THE MAKING OF A SOCIALLY-CONCERNED SCIENTIST: PERSONAL REFLECTIONS OF A MAVERICK

Amulya K.N. Reddy

It was 1942. I was 12 years old and determined to join the merchant marine training ship DUFFERIN at Bombay and become a sailor like my hero, my uncle C.G.K. Reddy. And then out of the blue I received this long letter written on rough handmade paper from Madras Jail. It was from CGK (in prison for anti-British activities) cautioning me against choosing a career based on whom I hero worshipped and urging me to do what I would love to do and what I believed in. This advice stayed with me throughout my life. But it was not easy to adhere to it because there were many things I loved to do.

Between 1945, my last year at school, and 1947, I struggled to choose between cricket and science. Chosen to play for Madras University at the age of 16, I was a promising opening batsman and a leg-break bowler. I was crazy about cricket; in fact, I still dream about it. As my father disparagingly said: "What do you see in his room? Books, pictures, clippings, etc. About what? How to hit a ball! Is that all in life?" But, cricket taught me two crucial codes of conduct: playing for the team and accepting the umpire's verdict.

And then my eyes were opened to the beauty of science -- first, by Mr. A. Alvares, an outstanding chemistry teacher in St. Joseph's School, Bangalore, and then in full splendour by my schoolmate, V. Radhakrishnan, Professor Raman's son, and his cousin, S. Ramaseshan. They were mentors who lit a lamp of scientific interest that has never died out. They encouraged me to read books such as Max Born's The Restless Universe (written, incidentally, in Bangalore), George Gamow's Mr. Tompkins in Wonderland and Mr. Tompkins explores the Atom, and Albert Einstein and Leopold Infeld's The Evolution of Physics. I still recall the talk that Ramaseshan gave me on the electronic structure of atoms. Even more clearly I remember worrying about the electronic structure of the transition elements when I was fielding at square leg in an inter-collegiate cricket match.

[But, association with the Raman family also instilled in me a belief that, while I could match them in my efforts, I could never compete with their intrinsic intelligence and creativity and therefore my ambitions must be humble. Looking back, I realize that in developing that humility, I was preventing the bitterness which comes from the frustration of unbridled ambitions. In fact, I was laying the basis for a happy life where achievements outstrip aspirations.]

Whilst still grappling with the conflicting attractions of cricket and science, I was pulled in yet another direction. After India became independent on August 15, 1947, the demand for Responsible Government was raised in Mysore State by the Congress. A strike was announced from September 1. I decided not to have anything to do with the strike, perhaps because my brother-in-law was the Private Secretary to the Maharaja's Dewan. But when I was cycling along to college, I was accosted by my classmate, Vimala Pawar. She asked me challengingly: "What are you going to do for Responsible Government?". I

don't remember what I mumbled, but she successfully shamed me into joining the strike and very soon I was on the Action Committee and one of its leaders.

Then in 1948, my uncle CGK, by then a confirmed socialist, came to Bangalore. He became an even greater influence on me than in my childhood. Thanks to him, social concerns became a powerful force in my life. Through him, I met great leaders, Jayaprakash Narayan, Achyut Patwardhan and others. Among them, Rammanohar Lohia was exceptional -- a brilliant intellect with tremendous compassion, a powerful man with a gentle affectionate nature, he had a magnetism that was irresistible. A student wing of the Socialist Party was formed. It ran a cyclostyled magazine which I wrote, typed, cyclostyled and hawked. But, the factional conflicts in the Socialist Party seeped into the student wing. Through this experience, I learnt two important truths about myself: (1) I love writing and (2) I do not enjoy politics and its infighting. Provided that what one loves to do does not hurt others, it is far better to do what one loves to do than what one ought to do.

1949 was a turning point in my life. I went to Madras to see the India-West Indies Test Match and on the rest day, I called on my former classmate, Vimala Pawar. We decided to write to each other and through the letters blossomed a love that led to marriage and a relationship that has been the most sustaining influence in my life.

I was less than 21 when I got married in 1951 and obviously not economically independent. I became a Lecturer in Chemistry in Central College on a grand salary of Rs.125 per month. Vimala and I overcame our penury and dependence with love and our first-born, Srilatha. Outside the home, I found fulfilment through teaching, cricket and philosophy. I came under the influence of a group of intellectuals who were deeply concerned about science and society. Amongst them, J.R. Lakshmana Rao and M.A. Sethu Rao who taught me Chemistry in my B.Sc.(Hons) course, and K. Srinivasan had a major influence on me. In the forty-odd years that I have known them, I have never heard them once complaining about the many injustices meted out to them by life. They had a reservoir of contentment, a greatness of character and a robustness of philosophy that I have tried to emulate. Lakshmana Rao also kindled in me an abiding interest in philosophy and social thinking.

