DISCUSSION PAPER

• *ABSTRACT*

Modern science, which was an indigenous product of Western culture, is now being practised in many non-Western countries. This paper discusses the peculiar social, cultural and intellectual problems which scientists of these non-Western countries face in adopting Western science in their situations, with special reference to India. It is pointed out that, in addition to money and communication, it is necessary to have a proper psychological gestalt to practise science satisfactorily. The author analyzes his experience as a physics student in India and in the United States to clarify the nature of this psychological gestalt, and to explain what makes it difficult for non-Western scientists to acquire it.

Practising Western Science Outside the West: Personal Observations on the Indian Scene

Arnab Rai Choudhuri

This article is frankly what the title indicates: personal observations, and some generalizations based on them. It does not make any claim to being a scholarly dissertation presenting the results of thorough research work. I am a theoretical astrophysicist by profession, and my interest in history, philosophy and sociology of science is only that of an outsider. To give the reader some orientation, perhaps I should begin with a few words describing how and why I came to write this paper.

As a boy I grew up in the urban society of India, and my childhood interests were in literature and the humanities rather than in the sciences. Only when I was exposed as an undergraduate to the beauty of theoretical physics, and realized that it could give me as much aesthetic pleasure as the most beautiful poetry, did I finally decide to become a physicist. In a country like the United States, good students are divided up amongst several competing

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universities. But, in India, most good students of physics try to go to two or three well-known institutes for undergraduate training. Since I was fortunate to do my undergraduate work in two of the most prestigious institutes, I believe that it will not be too preposterous for me to claim that I had the best possible physics education available in India. After that, I came to do my graduate work at the University of Chicago, and I have now been here for more than three years.

From the very beginning, I could perceive clear differences between the qualities of scientific research activities at the University of Chicago and at the Indian institutes I have been associated with. Here I have a few Indian friends with backgrounds similar to mine, and amongst ourselves we have quite often discussed various questions like these: What went wrong with Indian science? Why have our best institutes so far failed to reach the standard of the best Western institutes? What can we do to change the situation? If we want to practise science, then is it essential to stay abroad, or shall we be able to continue our research activities even if we go back to India? If many of the well-trained Indian scientists of our generation decide to go back to India, can we possibly achieve any kind of dramatic transformation of the whole scenario? I believe that these are problems common also to scientists of some other countries, such as China and Japan,[1] which, like India, already have significant scientific élites, but so far have failed to become part of the scientific metropolis of the world. This paper mainly discusses the peculiar problems of such societies, which almost seem to be on the verge of making the last leap of becoming truly independent centres of scientific creativity, but have not quite made it yet. These problems are somewhat different from those faced by countries where scientific life is just beginning.

To an occidental philosopher or sociologist of science, my questions may just be remote intellectual issues. But, for many of us, caught between two cultures, they are very much real-life questions. While musing over them, I have quite often been led to reflections on the nature of the scientific enterprise itself, and these reflections have gradually made me interested in the philosophy and sociology of science. Most philosophers and sociologists of science, who have studied the nature of the scientific enterprise, grew up in Western society and were trained in Western universities. For them, it is sometimes very difficult to visualize the peculiar problems which a scientist practising Western science outside the West may face. I think that we, who have seen Western science both from the outside and the inside, develop an unusual perspective, and become aware of certain aspects of the scientific enterprise which a Western philosopher or sociologist very naturally overlooks. Unless you ever have the experience of being locked in an airless chamber, you never realize how important air is for breathing! In this paper I shall try to present some of the ideas which have been taking shape in my mind over the last three years. Conversations with several friends were particularly important in the formation of some of these ideas.[2] However, the way I am presenting these ideas here is entirely my own, and I shall not be surprised at all if none of my friends completely agrees with me. One unavoidable limitation of this paper is the fact that it is heavily prejudiced by my own physics background. I have mainly discussed those issues which I happen to know in some depth. It is necessary for engineers, biologists, psychologists, sociologists and philosophers to study different aspects of the problem to get a fuller picture.

