Are Traditional Values in Scientific Research Still Relevent?

Tradition has sometimes been a great source of strength to certain people. One such person was Lal Bahadur Shastri in whose memory I am giving this XIX lecture in the series. If one sees how he dealt with failures and successes and shouldered great responsibilities without the benefit of education at a high level one will realise that much of the strength he showed at times of difficulty and adversity owed its origin to his faith in traditional values.

Tradition has also been a source of strength to Nations. They take pride in the old traditions. Younger Nations envy the traditions of Nations with a great past who have traditions extending over thousands of years. Invariably they adopted many traditional values of other Nations with much benefit to themselves. In India, we have adopted the 500-year old traditions of scientific research in European Institutions with much success.

Till recently, scientific research was carried out mainly in the Universities. It has been the job of University teachers to create new knowledge without regard to its possible applications. On the other hand, it has been the job of industrial organisations to apply this knowledge for the benefit of mankind and themselves. The intellectual and emotional appeal and the personalities of the two classes of people, *i.e.* those involved in creating new knowledge and those who apply it has been different in some ways. Because it is so profitable, creating new knowledge has now also become an industrial and national activity in USA. However, the great industrial

organisations of USA know that the creators of new knowledge cannot be treated like one of their common folk, e.g. executive directors or supersalesmen, but they have to be provided the atmosphere of the Universities which they try to establish in their industrial organisations. In spite of this in many organisations the scientific staff acquire more wealth than is good for them. This not only reduces their creative abilities but it also separates them from the true creators of new knowledge in the academic organisations. Similarly, in our science organisations e.g. the Indian Council of Agricultural Research and the Council of Scientific and Industrial Research (and similar organisations), we have by our superior personal, laboratory and other facilities undermind considerably the morale of those in the Universities from where all of us came. Here began the erosion of our traditional scientific values. But before going further I must tell you how I was stimulated to choose the present subject of the lecture.

The stimulii for this lecture have come from two immediate sources. They both provided me excellent examples of how we have allowed our scientific standards to decline by disregarding the established traditional values that have existed for centuries.

Traditional relation between supervisor and pupil

The first example is a letter from Prof. Sadasivan, who wrote to me as follows:

> "As your first objective is "to promote integrity, objectivity and ethical values in the pursuit of science", I have to make the following observations:
>
> In my long scientific career during which time more than 500 research publications came out of our CAS in Botany at the university, I have never used my official position and claimed joint authorship in our publications. Today, it seems to be a recognised practice to share even Ph.D. thesis

work, and invariably, the research guide even becomes a senior author! What are you going to do about this detestable unethical practice? We must do all in our power to curb this tendency. Our youth are revolting and we must look sharp and devise some effective measures to curb this tendency. With pranams."

The above letter relates to work in botany and so it is of immediate relevance to agricultural research. Most of you know more about Prof. Sadasivan's work than I do. In essence this letter states that scientists are disregarding some of the important traditions in scientific research regarding the relation between the teacher and the pupil. What these traditions? Some of the traditions are that the supervisor gives all to the student and takes nothing from the student. This puts the supervisor not infrequently in the same class as the student's parents and so where such a relationship has existed in India the students who might be 60 years old today continue to touch the feet of their old supervisors who may be 70 or 75 years old. The same kind of respect and relationship also exists in other parts of the world where the supervisor has been a classical type of supervisor and the student has also behaved in the traditional manner. Certainly, this kind of relationship between the supervisor and the student was quite common upto at least 1950 after which the tradition slowly began to be eroded. I will now give you a concrete example of the actual form of the relationship by quoting what a well-known life scientist has documented.