In 1955, I went to Imperial College, London, for my Ph.D. Vimala joined me there after a few months. I was a poor student and so Vimala had to support herself with a job. Nevertheless, those were wonderful days. We saw plays, visited art museums, participated in poetry reading, and made many friends. In particular, we developed a friendship with Hyman Levy, Professor of Mathematics, Imperial College, and a prolific writer on social thinking. I read his books, discussed them with him, and learnt how to think dialectically about dynamic systems. As Secretary of the Philosophy of Science Club, I organized my understanding in talks on The Scientific Outlook. To that period, I owe the honing of my analytical skills which proved invaluable in later years.

1956 was also the year of shattering of political illusions through the revelations of the horrors of Stalinism, the crushing of the Hungarian uprising and the Suez war. With all this political confusion, I withdrew into research and concentrated on my Ph.D. which

was on an electron-diffraction study of the structure and growth of electrodeposits. We started our second child and Vimala had to give up her job. There was no money. So, I looked for the highest-paying short-term job -- it was that of a British Railways porter at London's Waterloo station. I worked there for two months. It was the hardest physical labour I have performed in my life. I suffered both humiliating and honourable treatment from passengers whose luggage I carried; but they taught me to see the world through the eyes of physical labourers. Ever since, I have never won an argument with a porter about his charges, because I immediately see myself in his shoes.

I returned from England in early 1958 and got a job as a Senior Scientific Officer in the Central Electrochemical Research Institute (CECRI) at Karaikudi. I spent three years there. It was the first home that Vimala and I had (our London accommodation was only a student's transit flat). Many young friends adopted it and made it theirs. We were very poor. My take home salary was about Rs 450 per month. I moved around on a bicycle. Our monthly trip to the town was in a pony-drawn cart. But, Vimala and I have observed -- with sadness -- that we have never been as hospitable as we were then.

In Karaikudi, I met three outstanding electrochemists -- S.R. Rajagopalan (SRR) and S. Sathyanarayana (both of whom worked with me) and S.K. Rangarajan who I persuaded my Director to recruit into CECRI. SRR became a close family friend and we appealed in times of crisis to his encyclopedic knowledge on almost everything, for instance, when our second daughter, Amala, was late in speaking.

In CECRI, our group was excited by the research that we did. We worked mainly on the structure and growth of electrodeposits. But, CECRI performed well below its potential; it did not justify the investment on it. One of the main reasons was that hopes of royalties for work led to sub-critical teams. Also, the leadership was driven by ambitions of scientific glory rather than the best interests of the Institute. With no great vision generating the morale and zeal of the institution, the atmosphere was unpleasant. I saw there the ugliest infighting among scientists I have ever seen; I observed casteism at its worst; I experienced caste discrimination. For one who did not belong to the highest caste, the orthodoxy was suffocating. Until Karaikudi, I had the naive belief that people disliked you or were hostile to you if you did them some wrong; there, I learnt that people's attitudes to you were largely a response to what threat you represented to their ambitions and interests. Inter-personal relations among the senior scientists could not have been worse. I was generous in giving credit to my assistants, but this generosity was turned around to argue that I was incapable on my own. I began to doubt my scientific ability. My confidence plummeted to its lowest depth. I developed hyperacidity and was well on my to an ulcer when I received an offer of a post-doctoral fellowship from the University of Pennsylvania. I accepted the offer with alacrity. I left Karaikudi in 1961 with relief. I was, however, very worried about SRR's fate even though I had brought his outstanding abilities to the notice of Ramaseshan.

I arrived in Philadelphia and found the Electrochemistry Laboratory of Professor J.O'M. Bockris to consist mainly of foreigners. Later, I realized that the post-World War II proliferation of funds for R & D had produced a new breed of "scientific leaders" whose forte was not creativity but the ability to get funds. Once the funds were obtained, foreign

scientists -- Indians, Pakistanis, Sri Lankans, Yugoslavs, Australians, etc., -- could be bought with fellowships and studentships. And if they come on Exchange Visas, it was even possible to control their future. But nothing binds people together like a common scourge. There was a close camaraderie in the lab. But, there was little joy in the science that was being done. Science was not what it was in my youthful dreams, an exciting voyage across the trackless expanses of the unknown; it was an oppressive atmosphere of sponsors, results, deadlines, targets, long hours rather than productivity, vicious competition, dubious ethics, etc. In the everyday life of the laboratory, little attention was paid to social concerns, values and ethics. No wonder that outside, the world of the 1960s had grave doubts about the morality of science and serious concerns about its links with war and destruction.

From the point of view of my research, those were wonderful years. I was asked to develop ellipsometry, the study of surfaces through analysis of the changes in reflected polarized light. I developed a new technique called chronoellipsometry and made a well-received presentation at the National Bureau of Standards Conference on Ellipsometry in 1963. I also made presentations at the famous Gordon Conferences on Electrochemistry. I was able to rebuild my confidence which had been shattered in Karaikudi.