It is worthwhile to remember that when we look at the Western scientific tradition carefully, we no longer have the image of something uniformly spread over a homogeneous part of the globe. Within the Western world itself, centres of scientific creativity have shifted again and again during the last 300 years.[3] In fact, the United States, which was in an intellectual backwater at the end of the nineteenth century, faced a problem somewhat similar to what we are discussing here, when it tried to implant a scientific tradition modelled after Germany. However, the problem of the efficient implantation of science in modern India or China can almost be regarded as qualitatively different, for two reasons. Firstly, the social, economic and cultural differences between them and their Western models are enormously vaster than the differences that might have existed between the United States and Germany a hundred years ago. Secondly, the astonishing growth of science in the present century has made the problem of starting from scratch and catching up with latest research frontiers bigger by orders of magnitude.[4]

Science versus the Arts

The artistic works produced by a particular culture are usually very much defined in scope and content by that culture: certainly the *Iliad* could not have been composed in China. The extent to which the parent culture is reflected in a work of art, however, varies from one work to another. In any case, whenever the artistic works of one culture are judged by another, a different value system is used. There is a universal character in Shakespeare's genius for which his works will probably appeal to human beings of all countries and at all times. But non-Western readers sometimes find it difficult to understand why major Western critics have ranked Dante with Shakespeare. To a person not well versed in Western culture, the *Divine Comedy* is just an interminably long, dull poem containing occasional passages of fine poetical imagery, but quite often dealing with ideas which are — to say the least — downright silly! On the other hand, the contents of the scientific works produced in a culture are generally believed to be independent of that culture. It is true that recently a school of sociology of knowledge has challenged this view;[5] but their ideas do not seem acceptable to many people (including myself), and since a debate on this issue is not directly relevant to my theses in this paper, I will not pursue that issue further here.

Let us now imagine that there is a culture A with traditions of both scientific and artistic activities. Suppose it comes into contact with another culture B, which does not have those traditions. Now B tries to develop its own scientific and artistic traditions, modelled after A. Will it be easier for B to develop the scientific or the artistic tradition? Probably most Western philosophers of science would cast their votes for the scientific tradition, since science is supposed to be more universal and intercultural. But the surprising historical fact is that, when we study the impact of Western culture on India in the nineteenth and the twentieth centuries, the evidence clearly goes the other way.

India came into contact with Western culture at one of the lowest points of her history — at a time when there was almost no intellectual activity of any consequence going on in the country. At the beginning of the nineteenth century, the state of Bengal in the eastern part of India became the first to be exposed to Western culture. Very soon after this, there started a period of remarkable intellectual fermentation, usually known as 'the Bengal renaissance'. One of the odd features of this renaissance is that it was a period of rather unequal growth for different aspects of culture. Before the renaissance, Bengali literature consisted of a few pieces of medieval poetry, and Bengali prose was virtually non-existent. From the mid-nineteenth century, Bengali writers started using the Western literary forms of novel, short story and lyrical poetry, and, in a very short time, Bengali literature blossomed into unprecedented maturity, setting up a model for the other Indian languages. At least in two of the Western literary forms — the short story and the lyrical poetry — it produced works which can be favourably compared with the best works that were being produced in Europe at the same time.[6] It is true that not all artistic activities of the West had a similar impact on Bengali culture. The Bengal renaissance was a period of revival of the classical music of India, but Western music remained almost unknown. One of the main reasons for this ignorance is certainly the fact that, before the development of big gramophone companies, music was not easily accessible to anybody. An Indian musician of the early-twentieth century had no way of knowing what a Beethoven symphony really sounded like.

If we now turn to the sciences, then we find a most peculiar pattern. There were occasional stray works of high quality: two Bengali physicists, Saha[7] and Bose,[8] made two very important contributions to theoretical physics in the years 1920 and 1924 respectively. But we do not see any steady output of fairly good, though not outstanding work; in other words, we do not see any scientific tradition which could offer even remote comparisons to the contemporary scientific traditions of Western countries. A reader without any knowledge of the Bengal renaissance may wonder to what extent this is a personal opinion of mine. I can assure such readers that the statement that the Western literary tradition could be transplanted to the soil of Bengal much more effectively than the Western scientific tradition is a statement about which probably all *well-informed* Bengali scholars will unanimously agree. The Bengal renaissance was a renaissance of new artistic, religious and political revival — a renaissance virtually without creative science!