Walter B. Cannon was a well-known figure in Physiology. He worked in Harvard and followed the traditions of classical physiologists. In his book "The way of an Investigator" (1945; Norton and Co.), he uses Lombard's experience as an example to illustrate what I am trying to say. I quote below what he has written on page 95 of his book:

"The leader of a group engaged in research should always be generous in the care and treatment of

those with whom he is working. This is an attitude they thoroughly appreciate and always remember. Warren P. Lombard, long Professor of Physiology at the University of Michigan, has told an illuminating story of his experience in Ludwig's laboratory to which, when a young man, he went for training. He told the professor that he was interested in fatigue but knew nothing about how to study it. Ludwig then defined a concrete problem for him, assembled the apparatus, and set him at work. When he encountered difficulties the old teacher helped him. At the end Dr. Lombard wrote an account of the methods he had used and the results he had obtained and submitted it to the professor — preliminary to publication. In a short time the paper was returned almost entirely rewritten, with only Lombard's name at the top of it. He took the paper to Ludwig and protested. 'You have set the problem for me', he said; 'you have shown me how to use the apparatus and solve my troubles; you have rewritten the paper, and your name should appear here with me.' 'No', Professor Ludwig replied. 'You have done the work and you should have the credit. 'But', he added, 'if you never do anything more, people will think that I did it!"

The last statement is very significant because it is obvious that if a student is given a problem closely related to the main interest of the supervisor (which could well be the reason for choosing to work under him), then it is obvious that the student must have received a lot of guidance apart from the main idea from the supervisor. Thus it is unnecessary for the supervisor to put his name on the paper particularly when he is thanked profusely by the student in the acknowledgements.

Ludwig worked in the last century. What was it like closer to present day? A good example of the attitude prevail-

ing in the first half of the century is the attitude of Cannon himself. Here is what he says on page 93 of his book:

> "The treatment of his collaborators by the head of a laboratory may be put to a real test when the time arrives for publishing papers. Methods which have proved satisfactory in my experience through several decades are as follows. If I have merely suggested the problem to be investigated indicated the pertinent literature, demonstrated the method to be employed, and from time to time have supervised the work, I have not allowed my own name to appear on the published paper as a joint author. Although the beginner who has carried on research in these circumstances has usually thanked me graciously at the end of his report, that acknowledgement has never been requested. When I have participated in the experimental procedures, my name has appeared as one of the authors. If I have done a major part of the work my name has been placed first, but if my role has been secondary, it has not had that prominent position."

That then is the answer to Prof. Sadasivan's letter. It should be a general rule that supervisors should not put their names to papers that have arisen as a result of work done by a student for his Ph.D. degree. I expect this recommendation will make many people uncomfortable but if generally implemented, I assure you, it will mean the end of much frustration and bitterness. Getting back to old traditional values will usher again a most enjoyable life in the laboratory.

However, this will mean a reduction in the publications list of the scientists which is given such great importance by committees for giving grants, for prizes and positions. Long publication lists is a feature of more recent times — clearly a departure from tradition. It has led to several ill effects. It is, therefore, no wonder that on page 182 of last month's

"Nature" (issue of January 15, 1987) the editors concluded as follows with regard to the problems of fraud in science:

> "As the economists would say, the research community has externalised the problem of making judgements about the qualities of its own members by brooding on bibliographies. Everybody's interest is to find a less slovenly way of making intellectual judgements about people and institutions..... Decoupling the literature from promotion prospects would be a better goal."

One way of limiting the uselessness of large bibliographies is to ask people to submit only about five of their best papers for evaluation. This procedure automatically makes the large bibliography redundent. Thus you will see our traditional values in scientific research are particularly relevent in present times and I think will always be so.

The second stimulus for this lecture was a letter I got from the University Grants Commission (UGC) and I will now deal with that matter.

Traditional method of selection of academic and research staff

The usual method of selecting lecturers in science departments has been to go by the research work they have done. I was therefore astonished to learn from the UGC about the National Policy on Education, 1986 which lays down that the teachers will be recruited on the basis of a common qualifying test. It states that the methods of recruiting teachers will be reorganised to ensure merit, objectivity and conformity with spatial and functional requirements. The pay and service conditions of teachers have to be commensurate with their social and professional responsibilities and with the need to attract talent to the profession.

