind India's 1974 nuclear explosion. In matters related to aeronautics, he was not alone. I knew virtually all the SAs from the time of Dr Bhagavantam and had interacted with them on matters related to aeronautics. As stated already, aeronautics is a highly specialized field, driven by forward-looking high science and high technology, with long gestation periods. Each of these Scientific Advisers was professionally highly competent in his field of professional endeavour. But not one of them had any understanding of aeronautics to draw any sensible conclusions, and much less take decisions that would help the cause of aeronautics. They were handling responsibility for administering a field about which they knew little. The decisions taken by some of them turned out to be disastrous, as we shall see below.

Frankly speaking, the situation in CSIR was no different and in fact, may be considered worse. Its laboratories were not vertically integrated with any hardware development

organizations. They are so diverse in the scope of their activities, that no DGSIR would ever be able to have a deep understanding of all their activities. To build some accountability into their activities, the present DGSIR, Dr R A Mashelkar, made external cash flow a measure of their utility to the community. But no DGSIR has been really equipped to understand what goes on in NAL. NAL was pretty much left to itself, and its performance was judged by its external cash flow, which was good because of the LCA program. The real problems of NAL surfaced when it took up the development of the Light Transport Aircraft (LTA), SARAS, to obtain a measure of control over its destinies. Indications were that the DGSIR was let down by NAL management. More about this issue later.

I studied the files relating to the Jaguar. Three aircraft were considered. One was the Viggen from Sweden, the second was the French Mirage, and the third was the Jaguar. The Viggen was out of the running from the beginning because it had an American engine which would not be available to us; the French Mirage offered to us was an older generation aircraft; and the Jaguar was found to satisfy the Air Force requirements. I reported to the Minister accordingly after studying the relevant files and said that I would defend the choice, so to say, "before God". The Minister said that it would not be necessary, as he found that it was Mrs Gandhi who first approved the acquisition of the Jaguar on file, and had so informed her. The Jaguar lost its relevance as an election issue.

The LCA studies by the integrated team continued. Early 1981, when I was having lunch with Prof M G K Menon at the India International Center, I got a telephone call from Mr V S Tripathy in the Prime Minister's office, stating that he was calling on behalf of the Prime Minister, who desired that I should succeed Dr Ramanna, who reverted to BARC. I met Mr Tripathy in his office. He said that the PM was keen that I should succeed Dr Ramanna as the SA. I told him that my interests continued to be in aeronautics and obtaining a measure of self-reliance in this crucial field was the objective for which I strove all my professional life in India. I explained to him what I had in mind, and that without integration of the R&D institutions and the industry, there was precious little hope of ever achieving self-reliance. I also told him that others might well be able to handle the position of SA, and that at that juncture it was essential that I continue to do what I was doing. I made it clear to him that if I were to be appointed as the SA, my first task would be to create an Aeronautics Commission, serviced by a Department of Aeronautics, along the lines of Space and Atomic Energy Commissions. Mr Tripathy remarked that he also believed in the cause I was striving for, and said that he would help me in achieving that objective.

I then called on Mr Rao Sahib in the PM's office. He too said that he appreciated what I was trying to achieve, and asked me to give him an organization structure to achieve this objective. I gave it to him, and told him that the cause of aeronautics was what I would like to work for. I told him that I was not looking for a job but for a task, and was deeply committed to obtaining self-reliance in aeronautics. He too said that I should take over as the SA. He then told me to call on Dr Ramanna, who had returned to head BARC. It was a disastrous meeting as far as the cause of establishing an integrated aeronautical base was

concerned. I called on Dr Ramanna on 28th January 1981. I had extended discussions with him. He told me that I would weaken the office of the SA, were I to create the Aeronautics Commission. I then asked him if he was strong enough as the SA to make sure that the GTRE engine under development then, would be the chosen power plant for the LCA. I told him that I could find a hundred reasons why it would not be suitable, whereas I would have to make certain from the word "go", that the engine and the airframe would be compatible. As it turned out, the gas turbine under development by GTRE then was unfit for use as a possible power plant for the LCA. He said that he understood what I was saying, but I would be weakening the position of the SA, never mind even if it strengthened the cause of aeronautics. As I was leaving his office after lunch, I asked him how strong DAE would be if BARC was not an integral part of it. He understood what I was saying. I told him to find somebody else and that it meant nothing to me to be called the SA. Shortly thereafter, I was asked to meet Mr Shivaraj Patil, Defence Minister. He wanted to know what I was then doing in NAL since he saw me as the Vice-President of CSIR. I explained to him what I was doing. He had known me well. Nevertheless, Dr Ramanna's views prevailed. I was reliably informed that when the minister put up Arunachalam's name for the position of SA, the PM's office returned it to him for re-examination. Apparently, Dr Ramanna advised the minister to send it back to the PM's office. The PM approved the appointment of Dr V S Arunachalam. Dr Arunachalam, a metallurgist by training, earlier in BARC and subsequently an Assistant Director in the Materials Science Division in NAL, and at that time working as the Director of DMRL, succeeded Dr Ramanna.

The feasibility studies of the LCA by the integrated design team were progressing satisfactorily. The time came to conceive of an organization structure to handle the project further. A meeting was called at which the Secretary, Defence Production, Mahesh Sareen, Dr Arunachalam and I were present. It was unanimously agreed that the Aeronautical Development Agency (ADA) should be created to handle the LCA program. It was also unanimously agreed that its responsibility would be to fund, manage and monitor, the LCA program, and that it would be a lean organization, which could be closed at short notice. The idea was that HAL would be the prime contractor, with NAL, ADE, GTRE, and other organizations working in subcontractor mode, under the auspices of ADA.

I was informally being consulted by Vivek Sinha, the then Director of Aeronautics, while he was preparing the agenda paper for the LCA program approval by the Cabinet Committee for Political Affairs (CCPA). Just before submitting it, one day, Dr Arunachalam waved it in front of me and remarked that he would not have the courage to submit it to the Cabinet Committee were it not for the fact that I was behind the program. My response to him was succinct. I told him to go ahead and submit it and that I would be fully involved in the program. As it eventually turned out, he made it impossible for me to continue.

ADA was registered as a society. It was generally agreed that I would be the first Director General of ADA, with the rank of Secretary to the Government of India. But surprisingly, the proposed memorandum and articles of association, and the byelaws of ADA were not shown to me before the society was registered. Suspicion in the SA's office took its ugly

roots and continued to spread. I was still in NAL, but was managing the LCA program. One of the important decisions I took was to bring back Raj Mahindra, who had spent his entire professional career as an aircraft designer and was primarily responsible for the design, development, type certification, and keeping track of the production of HJT16. I noted that every drawing related to the production of HJT16 carried his signature. I considered him indispensable for the program, because of the wealth of his experience. He was, in fact, the moving spirit behind the feasibility studies of the LCA. I formally took charge as DGADA on July 2nd 1984, having retired from NAL end of June. The first thing that I did was to disclose my full financial status to Mr Rao Sahib, the then Cabinet Secretary.

In May 1985, I got a telephone call from Mr K S Bhatnagar, then Secretary, Defence, who wanted me to accompany him to Paris, as Air Chief Marshal (retd.) Latif, then Ambassador to France, was keen that we should meet the French Defence Minister. In his office, the Ambassador suggested that we should retain Marcel Dassault as our consultants on the LCA program, and inform the French Defence Minister accordingly. The Defence Secretary told him that the procedures for such decisions were very detailed, and that such a commitment could not be given at that stage. I returned to Delhi and formally submitted the LCA feasibility studies immediately to Air Chief Marshal Katre, and requested him to formally issue the ASRs based on the feasibility studies. Earlier, when he was the Chairman of HAL, I got to know him fairly well. I was a member of the Board of Directors of HAL for over 12 years.

There was a story behind Katre's association with HAL, which eventually had ramifications on the LCA program. There was reliable information that his predecessor, Baljit Kapur, as chairman of HAL, was misusing his authority by giving preferential consideration to a company started by his nephews. This came to the attention of the then Secretary, Defence Production, Mr Mahesh Sareen, who in turn brought the matter to the attention of the Defence Minister. It was decided by them, that Kapur should go. The Secretary called him to his office during lunchtime, and took his resignation after a brief meeting with Mr Venkatraman, the Defence Minister. The same evening, Mr Sareen took along with him to Bangalore Air Marshal Katre, who was then heading the Western Command. Kapur was relieved of his responsibility, and Katre took over as the Chairman of HAL. Mahindra and I knew Sareen well. Kapur thought that we had something to do with his dismissal. Sareen was a person of high professional integrity, and not swayed by friendships in living up to his administrative responsibilities. Kapur could not touch me. He arranged for questions to be raised in the Parliament stating that Mahindra's loyalties were suspect, as his wife was British. It was as if Rajiv Gandhi would be suspect because his wife was Italian! Mr Venkatraman defended Mahindra, and said we needed more people like him. His successor, Mr Narasimha Rao, also spoke in the same vein. We continued to work in ADA.

Shortly after taking over as the DGADA, in view of the importance of coordinating the development of an appropriate engine for the LCA, I proposed to the SA, Dr Arunachalam, that the monitoring and funding of the development of an engine suitable for the LCA in GTRE should be handled by ADA. He refused. The engine development would then have

had a better chance. The engine required as a power plant for the LCA was not even on the drawing board then. Its development started later. As of date (early 2006) it is still under development. It is most unlikely that the Kaveri engine currently being developed by GTRE will be available at least before the first thirty or so of the LCA are manufactured. This is again a classic example of an agency head being really unaware of the implications of his decision.

However, although I was the chief executive of ADA, Arunachalam as the chairman of the Governing Body started taking executive decisions. My position was getting to be untenable. I told him several times that I would be glad to leave if he wanted to appoint somebody else, that I had no interest other than successfully developing the LCA, and that I would ensure before I left, that there would be at least two persons for all senior positions for succession, and that I should be left alone to run ADA. It would seem to have fallen on deaf years. The issue reached a critical stage on May 31st 1985, when the Science Advisory Committee to the Cabinet (SACC) was about to adjourn for lunch. Arunachalam came to me and asked me to ease Mahindra out. I did not respond. I felt that no useful purpose would be served by my continuing in the position. That afternoon, Prime Minister Rajiv Gandhi chaired the last formal meeting of the SACC. I drafted my letter of resignation later in the evening. The following morning, I returned to Bangalore and asked Mahindra for his resignation, after informing him of what had transpired. I felt that if I did not command the confidence of the ex-officio chairman of the Governing Body, who had been working under me earlier, and if he felt compelled to interfere with ADA operations at the working level while knowing nothing about aircraft development, I had no business to continue. In the first draft of my letter of resignation I mentioned that I had asked Mahindra for his resignation as desired by the SA, and enclosed Mahindra's resignation letter also. Before sending it, I showed it to Air Marshal Wollen, the then Chairman of HAL. He suggested that I should omit any mention of Arunachalam asking me to ease Mahindra out. I deleted it accordingly. It was a mistake, and I should have retained the reasons for my resignation. I brought the matter to the attention of a person who was much respected in the administrative circles in Delhi. He too felt that it was the right thing to do. I mailed our resignation letters on 14th June 1985.

Early July, Mr Bhatnagar, Secretary, Defence wanted to be briefed on the progress in the LCA program. I arranged a meeting and told him that neither Mahindra nor I would speak on the occasion, as strength should be at working levels, and only they would make the presentation. He was deeply impressed by the progress made, and suggested that a similar presentation should be made to the Defence Minister. It was arranged for July 20th 1985.

But before the Minister came to Bangalore, Prof M G K Menon desired that Mahindra and I should meet him in Ashoka Hotel. Also present at the meeting were Dr Arunachalam and Vivek Sinha from DRDO. Prof Menon said how important it was that Mahindra and I should continue in ADA. I was taken aback by his comment. I told him that it was never our intention to resign, but that Dr Arunachalam wanted me to ease Mahindra out, and that as a matter of principle, I felt I too should resign, as I did not command the confidence of the SA, who was the ex-officio chairman of the Governing Body of ADA, and that if he

questioned my judgment in my sphere of responsibility, I had no business to continue. Arunachalam knew too little about aeronautics to take any sensible decisions, as subsequent events amply proved. Then I got the shock of my life. Dr Arunachalam told Prof Menon that he had never asked me to ease Mahindra out; a blatant lie if ever there was one. I told Prof Menon that if ever I had any doubt about the wisdom of my decision, it was cleared by this blatant lie. Prof Menon then said that he knew nothing about this. There ended this sordid meeting. I made a serious mistake in deleting the mention of Arunachalam's desire to have Mahindra eased out, in my resignation letter.

On July 20th 1985 Mr Narasimha Rao, the Defence Minister, was briefed by the staff at the working level about the progress made so far. He left afterwards for another meeting and came back at 4 pm to continue discussions. He looked visibly pleased with the progress made till then. He wanted to know what I wanted next. I told him that the Air Staff Target must be converted to Air Staff Requirement to proceed further. He said that it would be done. Air Chief Marshal La Fontaine, who succeeded Katre, said that he fully supported the program. The Finance Secretary sitting next to me said that I should approach him for all the money I wanted. The Minister wanted to know if there was any engine other than the Rolls Royce RB199 that would satisfy the requirements of the LCA. I told him that the older generation GE404 also had the necessary thrust. There were no proposals to up-rate it at that time, while such a study was being made on the RB199. The Minister asked Dr Arunachalam to look into the matter. As it turned out, it would seem that other considerations ultimately decided its choice as a power plant for the LCA. The meeting adjourned, and the Minister wanted me to accompany him to see NAL. As I was leaving, I told Dr Arunachalam that I had done my job, and he should find a successor for me, and that I was not going to continue after what had happened. He then asked Mahindra. He too said the same thing. He then asked me what he should do then. I told him that he should have thought about it when he asked me to ease out Mahindra. He then said he could not quit, as he needed the job. As a matter of fact, from the time my resignation was accepted end of November 1985, till I started getting my US Social Security benefits, I had a very difficult time financially, what with a meager pension of less than Rs 4,500 per month from CSIR. But principles were important to me and I could not conceive continuing as DGADA under the circumstances. While accompanying the Minister to NAL, I told him that there were important policy matters, which could be decided only by him, and asked whether I could bring them directly to his attention. He said that he was surprised by my question, as I was the chief executive of ADA, and he was the president of the society of ADA, and that it was well within my rights to bring them directly to his attention. There was a reason for this question. After the first general body meeting of ADA presided over by the Minister, I issued a brief press release, and sent a copy of it to him and to Dr Arunachalam, the SA and ex-officio chairman of the ADA governing body. Arunachalam informed me that the Minister did not desire me to have direct contact with him. I told the Minister in the car that to have a halfway decent chance of self-reliance in aeronautics, there should be formal integration of activities among the R&D institutions and the industry.