1964 was a crucial year. With success came offers of jobs in industry. I went interviewing and became more and more demanding with regard to perquisites. But the salaries were not a measure of the freedom to choose problems for work. Scientists in industry seemed to work in a straight-jacket atmosphere. After an interview at one of the giant companies where the desks for the scientists were lined up in a endless hangar, I came back and told Vimala that the work would be soul-destroying and I was becoming mercenary. She said: "Let's go home!"

It was at this stage that I was asked by Bockris to help him edit for publication about 150 typed pages of his lecture notes on Electrochemistry. At the very first discussion, he said that what he wanted was something new and not based on his notes. He drew up a contract making me a second author. That was how I got into drafting Modern Electrochemistry by Bockris and Reddy -- a two-volume 1400-page book that a reviewer called the bible of electrochemistry.

The book was both an agony and an ecstasy. The agony consisted of interminable discussions ending up with marginal changes, the draft after draft, the continuous expansion of content, the weeks stretching into months and the months into years, the tension, the massive intrusion into family life (for instance, my youngest daughter, Lakshmi, began to hate anything intellectual because that was what deprived her of her father's companionship), etc. The ecstasy consisted of my discovering electrochemistry for myself, being excited about it and coming up with a very fresh account of that discovery. It was this excitement and freshness that readers found stimulating. They found it excellent for self-study. The book is frequently stolen from libraries. The book dragged on from 1964 till I returned to India and it was finally finished only in 1969. For a technical book, it was a best-seller and a money-spinner. Above all, it made me famous in the world of electrochemistry.

We returned to India in 1966. After six years in the USA as a Post-doctoral Fellow and Research Supervisor and many research contributions, particularly to ellipsometry, I became an Assistant Professor in the Department of Inorganic and Physical Chemistry, Indian Institute of Science. But, it was wonderful returning to Bangalore and giving the children a happy home and atmosphere to grow up in.

I began research with a team of students. Experimental work was hard going. Facilities had to be built up. And money was scarce. Eventually, most of the students got their Ph.D.s. Modern Electrochemistry finally came out in print. The one-to-three invitations per year to speak at international conferences were some measure of success. But, my research had no grand theme. It dawned on me that most of the fundamental discoveries had already been made in electrochemistry; it had, therefore, become an applied science. I tried to give my research an applied thrust. Sathyanarayana and I took up the indigenous development of the magnesium-manganese dioxide battery system and started getting some success.

I also became a consultant to Sandur Manganese and Iron Ores, Ltd., the company of my college-mate, M.Y. Ghorpade. I helped to recruit and train a team of engineers. We used reverse-engineering; we started from the imported technology and tried to understand its design basis. Design know-how is the highest form of know-how; it is superior to construction know-how, maintenance know-how and operational know-how. The team became extremely competent in the field of electrometallurgy and is now in leadership positions in the company.

1973 was a year of personal crisis. Firstly, we came to know that the magnesium-manganese dioxide system that we were developing was being tested in Ladakh. That information upset me because I realized that our work was part of a defence effort against the Chinese and I felt that the people of India had no quarrel with the people of China. I did not want any part of this type of scientific relevance, if this is what it would lead to.

Secondly, it became clear that the electrochemists I had brought into the Department of Inorganic and Physical Chemistry to build a centre of excellence in electrochemistry would do outstanding individual work, but they would never gel into a school. That dream of mine would not be achieved. It was only much later that I heard the saying: "One Indian = three Japanese; one Japanese = three Indians" which is meant to indicate that, in typical situations, Indians are individually brilliant but hopeless as a team because they cannot work together. Not only do they produce no synergism, but the whole may even be less than the sum of the parts.

In this crisis, there occurred a rare event, a single experience that altered my whole pattern of thinking. I heard a lecture on Poverty in India by Professor C.T. Kurien (then of the Economics Department, Christian College, Madras) at the Ecumenical Christian Centre, Bangalore. Referring to the book by Dandekar and Rath on the subject, Professor Kurien said that poverty had increased with industrialization. This observation shattered my Nehruvian faith in the dictum: more science and technology --> more industrialization --> less poverty.

A period of intense searching began. It was neither organized nor focused. I was really groping. I took a small step forward when I presented a paper entitled An Asian Science to combat Asian Poverty at the One Asia Conference in Delhi organized by the Press Foundation of Asia. I argued that the industrialization-poverty nexus arises from the capital-intensive labour-saving nature of the pattern of industrialization based on imported Western technology and that an attack on poverty required a different science and technology, an "Asian Science". The paper attracted favourable attention at the One Asia Conference from several scholars there including the Myrdals.

The real personal "break-through" was achieved at Bangalore. C. Subramaniam was organizing conferences of scientists to get reactions to the National Committee on Science and Technology (NCST) document "An Approach to the Science and Technology Plan". [Was that the last time that scientists were consulted so widely? Certainly, the Science Advisory Council of Rajiv Gandhi did not ensure country-wide discussion of its papers.] Ramaseshan was so impressed with my paper to the One Asia Conference that he urged Satish Dhawan, the host for the Bangalore Conference, to include me as a discussant of the NCST paper. I presented a paper on the Choice of Alternative Technologies where I argued that India was a dual society with "... islands of elite affluence amidst vast oceans of poverty of the masses ...", that this poverty was primarily due to inadequate income-generating employment in the rural countryside and that such employment would not come from capital-intensive industrialization. I attacked Indian science and technology for firmly allying itself with the elitist pattern of industrialization and demanded that it should devote itself to the generation of an alternative pattern of capital-saving labour-intensive technologies of relevance to the rural poor.