Scientific Practice: Total and Partial

In order to judge whether a scientific community is functioning properly, we have to consider whether it satisfies the following three criteria: (a) there are members of the community well versed in the well-established scientific knowledge of the past; (b) there are members of the community who are keeping themselves abreast with the current scientific developments; and (c) there are members of the community who are *steadily* making worthwhile research contributions to science. The word 'steadily' in the last criterion is of the utmost importance. If the community produces occasional works of outstanding quality, but no steady output of average research works, then criterion (c) is not satisfied. The main difficulty is that we cannot provide an entirely objective method of deciding to what extent a community satisfies any one of these criteria. However, let us suppose that we compare various scientific communities and develop a grading system, such that a community would get (say) a score between 0 and 10 for each of the criteria (the maximum possible score for a community being 30). A more sophisticated system may consider weighting the three criteria differently, but, for our present discussion, such elaborate sophistication is not necessary. Let us now introduce a definition: *if a scientific community gets high scores in all three criteria, then we shall say that it is practising 'total science'*. There is some kind of hierarchy amongst our criteria. It is highly improbable that a community will score highly in one particular criterion without high scores in preceding criteria. But the opposite is certainly possible. *If a community either gets a high score in (a) without high scores in (b) and (c), or gets high scores in (a) and (b) without a high score in (c), then we shall say that it is practising 'partial science'*.

We now come to the crux of the matter. In some of the best institutes of India, China and Japan, there are scientific communities which have so far failed to practise total science satisfactorily. The question is: are they practising partial science, or do they get uniform medium scores for all the three criteria? I am not in a position to give a final verdict for China or Japan. But I believe that I can say something about India, based on my personal experience. All the physics departments I have been associated with in India tend to show the same general pattern of partial scientific practice — scoring very high in (a), but rather low in (c). As a result, a physics undergraduate in a place like the Indian Institute of Technology at Kanpur is given a particularly thorough and rigorous training in the well-established branches of physics, but with absolutely zero exposure to really creative research. The danger of such a training is that it develops a misleading philosophical conception of the nature of the scientific enterprise in the student's mind. When such a student has to compete with Western students in *courses and tests*, he usually fares extremely well,[9] but is quite often at a loss when beginning research. This point will be discussed at some length in this paper.

My claim is that there are three main factors responsible for this malady:

(a) money and organization;
(b) communication (that is, the problem of being isolated); and
(c) proper psychological gestalt.

I think that these three factors cannot be weighted relative to each other in any absolute way. Their relative weights vary from one particular case to another, depending on the circumstances and the nature of the scientific pursuit under consideration. The first two factors are the more obvious ones and would probably spring to the mind of any reasonable individual giving a thought to this problem. But the third factor is more subtle in nature and demands a much more elaborate explanation. The main aim of this paper is to elaborate on this notion of 'proper psychological gestalt'. Even when the sociology of science was in its infancy, that remarkable chemist-turned-sociologist, Michael Polanyi, observed:

> Modern science is a local tradition and is not easily transmitted from one place to another. Countries such as Australia, New Zealand, South Africa, Argentina, Brazil, Egypt, Mexico, have built great modern cites with spacious universities, but they have rarely succeeded in founding important schools of research . . . Those who have visited parts of the world where scientific life is just beginning, know of the back-breaking struggle that the lack of scientific tradition imposes on the pioneers. Here research work stagnates for lack of stimulus, there it runs wild in the absence of any proper directive influence. Unsound reputations grow like mushrooms: based on nothing but commonplace achievements, or even on mere empty boasts. Politics and business play havoc with appointments and the granting of subsidies of research.[10]

It has been pointed out again and again, by many authors, that it is immensely difficult to organize research institutes in underdeveloped countries and to keep abreast with latest developments.[11] However, the point I want to make here is that, if we tacitly assume that these are the only obstacles in the way of starting a new scientific tradition, and that science would start blossoming in an underdeveloped country as soon as efficiently organized institutions are set up with good libraries and adequate research facilities, then we would be missing a most vital aspect of the problem. Science is one of the profoundest forms of creative expression of the human mind. Unless we have human minds properly conditioned to create science, it is absurd to expect science to stream out of buildings, libraries and laboratories, however, well-equipped they may be.