In August 1985 I got a call from the Minister's secretary, Mr Ramu Damodaran, an IFS officer, saying that the Minister wanted to see us. Mahindra and I had an extended meeting

with him on a Sunday morning in his office. The Minister remarked that he was not talking to me as the DGADA but as a scientist, and would like to have an honest assessment of our ability to build the LCA. I told him that I would speak to him as a scientist. I told him that while we were capable of designing the LCA, we needed some critical technologies that were essential to build it. These were composite technology to build parts of the airframe, a thorough understanding of relaxed static stability, and ability to build fly-by-wire control systems. I told him further that following the delineation of responsibilities among NAL, ADE and GTRE, NAL had acquired enough capability to handle the composite structures in the LCA without any problem. As it turned out, we did not have an autoclave big enough to make the CFC wing skins, and we had to import them. Subsequently, NAL supplied on contract the autoclave to HAL. I told the Minister that in its assigned area of responsibility for FBW systems ADE did not seem to have made any progress, and that we would have to import the technology. I drew his attention to how NAL placed its staff to work with a foreign company while designing the 4 ft trisonic tunnel, and suggested that we should use a similar approach for the FBW systems needed for building the LCA. He was satisfied with my clarifications. I reiterated to the Minister that GTRE would not be able to supply a suitable engine in the near future, and that at least for the first thirty aircraft or so, we would be compelled to use a foreign engine, and the Rolls Royce RB199 was a strong contender. There were proposals for increasing its thrust further. As mentioned already, GE404 was chosen as the power plant. He wanted us to meet him again at his home that afternoon on our way to the airport. The Minister stated that he understood what needed to be done and would see us again in September when I would be in Delhi for the Governing Body meeting. He wanted me to prepare a note on restructuring aeronautics and bring it along. When the meeting of the Governing Body was taking place end of September, I got a message that the Minister would not see me later that day, but only the following morning. The following morning, I got a telephone call from the Secretary, Defence Production, Mahesh Sareen, saying that Mr Narasimha Rao was no longer our Minister, and Arun Singh had taken over. I sent him a copy of the structure for reorganizing aeronautics as an integrated activity in the country as desired by his predecessor, and requested him to relieve us at an early date. We were relieved end of November 1985. Shortly thereafter, I wrote a letter to Mr Rao Sahib, who had by then retired and become a Director in the World Bank, explaining the circumstances under which I felt compelled to resign. He took interest in trying to get me to join as the SA. He was made fully aware of Raj Mahindra's capabilities as a designer.

At the first general body meeting presided over by Mr P V Narasimha Rao as the Defence Minister, Mr V P Singh, the Finance Minister, wanted to know the cost details of the LCA program. My colleagues told me not to give any actual figures, but give only rough estimates. I told them that I would not do such a thing, as it would compromise my credibility and integrity. I told the Minister, that we made the cost estimates based on the costs in HAL at that time, and provided for inflation within the country and abroad. We estimated that the first prototype would fly in about eight years, in 1991, and that we would require six prototypes to complete the flight tests in about three years, and hopefully start production around 1994. I informed the Minister that the estimated cost was Rs 1250 crores (Rs 12.5 billion), and that every year of delay would cost another Rs 150 crores (Rs 1.5

billion). He looked satisfied, and left immediately after the clarification. Apparently, instead of six prototypes for flights, only two were approved. This would inevitably increase the time for the flight tests alone to about nine years, apart from any other reasons for delay. As it turned out, the first prototype flew ten years later in January 2001, and the reported cost by then was Rs 2750 (Rs 27.5 billion), not too far from the estimate given to the Finance Minister.

The ASRs stipulate some important parameters to be achieved by the LCA before the Air Force can accept the aircraft and order series production. If there are any shortfalls, these have to be negotiated by the ADA with the Air Force. The critical parameters are: time for level acceleration from 0.8 Mach number to 1.2 Mach number, rate of turn, rate of climb, range, and war load. If there is a shortfall in any of these parameters, it would amount to deterioration in the stipulated performance and compromise the fighting capability of the aircraft. Typically, about 2500 flight tests would be needed to establish these parameters. Apparently as of date, about 500 flight tests have taken place. It is clear that the LCA has a long way to go before it can be considered acceptable to the Air Force without any compromise in the original ASRs and without any concessions. If newspaper reports are true, the Air Force has apparently placed an order on HAL to supply 40 LCA of which 20 were firm orders, with series production booked for eight aircraft (Vijay Times 10/1/06).

This is a significant development. Nothing would give me greater pleasure than to see the LCA fully living up to the expectations of the Air Force, and becoming the backbone of our fleet for air defence. One would hope that ADA will fully satisfy the ASRs, with such minor concessions as the Air Force may agree to, so that the LCA will not meet the same fate as the HF24. It is essential that the LCA fully satisfies the ASRs, even if it was too late to really respond to the Air Force's needs, if only to know how to design such aircraft, without repeating such mistakes again. In this connection, I recall a visit to the Air Force museum at Wright Patterson Air Force Base in Dayton, Ohio. What struck me was the number of aircraft that were built, but did not go into production. Each one of them had added something to the pool of knowledge about how to design aircraft.

The design of gas turbines is more complex. It depends on enormous test data generated by the engine manufacturers. As a Pratt and Whitney designer told me, engine designers tend to be a mafia, and keep their knowledge close to their hearts. Prof Howard Emmons from Harvard University, a member of President Reagan's committee to examine whether the US should go in for the development of a supersonic passenger aircraft, confirmed this during his visit to NAL, and highlighted the importance of extensive testing in the development of advanced-technology gas turbines. It may well be necessary for GTRE to enter into an agreement for the successful development of the Kaveri engine, the proposed power plant for the LCA. GTRE would learn much from such collaboration. I think it was a serious error of judgment in not retaining AVM Roy Choudhuri. I greatly admired Dr Krishnan, who became the Director of GTRE some time later. But he was a metallurgist, and did not have any knowledge in gas turbine design. It is too expensive to try to learn on the job in such projects. I think Dr Kota Harinarayana did a commendable job, considering the heavy odds against which he had to function as the LCA Program Director. He had to

learn a lot on the job, with inevitable delays, but he did succeed in making the LCA fly. He should have been made the DGADA. It was a pity that his contributions were not sufficiently appreciated by the SA's office.

I believe the lesson to be learned from the history of aeronautics in India is that it is essential to restructure aeronautics so that we do not repeat the same mistakes. All the R&D laboratories primarily concerned with aeronautics, such as NAL, ADE, GTRE, and the HAL Design Bureau as well as its corporate R&D base, have to be brought under the authority of the Director General of the Aeronautical Development Agency. It will have to be headed by a person who has a good understanding of R&D and the design of aircraft, and a flair for administration. It is the *relevant* scientific and technical knowledge and experience that will make or break an institution in fields of high science and high technology, such as aeronautics, and not *any* knowledge and *any* experience. It is not surprising that in the technologically successful institutions like Space and Atomic Energy, people who have the potential are groomed within the organization to occupy senior positions of responsibility, including the position of the agency Head. It was the lack of such experienced people in senior management positions that caused the enormous problems in the development of the LCA and aeronautics in general, in the past.

The Governing Body of ADA should have among others the SA, the Secretary, Defence Production, MD of the HAL Design Bureau, the Deputy Chief of Air Staff, the Director General, Aircraft Production Agency, and the Member, Finance in the proposed Aeronautics Commission. If any aircraft are to be built under license, ADA shall be the primary agency to absorb the imported technologies to build thereon, to make the country less and less dependent on licensed production. It would be an unfortunate error of judgment to keep the Air Force out of any committees in ADA concerned with the development of the LCA. I hope the information that was given to me was not true.

The Director General, Aircraft Production Agency (APA) should be a serving Air Marshal who has earlier headed the Maintenance Command, as the Air Force would be the primary customer for any aircraft designed and developed, or produced under license. The APA Governing Body should include, among others, DGADA, the SA, the Secretary, Defence Production, MD Production, HAL, the Deputy Chief of Air Staff, and Member, Finance of the Aeronautics Commission.

The Aeronautics Commission should be headed by a professional who has a good understanding of the various aspects of research, design, development, and manufacture of aircraft, preferably groomed within the organization. He shall also head the Department of Aeronautics, much along the lines of the Space Commission. Like Space and Atomic Energy, the Commission shall have not less than four and not more than seven members, including the Member, Finance. The Commission shall report to the Defence Minister.

Perhaps a simpler procedure would be to upgrade the present Aeronautical Development Agency (ADA) to that of a Commission, responsible also for research, design, development, and manufacture of aircraft, with the appropriate supporting organizational

structure reporting to it. Without such restructuring of the aircraft industry and the associated bodies, it would be impossible to obtain a measure of self-reliance in the aircraft industry. This is essential for a country of our size and with its avowed independent foreign policy.

MISTAKES AND ERRORS OF JUDGMENT

Looking back over the years of my professional life, if I ask myself could my decisions have been any different, my answer would be NO. I perhaps made a mistake in being so transparent about my desire to restructure aeronautics. Perhaps I should not have disclosed my views on the need for restructuring aeronautics, when I was invited to succeed Dr Ramanna as the SA. Since he was from DAE, which was integrated similarly, I thought I would get his support. I was confident of getting the support of the PM's office, and from Mr Shivaraj Patil, the then Defence Minister, had Dr Ramanna agreed to my suggestion. He did not want to have his (concurrent) authority as the SA compromised, never mind that he knew little about aeronautics, as he himself admitted, when both of us were asked by the then Defence Minister to look into the issue of the acquisition of the Jaguar fighter aircraft, when it was becoming an election issue. I found that the bureaucrats, who are not familiar with issues, desire unanimity among the professionals whose advice they seek, before they take decisions. But transparency is a trait I suffer from, and sometimes it even led me to make tactless comments and decisions, which I later regretted. Clearly, I made a mistake in expressing my views to Dr Ramanna, thinking that he would appreciate my arguments and agree to the restructuring of aeronautical activities in India along the lines of DAE. It was ironical that he did not agree, since it was precisely in such an integrated environment in Atomic Energy under Bhabha, that he grew. I have to regretfully conclude that he was no Bhabha in his vision for administration of highly specialized S&T institutions. I had no regrets for rejecting twice the offer of DGSIR, as my heart was not in it. Restructuring aeronautics was close to my heart. It was for this purpose that I returned home. Restructuring this field is essential if we wish to have a measure of control over our destinies in times of war.

Chapter 4

COMMITTEE WORK

THE VALLURI COMMITTEE REPORT - RESTRUCTURING PROMOTION POLICIES FOR CSIR SCIENTIFIC AND TECHNICAL STAFF

The origins of the proposal for restructuring the promotion policies for the scientific and technical staff of CSIR can be traced back to the Directors conference convened in 1979 by the then DGSIR, Prof M G K Menon,. This issue came up for discussion when I pointed out that the CSIR bye-law 71 b, which provided for five-year assessment of scientific staff, was not fair to the supporting technical staff, whose contributions were decisive for the success of the R&D programs in the CSIR labs. I suggested that we should review the whole policy and that something should be done for the supporting staff also. The DGSIR then appointed a committee under my chairmanship to look into the matter. The background that led to the recommendations is as follows.

At the July 1966 CSIR Directors conference, Dr Zaheer, the then DGSIR, drew attention to the International Council of Scientific Unions (ICSU) conference held earlier in Bombay, and in particular to the lecture given by Dr H J Bhabha, Director of TIFR and Chairman of the Atomic Energy Commission. In his lecture, Bhabha apparently drew attention to the manner in which he built his institutions, and how he did not compromise quality in recruitment. He stated that in contrast, in building CSIR labs, an organization structure was first drawn up and minimum qualifications for each post were specified. He further stated that in such a scenario, the minimum stipulated qualifications frequently became the maximum qualifications, with the inevitable lowering of standards. In the DAE, the research work that was taken up had well-defined end purposes and more often than not, became a direct input to some other DAE project. In other words, there is vertical integration of the bulk of the activities with specific end objectives. Bhabha implied that the structure of CSIR laboratories came in the way of achieving maximum benefit.

Any dispassionate assessment of the functioning of the CSIR labs would lead one to acknowledge that there was some substance to this conclusion. It was felt that much more could and should be achieved by the CSIR labs, and that its policies should be reviewed. Some basic facts became readily apparent. The first was, of course, the fact that to the extent CSIR labs were not vertically integrated with any specific agency, there was no certainty that their research output would necessarily become a deliberate input downstream to some other body. Secondly, this implies that they should take up primarily those research programs that would attract the attention of potential users. The potential users would be there for their health, and not for the labs' health, and labs should therefore take up research that would attract the attention of potential users. Third, to attract such potential users, a CSIR lab should have highly competent scientific and technical staff at

its senior levels. Fourth, it had to recognize that if a second-rate person is appointed or recruited today, the first-rate staff would tend to leave, inevitably making the laboratory a second-rate institution, sooner or later. Fifth, it therefore inevitably follows that you cannot pursue excellence by compromising quality of the staff in recruitment or promotion. Sixth, the supporting staff should have an equally important role to play in the functioning of an R&D institution, and their interests should not be neglected in formulating the promotion policies. Seventh, it should be recognized that no system is infallible, and the scheme should therefore provide for correction of errors in initial recruitment. Eighth and finally, the promotion policy should not invert the staff pyramid.

These were the primary considerations that led to the restructuring of promotion policies in the CSIR labs. The recommendations covered the promotion opportunities of about 20,000 scientific and technical staff. The staff were divided into four groups based on their education, and each group had five grades. The lowest were the grades in Group One, the so-called 'class four' staff, with a minimum qualification of eighth standard education. They were expected to learn some trade on the job. The promotion from the lowest grade to the next higher grade in the Group was up to 100%, but not necessarily to 100% of the eligible staff. The promotion from that grade to the next grade was fixed at 75%, and so on up the promotion ladder to 50% and then to 25 %. If a person did not make it the first time, he had three more chances, once a year. If he did not make it in four chances, he was not considered fit for promotion to the next grade. The system made it possible to weed out those who were not pulling their weight. It also made it clear that those who were not pulling their weight in the relative assessment would not be promoted.