Remembering the hero of Jack London's The Iron Heel addressing the capitalists club, I expected to be crucified by the scientists, but to my amazement, my presentation was received with thunderous applause. One is fortunate if there are a few such moments of glory in a lifetime. But the applause was not for me; it was primarily because I had echoed concerns shared by a large number of people. There was an interesting episode during the ensuing discussion -- a well-known scientist attacked me with the words: "Reddy is asking us to go backwards!" and C. Subramaniam, who chaired the session, jumped up and said: "No! No! He is taking us forwards!"

But, what was really gratifying was the large number of faculty from the Institute who came to me after the presentation to express agreement. Even more important, they declared a desire to do something to implement an alternative science and technology -- since they were not social scientists, they could not present their critique and "go home and have tea"; they had to alter their focus of work.

It was then that I made the decision to quit electrochemistry. I felt that I had to burn my bridges. Otherwise, I felt that if things became difficult in rural technology, as I was sure they would be, I would escape into the expertise that I had built up in electrochemistry. At that time, I could start from "zero" and derive any one of the equations in the two volumes of Modern Electrochemistry.

Thus ASTRA was born in 1974 to initiate and promote work of rural relevance in

the Indian Institute of Science. Quite deliberately, it was designed as a multi-disciplinary effort drawing on the expertise from the various discipline-oriented departments. Major presentations were made to the faculty and students and at the instance of the Institute Director, Satish Dhawan, to the Senate Committee on Research and Academic Policy. Those were heady days. The best and the brightest in the Institute worked for or supported ASTRA. ASTRA's open seminars were widely attended. A number of projects were initiated. There was camaraderie. The support and friendship of Krishna Prasad and Jagadish were particularly precious. ASTRA was an interacting community of scientists and engineers. We had discovered how to build a team out of Indians -- create a shared vision. But, the vision must be grand enough to inspire and the vision must be shared.

But, the student support for ASTRA was never adequate. The whole outcome would have been different if a political, or student, movement had backed us. The left must assume a large measure of the blame for this situation -- by and large, they preferred a pattern of activity where, between office hours, they did not question their work -- even if it benefitted the elite -- as long as, after office hours, they got worked up over remote causes such as Cuba. More fundamentally, the Indian communist left never questioned capitalist technology. In contrast, I was arguing in 1973 that "... technology is like genetic material; it carries the code of the society in which it was conceived, and given a favourable milieu, reproduces that society ...." Later, I humbly learnt that I was only elaborating concerns that Gandhi, Kumarappa and Lohia had articulated.

In 1975, I was involved with M.Y. Ghorpade, then Finance Minister of Karnataka, and Satish Dhawan in setting up the Karnataka State Council for Science and Technology (KSCST) to bring together government and scientific institutions to address the problems of poverty in Karnataka. Whereas ASTRA concentrated on the generation of technology, KSCST would focus on the dissemination of technological solutions. Since then, KSCST has become a model for state councils. It has come up with several novel programmes and activities -- Karnataka Rajya Vijnana Parishad (the science popularization and people's science programme), the Student Projects Programme (to fund relevant student projects in the engineering colleges of the state), the Product Development Centres (to commercialize the products/devices from successful student projects), the Drought Monitoring Cell (a database for information necessary for decisions on drought), etc.

One of ASTRA's first outputs was the 1974 paper on Biogas Plants -- Problems, Prospects and Tasks published in Economic and Political Weekly by C.R. Prasad, K. Krishna Prasad and myself. The paper said many important things which remain valid. For instance, it showed that the official biogas programme based on family-scale biogas plants would neither make a dent on the energy problem nor spread beyond the rural elite. It showed the economies of scale associated with community biogas plants. Though it was merely a paper exercise, it immediately attracted international and national attention. On the international front, it was widely cited.

Unfortunately, the national biogas programme felt that we were poaching into its territory. And so, I discovered an important problem with working in India -- subjects become territories, and when "outsiders" work on a subject, they are treated as invaders. A good deal of the problem arises from the fact that these "outsiders", with extremely

limited manpower, money and resources but with dedication and the freshness and innocence of newcomers, achieve far more than large establishments set up for the subject. Thereby, they expose the ineffectiveness of Big Science and its bureaucracies; hence, they are a threat. But, their competition is essential for progress, and it can come mainly from universities which is why they must be nurtured. They must also keep their spiritual distance from the powers at Delhi.