In pursuance of this policy statment, it has been suggested in the Programme of Action for National Policy on Education that teachers will be recruited on the basis of a

common qualifying test, the details of which will be formulated by the UGC. I was told that the Commission had constituted a committee with a view to working out the modalities for the conduct of such a test.

The thought of a university lecturer being selected on the basis of a qualifying test was quite unpleasant. I discussed the letter with some colleagues and they also agreed that by the time a bright young man interested in doing good research had finished a Ph.D., he would have lost to a large extent the knowledge that he needs to perform well in a competitive examination. In my reply I drew the attention of the UGC to my Zakir Hussain Memorial Lecture in which I had given an indication of the mistakes we had made in the past by instituting untried changes in the organisational structure of academic systems. For example, in 1982, the Delhi University approved a merit promotion system. This scheme has proved disastrous. Similarly, I told them that selecting university teachers through a test would be another terrible mistake. However, the situation in colleges is different and the qualifying test might help to remove mediocrity and corruption in colleges where no research is expected.

The aim of the National Policy on Education is praiseworthy *i.e.* to try to reduce corruption rampant in certain universities particularly in some universities in the north of India. However, inadvertantly, the execution of their plan of selecting university lecturers through a competitive test would have eliminated the best people with ideal qualifications *i.e.* those with the ability to produce new knowledge and teach students how to generate new knowledge. No doubt the university lecturer has also, as a part of his duties, to impart known knowledge but that is relatively easy. I have discussed the matter informally with the chairman of the UGC and some members of the Commission. They agreed that what I said made sense. I do hope it will be possible to

reconsider the problem not only because it is risky to abandon traditions so suddenly but also because such a procedure for selecting academic and research staff has never been entertained by the best universities of the world.

Tradition relating to filling academic posts

There has been a tradition in many universities e.g. Delhi to fill acadamic positions by getting the best men in India. Many of the best professors appointed in the past have come from outside the universities both in India and abroad. This is still true of the European and American universities. In fact in Germany there is a rule that the highest grade of professor has to come from outside no matter how good the departmental faculty members are and how well they deserve the position. Thus, whereas the European and American universities have stuck to tradition, we in India have abandoned well-established traditions and have ended up in a mess because of the institution of the time bound promotion scheme. Combined with the rotation of headships on the basis of seniority of service the university system has just about been totally destroyed. One could even say that the kind of universities that existed in India in the sixties and the early seventies do not exist anymore. Now they are almost like intermediate colleges where the teachers are no longer interested in research but they take on Ph.D. students not for promoting research but in order to show that they deserve the next promotion after eight years or so. All professorships and readerships are now filled from within the departmental faculty.

Because of the security that the time bound promotion system provides, no member of the faculty (with few exceptions) is actually interested in doing research. He succeeds in remaining alive as a third-rate scientist by ensuring his place in the international fraternity by various methods, such as inviting foreign scientists to India and getting invited reciprocally. Few people find his papers of any value and so he remains mostly uncited in spite of scores of papers published

in national and international journals without a reputation. No longer is the department known in India and the world by the reputation of the head of the department traditionally appointed because of his fame and his standing in science and his ability to bring in grants and promote the academic growth of the faculty in the department. The British still do not have rotating heads of departments. So is the case in most German Universities. In the USA because some of the most productive scientists find that chairmanships take a lot of their time they have given up being chairman but at the same time they have assured that the chairman is a competent man, not just the senior most individual.

Thus the tradition of appointing professors from outside is still relevant and we must revert to this practice again. In the best universities this will mean that the professor they attract from outside will invariably already be a professor. In the case of lesser known universities the person from outside will be a reader who has built up a reputation in research and who has grants and apparatus that he could bring with him. It would follow that the reader in the top universities would need to seek professorships in the lesser universities. This movement will do a great deal of good to the latter who are seldom able to get good people.