The next level, Group Two, also has five grades, with an entry-level qualification of SSLC/ITI trade certificate. Their promotions were based on trade tests with 70% for the trade test and 30 % for performance in the interview. The percentages for promotion from one grade to the next remained the same. The next level, Group Three, also had five grades with an entry-level qualification of a BSc or three-year diploma in engineering. Their entry level was JSA/JTA with promotions to SSA/STA, Scientist A/A1, Scientist B1 and on to Scientist C1, with the same percentages as above, for promotion from one grade to the next.

The next level, Group Four, also had five grades, and an entry-level qualification of a first class MSc or a first class degree in engineering. The entry level was Scientist B and the promotion ladder led to Scientist C, Scientist E1, Scientist E2, and then to Scientist F. During those days there was no grade of Scientist G or H; they were introduced later. Percentages were the same for promotion from one grade to the next, as in the lower group of grades. Staff in this group of grades were considered the most important of the staff in the laboratory, and it was considered essential to maintain high standards in promotion from one grade to the next, in this group. It is on the excellence of the scientists in this group of grades and their performance that the future of a laboratory depends. To ensure this, it was felt that only the very best should be promoted to the higher grades in the ladder. It was also proposed that an outstanding scientist need not become a director to draw the director's salary, and that only the truly exceptional would be promoted to the director's rank.

It was also felt that staff in the lower grades who improve their qualifications to the entry level stipulated for the next group of grades, should be immediately assessed for promotion to the next group. In NAL, I felt it was important to identify talented people and encourage them to improve their qualifications. There is an example in NAL, where a person with a diploma in draftsmanship went on to obtain a PhD, and was eventually promoted to the Deputy Director's grade. As people improve their qualifications, not only they, but the lab also is benefited. I made it a point in NAL to identify talented people and nurture them, so that as they grew, NAL also grew in its capability. Provision was also made for handling some rare circumstances. It was proposed in the committee report that these should be placed before the DGSIR for consideration. For example, if the assessment committee felt that there was more staff fit for promotion to the next higher grade than stipulated by the percentages. As it turned out, Dr Varadarajan, then DGSIR, did not want to take on this responsibility. Incidentally, computer studies of the implications of this promotion policy were made, particularly for the Group Four grades. The study showed that the staff pyramid would not be inverted in such a scheme.

The report was submitted to Prof M G K Menon, then DGSIR. He went through it, and desired that I should brief the Member, Finance of the Governing Body of the CSIR. So the chiefs of administration and finance and I met Mr Ramachandran, the then Finance Secretary, and explained in detail the proposal. His comment was succinct. He stated that if a comma were to be changed in the body of the report, I should give my consent. I was happy that he fully supported the proposal for restructuring the promotion policies for the scientific and technical staff of CSIR.

The matter was next placed before the Governing Body. It faced opposition from Dr Nityanand, who stated that Central Drug Research Institute (CDRI) had scientists who were as good as he was, and there should be no percentages in promotion. As it turned out, this was broadly the feeling of the chemistry-based labs. The Governing Body approved the report without any changes. There was happiness among the staff in the lower group of grades. The policy recognized that they too have a role to play in the performance of the labs. Group Four staff resented the system, because they could not take their promotion for granted, as they had been doing in the past, and would have to work hard for it. As it turned out, Dr Sidhu, who became DGSIR after I refused the offer from Nurul Hasan, then Vice-President of CSIR, appointed a committee under the chairmanship of Dr Thyagarajan, the then Director CLRI/IICT. They diluted the promotion system and added one more grade to Group Three grades, and made people with MSc second class eligible for recruitment to this group of grades. The very basis on which the promotion policy was conceived was compromised. I understand that even the system of percentages for promotion from one grade to the next was done away with subsequently.

There was no real performance accountability in the CSIR labs. In 1995 Dr Mashelkar, DGSIR, proposed changing this, by making the external cash flow (ECF) into a lab a measure of its performance and utility to society. I believe it is a good idea for the CSIR labs, since they are not vertically integrated with a user agency, as, for example, Space and

Atomic Energy are. It is a measure of the utility of the labs' output. He said that he had seven years to go before he retired, and hoped that the ECF would reach Rs 700 crores (Rs 7 billion) by that time. There was no penalty if a lab did not succeed in getting much external cash flow. Incidentally, Dr K N Raju, when he was Director of NAL, said that NAL was one of forty labs in the CSIR, but it contributed 25% of the ECF, much of it due to the Light Combat Aircraft program. Apparently, the annual ECF of CSIR currently is nowhere near the target set by Dr Mashelkar ten years ago. One needs to ponder the implications of this disparity. It would seem that the basic point made by Bhabha about the recruitment and promotion policies, and perhaps by implication the lack of well-defined linkages with other agencies, was not entirely without foundation. CSIR needs to do deep introspection on this issue.

RESTURUCTURING THE PROMOTION POLICIES FOR CSIR ADMINISTRATIVE STAFF

Shortly after he took over as the DGSIR, Dr S K Joshi desired that I should review the promotion policies for the CSIR administrative staff also. I found it was easier said than done. The administration cadre did not want any changes that would dilute their promotion opportunities to the rank of administrative officers, the highest grade then prevailing. The administrative staff of CSIR comprised administration, accounts, stores and purchase cadres, apart from security, etc. The qualifications for recruitment were SSLC and upwards. The committee felt that a person with SSLC would not necessarily be bad, but that exposure to formal higher education would better equip a person for handling administrative responsibilities. During those days, people from the accounts cadre also sometimes became Administrative Officers (the top position in administration, now re-designated Controller of Administration), but hardly any from the stores and purchase cadres. Furthermore, the responsibilities in these jobs were based on very well defined rules and regulations and precedents. They were specific to each cadre, so much so, that the exposure of the staff of one cadre to the knowledge of the other was limited.

To correct the situation, it was proposed that all the staff in administration should belong to only one cadre, and that the entry level qualification should be a bachelor's degree in arts, science or commerce, and all future recruitment should be at the upper division clerk (UDC) level, with transfers about once every three years among the categories cited above, so that they would be well exposed to the various aspects of administration. It was also proposed that to protect the promotion opportunities of the existing staff, the promotions should have the same percentage as the then staff strength among the various cadres, till they retired. Thus, if initially there were forty staff in administration and twenty in the accounts section, the ratio of promotion among them would be two to one until the existing staff retired. Subsequently, percentages for promotion would apply to these cadres also. The matter was explained to the then Member, Finance of the CSIR Governing Body, Mr Ganapathi. He appreciated what was being proposed and wished that the promotion policy for the staff of the Government of India could be along the same lines. He went on to say

that it was an impossible objective to achieve, and he would have difficulty in agreeing to its implementation. He also stated that one way of getting around the problem would be to create vacancies for deserving people, and that he would readily agree to such a proposal There ended the efforts to restructure the CSIR administrative staff structure.

But there was an aftermath to this. The then Chief of Administration of CSIR, Mr Omesh Saigal, an IAS officer, did arrange to make some changes based on the principle 'anybody can do anything, and no prior experience is needed'. As it turned out in this scheme, a stores officer who did not have an iota of experience in accounts or administration was posted as the Controller of Administration in NAL. It played havoc. An integrated administrative structure would have avoided this situation. But who cares?

SCIENCE ADVISORY COMMITTEE TO THE CABINET (SACC)

When Dr M S Swaminathan was the Secretary, Department of Science and Technology, I was invited to be a member of SACC. It was the highest body in the country to advise the government. You feel you have arrived. But the reality was somewhat different. I was a member for two (or was it three?) terms, and do not recall any major recommendations of SACC being accepted by the government. When Prof Menon succeeded Dr Swaminathan, the SACC took up for discussion a report I had prepared as the chairman of a sub-committee of the Administrative Reforms Committee, about restructuring the administration of S&T institutions. This was done under instructions from Mr C Subramanyam, the Cabinet Minister. I must have spent more than a year preparing the report. Dr A Ramachandran, then Secretary, DST, and I met the Minister and submitted the report. I was instructed to get the report implemented, and Dr Ramachandran assigned one Mr Belliappa, an IAS officer and a joint secretary in DST, to assist me in this matter.

One of the recommendations was that in those departments of the government where science and technology have an important role to play, as in the Ministry of Defence, there should be a scientific adviser reporting to the minister directly. Mr Belliappa opposed this, and said that it was the sole responsibility of the IAS to advise the ministers, and there was no place for others to do so. In the conversation that followed, I said that I would like to assume that he was an expert in aeronautics, and so he was well equipped to advise the Minister on aeronautical matters. I further said that as his position was transferable, I would like to assume that he was transferred to the Department of Electronics. I then asked him what his competence was to advise the minister on matters related to electronics. He said that somebody could explain the issues to him and he would then explain it to the minister. I then asked him how he could understand the issues and the implications of the proposed solutions, when he had no background in the field. He could not respond. I never met him again.

The report on restructuring the administration of S&T institutions surfaced again as an agenda item before the SACC, and was discussed threadbare. The issue of having scientific

advisers came up for discussion again. Prof Menon nominated Dr Raja Ramanna from DAE and me as a two-man committee to look into the matter. We unanimously recommended the proposal. The bureaucrats did not accept it. I also recall proposing that budgeting for research should be project-specific to respond to specific felt needs and to build accountability at all levels. Nothing came of it. It would have required that bureaucrats who sanctioned the projects also be made accountable for the results obtained, and would have ensured that they provided desirable inputs to help the S&T base of the economy in a tangible manner. The convention at that time was that the various S&T departments of the government would appoint expert committees to study the R&D proposals submitted to them, and if the committees approved them, the departments would sanction grants to support the projects. The expert committees were rarely concerned about the utility of the end results in strengthening the economy. For example, in the US, no more than 15 to 20% of the federal funds are provided for openended basic research, and the balance of the funds are project-specific to respond to welldefined needs. The proposal was not agreed to, as it would have meant specific accountability from the S&T departments.

Another important issue I brought to the attention of SACC was the tremendous increase in population, and that there were no real solutions to our country's problems if this issue was not tackled. I pointed out, in this connection, a finding by J R D Tata that it would cost the government about Rs 5000 to develop the infrastructure to support every child that was born, and that it would be less expensive in the long run to pay that much money to a woman who would undergo the operation to prevent the birth of more children after the first two. I proposed to SACC that instead of giving all the money at one time, the women who underwent the operation should be given immediately Rs 1000 as an incentive, and the balance should be given in the form of Postal Savings Certificates with accrued annual interest, which could be paid in installments after the women reached retirement age, along the lines of monthly pensions. In our country, particularly among the poorer class, children are looked upon as a social security in old age. SACC approved it unanimously but it was turned down by the government, saying that they had no money for it, whereas in reality it would have saved the government money by reducing the outlays for providing more infrastructure for schools, hospitals, etc. I was equally disappointed that the government rejected another proposal unanimously approved by the SACC. I pointed out that the existing government policy of building more and more staff quarters to provide accommodation to staff while in service was counterproductive in the long run. In particular, when a government servant occupying staff quarters retires, he would probably have to move to a hovel, unless he inherited some property or had taken a loan from the government while in service to build a house for himself. I proposed that instead of building staff quarters, government should build multi-storey buildings, and sell flats in them to the staff, so that instead of paying rent, they would pay off the cost of owning the flat, while they were still working, and have a home of their own after retirement. The proposal was that the government should give the money to its institutions as a revolving fund and permit them to reinvest the receipts, including the accrued interest, for building more flats. A simple calculation showed that this way, about three times more flats could be built. For example, if the government provides Rs 10,000,000 to be given as loan for one hundred people, it would satisfy 100 people. However, if the monthly payments on the loan along

with the interest were to be reinvested for building more flats, something like 300 flats could be built. As could be expected, it was unanimously approved by the SACC. In the United States building construction is one of the biggest employment generators for the working class. In a poor country like ours, it would have created a lot of employment among the poorer sections. The bureaucrats rejected it. Unless I am greatly mistaken, few, if any, of the recommendations of SACC received the consideration they deserved. It is a pity. I got the feeling that the principle of NMH (not made here) would seem to have operated with the bureaucrats deciding their future.

I cannot help recollecting an amusing incident that took place at one of the meetings of SACC, chaired by Prof Menon. He placed before the committee the annual report of the SACC before forwarding it to the cabinet secretary. After quickly scanning through it, Dr C N R Rao commented that the report was not good, as it did not mention anything about his work. It gave a deep insight into his personality. This comment was received with thundering silence by the other members of the committee, who were obviously amused by his desire to have his contributions highlighted in the report, which was primarily intended to summarize the progress of scientific research in the country. Prof Menon responded by stating that it was not intended to highlight the contributions of individual scientists, but was a summary of the scientific progress in the country. It is not pleasant to note that Dr C N R Rao is not alone in openly craving publicity from outside his own community of scientific peers. Another instance that gives an insight into his personality and value systems, and his scant respect for ethics in the practice and management of science, was his tendency to take unfair advantage of the contributions of some scientists. For example, this he did by inviting some reputed scientists as honorary faculty to the Jawaharlal Nehru Center for Advanced Scientific Research (JNCASR) in his capacity as its President, and listing their publications based on work done in their own institutions, as if it had been done in the JNCASR, omitting any mention of their parent organizations in the Centre's annual reports. Prof Rao clearly established his reputation as an eminent scientist in his own field of scientific research, but these practices inevitably raise questions about his scientific integrity.

FIFTH REVIEW COMMITTEE OF THE INDIAN INSTITUTE OF SCIENCE

In 1970 I was nominated as a member of the Fifth Review Committee of the Indian Institute of Science, from where I graduated in 1948 as a student in the Aeronautics Department. Prof T R Seshadri, FRS, distinguished chemist, then at the Delhi University, chaired the committee. Being a local person, the responsibility of drafting the committee's report fell on my shoulders. At its first meeting, Dr Satish Dhawan, then IISc Director, gave a brief review of the past and the functioning of the IISc. He arranged for the committee to visit the local Rajaji Nagar Industrial Estate, and proposed that we should also visit TIFR in Bombay. I imagine that he wanted us to be exposed to the two ends of the spectrum of IISc activities. They were revealing. Incidentally, I made a casual visit to his home that evening, as I frequently used to do during those days. He advised me not to do so in future while I was associated with the Committee, lest it should be misunderstood - a measure of Prof Dhawan's integrity. I met few such people in my life.