The biogas paper also revealed that there were new allies of whom we had been unaware. Professor K.N. Raj called on me at my home to commend the biogas paper and to encourage us to continue work at the technology-economics interface. He went on to invite me to give seminars at the Centre for Development Studies, Trivandrum, and join the Governing Body of the Centre, an association which continued for over 16 years. I owe an immense debt of gratitude to Professor K.N. Raj for making me feel that what we were doing was important and the way we were doing it was right. This inspiration and encouragement from a distinguished economist was extremely important because rural technology was forcing us to work in new areas with economics implications. We went in with great trepidation thinking "Fools rush in where angels fear to tread!" But, many eminent economists were very positive to our writings. I particularly recall the great Cambridge economist Joan Robinson telling me when I diffidently expressed my ignorance of conventional economics: "Don't learn that stuff; you are doing fine!"

In 1975 I went on sabbatical to the United Nations Environment Programme at Nairobi, Kenya. Things did not work out as planned. Before I went there, I was assured that I could make several trips to India to keep in touch with ASTRA. After landing in Nairobi, I found that the management had changed. I was grounded. Two of my ASTRA colleagues threatened me that if I did not come back in a year, they would not support the proposal that we had jointly submitted to the Department of Science and Technology. I came back in a year, but both of them quit -- one to build his own empire and the other for alien climes. The good news was that UNEP asked me to develop the conceptual framework for environmentally sound and appropriate technologies and thereby provided me with a tremendous opportunity to learn about the genetic characteristics of western technology and about development.

The first thing I learned was that development must not be equated with mere growth (measured by GNP). What had taken place in India was a distortion of development. Genuine development is a process of growth that is directed towards (a) the satisfaction of basic needs, starting with the needs of the neediest, (b) the strengthening of self-reliance and (c) harmony with the environment. I have gone around with this understanding of development for almost two decades and found no reason to abandon it. However, the Narmada controversy has forced me to include in the definition an insistence that the benefits of development projects must start with the people at the project sites and then radiate outwards; otherwise, these people at the epicentre become the victims of development.

I also came to the view that however attractive modern technology may be, it has certain intrinsic unwelcome tendencies -- it tends to amplify inequalities, to alienate people from their work and from each other, and to degrade the environment. All this went into

an UNEP publication Technology, Development and the Environment -- a Reappraisal that is little known; for my evolution, however, it was seminal.

I returned from sabbatical in 1976 after resisting the temptations of a UN job with its vulgar salaries and perquisites -- Vimala said to me in Nairobi: "If you continue here, you will be destroyed!" I plunged into ASTRA work. The Extension Centre was established at Ungra and we began our studies of the Ungra ecosystem with an excellent team organized by N.H. Ravindranath (Ravi). We did what was probably the first study of energy consumption patterns in villages.

For this, we owe a great deal to that wonderful person who is no more, J.P. Naik, then Secretary, Indian Council for Social Science Research. During coffee break at a Delhi meeting, I mentioned to him that we knew far more about how energy is used in London or New York than we knew about energy in villages 10 kms away from the Indian Institute of Science and therefore I would like to study the sources and end-uses of energy in villages. He promptly asked me how much money we needed and in a few days we had an ICSSR grant. Such visionary and generous people men are rare -- but for them, pioneering and non-conventional work would not take place.

From energy consumption patterns in villages, we went on to deepen our study of village ecosystems and to design and build rural energy centres. The ecosystem work required a great deal of survey work and analysis of data. The team lived in the Ungra Extension Centre.

At the height of our activity, I made weekly visits lasting a couple of days at a time. Vimala and I used to live in a 30 square metre house with no furniture, electricity, and flush toilets, but these were among the happiest days of our life. It is not irrelevant to mention here the importance of the spouse in unorthodox ventures such as ASTRA -- you cannot fight a battle in society unless there is unqualified support at home for these crazy ventures. And Vimala gave me this in abundance!

The discussions were excellent and the learning process was intense. We gained many insights. Learning from the environment is certainly a more powerful heuristic than copying from the West. Unfortunately, much of the work (at least half a dozen papers) was not published even though it was written up by Ravi and his colleagues. The blame was entirely mine for this sin of omission, viz., quitting a field/activity before writing up the papers. In fact, I had committed this sin twice before in my career -- when I left Karaikudi and when I quit electrochemistry. By taking up a new venture, viz, global energy strategies, before completing the previous one of publishing our ecosystem studies, I landed in a situation where the urgent new tasks took precedence over the important old commitments.

1978 was an important year for my future. I met Theodore (Ted) Taylor at an INSA meeting at Delhi and was greatly impressed by him. Here was a nuclear physicist who after designing a whole generation of atomic bombs at Los Alamos gave it all up to lead a crusade against nuclear weapons and for solar energy. That major changes could occur in professional lives impressed me. We became good friends and from him I learnt

the importance of what he called in any context: "thinking it through". Most implementation fails because the implementor has not "thought it through".

In 1978, I also met Jose Goldemberg at a meeting organized by him in Sao Paulo where I presented the results of ASTRA's study of energy consumption patterns. We discovered an identity of outlook and affinity of views and began a lasting friendship which resulted in an important on-going collaboration.