It is not difficult to provide empirical evidence that factors (a) and (b) are insufficient to solve our problem just by themselves: we do need something more. There are at least a few places in India (the Indian Institute of Technology at Kanpur, where I did my master's degree, is one example) which have sufficient facilities to maintain a tradition of scientific research. But these places show strong tendencies to grow into outstanding centres of *partial science* rather than true centres of creativity. If we take a historical vantage point, then the matter becomes even clearer. Only after World War II did the sciences start becoming big business, in which money and communication became overwhelmingly important factors. But during the period between the two world wars, money and communication did not have the importance they have today. Scientific research was still being done, in most places and in most branches of science, by university professors who did not have much research money apart from their salaries. Hence, if we find that some communities practising partial science in this period failed to achieve total science, then we have to look for explanations of a different nature. During this period, India and Japan could boast of a few physicists who made outstanding contributions to physics: Bose, Saha and Raman from India, and Yukawa and Tomonaga (a little bit later) from Japan. These are all physicists who remained in their native lands and did their research work there. In this list I do not include Indian physicists like Bhabha and Chandrasekhar,[12] who did important work at Western universities during this period. Because of my own physics background, I am citing examples only from physics: I do not have sufficient knowledge or competence to judge the situation for the other sciences. However, these examples should be enough to illustrate my point. The achievements of this handful of brilliant Indian and Japanese physicists show that the scientific communities of India and Japan must have already been fully exposed to Western science. But these scientific communities did not satisfy our criterion (c): they did not produce a steady output of competent, though not outstanding, research. Hence we have to conclude that they were just practising partial science and failed to achieve total science even at a time when money and communication were not overriding concerns; and hence the necessity of a third factor to explain the incomplete nature of these scientific traditions. The scientific careers, research outlooks and almost inevitable ultimate frustrations of the physicists in my list can provide the subject-matter of a fascinating study that will certainly illuminate many points which seem rather obscure now. This is a gold mine still mostly unexplored.

The Unusual Nature of the Scientist's Calling

I can still very vividly remember my feelings on one particular day. It was during the time I was writing up my first paper on theoretical astrophysics at Chicago. I was working in my office, which has large glass windows overlooking the Stagg Field playgrounds. It was a lovely afternoon in late summer. The fluffy clouds almost seemed to be set aflame by the rays of the setting sun. There were children playing on the field, boys playing soccer, men and women jogging. Beyond the playground, I could see the busy traffic of 55th Street and far, far beyond, the skyscrapers of downtown Chicago on the clear horizon. Suddenly I was gripped with an almost intolerably agonizing feeling that at such a time I was spending my hours on a work, about which this bustling, joyful world, teeming with life in this summer evening, does not care at all. All over the world my paper will probably be read and understood by twenty professionals who bother about magnetic fields in the sun. A novelist or a poet would certainly consider his book a failure if only twenty people read it! But we, theoretical physicists, are doomed to live in another world — the world of Eddington's famous second table.[13] We cannot share our intellectual excitements even with our spouses or closest friends; we are perpetually estranged from our fellow human beings. A good scientist, of course, has personal moments of intellectual triumph, but is forever denied those thrills which a triumphant musician, standing in front of the stage, feels when the whole audience bursts into applause. In popular stories, scientists are very often depicted as awkward individuals, unsuited to everyday life — people who boil clocks for breakfasts, and keep on doing calculations on sand even when their lives are in immediate danger. If all scientists were really like this, then they would probably have been eliminated from our society by the processes of natural selection! However, I believe that these popular stories tell us one truth — the fact that science is a very strange enterprise, although scientists themselves may not necessarily be strange people. (Curiously enough, some writers have also tried to argue the opposite extreme: that scientists always have to be 'normal guys'. E. T. Bell attempted to show that no great mathematician was ever a sex pervert![14])