By any reasonable measure of evaluation, IISc has been, and continues to be, an outstanding academic and research institution, unexcelled in the country even today, long after the government created many IITs and R&D institutions. The visit to the industrial estate was interesting and indicated the possible scope of, and a desire for, interaction by IISc with low-technology industries also. I was therefore not particularly surprised that IISc started a section to study the Application of Science and Technology to Rural Areas (ASTRA), under the inspired leadership of the late Prof A K N Reddy. He was also associated with the Karnataka State Council for Science and Technology (KSCST), of which Prof Dhawan was the Vice-Chairman, and I was a member for several years. ASTRA's concern for the rural poor could be gauged by the fact that it took considerable interest in developing economical, high-efficiency cooking stoves using firewood and technology for making inexpensive bricks for house construction, which are now being used fairly extensively.

But far more interesting to me personally was the visit to TIFR, where the Director showed us around. Dr Vikram Sarabhai, then Chairman of the Atomic Energy Commission, hosted a lunch for us. I sat next to Dr A S Rao, who played a crucial role in obtaining India's first sustained nuclear chain reaction in the nuclear reactor APSARA. He told me that Homi J Bhabha, then heading DAE, requested Sir John Cockroft to help obtain nuclear fuel elements, so that India also could build a chain reactor. Cockroft said that he would help Bhabha get the fuel elements, but would not be able to tell him how to build the reactor. Apparently, Bhabha said that we would be able to build it on our own in three years. Cockroft arranged to supply the fuel elements. On the day when the experiment was about to start, Dr Rao apparently told Bhabha that there was only one log flux meter, a crucial facility to ascertain when the sustained chain reaction started, and asked what he should do, if it were to fail. Bhabha told him that he should test the meter again and again, but the test must go on. After 48 hours of the experiment, the reactor finally went critical. But that was not all. When Cockroft visited Bhabha again, they compared notes about their experiments, and Cockroft told Bhabha that their experience was similar. It was an important lesson for all of us. It meant that if there was political support with financial backing, there was much that we could achieve in areas of high science, and high technology.

I visited all the departments of the IISc and held detailed discussions with many of the members of the faculty before preparing the draft report. I learned much and realized why IISc was so good. It was clearly due to the quality of its academic staff. The committee finally recommended that the government should sanction about Rs 17 crores (Rs 170 million). Apparently, only Rs 4.5 crores (Rs 45 million) was sanctioned. I was disappointed. I am glad that recently the government has sanctioned Rs 100 crores (Rs 1 billion) to this outstanding institution.

INTERNATIONAL AIRPORT FOR BANGALORE

Improving the air travel facilities in Bangalore was a long-felt need. Once, in the late 80's, Dr R Narasimha, my successor as the Director of NAL, and I were returning from Thiruvananthapuram after attending a meeting in ISRO. We noted how crowded the airport was and how badly it was designed. We noted that more often than not, Indian airports were obsolete even before they were opened for operations. There was no forward planning

in anticipation of future requirements. I proposed to Narasimha that we should do something about it, and that I would be glad if NAL were to formally obtain the approval of the Secretary, Ministry of Civil Aviation to study the issue, and that I would be glad to take on the responsibility of studying the problem. The then Secretary, Mr Ganesan, readily agreed.

As a prelude, I studied the plans of several international airports from all over the world. I also studied the air traffic growth pattern over the years in Bangalore and the potential increase in the national and international traffic in the years to come. It appeared to me that the design of the airport should be based upon a modular concept, with more and more arrival and departure gates with aerobridges being built to respond to increase in traffic as time goes on. I also felt that to reduce the traffic congestion near the airport, it should be channeled near the airport itself to different parts of the city, while providing adequate parking facilities. I used HAL airport in Bangalore as an example to sketch a modularly designed terminal.

As soon as the feasibility report was submitted, I got a telephone call from the then chairman of HAL, saying that they would not like civil operations to continue forever from their airport. I told him that ours was only a feasibility study about how to build terminals with potential scope for expansion. We submitted the study to the Karnataka government also. Concurrently, Mr Ramanathan, earlier Chairman of the International Airport Authority, also put up a proposal to the Karnataka government. Following these developments, the Ministry of Civil Aviation constituted a committee under his chairmanship with wide representation from the interested parties. The Chairman, HAL retired and another one took over. The new chairman took the view that civil operations need not be shifted. At that time, HAL was apparently earning about Rs 15 crores (Rs 150 million) per year as landing fees, certainly not a small sum. But it was too late. The decision to shift the operations had been made.

The Karnataka government offered land about 75 km from the city center. I could not visit the place. The rest of the committee members approved it. Clearly, they did not do their homework or appreciate the problems air travellers would face if the airport were to be located so far away. The block time for travel from city center to airport for travel to nearby cities like Chennai, would double; clearly not an acceptable solution. Mr Ramanathan then left for the US on a vacation and asked me to take over the responsibility of conducting the meetings in the interim. I met the then Chief Secretary of the Government of Karnataka and asked him whether the government would be willing to give alternate land and compensation, if the Air Force would be willing to give the Yelahanka Training Command Air Force Base to be developed as the international airport. He said that if the Air Force would agree, he would arrange for them to get as much land as they wanted, anywhere in Karnataka, and full compensation for the facilities they would be leaving behind. Rough estimates indicated that the government would have to pay about Rs 500 crores (Rs 5 billion) as compensation to the Air Force. It was clear to me that sooner or later, the Air Force would have to shift the operations of the Training Command from Yelahanka, as Bangalore by then had already started expanding rapidly in that direction. With the

assurance from the Chief Secretary, I called for a meeting of the committee in Delhi to examine this alternative. The approval was unanimous, with the Air Force representatives stating that while they agreed in principle, they had no authority to speak on behalf of the Air Force. The Air Force said, "We always take and never give". So ended the efforts to develop Yelahanka Training Command AFB as an international airport.

I then once again approached the Chief Secretary and briefed him about the development and sought an alternate location closer to the city, as the one offered was too far. Land near Devanahalli was offered as an alternate site. It is about 35 km from the city center. By that time, Mr Ramanathan returned, and we all went to see the place and concluded it would be a good alternative. Proximity to Yelahanka and the HAL airport would pose some air traffic control problems, but the committee felt that a common air traffic control for the three airports should be able to handle the problems. It was also recognized that a new satellite city of about 100,000 people would come up for setting up high-technology industry near the airport. It was also proposed that a six-lane highway as well as a train shuttle should be built to connect the airport with a city check-in terminal where passengers could check in and obtain their boarding passes. All these recommendations were apparently accepted.

While approving the airport, the government proposed that it should be self-supporting. In a sense, this was a tall order. The airport was expected to cost about Rs 1250 crores (Rs 12.5 billion) and that has been currently updated to Rs 1900 crores (Rs 19 billion). Estimates of earnings based on current landing charges and possible income from leasing space for commercial purposes suggest that they would not be adequate to cover the cost of operations and make the airport a self-supporting venture. Rough estimates indicate that to make the airport self-supporting, there would have to be about 1000 landings a day, based upon the current landing charges. Currently there are about 150. It is inevitable that to cover the cost of operations and running the airport, the operators will have to charge about Rs 500 per passenger, if the terminal operations are not subsidized. It remains to be seen what the airport operators will eventually do. Incidentally, it was also found that the HAL airport would be able to handle the traffic for the next 25 years or so. But it became a matter of prestige to have a separate international airport, and the politicians had their way. They also acquired twice the land (about 4500 acres) recommended by the committee for twin runway operations. It raises questions about the motives of politicians. At the time of writing, the developers have just started the work and hope to complete it by 2008. One would hope that an airport of truly international standards would eventually be built for Bangalore.

REVISING THE HOUSE NUMBERS IN THE CITY OF BANGALORE

Bangalore house numbers are not citizen- or service-friendly. If one were to have the number of a house in the city, he would not necessarily be able to find it, as the numbering is not sequential. For example, my house number is 659. The next is 660A, then 660, and then the next one is 284, with a road in between! This system is all too common in our city. I brought this matter to the attention of Prof Dhawan in 1980, and suggested that the Karnataka State Council for Science and Technology, of which he was the Vice-Chairman,

should look into this matter. He nominated me as the convener of a committee to look into the matter on behalf of KSCST.
I called for a meeting of all the interested parties from the city administration. The opinion was unanimous that the system should be revised. I explained the American system, where each street is given a specific name, and the house numbers have at least three digits, of which the last two are specific to the house, and the first digit (or first two) jumps by one, when a street intersects the street in question. Thus, if a house number is, for example, 800, the adjacent house on the same side would be 802, and the next would be 804, and so on. Correspondingly, the numbers on the opposite side of the same street would be 801, 803, 805, and so on. If then a street intersects with this street after 804, the house number on the same side of the street, would be 900 and then 902, 904, and so on. On the opposite side, the numbers would be 901, 903, 905, and so on. If there is another intersection after 904, the next number on the same side of the street would be 1000 and then 1002, 1004, etc.

The committee unanimously approved the introduction of the new system, placed the matter before the KSCST and obtained its approval. Rs 10 lakhs (Rs 1 million) were provided by the Karnataka government to undertake an aerial survey of the city to produce an up-to-date map of the city as a basis for revision of house numbers. Every BMP Commissioner approved the introduction. But they were not in their positions long enough to see that it was expeditiously introduced. It was during the time of Mr M R Srinivasa Murthy, IAS, as the Commissioner, that the project really got going, with his suggestion that it should be first demonstrated in Sadashiva Nagar, which had about 600 houses. These were illustrated on the aerial survey maps, which were up-dated by the Corporation through NRSA. The BMP decided to make this a part of the comprehensive data compilation for the city of Bangalore. I hope this revised house numbering will see the light of day, some day, and not remain a paper exercise. It would be of tremendous help to the citizens of Bangalore.

CHAPTER 5

THE STRUGGLE FOR ETHICS IN THE PRACTICE AND MANAGEMENT OF SCIENCE

In all my professional life, I felt deeply concerned about nurturing good R&D management practices. I felt a sense of satisfaction when I found that Dr Phil Francis, who worked with me in Douglas Aircraft Company 40 years ago, and subsequently became an expert in R&D management, dedicated his book on management and productivity to me along with one other person. No matter how good our scientists are, you can bring out the best in them only by building and developing good management practices. I feel that if people in senior positions of responsibility knowingly ignore, or deliberately indulge in, unethical practices, they will cause serious damage to the cause of science. Without a healthy code for the practice and management of science, there is not much hope of establishing strong foundations for science, on which crucially depends our ability to join the cadre of developed nations.

All my life here as an administrator, I argued for ethics in the practice and management of science, and clearly did not succeed. I spoke about it from various platforms and stressed its importance for a halfway decent chance to obtain a self-reliant and selfgenerating high-science, high-technology industry. I wrote letters to the science academies that they should take the lead and do something about it. As far as I know, they too did not pursue the matter vigorously. The Indian Academy of Sciences appointed a committee to look into the matter. Their findings were unexceptionable, in that they identified what needs to be done to maintain integrity in the practice of science. But they did not pay adequate attention to the management aspects of it. For example, in my letters to the Presidents of the science academies, I proposed that for election to the academies, their councils should stipulate compliance with an agreed code of ethics, and the scientists who propose names for election to the academies should confirm that the candidates whom they are proposing, do in fact comply with the code. The suggestion was received with thundering silence. No action has been taken so far to ensure that scientists who are to be elected as fellows of the Academy have an unblemished record in the manner in which they did their research.

The problem is complicated by the self-defeating system of promotion and assessment that prevails in the country. It does not necessarily encourage scientific integrity. Academic staff are judged on a system based on the number of publications and PhDs they produce. This itself tends to encourage compromise in the practice of science. It inevitably encourages the faculty to take on students who may not really be competent to do original research under the broad guidance of the thesis adviser. Since the promotion of the adviser is based on the number of PhD students he guided, the adviser not infrequently ends up doing the major portion of the research for award of the degree. He then tends to feel that it is perfectly appropriate to have his (or her) name included as a co-author of any papers resulting from the thesis, and not without some substance! All too often, a PhD thesis is

submitted to the university, claiming that it is an original contribution by the candidate to the advancement of knowledge. But as soon as the degree is awarded, its contents frequently appear as research publications with the thesis adviser as a co-author, thus inherently contradicting the original premise on which the degree is awarded. To avoid this contradiction some US universities award the doctor's degree based upon the contents of articles in journals of repute, under the candidate's own name. There are innumerable instances where the faculty thesis advisers, and even the heads of departments feel that it is their right to add on their names as co-authors to any paper published from their departments, and to which their contributions were nil. I am personally aware of instances where the names of the directors of research labs are routinely added as co-authors without the directors feeling any sense of shame.

The fact that such practices are prevalent in the developed countries also is clearly no excuse to condone them in our country. Instances of scientific misconduct are not always exposed and salutary action taken in time. But there are several instances abroad where, when peer groups expose issues of scientific misconduct, thorough investigations are conducted, and sometimes such scientists are dismissed. A recent example is the dismissal of a professor by MIT. Another is the dismissal of a professor from Korea, Hwang, who falsified data in stem cell research. The editorial in the March 2006 issue of Scientific American said, "It is important not to brush off the Hwang case as a fluke without considering its lessons for the future. For instance, Hwang's papers had many coauthors, few of whom seem to have been a party to the cover-ups. But what responsibilities should co-authors have for making sure that papers bearing their names are at least honest?" The editorial went on to say, "We should also think hard about (how) Hwang's deceit went undetected for months because so many scientists and science journalists wanted to believe that ESC (Embryonic Stem Cell) research was progressing rapidly … Extraordinary results need to be held suspect until confirmed independently. Hwang is guilty of raising false expectations, but too many of us held the ladder for him". In our country, tragically, the tendency more often than not is to indulge in a cover-up. I will examine later in some detail, a clear instance of scientific misconduct, with the management whose attention was drawn to the issue, indulging in a cover-up by stating without any scientific basis, that the scientist in question only committed mistakes, but did not indulge in scientific misconduct.