In that same year, I visited, on my way back to India, the Center for Energy and Environmental Studies, Princeton University, at the instance of Ted Taylor who was then teaching there. I established instant rapport with a number of distinguished scientists -- Bob Williams, Rob Socolow, Frank von Hippel, Hal Feiveson, Gautam Dutt and others -- all of whom had turned their back on conventional physics for studies on energy and the environment. I found like-minded souls with deep social concerns and a determination to pursue science with a humane touch. I found unexpected reactions -- for instance, a leading physicist Freeman Dyson from the Institute of Advanced Study saying to me after my seminar: "I envy you!"

Their appreciation of ASTRA's work and efforts was in sharp contrast to the scorn and disdain of the bulk of the scientific establishment in India. It was alright to make at the NCST meeting -- as Ramaseshan did -- an insightful and passionate exhortation: "We as scientists are intelligent observers. What we lack is direct exposure. So, all that we need to do is to live for some time in a rural environment and we will be able to identify the problems." But, once ASTRA tried to implement the suggestion, exhortation became denigration. Was it because ASTRA was rocking the boat of conventional science, setting an uncomfortable example and demanding a new and threatening orientation to science and technology? In short, was it because ASTRA was changing the paradigm for scientific work?

The Director of a prestigious institution publicly declared that those who were failures in science took to rural technology. The Editor of an Indian scientific journal said: "What Reddy is doing is not science. I will never publish him in my journal!" It was not easy going. Mentors became tormentors, friends became opponents, and colleagues became critics. The intensity of the critique increased as national and international recognition for ASTRA's work grew. The situation was aggravated by the BBC film West of Bangalore which publicized ASTRA world-wide. At the national level, I was awarded the Rathindra Puraskar at Shantiniketan, and after hearing the citation, Indira Gandhi said to me whilst giving the award: "It must have required rare courage!" Communist fellow-travellers jibed: "This rural technology is a trick of the industrialized countries to keep us in the bullock-cart age! See, the World Bank is supporting it." By the same token, they should have rejected the dams, the power stations, etc., all of which were funded by the World Bank.

Those who want to change a paradigm must be prepared to struggle and to be lonely. There was neither a Gandhi nor a Raman to turn to for support. However, there were some steadfast, albeit tacit, supporters among the scientists -- Satish Dhawan was a beacon among them -- and other scientists like C.V. Seshadri also decided to shift their

concerns to rural problems. And in my case, Vimala remained "constant as the Northern star!" What the ASTRA workers had in abundance was conviction in the path they had chosen and faith that they would succeed. This faith was a crucial source of strength -- in the ultimate analysis, faith is what keeps us going when there is no hint that our efforts will succeed or no evidence to justify what we are doing. Fortunately, the villagers of the Ungra region never lost their faith in ASTRA.

And ASTRA maintained a publication record. I edited a monograph on Rural Technology which attracted wide attention. It was even suggested that rural technology could become the theme of a separate journal but those struggling to get articles for conventional journals felt that this would become a threat.

My 1978 visit to Princeton led to annual spring visits during the course of which old friendships deepened and new friendships were formed. It was in 1980 that I had the good fortune to meet Thomas Johansson from Lund University.

Jose Goldemberg, Thomas Johansson, Robert (Bob) Williams and I began a collaboration that was to play a major role in my life. What initiated and sustained the collaboration among the four of us is of considerable relevance. Each of us started his career as a physical scientist and turned eventually to energy research. Also, we live and work in different countries -- Brazil, Sweden, India and the United States -- located in four continents. And, our cultural backgrounds and experiences are diverse. Despite all this, we forged bonds and functioned as a well-knit team achieving together what no one of us could have achieved individually.

Our meetings at various international meetings and Princeton visits soon revealed a remarkable measure of shared values and concerns about the interaction of technology and society. They also showed an identity of outlook and similarity of approach on matters concerning energy in society. These early interactions showed that the four of us could work together with mutual respect and equality and avoid the hierarchical modes of functioning which nearly always vitiate international collaborations.

As we combined our efforts, we were led from a critique of conventional wisdom to a new approach to energy. When significant progress had been made we felt that we should expound and elaborate the new approach -- and that is how our book Energy for a Sustainable World came to be written.

We suggested that, contrary to widely held beliefs, the future for energy is very much more a matter of choice than of destiny. Energy futures compatible with the achievement of a sustainable world are within grasp. The joy in our endeavour came from the feeling of being harbingers of hope rather than prophets of doom.