I, therefore, recommend that the whole mechanisms of appointment of lecturers, readers and professors be re-examined by the UGC and that steps be taken to revert to the traditional system prevailng before the seventies. This needs to be done most urgently.

Tradition of humility

Traditionally, humility has been an outstanding characteristic of scientists. This has been gradually declining throughout the world. In the fifties I recall that even as a professor I didn't have the nerve to say that I was a physiologist. To me a physiologist meant people like Adrain, Katz or Eccles. In fact, I still cannot bring myself to say I am a physiologist when stating my occupation in a form. I know it is

foolish but it is part of the tradition in which I have lived. And so I often wonder how people can write on their letter-heads or sign as "Professor of Eminence". This is a feature only of the Agricultural Research Council and I think it should be reviewed. I know of some people who have this unfortunate designation and they feel extremely embarrassed outside the agricultural environment. This departure from tradition by the ARC must be remedied. Similarly, the CSIR should consider providing another designation for their so-called "Distinguished Scientists". Certain scientists are quite embarrassed by this designation. One is surprised by what we have done in the last twenty years because humility has been part of our Indian culture — a tradition about three thousand years old. We must promote it as it (humility) is an essential ingredient for good scientific work.

Tradition of 'small' research and interdependence

The tradition in University laboratories upto about 1950 was to try and carry out ones experiments meticulously with whatever one had or could borrow — mostly borrow. This meant having amiable relations with one's colleagues. Thus interdependence was an essential part of life in the laboratory. The change in the laboratories began with the large grants given to certain scientists in the USA and these grants kept on increasing. Soon in the sixties even physiologists talked of millions of dollars worth of grants. "Big Science" type of atmosphere became attractive and this attitude spread to several countries. This has now gripped Indian scientists who think doing research with little is below their dignity. It is not good work that concerns them but power and dignity. There is plenty of money for those who can write impressive grant applications. Such people, some of them with an unenviable past record acquire expensive equipment. Often due to the successful efforts of high pressure salesmanship, they buy equipment which they don't actually need and it therefore lies in the original packing cases for years. Greed and jealousy are now common features. You don't have to be nice to your colleagues anymore, perhaps invite him over for

a cup of tea, so that he will let you use his fantastic precision balance. After all you have two of your own, why bother with him!

The tradition of making do with little and building parts of your own set-up is essential for good work. They say you cannot do outstanding work with a 'small science' set-up. That's humbug and in my opinion a case of one-upmanship by the equipment manufacturers. High pressure salesmanship is not easy to resist particularly when 'big science' approach is promoted. I know what it is like but we still never buy new equipment unless it is actually needed desparately. Most of our equipment is more than 20 years old, some of it actually more than 35 years old. We repair this whenever it goes wrong.

It is necessary to encourage the traditional approach with regard to the equipment that any scientist and his team may need. It should be noted that some of the best work in life sciences in many countries, certainly in physiology, is still done by scientists with small budgets.

The same situation holds for size of research teams. It has become fashionable to have a large empire that does the work while you go on jaunts all over the globe yourself. In India at present the hallmark of a successful scientist, in the minds of laymen or even scientists, is the number of times that he goes abroad for committee work or for conferences. Among scientiest the thing in fashion is the so-called 'key-note' address and being invited to chair a session. Again this, *i.e.* big team approach is a departure from tradition. The best work is still done by one or two people working together with some help from post-doctoral fellows.

In this lecture I have given you an indication of some of the traditional values that we have ignored and the difficulties, some of a serious nature, that this departure from tradition has caused to the universities and scientific research in India. This has come about because we have chosen to take the easy path instead of overcoming obstacles. The situation

is still remediable but no time must be lost. There are people in India who can help us put our "scientific house" in order. We must take their guidance. I too have some concrete suggestions which I will be glad to share with those made responsible for providing a suitable plan of action.