Some time ago, this issue of scientific misconduct attracted general attention in some newspapers. It was proposed by a reputed scientist that if a senior scientist/faculty member loans his equipment, bought from government funds, to another for research, his name should also be included as a co-author! In a response to this, I proposed that we need not stop there but make the secretary to the concerned department who sanctioned funds for buying the equipment, and for that matter, the minister himself, co-authors. Undoubtedly there are instances, as for example in fields like particle physics research, where papers are published with several co-authors, sometimes tens of them. I would suspect that though in a small way, such people also contributed in substance to the contents of the paper, either experimentally or theoretically. Against this background, it is dismaying to see vice-chancellors and directors of research establishments blithely having their names added as co-authors. When such instances of scientific misconduct attract public attention, these

gentlemen claim that they knew nothing about it, and their permission was not sought. The research laboratories under agencies like CSIR, DRDO, etc are clearly no exception.

There is urgency and a need to specifically introduce a code of ethics for the practice and management of science at all levels and in all institutions involved in teaching, research and management of science. This is not to say that there are no honest scientists in India. But they seem to feel that they themselves will not indulge in scientific misconduct, but that it is not their concern if others indulge in it. They are tarred with the same brush by the community, which cannot distinguish the dishonest scientist from the honest scientist. The purpose of peer group review of research papers is primarily to ensure that members of the scientific community do not indulge in unethical practices. There is therefore a moral obligation on the part of honest scientists to formally take a stand whenever they come across clear instances of scientific misconduct. Tragically, scientists who expose scientific misconduct tend to get criticized for their stand, instead of being complimented. It is a pity.

It is instructive to once again recall in this connection, the definition of scientific misconduct framed by a committee of scientists, and issued as a "presidential finding" by Mr Clinton. It is hard to improve upon. It reads, "Research misconduct is defined as 'fabrication, falsification, or plagiarism in proposing, performing, or reviewing research results. Fabrication is making up data or results, and recording or reporting them. Falsification is manipulating research results, equipment or processes, or changing or omitting data or results such that the result is not accurately represented in research record. Plagiarism is appropriation of another person's ideas, processes, results or words without giving appropriate credit. Research misconduct does not include honest errors or differences in interpretation". "Finding of research misconduct requires that there be a significant departure from the accepted practices of the relevant research community; and the misconduct be committed intentionally, or knowingly or recklessly; and the allegation be proven by preponderance of evidence". The finding further went on to state that when proven by preponderance of evidence, such scientists and the institutions to which they belong should be denied federal funds. Apparently, wide publicity is given to this presidential finding, and scientists receiving federal funding are required to sign a declaration accordingly. It is essential that we too introduce such a procedure for developing healthy traditions. It would seem that as of now, it is an uphill task to expect the present senior scientific community to actively fight for this cause. Asking them to take the initiative would be like "asking the goat to guard the cabbage patch", to quote a Russian proverb. Perhaps the basic principles in the methodology of science and the ethical principles that guide it, should be taught at the high school level itself.

While I took decisive action in NAL in all cases of scientific misconduct that I came to know of, I made a serious mistake in not formally defining scientific misconduct and requiring that all the S&T staff in NAL become signatories to an honor code. After I retired, Dr Bhogle of NAL drew my attention to an instance where apparently the head of a division took unfair advantage of the work of one of his staff members, to obtain his PhD degree. I was helpless. Nobody brought it to my attention while I was in NAL. One of the unfortunate

things that heads of institutions face is anonymous letters and sometimes, even signed letters. Two instances readily come to my mind. In one, a staff member gave me a letter making serious accusations against the Head of a Division, and desired that I should investigate it and take suitable action. I wanted him to put his signature, and he asked me the reason for it. I told him that if his accusations could not be established, I would charge-sheet him and take disciplinary action against him for making reckless accusations. He promptly left my office. A young research fellow once complained that his group leader, who knew nothing about his work, demanded that his name should be added as a co-author in a paper. I told him to leave his paper with me and called the concerned group leader and asked him to very broadly describe the contents of the paper. He was unable to do so. I then asked him whether he realized that if there were an error in the paper, his name would be tarnished. He said that he did not realize that. I told him that I was ashamed of his principles in such matters, and reprimanded him for his audacity in trying to take advantage as a co-author of a paper to which his contributions were nil.

I was glad that Prof Balram, the editor of Current Science, opened its pages to espouse the cause of ethics in science. One would hope that eventually, enough scientists occupying senior positions of responsibility would support and fight for it. Regrettably, it is not so at present. It would appear that as the Director of the IISc, he has not been able to formally implement the value systems of science, which he so eloquently argued, as a policy of the Institute which he now heads. As a member of the promotions and assessment committee of the Institute Council for over twelve years, I am aware of instances of scientific misconduct among the faculty. Prof A S Paintal founded the Society for Scientific Values, hopefully to cultivate this honour code and examine instances of scientific misconduct that are brought to its attention. I understand that they approached a government department with a proposal along the lines of the Clinton committee findings. It would appear that this government department was unwilling to take a formal stand in the matter of compliance with a formally defined code of ethics for the practice and management of science. I strongly believe that it is essential to establish an Office of Research Integrity under the Office of the Principal SA to the Government of India, as the ultimate appellate authority to evolve uniform policies to handle such matters.

In matters of science and its management, I have no permanent friends or permanent adversaries, and it is the scientific truth that is sacred to me. I differed with some of my closest friends in such matters, and stood vindicated. In the instances noted below in some detail, some people, hurting the cause of science and technology in NAL, and compromising our ability to obtain a measure of technological self-reliance in some crucial areas, deliberately ignored these values. Some very senior people indulged in these acts; and I suspect, not unknowingly, but deliberately. President Clinton's finding on scientific misconduct unconsciously became my guiding principle in such matters. To underline the issues involved, I would like to describe a couple of events with which I was personally familiar.

THE VISHVESHWARA CASE

Perhaps one of the most unpleasant issues I faced in all these years was the case of Prof C V Vishveshwara, then of the Raman Research Institute. He was assigned the responsibility on deputation, of building the planetarium in Bangalore. When he completed this task, he approached the Director of RRI, Prof V Radhakrishnan, and apparently expressed his desire to return. The Director told him in so many words, that he was not wanted, and he should have no difficulty in finding a job elsewhere by virtue of his record. Thereby started a series of extraordinary events in the annals of higher education in India.

Dr P N Shankar of NAL drew my attention to this issue. Services of full professors, particularly of those with long tenure, are never terminated, except possibly for moral turpitude. In the years I was associated with the IISc's Promotions and Assessment Committee (PAC), there was a standard instruction from the Director, Prof Satish Dhawan, that reviewing the performance of a full professor and renewal of his contract was to be treated as a formality. At the time Dr Shankar brought the matter to my attention, I had not met, or known Prof Vishveshwara. It was a matter of principle to me, that decisions for which precedents were well established, should not be violated. I signed the letter of representation against the virtual dismissal of Prof Vishveshwara by the Director of RRI, and also addressed a letter to the chairman of the RRI Research Council, Prof Satish Dhawan. I informed him that it was against the very policy he had laid down for reviewing the performance of full professors at the IISc, and that the decision of the Director of RRI violated well established principles. I immediately called my erstwhile NAL colleague Dr Ramaseshan, who was then a distinguished emeritus professor in RRI, and closely associated with its Director on various aspects of management, as well as a member of the RRI Research Council, and told him that what they were doing was utterly improper. He said that he would call on me. He never did. My letter to Prof Dhawan, calling his attention to the impropriety of their proposal received wide public circulation all over the country. I also wrote a long personal letter to Prof Dhawan giving reasons for my stand, and told him that if the RRI had any valid reasons, they should frame charges and charge-sheet Prof Vishveshwara, and conduct an inquiry to establish his guilt by preponderance of evidence. I told him that I would then fully support them in such a move. Prof Dhawan alluded to an instance where Vishveshwara apparently did not put up an advertisement notice for qualified people for a research fellowship abroad. Apparently an RRI junior staff member could have been an eligible candidate. I was surprised by this information and its basis for terminating the services of Prof Vishveshwara.

It was also implied that Vishveshwara did not have enough publications to his credit. His field of study, General Relativity and Gravitation, was not everybody's cup of tea. I looked into the number of publications listed in the then released annual report of RRI. It amounted to a grand total of 18 for the whole of RRI. I also recalled a casual comment Dr Ramaseshan had made when he was taking a visitor around, that in RRI, they did not consider publishing papers important! Perhaps it was not important for the RRI authorities that Vishveshwara was the only person till that time from the whole of Asia to have been a member of the International Council for General Relativity and Gravitation. The IISc faculty was up in arms about this development. Prof C N R Rao, then Director of IISc, was at that time in the

Raman Research Trust, the authority to which the RRI Council reports. Prof Mukunda from IISc called for a meeting in the IISc to discuss the issue. The opinion was unanimous that RRI was wrong in what it was trying to do. I recall a question from Prof Balram of the Molecular Biophysics Unit and currently the IISc Director. He asked me whether I would take the same stand if any other person were involved. I told him, most certainly; to me it was the principle that mattered, and not the individual.

The RRI council renewed the contract, but with the condition that Prof Vishveshwara should comply with all instructions issued by the Director, Prof Radhakrishnan! It shocked me, as the very foundation of academic freedom was trampled upon. It was a humiliating offer. Vishveshwara asked me if I would accept such an offer. I told him that I did not know what he would do, but if such an offer were made to me, I would not accept it. He said that he intended to appeal against it to the RRI Trust then headed by Prof M G K Menon, a close personal friend of Dhawan, Radhakrishnan and Ramaseshan. I felt that ethics in the practice and management of science were at a crossroads, and the issue called for a stand by all people who respected the cause of science. In fact, Prof C V Raman founded the Indian Academy of Sciences and the Current Science Association to further the cause of science, something that was holy to me. The appointment order violated the very cause of science, which Prof Raman desired to nurture by founding RRI and the Indian Academy of Sciences. As a matter of principle, I therefore sent my letter of resignation to the then President of the Academy, Prof C N R Rao, who was also an ex-officio member of the Trust, and released it to the press, where it attracted wide attention.

At the Trust meeting, apparently Prof Rao strongly argued against the humiliating letter of appointment. The Trust reversed the judgment of the RRI Council and reinstated Prof Vishveshwara without any conditions. My letter of resignation was rejected. Dr Dhawan called all the parties involved to a peace-making breakfast, so to say. I do not know to what extent it healed the animosity the issue had generated.

I regret that I had to take a stand against a person whom I revered all my life and looked upon as a person to emulate. More often than not while running NAL, when I had to take a critical policy decision, I used to ask myself, "Would Satish approve of it?" I have no doubt in my mind that my stand would be the same, if I were to face the same issue again. Perhaps Dr Dhawan's compulsion of friendship with Radhakrishnan and Ramaseshan compromised his duty of upholding the cause of science. By the decision of the Trust, my stand was vindicated. I was surprised by a brief telephone call I received from a close friend of these gentlemen who wanted to ease out Vishveshwara from RRI, complimenting me for the public stand I took in this case. It was not the most pleasant thing for me to take a stand against close friends, but in matters of science, I had no permanent friends or permanent adversaries. I had always felt that ethical principles in the practice and management of science, on which the progress of science and its offshoot, high technology, depends, must be protected at all cost.

Shortly thereafter, Vishveshwara called me and asked what he should do. I told him that he had taken a stand on a matter of principle and had been vindicated, and that it was time for him to move to some other place to have peace of mind. He agreed. I talked to Prof Ramnath

Cowsik, then heading the Indian Institute of Astrophysics, and suggested that he might consider having Vishveshwara on his staff. He told me that his council chairman, Prof B V Srikantan, had also suggested the same. Vishveshwara later joined the IIA, from where he retired. Vishu, as his friends and I call him, apparently tended to be an introvert in his professional work. It was not a crime, although it would perhaps help to be an extrovert to interact with others. In the following years, I attended several lectures by him. He is an extraordinarily good speaker. Looking back over the years, I asked myself if my views and decisions could have been, and should have been, any different in such matters. My answer was, NO. You cannot trample on the very ethical foundations on which the cause of science and its future in the country rest, and expect science to thrive.

EVENTS THAT LEFT LASTING AND UNPLEASANT MEMORIES

A MISTAKE OR SCIENTIFIC MISCONDUCT?

There was an allegation of scientific misconduct indulged in by Dr B K Parida, then heading the Structural Integrity Division of NAL. He had the primary responsibility as a manager of science, to ensure that scientific misconduct did not take place in his Division. It was all the more serious because the Head of the Division himself was alleged to have indulged in the misconduct. The events are as follows.

Dr R Sunder, who earlier headed this Division, showed me in early 1998, a fractograph of a striation pattern on a fracture surface, apparently obtained from a fatigue test by him and his colleague, Dr Raghu Prakash, in 1995 by using a particular fatigue load sequence. This fractograph, with an entirely different load sequence, was included in a paper presented by Parida at an ASTM International Conference held in San Diego, California, in November 1997. The paper essentially claimed that it would be possible through strain gauge measurements at the crack tip, to draw certain important conclusions about crack growth, and the striation pattern of crack growth as revealed from the fractographs would not be necessary to draw the same conclusions. If this were to be so, it would be an important development and a significant contribution to studies in crack propagation. I was involved in fatigue crack propagation studies during my professional research career, and naturally this hypothesis intrigued me.

After the conference, ASTM apparently decided to bring this paper out as a Special Technical Publication (STP) in view of its potential importance. This then became a serious matter, and not the less serious case of plagiarism, but an instance of deliberate falsification of data by Parida, if proven by preponderance of evidence. After studying the fractograph and the load sequence contained in Parida's San Diego paper, I had no doubt that the alleged fractograph could not have been obtained by the stated load sequence. But I always believed in the aphorism: "Trust but verify". While I trusted Sunder's integrity in such matters, I felt it necessary to try to reproduce independently, the alleged fractograph, claimed to have been obtained by Parida. I used an identical test specimen and his stated load sequence. It is important to note that if a different aluminum alloy were to be used,

but with an identical load sequence, the spacing widths of the striations may not be the same, but the number of striations themselves would digitally correspond exactly to the load sequence. Thus, if there were ten load sequences, there would have to be ten striations, and not nine or eleven. They are like the growth rings of a tree. There will be only one ring for each year, though their spacing width may vary depending on the weather. It is evident that cladding of the aluminum sheet metal does not change the number of striations, as compared to unclad sheets.