I have always been impressed by the saying: Think globally; act locally. The challenge of designing and building rural energy centres led ASTRA as early as 1979 to the community biogas plant project at Pura village, 2 kms from our Ungra Extension Centre. During the first phase of this project, we attempted to provide all the households of the village with piped biogas for cooking. We failed because of an overestimation of cowdung

resources and an underestimation of biogas requirements. When I was away on sabbatical, the project came to a standstill in 1984, but on my return from my sabbatical, the villagers wanted the project to be restarted with the emphasis on drinking water. The scheme consisted of the villagers supplying dung to the biogas plant where it would be anaerobically fermented to yield biogas which would fuel a modified diesel engine which in turn would run a generator. The electricity thus produced would run an electrical submersible pump and lift drinking water for the village and in addition be supplied to the households to provide electrical illumination. This modified scheme has been successfully operated by the villagers since 1987. When every household was illuminated with a fluorescent tubelight on Gandhi Jayanti, October 2, 1989, we felt that we were implementing Gandhiji's vision of the role of science and technology. The Pura scheme is now being replicated in other villages.

The interaction with the villagers of Pura has been one of the most rewarding experiences of my professional life. I learnt from them the difference between mere popularization of science and democratization of innovation. Their understanding that technological progress occurs via mistakes was far superior to that of my colleagues in the Institute who cheer when the satellite goes up and jeer when it crashes into the sea.

When I returned in 1985 from my sabbatical, I was persuaded by the new Director of the Institute to take up the chairmanship of what was to become the Department of Management Studies. There I turned my attention to the dissemination of technologies. I learnt the importance of innovation which is the process of converting an idea into a product in the economy as distinct from invention where the process ends with a working device. But if a device works, that does not mean that it will be produced, distributed and accepted by end-users. It was amusing to find that technology generators consider themselves a superior caste to the technology disseminators. In particular, there are important actors in the innovation chain -- those who productionize and develop the method of making thousand-off as distinct from making the one-off prototype -- who are largely missing in India. I had a fruitful and enjoyable collaboration with Professor K.N. Krishnaswamy to produce a model backed by several case-studies on the factors governing the success and failure of rural technologies.

In 1988, our book Energy for a Sustainable World was published. It attracted international attention. It was referred to in the Brundtland Commission Report. It led to an invitation from Scientific American to write an article. But, our local scientific journal ignored it for several years. What a contrast to the reception that Professor Raman gave me when I presented him Modern Electrochemistry; he promptly sent his secretary to hand-deliver to me his books all autographed with: "With kindest regards ... from one author to another".

But, the most productive part of my stay in the Department of Management Studies was the energy work. During the 1980s, energy had become an increasing concern of mine. It has been a rare privilege and a good fortune that I have been able to work on energy problems at the global, national (India), state (Karnataka), city (Bangalore) and village level. I built up a small team for energy analysis. Starting in 1986, Dr. Gladys Sumithra (from the Planning Department, Government of Karnataka), two project

assistants, P. Balachandra and Antonette D'Sa, and I constructed a detailed development-focused, end-use-oriented and service-directed electricity demand scenario for Karnataka. We then did a detailed comparative costing on the same terms of fifteen technologies of electricity saving, decentralized generation and conventional centralized generation of electricity and used the results to construct a least-cost supply scenario. It turned out that the least-cost plan consisted of a mix of end-use efficiency improvement and electricity substitution measures, decentralized generation and centralized technologies (hydroelectricity, natural-gas-based and coal-based thermal power, and nuclear power.

The detailed scenarios attracted international attention, but many large national energy institutions were upset with us for stealing the limelight. What they did not realize is that our team at the Institute had put in about three years of sweat to do the end-use analysis for Karnataka, the comparative costing and the scenario construction. During this time, these institutions strived for influence in the capital and their leaders sought to be in the corridors of power. There were two lessons here. Firstly, you can either strive for analytical excellence or for political clout, but not for both. Secondly, political influence is seductive but it is ephemeral; in contrast, new ideas and sound analysis have a long-term sustainability.

Our in-depth analysis of the economics of nuclear power was invaluable when a debate on the Kaiga nuclear power plant was organized by the Department of Science and Technology, Government of Karnataka. I showed that nuclear power is neither necessary nor economical -- in fact, it is the most expensive technology of electricity generation. Its proponents claim that it is safe, cheap, appropriate and modern; the resulting acronym SCAM is a better description.

Up till that debate, I had been silent on nuclear power -- much to the disappointment of many anti-nuclear activists. They wondered whether I was afraid. But, my silence arose from a nasty experience that I had several years earlier. I had been generally in favour of a scientific outlook. I had even written a paper on the subject. What little disdain I had for the faith and belief of ordinary people (who are not at all ordinary if one considers how they face the world despite their economic and social handicaps) disappeared after I became involved with ASTRA. Despite this, when a leading scientist said to me as we were about to enter a meeting: "I say, Amulya, that Pushpa Bhargava is pestering us, so please sign his Scientific Temper declaration", I signed the declaration in the same way that I used to buy charity-show tickets to get rid of the ticket-seller. In doing this, I was certainly irresponsible on a major issue. Then, articles started appearing in the newspapers attacking the declaration and its signatories. To my surprise, I found myself agreeing with many of the points made by the critics particularly those pertaining to the arrogance of modern science and the disrespect for traditional knowledge. In fact, I had written a paper entitled Some Thoughts on Traditional Technologies in which I had said some of the things that the scientific-temper critics were now saying.