Reproducibility is the hallmark of scientific research. Thus, if a stated 'experimental result' could not be produced under stated 'identical conditions', the reported result cannot be true. I could not reproduce the results reported by Parida. If the paper came out as an STP, and if the falsification of data came out, as it would have undoubtedly, if others repeated the claimed tests as I did, the name of NAL, and for that matter of CSIR also, would have been mud. Then started the problems.

I brought the matter to the attention of Dr Prahlad, then Director of NAL, and suggested that he should look into the issue, because of the seriousness of the matter. I deliberately refrained from telling him that I had come to the conclusion that misconduct was indulged in only after I had got the exact reproducibility test conducted, and could not reproduce the reported results. I felt that he too should independently get the test conducted and draw his own conclusions. At his request, I had a long conversation with him one evening in the NAL guest house. I was shocked by his solution to the problem. He said he wanted the issue to be settled amicably, and that he would be willing to instruct Parida to include Sunder's name also as a co-author! I told him the issue was not one of including Sunder's name, but of establishing scientific facts. The issue of amicability and including Sunder's name was an administrative decision, and I did not come into the picture, and certainly did not hold a brief for Sunder. Most certainly, the issue was not one of plagiarism, but the far more serious case of falsification of test data. I felt it was Dr Prahlad's first cause as the Director of NAL to protect the reputation for scientific integrity of any research in NAL, when any instances of scientific misconduct were brought to his attention. Instead, he was talking of an amicable solution! I felt ashamed listening to him, and realized the level to which he was willing to bend scientific truth! Clearly, he was more concerned about getting along with people, rather than upholding the ethics and the culture of scientific practices in NAL. I then took up the matter with Dr Mashelkar, DGSIR. I explained to him the seriousness of the issue and suggested that he appoint a committee to look into the matter. He finally appointed a oneman committee under the chairmanship of Dr P Ramachandra Rao, the then Director of NML. It was only after Dr Mashelkar told me this that I revealed to him that I had got exact reproducibility tests conducted, and could not reproduce the results claimed by Parida in his San Diego paper.

As per the report that was submitted, Dr Ramachandra Rao called for a meeting on 11th May 1999, at which were present Parida and Sunder, who first raised the issue. After an extended discussion**,** Parida explicitly admitted that the load sequence contained in his San Diego paper would not be able to reproduce the fractograph contained in his paper. It was thus established beyond reasonable doubt that Parida, by his own admission, indulged in

scientific misconduct. Dr Ramachandra Rao should have closed the proceedings, but then apparently asked him, if the load sequence given in the paper could not produce the given striation pattern, what load sequence had he used to produce the pattern. On the spur of the moment, Parida apparently drew a load sequence, which he claimed to have actually used. This question was of no consequence, and in fact irrelevant for answering the crucial question of falsification of data, which Parida had already admitted explicitly!

Dr Ramachandra Rao then asked Parida and Sunder to give in sealed letters, the estimated number of hours to failure. Apparently Parida stated that the specimen would fail in about 5 to 6 hours, and Sunder stated that it would take about 15 hours or more for failure. I was informed that in the actual test, one specimen failed after about 15 hours and the second specimen did not fail even after 20 hours. About a year later, Dr Ramachandra Rao sent his report to Dr Mashelkar, stating that Parida's result was not reproducible. Another independent test (not to speak of mine) thus established that Parida's result was not reproducible, and Parida himself had already explicitly admitted that the load sequence given by him in his paper could not have produced the stated fractograph. These facts established beyond reasonable doubt and with preponderance of evidence that Parida had indulged in scientific misconduct, an inescapable conclusion.

The issue should have been closed then, but surprisingly, Dr Ramachandra Rao apparently recommended to Dr Mashelkar, the appointment of one more committee, after explicitly stating in his conclusions, "the only meaningful conclusion that can be drawn was that the results claimed by Dr Parida et al could not be reproduced under conditions agreed to". These were the conditions which Parida claimed to have actually used in his tests reported in his San Diego paper! This was the primary issue. It was therefore surprising that Dr Ramachandra Rao suggested, apparently of his own volition, the appointment of one more committee with wide latitude in the proposed terms of reference, which really had no bearing on the issue of falsification of data. These terms of reference did not oblige this committee to reestablish the reproducibility of data claimed by Parida, using the load sequence (claimed to have been used by him) in his paper.

This recommendation, to say the least, was surprising, after Parida had explicitly admitted that the fractograph in his paper could not be reproduced by the load sequence given in his paper, and not even by his changed load sequence! As noted below, the report of this new committee did not at all indicate that it undertook exact reproducibility tests to confirm or reject Dr Ramachandra Rao's finding. In fact, Parida was apparently given freedom to use any load sequence he wished, to produce the striation pattern he included in his San Diego paper. If he had really obtained the fractograph in his San Diego paper by himself, he should certainly have known the load sequence he had used earlier, and reproduced the fractograph in his San Diego paper. Identical load sequences produce identical striation patterns on the same material. It is possible to reproduce digitally accurate striation patterns in such tests. If the aluminum alloys used in such tests are clad or unclad, fully heat-treated or only partially, the width of the striations and their spacing might be different but not the number of striations. As stated already, if ten load sequences are applied, there would be ten striations, not nine or eleven.

Purely by studying the load sequence and the striation patterns in the fractographs, Dr P N Shankar, a fluid mechanics scientist from NAL, independently established the impossibility of producing the stated results. This he indicated in a letter to DGSIR. Dr P K Dash, an expert in fracture mechanics, also wrote an "anonymous" letter to Dr Prahlad, giving reasons for the impossibility of confirming the claims made by Parida. If Dr Prahlad believed in protecting the scientific integrity of NAL, he could easily have established the facts himself by independently getting the reproducibility tests conducted. This he did not do, and as the Director of NAL it was unfortunate that he did not. Apparently he did not care to protect the integrity of NAL in the practice and management of science, which was his primary responsibility as the Director. It would appear that he treated such concepts with contempt, as he wanted to get along with people.

Dr Mashelkar, as recommended by Dr Ramachandra Rao, appointed one more committee under the chairmanship of one Dr Kutumba Rao, at that time heading the Aluminium Research Center in Nagpur, and earlier apparently a student of Prof P Rama Rao, another metallurgist, in BHU. This committee was also given wide latitude in its terms of reference. These terms as stated already, had little to do with establishing whether or not Parida had indulged in scientific misconduct. In fact, the Kutumba Rao committee report indicated that the committee did not care to conduct the exact reproducibility tests. Instead, it permitted Parida to use any load sequence he wished, to show the relevant striation patterns.

When it was pointed out by Dr Raghu Prakash, who was present while the tests were being conducted, that the tests that Parida was conducting had nothing to do with establishing whether or not he had falsified data in his paper, Dr Kutumba Rao is reported to have stated to Raghu Prakash, "You short people are dangerous". Raghu Prakash was representing Sunder in supervising the tests being conducted by the Kutumba Rao committee, and his responsibility was to ensure that Parida was in fact conducting exact reproducibility tests as per the load sequence in his San Diego paper. It was clear, that Kutumba Rao was not particularly concerned about establishing facts relevant to the issue of reproducibility of the test results claimed by Parida. The conclusion was inescapable that the Kutumba Rao committee had deliberately indulged in, and was a party to, the cover-up.

This was the crux of the issue, and not striation spacing, rate of growth and what have you, or "any other matter" included in the terms of reference to the Kutumba Rao committee by Dr Mashelkar, DGSIR. After conducting completely irrelevant tests that had nothing to do with establishing exact reproducibility, under test conditions claimed by Parida in his paper, this committee concluded that Parida committed mistakes and did not indulge in falsification of data! I conclude that the Kutumba Rao committee was even guiltier of scientific misconduct than Parida. Dr Mashelkar had apparently sought the opinion of an expert about the Kutumba Rao committee's findings. Dr Mashelkar in his finding stated that this expert said that Kutumba Rao's committee did an outstanding job, and that the report may be accepted. The Kutumba Rao committee report included the Dr Ramachandra Rao committee report also as an annexure. Had this expert cared to read this annexure, and noted that the Kutumba Rao committee did not conduct exact reproducibility tests, he

would have been obliged to reject the findings of the Kutumba Rao committee report, and declare them irrelevant. By not doing so, this so-called expert, knowingly or unknowingly, and willingly or unwillingly, also became a party to this sordid cover-up. It was not difficult for me to guess who this expert could possibly be.

Dr Mashelkar accepted the report, apparently based on the expert's recommendation, and ordered that Parida be removed from the position of Head of the Structural Integrity Division. If Parida had made only mistakes and had not indulged in scientific misconduct, strictly speaking, it was unfair to remove him from the post of Head of the Division, as honest mistakes are certainly possible in scientific research. Dr Prahlad made him his adviser, and never filled up the vacancy! So much for Dr Prahlad's commitment to protecting integrity in the practice and management of science in NAL!

A reporter from Asian Age, getting to know of this sordid affair, came to see me. I related the incontrovertible facts based on tests that I got conducted, and independently confirmed by the Ramachandra Rao committee. The paper reported them. Apparently Parida and some other scientists from NAL met the local editor, and told him blatant lies about the matter. The editor did not care to investigate the veracity of the so-called facts given to him by these 'scientists'. These were printed and their rebuttal was not published. So much for the journalistic credibility of the Asian Age. Incidentally, over the years, I wrote many articles of general interest and sent them for possible publication, or reacted to articles that appeared in the press. But I never approached the press on my own to talk about any developments in NAL. It was the press that approached me and not the other way around.

I would have been deeply concerned if any staff member of NAL had indulged in such scientifically unethical practices in my time. When such instances were brought to my attention, I never hesitated to make a thorough investigation. If this incident had occurred in my time, I would have recommended Parida's dismissal without a second thought, after establishing facts. What was at stake was the professional integrity of NAL and protecting the confidence reposed in it by its customers such as the Air Force, its valuable client. Why should the Air Force now believe any results NAL submits to it, if its senior management itself does not take adequate precautions to maintain integrity in its scientific investigations and instead knowingly indulges in cover-up?

So ended this sordid tale of scientific misconduct. It would not be wrong to state that all those who played a role in declaring that Parida committed only mistakes and did not indulge in scientific misconduct, themselves also indulged in scientific misconduct. Parida did it in practice, and the rest did it by defending him blindly without ascertaining or establishing facts. So much for their respect for the culture of science! I was astounded to see a letter from Dr Bhogle to Dr Shankar (both in NAL) about the matter, virtually accusing me of irresponsibility for the stand I took. My stand was based on irrefutable facts, obtained by me from exact reproducibility tests, something that none of the investigating committees did. I regret to state that their stand had no basis whatsoever on established facts relevant to the issue. In matters of science I always remember the aphorism "if you don't get facts, facts will get you". All these gentlemen had let down the cause of science.

The ultimate arbiter in such matters is one's conscience. I hope the scientists who defended Parida without really ascertaining the facts and got involved in this sordid matter, are clear in their conscience. Over the years, I thought a great deal about this issue, because of its ramifications for the cause of science. I have no reason to change my view. NAL and CSIR would have been respected more if they had taken a public stand on this matter of scientific misconduct by one of their senior staff members, instead of covering it up. In the case of scientific misconduct by Hwang in stem cell research, South Korea did not cover up the misconduct but exposed it. CSIR/NAL too could have set new standards and a code of conduct for others to emulate. If the ASTM had published Parida's paper as a Special Technical Publication (STP), and somebody else could not reproduce the reported results, what would have happened to the reputation of NAL/CSIR?

There was an unfortunate aftermath to this sad tale. Shortly after this event, I decided to withdraw from any formal association with NAL by vacating the office that had been assigned to me a long time ago, and wrote a farewell letter to my erstwhile colleagues, whom I had tried to nurture, and in whom I had tried to inculcate the culture of science. Apparently Dr Prahlad, the Director, placed my letter as an item for discussion at a meeting with some of them. It would seem that a view was expressed that I should not have taken a position against Parida. Not one of them came to talk to me or cared to independently get exact reproducibility tests conducted before they took their stand. Incidentally, none of them knew anything in this field, nor did anyone seem to have made any effort, as Dr Shankar had done, to draw any logical conclusions based on their own studies of Parida's paper. Becoming a party to such cover-up of misconduct can only encourage others also in the same institution to indulge in similar practices. I feel sad to see the degeneration of standards of scientific integrity in NAL/CSIR.

It would appear that some of the NAL scientists felt that Sunder was not above board, and that he had also indulged in unethical practices, and that he took away equipment from NAL. The facts, as I knew, were different. Parida wrote to me to say that Sunder indulged in an instance of scientific misconduct in a paper published by him, and wanted me to investigate him. I asked Parida what exactly was the scientific misconduct indulged in by Sunder so that I could look into it. He could not pinpoint the alleged misconduct. Some staff also complained that Dr Sunder was abstaining from NAL to build his own company. I looked into the matter and asked Sunder for facts. Apparently Sunder wished to take advantage of a CSIR provision for its scientists to take three years' entrepreneurial leave to set up a company of their own to exploit any development they made while in service, and return if they did not succeed. As I understand it from Sunder, he wanted to take advantage of this provision to start a company of his own, to exploit the know-how he had developed in improving the performance of fatigue testing machines, and submitted his leave application to Dr Narasimha, the then Director. Apparently, Dr Narasimha told him that as he would be leaving shortly, it would be more appropriate for his successor to handle the issue. As far as I am aware, the rules provided for such leave, and it had not been rejected till then. Assuming that he would get it, Sunder, after applying for leave, went ahead to start his own company under the provisions of applicable CSIR rules. Apparently, Dr K N

Raju, the then Director, was reluctant to forward his application to the CSIR for approval, saying that during his service, nobody was going to leave NAL, even if it was a CSIR policy to encourage its staff to exploit their experience this way. Apparently CSIR rejected his leave application, saying that the technology he developed was in the public domain! Sunder then resigned from NAL to build his own company based on the technology developed by him while in NAL and duly licensed from NAL. To the best of my knowledge, he had no competitors in the country, even if the know-how he developed was in the public domain. I note Sunder has also been exporting his equipment abroad. As far as I know he is the only one from NAL who has done so.

Some time thereafter, Dr Raghu Prakash, who worked with him in NAL, and whose position had not been confirmed, decided to leave, and join Dr Sunder. As per the rules, staff whose positions are not confirmed, are not obliged to give notice of their intent to quit. In spite of it, Raghu Prakash apparently gave two months' notice. At the end of two months he quit NAL. Prahlad issued a show cause notice, and instructed the State Bank of India, NAL branch, to withhold payment of his salary, claiming that he had not accounted for funds given to him for buying some equipment for the Division. Raghu Prakash informed me that he had rendered full account for the money advanced to him. The Controller of Administration confirmed the same to me personally, when I talked to her. Raghu Prakash came to know about this affair only when the bank rejected a cheque he had issued, for want of funds to honour it. Dr Prahlad appointed a committee to do stock verification in the Structural Integrity Division, as it was reported to him that some of the equipment issued to Raghu Prakash was missing. One piece of equipment which was reported missing was apparently found in Parida's possession. So much for the accusations leveled against Drs Sunder and Raghu Prakash.

Incidentally, the SBI violated its well established rules to comply with the 'order' of Dr Prahlad, and deducted more funds than they were legally entitled to debit from Raghu Prakash's account for excess salary payment, and that too, without informing him. When I brought the matter to the attention of the Senior General Manager of the SBI, he promptly ordered the NAL branch manager to credit to Raghu Prakash's account the amount illegally debited, stating that to indulge in such practices was an aberration at working levels. Prahlad should have done his homework before he gave his order to the bank, and the bank manager should have functioned in a more responsible manner, rather than blindly following the instructions of Dr Prahlad as the Director of NAL. Clearly, Dr Prahlad was not his boss, only a customer.

I was mistaken in assuming that good traditions endure. It is hard to believe that the NAL management, which indulged in such practices, was not aware of the implications of what it was doing. I have now sadly come to realize that good traditions will endure only as long as the people who believe in them conscientiously practice and nurture them. This has apparently not been the case in NAL, in this instance. And the cause of scientific research in our country depends on such scientists and such institutions, and their agencies!

Some time ago, I was invited on the spur of the moment to address some recently recruited staff of CSIR in Chennai. In my talk I stressed the importance of upholding ethics in the practice and management of science, and the impropriety of taking credit for work done by others. I had a rude awakening. One of them said, "It is easy for you to say it. Our seniors insist that their names be added as co-authors in papers and reports to which their contributions were nil". I did not know what to say. It is a fact that many Directors and heads of S&T divisions across the country (not only in CSIR) insist that their names be included as co-authors of papers about whose contents they are ignorant. Such is the prevailing culture of research in our country. The cancer of scientific misconduct would seem to have penetrated very deep into the innards of the Indian scientific community.

THE CASE OF SARAS

"For successful technology, reality must take precedence over public relations, for nature cannot be fooled"

So said Richard Feynman, the Nobel Laureate, commenting on the reasons for the catastrophic failure of the Challenger shuttle. People down the line apparently knew that if the shuttle was launched on a cold day, the O-rings that seal the joints between stages might not have enough flexibility to function properly. Apparently, they brought the matter to the attention of their immediate supervisors. But in the anxiety to launch 'on schedule', the management ignored this fact, resulting in a catastrophe killing all the astronauts. Designing of sophisticated systems is unforgiving. Furthermore, while a research scientist is trained to look into a problem in finer and finer detail, the design of systems demands synthesis of knowledge relevant to the design of the hardware and knowledge of where to draw the line from the standpoint of safety and commercial viability.

NAL has the reputation of being an outstanding research organization, but it had no experience in design synthesis. It had to learn this the hard way, when it took on the responsibility for the design and development of the Light Transport Aircraft (LTA), SARAS. In such matters there is no substitute for experience. NAL did not have any prior experience in design of such aircraft, although it had recruited some people with design experience from HAL, who were apparently sidelined by the management from this program. Typically, an engineer with a feel for design has to spend about 15 to 20 years learning on the job, before he can take on independent responsibility for the design of sophisticated hardware such as the Light Transport Aircraft, SARAS.

It is also instructive to recall what von Karman, the high priest of aeronautics, said a long time ago: "Good decisions come from experience, and experience comes from bad decisions". It would appear that in their anxiety to show that they were successful in designing SARAS, the management put aside the basic reason for developing it, namely to develop a commercially viable passenger aircraft. To fly an aircraft that would not be able to carry the design payload and declare that it was successfully flown is stretching the definition of success a little too far. Well before SARAS flew, it was known to many in

NAL that the aircraft was overweight and required substantial and expensive changes to make it commercially viable. They were not willing to face the truth and take corrective action before it was too late. We need to know what went wrong in the design and development of SARAS, so that NAL will not repeat similar mistakes. The general attitude would seem to be to "somehow fly the aircraft and make appropriate changes later on, and get money from the government for changes". Apparently they did get additional funds from the government. But it did not seem to bother the decision-makers in NAL that such an approach could virtually make the LTA commercially unviable, with the inevitably high amortization costs spread on each aircraft, if it ever goes into production.

I had earlier mentioned some of the events leading to NAL taking up the development of SARAS, a 9- to 14-passenger aircraft intended for commercial aviation. The project was mooted during the time of Dr Narasimha as the Director of NAL. NAL has been utterly dependent on other client organizations, notably Defence, to make its work purposeful, and become a desirable input downstream. This has not been easy, in spite of having established itself over the years as a first-rate research organization. But that meant nothing, if it could not materially contribute something tangible. DRDO has captive clients in the armed forces. NAL has none. This was the situation that Dr Dhawan as Chairman of the NAL Research Council had hoped to resolve by making NAL take up a project that had commercial potential, to make it less dependent on the military aircraft industry and obtain a measure of control over its destinies.

It was evident that the development of any commercial aircraft in the class of Boeing 737 or even Avro 748 was beyond the reach of our country, let alone of NAL, because of the high cost of development and the limited international market due to foreign competition. It was this that prompted the late Raj Mahindra to study the design of a fourth (feeder) level aircraft as a possible commercially viable venture, after he resigned from ADA and the LCA program.

Dr Dhawan got to know what Raj was doing, and recognizing its commercial potential, brought the matter to the attention of Dr Narasimha. He too could see the potential, and invited Raj to join NAL as a consultant, and take up the feasibility studies of what eventually turned into a 9- to 14-passenger aircraft. Apparently the Russians were also interested in this class of aircraft, as they felt that they too needed such an aircraft in large numbers, and a joint project would be mutually beneficial to both the countries. The idea apparently was that the MDB of Russia would handle the design and development of the fuselage while NAL would take up the responsibility for the wing. Apparently Dr Dhawan tried to impress upon the powers that be in Delhi to sanction the project. He did not succeed, and gave up hope of ever realizing the dream.

Meanwhile, studies went on, although the Russians later withdrew. Market studies by a private company apparently indicated a demand for about 250 aircraft of this size, with a potential demand from abroad also. When things looked hopeful, Dr Narasimha wanted to formally name the aircraft to emphasize that it was going to be not simply a feasibility study, but a definite program for building it. He told me that he wanted to name it SAURUS,

after the bird. I suggested it should be spelt SARAS so that in the event it was not successful, people would not say NAL tried to build a dinosaur. He readily agreed to name it SARAS.

The project was finally sanctioned only during Dr Prahlad's time as Director, NAL. Apparently it was the first time any project of CSIR had to receive cabinet approval, because of its financial implications. It was the first CSIR project of such magnitude and high visibility. Well before the project was approved at a cost of about Rs 135 crores (Rs 1.35 billion), Raj Mahindra, who had hands-on experience in designing aircraft and did extensive feasibility studies of the LTA, died suddenly of a heart attack in Delhi while on his way to Kanpur, for holding discussions with the HAL on LTA development and production there. He was the chief architect of the LTA while he was alive, and the project definition did not come to a stage where simply giving and placing orders would ensure the success of the program. The hands of an experienced person to steer the program were no longer available. The responsibility to steer the program was then taken over by Dr Prahlad. His earlier responsibility in ADA in the LCA program apparently did not call for hands-on experience in the design of aircraft.

After the project was sanctioned, others handled the bulk of the fabrication of the airframe, while the design responsibility was largely handled by former HAL staff on contract to NAL through NALTECH. These people had experience in the design and development of military aircraft, which apparently get to be designed in a conservative manner, because of the potentially high g's they are expected to experience in combat. It would appear that these designers designed the LTA airframe more as a fighter aircraft airframe, than that of a commercially viable passenger aircraft. It is to be noted that every kilogram of excess weight would mean at least that much less of payload/range. The weight control is of utmost consequence in the development of commercial aircraft. Even the weight of the paint has to be carefully considered.

For example, I noted while I was associated with the Douglas Aircraft Company in the US in the early sixties, that in their design section, the weight control group had the last word, before any metal was cut. Every pound of weight of all components that would go into the aircraft was carefully taken note of, as it would directly affect the payload that the aircraft would be expected to carry, or reduce the range. Apparently, such a weight control group was constituted for the LTA program also, but it did not seem to have had any significant role to play in weight control. It would seem that as in the Challenger program, people down the line knew that the weight of the LTA had gone out of control, when it was weighed about six months before the first flight, but felt that they could do nothing about it, as it was considered more important to somehow fly the aircraft, even if it were to carry no passengers, just so that it could be declared that SARAS had flown successfully. Apparently, the expectation was that if somehow the LTA could be flown, money would start pouring in, and so no attempts were made to control its weight.

It would appear that the DGSIR was not informed of this weight problem. So to say, public relations took precedence over the realities of developing successful technology. As of

now, LTA has flown a few times. But it is nowhere near becoming a commercially viable civil aircraft without substantial changes. I understand that the weight of the structure went up by about 1000 kg. For a 14-passenger aircraft, the weight of the passengers would be of the same order. This means that the LTA will not be able carry any passengers. As it stands, it is not commercially viable. Trying to reduce the weight by using materials such as carbon fibre composites in significant quantities would make it very expensive, thus making it commercially even less attractive. Major changes costing several tens of crores of rupees would be required to make it commercially viable. It is not certain that many SARAS aircraft can be sold as commercially attractive passenger planes, what with tough foreign competition.

In the western world, before such projects are mounted, an enormous amount of marketing effort is made, and enough orders are obtained to ensure commercial success. This is because money is borrowed to build such aircraft, or the companies spend their own funds. Recouping them through sales gets priority in decision-making. This has not been the case in the LTA program. Government funding was made available, and also apparently a government loan of Rs 50 crores (Rs 500 million). According to press reports, a senior staff member of the team commented at a conference that commercial viability was not given enough importance while designing the SARAS, although it was the primary justification for launching the project. Attention of the senior management was drawn to the importance of weight control and of weighing the components as and when they were being delivered, by one of the NAL scientists fully knowledgeable about the issue. It would appear that no attention was paid to his views. The stand taken was 'nothing can be done now'. The few people who were knowledgeable about the design of aircraft and had hands-on experience left or were eased out. They had been in NAL for a long time and later were consultants to the lab. Some of these people were so good, that in my time at NAL, Lockheed asked me to release people like Dr Shiva Kumara Swamy to work in their company in the US. So to say, 'public relations took precedence over reality, with an effort to fool nature'. It was even reported that the Air Force would be interested in ordering six SARAS aircraft. It is to be noted that DGCA type certification and approval is required before the Air Force could place such orders.

After it became evident that SARAS will not be commercially viable, it would appear that renewed efforts were being made to see if the aircraft could be made commercially viable by carrying out substantial modifications, such as increasing the passenger capacity by extending the fuselage length, increasing the wing area, use of carbon fibre composites, changing the engine and the gear box, etc with an additional sanction of about Rs 90 crores (Rs 900 million). For practical purposes, this will be an entirely new aircraft.

But the fundamental question remains. Should these modifications be made? The answer is yes. If the program fails, it will be a serious setback for NAL, not to speak of CSIR. The senior management handling the program has clearly failed. The program required inspired leadership, and not blindly setting deadlines and expecting results. I find it hard to believe that the senior management expected the first flight of LTA in about two years. Only people who were unaware or ignorant of what is involved in the development of such an aircraft

could have set or accepted such a deadline. Considering that NAL had no experience in building aircraft, about five years or more would have been more appropriate. It would seem that here again, public relations took precedence over ground realities.

NAL has a long way to go before it can successfully develop the LTA and obtain DGCA clearance. It is essential that it succeeds. In the public perception, the LTA is an NAL program, and its Director is totally responsible for its success or failure. Clearly, the reputation of NAL and its ability to obtain a measure of control over its destinies greatly depends on the success of this program. It is NAL, which will eventually be held responsible and accountable. Director, NAL should therefore have the ultimate authority and the last word on LTA's progress. But apparently this is not so. I understand that the DGSIR delegated full authority to Dr Prahlad to handle the program. It is not clear what his responsibility would be, if the program does not succeed. So to say, success has a thousand fathers, and failure is an orphan. These problems demand an in-depth study by the DGSIR, if the LTA program has to have a halfway decent chance of success. The fact is that the present DGSIR and Director, NAL may change, but CSIR and NAL will continue, and will have to take tough, and even unpleasant, decisions.

In the Douglas Aircraft Company, whenever a new aircraft project was launched, an integrated team was constituted by drawing S&T people from the various specialist groups, such as aerodynamics, structures, materials, propulsion, systems, etc. They remained with the program till the type certification was completed and production drawings were released. The team reported to the chief designer in charge of the project. It was only after the aircraft went into production that the various specialists in the team reverted to their respective groups in the design bureau, etc. In essence, capability rested with the specialists in the company, and as and when required, integrated teams were constituted to handle an aircraft design and development program. It is not clear if NAL followed this system or had to refer the assignment of tasks to the heads of already existing divisions in NAL. For example, as the DGADA, I constituted such an integrated team for the development of the LCA, and the team was expected to report to Mahindra, who was in charge of development of the LCA. I recall an instance when an NAL staff member who was assigned to the team desired to revert back to NAL. I had to tell him that he should approach Mahindra for permission to be relieved. It would have played havoc if I had interfered in this matter, as the responsibility to deliver the goods rested with Mahindra, and he should have the last word in such matters.