For quite some time, I had been worried about the conventional view that science is amoral and neutral. The view may have been a clever way of rejecting responsibility for a Hiroshima after computing and prescribing the height at which the bomb must be exploded to maximize the number of people who would die. But, like the youth of the 1960s, I

rejected that sophistry. I was concerned that values, feelings and emotions were considered unmentionable in scientific discussions. Since ASTRA, however, I did not hesitate to refer to them in my seminars, even in western centres of excellence. The scientific temper debate raised my level of understanding of the very fundamental issues involved.

Western Science has been based on two dichotomies: (a) separation of the subject from the object and (b) separation of emotion (the non-cognitive self) from analysis (the cognitive self). Thus, science is claimed to be objective and amoral. The first dichotomy leads inevitably to degradation of the objects of study (even humans) into things, and the second, to the removal of feelings for objects. We must not forget that Oppenheimer said that the first nuclear device/bomb was "technically sweet" or that at the Bangalore Kaiga debate, a Department of Atomic Energy scientist said: "Hiroshima provided us with a fortunate opportunity to study radiation effects!" The relationship between subject and object must be dialectical so that initial separation ends in subsequent unification. The suppression of emotion during analysis must give way to emotion after analysis. The functioning of scientists as individuals, groups and institutions must be constrained and limited by moral strictures and taboos. Otherwise, the synergism between the isolation of the subject from the object and the removal or absence of emotions and feelings leads inevitably to science becoming the instrument of violence, oppression and evil. Science, therefore, is not neutral, but it can be -- and must be -- encoded with life-affirming values.

After the Scientific Temper episode, I made a vow: "I will go into Advocacy and Action only on issues where I have done Analysis myself!" This vow has been a handicap but it has increased my effectiveness. I determined to follow the sequence: Analysis --> Advocacy --> Action. At the analysis stage, it was crucial to isolate myself, the subjective analyst, from the object of analysis and also to remove emotions from the analysis. But, once the objective, dispassionate analysis is over, it is vital to reconnect with the object and bring in values into the advocacy and action based on analysis.

On July 31, 1991, I retired from the Indian Institute of Science. The Department of Inorganic and Physical Chemistry, the Department of Management Studies and ASTRA arranged separate symposia in farewell to me. At the ASTRA seminar, I said: "I have tried to follow in my own life the philosophy of nish kama karma, doing one's duty without thought of the success thereof. But, success is a function of two variables, internal effort and external support. External support is a probabilistic factor which we cannot control, though there are many people who direct the bulk of their internal effort trying to control external support. By and large, what is within our hands is internal effort. But, there is also a very interesting "stochastic" relation between success and internal effort -- the more intense your internal effort, the greater the chances are that you will do better work which in turn will earn success.

Looking back, I do not know whether my switch over from electrochemistry to rural technology and energy analysis has brought me more or less success. If success can be equated to making oneself redundant, I have been successful in ASTRA and KSCST because younger people -- K.S. Jagadish and S. Rajagopalan -- took over and the institutions have survived and grown. What I do know is that what I have achieved is ten times more than the early dreams which my wife and I had about our future. But, the

switch has certainly brought me more happiness. Some of the best people I have met are those I met after I started work with ASTRA and in the field of energy.

I read out to the ASTRA seminar audience an extract from a letter that my teen-age grandson had written to his parents: "Another bright spark in my project (on nuclear power) was our trip to ASTRA and a great discussion with thatha (= grandfather in Telugu) on the problems of the nuclear power. When ASTRA was being set up, I was much too young to understand the whole concept and what it stood for, but now all of what I have understood makes perfect sense." I think that is indeed a tribute. The young man made me feel immortal if immortality consists of one's ideas and influence acquiring an independent existence." It made me conclude by quoting from The Tale of Two Cities: "It is a far, far better thing that I have done than I have ever done before!"

This account would have stopped here if I had gone into retirement after retiring from the Indian Institute of Science. I did not. After our book Energy for a Sustainable World was published, there were frequent jokes from supporters and sponsors on the lines of "You have produced the bible. Now, where is the church?" Meaning, why not implement the ideas? So, the International Energy Initiative (IEI) was set up in 1991, and since I had retired, I was persuaded to become the President with Jose Goldemberg as the Chairman. IEI is a Southern-conceived, Southern-led, Southern-located South-North partnership -- a small, independent non-governmental public-purpose international organization that networks existing energy-related institutions and groups, particularly those functioning in developing countries. IEI's objective is to promote the efficient production and use of energy for sustainable development. Its focus is the developing countries. IEI's activities span information exchange, training, analysis, advocacy and action. I have enjoyed -- and been somewhat successful at -- designing institutions and establishing them -- ASTRA, KSCST, etc., and now, IEI is the new baby. I am busy nurturing it.

FILENAME: SEMINAR1 8288 words 16 July 1993