It would be instructive in this connection, to study how Embraer Aircraft Company of Brazil made inroads into commercial aviation in the world at large. This company too started its life by manufacturing aircraft under license from the US. The Brazilian Air Force was running a school of aeronautics in San Jose dos Campos, where the students were being taught aircraft design by Ozires Silva. Years later, I found that he too was a Caltech graduate like me. In an informal chat there in 1975, he told me that he was informed by his bosses in the Air Force that simply designing an aircraft on paper would not be sufficient, "he should take his team to the aircraft company, and build it there to understand what it meant to build such an aircraft". The result was Bandeirante, in which I flew from Rio to

San Jose dos Campos. It was very similar to SARAS, but was already flying as a commercial aircraft as early as 1975. It is interesting to note that the Brazilian aircraft industry was started much later than ours, but that they made significant inroads into the world commercial aviation sector. I understand that Embraer even sold some commercial aircraft for VIP operations to our Air Force. Brazil is a very big country, and its government decided that commercial aviation has an important role to play in connecting distant places and in agriculture. One of the results was the Embraer aircraft development, with its penetration into the international markets. Such has not been the policy of our government.

SOME EMBARRASSING INCIDENTS

The chief executive of an R&D institution must have a clear vision for his institution and take measures to implement it. It has been my experience that it is difficult to build a good institution, but easy to destroy it by taking wrong decisions, unintentionally, or by not having the courage to take tough decisions, or by not taking crucial decisions just so as to get along with people. No chief executive takes decisions with the intention of destroying the institution that he heads. More often than not, it is a case of not applying his mind to think through the long-term consequences of his decisions and the precedents that he would be setting, unwittingly, and which would compromise and hurt the institution's future and its image. I found this to be the case not only in NAL, but also in several institutions with which I have been involved. I think there is a necessity to probe candidates for the position of chief executive about how they would run their institutions, were they to be selected. I had in fact proposed to one of the DGSIRs that apart from the scientific experts in the relevant fields, it would be desirable to include a management expert also in the selection committee. The suggestion was ignored. If a potential candidate for chief executive does not have a vision for the future of the institution, and wishes merely to maintain the status quo, he will inevitably destroy whatever is good in the functioning of that institution.

In all the years that I was associated with NAL/NALTECH, nothing hurt me as much as an incident that made me feel humiliated due to no fault of mine. It had to do with the issue of a gate pass to me after my retirement. Dr Prahlad as Director of NAL was kind enough to provide me with an open-dated gate pass which made it possible for me to enter NAL without signing the entry register. The pass indicated that I was associated with NALTECH. When I resigned from NALTECH, I felt it was improper for me to use that pass. So I requested the then Director, Dr Ramachandra Pai, to issue me another open-dated gate pass, which did not mention my association with NALTECH. Instead, I was issued a pass that expired by the end of 2003. Dr Pai told me that as he was retiring by that time, he could not issue me a pass valid beyond that date. The fact of the matter was that he retired later. And he refused to reconsider my request. From then on I had no alternative but to sign the gate register whenever I entered the NAL campus, much to the embarrassment of the security staff. But I did not wish to violate the prevailing rules. I had known Dr Ramachandra Pai from my IIT Madras days, when he was still a student there. He had in fact asked me for a letter of recommendation, when he was applying for a foreign scholarship, which I gladly gave. I considered him to be a good scientist and guaranteed

him an opening in NAL if ever he wanted to return to India. He did join NAL as an Assistant Director in the Propulsion Division, and eventually became the Director. I most certainly did not expect any out of turn favour from him, but only a routine re-issue of the pass, based on a precedent already set by his predecessor. To this day I cannot understand the reason for his refusal, as a precedent already existed for it, and I was not a security risk! I do not know what role his Adviser, Management & Administration (M&A), Dr R V Krishnan, played in this matter. But the ultimate responsibility for the decision was his and his alone. I do not believe there was any malice in his decision, but a certain lack of appreciation of the implications of his decision. Dr Pai's successor as Director of NAL, Dr Upadhya, was gracious enough to issue an open-dated pass, when the matter was brought to his attention by Dr Nayak, his Adviser (M&A). I am thankful to them.

There is a convention in the CSIR labs, that if a former Director of any CSIR lab happens to visit a city where a CSIR lab is located, the lab extends to him the courtesy of providing transportation, on request. A former Director of a CSIR lab was visiting Bangalore and he and his wife were staying with me. Another former CSIR Director invited him to lunch. As I had no car of my own, I requested NAL to provide transport, on behalf of the visiting Director. To my utter surprise, I was informed that the visiting Director should apply on his own to Dr Pai, the NAL Director! Presumably, Dr Pai could not take cognizance of my request on behalf of the visiting Director! The visiting Director was very embarrassed, and so was I, for this lack of common decency in responding to such a request. After my formal disassociation from NAL/NALTECH, I made it a point to go by auto to the NAL gate, and then walk towards the administrative campus gate to sign the register. Therefore, it is inconceivable that I would take advantage of the visit of another former CSIR Director to procure NAL transport!

In another instance brought to my attention, rules say that if a staff member owns a home in his/her place of work, he/she is not entitled to staff quarters. Accordingly, Dr Pai ordered one of his staff members to vacate the staff quarters. Two former senior staff members of NAL who are her relatives and lived with her, and who continued to work in NAL after retirement, also had to vacate these staff quarters. They are outstanding scientists and routinely used to work long after the formal working hours. But I imagine rules are rules in such matters and should not be violated. But it so happened that there was another scientist in exactly the same situation, staying in staff quarters while his own house was within a few hundred yards of the campus. But he was permitted to stay! I did not know what to make out of it.

When heads of establishments indulge in arbitrary decisions, their staff lose confidence in them, and such heads attract unstated contempt from their staff. Such incidents only go to show that not all good scientists are necessarily good managers of science. We have to always remember that when we spend public money, we are trustees for public good, and that we cannot betray that trust by taking arbitrary and unhealthy decisions. It was not that I did not make mistakes in running NAL. The last thing I normally used to do before going to sleep was to ask myself whether I had unintentionally hurt anybody that day. If I found that I had done so, I invariably called that person to my office the following morning and

conveyed my apologies. I did that even to the gardeners and office boys. I feel it is better to accept your mistakes and apologize. I have no pride in such matters.

Chapter 6

CONCLUSIONS

About four decades of active involvement in aeronautics in the country, and efforts to make the scientific community aware of the importance of complying with a code of ethics for the practice and management of science, have been eventful years for me. Looking back, I tried my best to see if a formally integrated structure along the lines of Space and Atomic Energy could be built for aeronautics also. I failed. But I feel it is better to try and fail than not try at all. I find that it is easier to create a new department of the government rather than restructure the existing ones. The vested interests are too powerful, and there is no accountability in the existing system. The serving bureaucrats stoutly resist any changes that demand accountability or compromise the extent of their authority, even if they do not understand the full implication of their decisions. Perhaps it would not matter, but for the fact that aeronautics is a field which we should be able to call our own. In fact, more so than Space, as our ability to defend our skies in times of war cannot, and should not, be mortgaged to other countries. This is what is happening now, with a preponderance of production of military aircraft under license. The government needs to ponder over this problem at the highest levels, and decide if this is the way to build even a measure of self-reliance in this crucial field, for a country of our size and scope.

To make millions of scientists in the country consciously accept a code of ethics for the practice and management of science would appear to be even more difficult. I have tried it ever since I returned home in 1964, but without success. The fact of the matter is that in discussions, hundreds of them agree on the need for an honour code, but even the science and engineering academies are unwilling to stipulate compliance with such a code as a requirement for election to their academies. I raised the issue with them, but they seem to have buried it by referring it to a one-man committee. As mentioned already, the report of the committee constituted by the Indian Academy of Sciences, which was recently released to its fellows, is unexceptionable in its findings. The report has laid out detailed procedures for investigation of scientific misconduct indulged in by its fellows, but is unwilling to frame a formal code of ethics (along the lines of Clinton's "presidential finding", for example) and make compliance a precondition for election to the Academy. I am personally aware of many instances of scientific misconduct by fellows of the academies, as indeed by so many others. My experience suggests that it is difficult to take any action in such matters along the lines suggested by the Indian Academy of Sciences. It would seem the aphorism 'Prevention is better than cure' is equally valid in science, and we should start instructing students of science, at an early and impressionable age, on the importance of following an ethical code. Perhaps a chapter should be included in the books prescribed for school students, to inculcate in them the value systems of science.

After arguing for this cause actively for almost three decades, I do not feel hopeful that the present senior scientific community will ever take the initiative to introduce healthy

practices. I suspect that many of them indulged in unethical practices themselves. Asking them to do something about it would be like "asking the goat to guard the cabbage patch", to quote the Russian proverb once again. Or to quote another Russian proverb, "fish start rotting from the head". I made it a point to instruct my colleagues in NAL who participated in various investigating committees, not to compromise scientific integrity in their work, and to always speak the truth and let the chips fall where they may. This helped NAL to establish in my time its reputation for uncompromising commitment to scientific integrity.

Without such a code as a living principle, there is not much hope of establishing in our country strong foundations for science and its offspring, modern high science and hightechnology industry. But this is essential if we want to join the cadre of developed nations in our own right. Not all the foreign direct investment (FDI) in the world will ensure this. Such investments are made to take advantage of the lower manpower costs in our country. When all is said and done, it is to be noted that foreign countries invest in our country for their own health, and to protect their interests, not ours. The tremendous contribution made by the migrant Indian professional community in the developed world is an eloquent testimony to our inherent capability. It is this capability that the Government of India needs to tap, as a small country like South Korea did. What is required is not simply funding diverse R&D programs proposed by scientists and technologists, but integrated planning, which is singularly absent at present.

The government should seek answers to the fundamental questions: Where are we now? Where do we want to go? Why do we want to go there? And finally, how do we want to get there? In the major S&T sectors of our economy, there is an urgent need for supporting more and more mission-oriented programs, and not simply giving grants-inaid in response to the research projects submitted by the scientific community. This is hard work for the government agencies, and also demands accountability from them. But there is no alternative to it, if spending by the government is to yield some desirable results. There is certainly a need for more basic research in the country. But even in countries like the US, no more than 15 to 20% of federal funds are given for open-ended and basic research. The balance of the funds is given for supporting applied research and technology development in the academic and R&D institutions, and the industry. If past experience is any indication, simply investing thousands of crores of rupees to build new S&T institutions for open-ended research, as is now being proposed by some high-level committees, and granting hundreds of crores of rupees as grants-in-aid, most certainly will not achieve these objectives.

It is hardheaded applied research and technology development to support industry that builds the technological base of a country, not basic research. Regrettably, this is missing in our country, and much of the industrial production is based on imported know-how. The suppliers of such know-how do not reveal crucial information that would help us stand on our own feet. Regrettably, industry does not involve R&D institutions to help absorb such advanced technologies and make them our own, and use them as a base to develop our own next generation products. The kind of interaction between R&D and industry that one observes in the developed world, not to speak of substantial R&D investment by the

industry, is singularly absent in our country. Whereas typically, something like 5 – 10% of the earnings are ploughed back into in-house R&D in the developed world in high technology areas, it rarely exceeds 2% in our country, if that. Reputed academic scientists are critical of S&T people joining the industry, saying that they are sacrificing basic research for moolah, while they themselves are spending government funds for their own research. It is a clear lack of understanding of what builds an industrially advanced country. Basic research supported by the government is necessary. But it is project-specific investment by the government to achieve specific industrial and technological objectives that builds a developed nation. It is this that the government and the leaders of science who advise the government need to ponder. There is no dearth of intrinsic capability in the country, and it is the primary responsibility of the leaders of science to draw upon this tremendous wealth to build the industrial base of our country in a purposeful manner, and not belittle it by giving greater importance to publishing papers inspired by the research taking place elsewhere in the world.

In everyone's life, there are always events that get etched in the memory for good or for bad. One event that brought home to me how tenuous one's hold on life is, took place on the morning of 13th February 1989. I got up with a mild fever. Dr Nanjunda Swamy, Chief Medical Officer of NAL, came home, and after examining me said that it did not look like the 'flu, and that he would wait and see before he prescribed any medicine. Around 12 noon, I took a cup of coffee, and that was the last thing I remembered. Shyam felt I was sleeping, and left me alone. I did not wake up even after several hours, and even after she tried to wake me up. Apparently there was no response from me. Around 7 pm, the lights failed. She called Indira Rajgopal from NAL, who managed to bring along with her Dr Lalitha, who found my pulse was weak. I was carried downstairs, put in a car, and taken to the Jayadeva Institute of Cardiology. They took an ECG and found that my heart was all right. By that time, Dr Nanjunda Swamy came to the hospital and decided to shift me to NIMHANS. It was midnight. On hearing about it, Dr Narayana Reddy, the Director of NIMHANS, came immediately from his home and put me on intravenous crystalline Penicillin. When they took my lumbar fluid, it was cloudy, and the doctors' diagnosis was that I was suffering from meningitis. They continued with the intravenous drip. I was in a coma for ten days. My wife, Shyam, and my daughter, Monica, kept a round-the-clock watch on me to report any changes to the doctors. My brother, who was also a doctor, felt that my condition was critical, and suggested to Shyam that she might consider asking our son, Siddhartha, to come home immediately from Kanpur. But she felt confident that I would pull through, and did not want to call him. But Siddhartha, who was a student in IIT Kanpur, came and kept company with them. Apparently I was having epileptic fits while in coma. I woke up from the coma ten days later, on 23rd February, and recognized all my relatives standing at the foot of my bed. The doctors declared that I had not suffered any brain damage, common in such cases. Two weeks before I fell sick, I was in Pune to give a lecture. Pune was in the endemic belt of states where typically, about 2500 people die every year from this disease. I was fortunate that I survived because of the timely attention by NAL doctors and the treatment in NIMHANS. It made me realize how tenuous one's hold on life is, and that you cannot take anything with you when the time comes. In a sense, I realized that I had no desires, and felt that what little money I had should be shared with

others, as far as possible. Since that time, this has become a compulsion, and we have been donating about Rs 30 to 50 thousand each year to worthwhile causes. We wish we could give more. Most of the donations are for educating children. It gives us a sense of satisfaction that we are able to help the deprived.