Nearly everyone likes to tell stories attractively; most people like to hum tunes at least to themselves; almost all children like to draw pictures, though this tendency may eventually get suppressed in a mature adult for various reasons. In a sense, all literature, music and art can be thought of as just supreme manifestations of these natural instincts. In all societies, there are natural demands for products of such artistic creativity. Storytellers and singers have important roles even in the most primitive of all societies. But most societies do not have a similar role for the scientist.[15] Science cannot be thought, in the same way, to be the quintessential product of some natural human instinct. One may try to argue that curiosity about nature is the natural instinct leading to science. But that does not work too well. This instinct may have been the original cause of scientific speculations in ancient civilizations, but most of these civilizations did not create science in the sense we understand it today. Science, in the modern sense, was possible only in certain cultures under certain special circumstances. Many civilizations continued to produce instinctive scientific thinking which did not crystallize into scientific enterprise. Most scientists, in sophisticated branches of science, usually work on secondary problems derived from the basic paradigms of the particular discipline. These problems very often do not make any sense independent of the paradigms, and the scientist working on them is not motivated by curiosity about nature in any direct way. Such a scientist has to create for himself a dreamworld of abstract formalisms, cryptic symbols and ideas which are very remote from everyday life. Then he becomes so much engrossed with this world of shadows that it becomes a real world for him — a world with which his hopes and fears are tied up. Though a particular problem of this world may interest less than a dozen people, he still feels compelled to invest all his time and energy on it. *This is not a very natural inclination for a human being*, in the sense that literature or art are, even when grant money is readily available to support the endeavour.[16] As a result of this artificial orientation, scientists may become obsessed by the problems in their own dreamworld, but may remain completely unmoved by a problem of similar nature which just happens to lie outside that world. Let me clarify my statement with an example. Physics is the study of inanimate nature, and it may normally be expected that somebody curious about inanimate nature would be extremely anxious to know why the earth has a magnetic field. But most physicists, working in areas of physics other than astrophysics, are amazingly ignorant in this matter and, what is worse, most of them show no particularly strong, burning desire to learn about it. Certainly, an average physicist is not a person with any special kind of overwhelming passion to understand nature, in general. We thus have to rule out the possibility that curiosity about nature is the impulse that motivates a typical practitioner of a mature science.

Once we have grasped the fact that scientific research is, in the sense described above, a rather artificial activity and not arising from any natural instinct and, under most circumstances, not corresponding to a natural demand in society, we are almost ready to understand the notion of 'proper psychological gestalt'. A poet may certainly feel an inner urge to create poetry, but most poets would not have gone very far unless they felt that there was a demand for their works amongst people around them. A poet banks on posterity for appreciation only when he fails to draw the attention of contemporaries. But why does a scientist create science? Under certain circumstances, it seems that a scientist just comes to regard himself as a person whose job it is to create science; science becomes a *calling* for him. Almost like the early capitalist entrepreneur described by Max Weber, the scientist's chief reward is 'the irrational sense of having done his job well'.[17] Once science has become a calling for a scientist, in spite of the fact that scientific research is such an unusual pursuit, it is possible for a scientist to get emotionally involved with his work.[18] It is very fortunate that an average scientist normally does not have to proceed always through bold conjectures and ruthless falsifications in accordance with Popper's remarkable scheme of science.[19] Such a procedure would have produced enormous mental stresses on an emotionally involved scientist. Only at certain critical moments must scientists advance in a more or less Popperian way. While describing Heisenberg's work on the formulation of quantum mechanics, Dirac gives a clear expression of the decidedly anti-Popperian psychology of a creative scientist: 'I think it is a general rule that the originator of a new idea is not the most suitable person to develop it because his fears of something going wrong are really strong . . .'[20] Other examples of the emotional momentum of scientific work are not difficult to find.

How does a modern scientist come to have a calling? This is a provocative question which does not seem to have been analyzed adequately. To answer it, we have to fit the two pieces of our jigsaw puzzle together. We have seen that (1) human beings are not born with any natural instinct which may provide a motivation for scientific research, and (2) to practising scientists, their dreamworlds become so real that scientific research seems to be a natural activity to them. The only way of reconciling these two facts is to assume that during the process of intellectual development to scientific maturity, a scientist undergoes a mental transformation which changes his outlook completely. He now has a totally different way of looking at things. His particular 'dreamworld' becomes very real to him, and he is mentally prepared to practise normal science. The mentality acquired in this process is what I call 'proper psychological gestalt'. I hope that this notion will become clearer in the next two sections, where I go into more detailed discussion of its various aspects. If a very brilliant person possesses the necessary scientific knowledge, but has not undergone this transformation, then he may once hit upon an important scientific discovery, but he will not be able to practise total science steadily. We are now going to look at the learning process in science in order to discover the origins of this transformation.

The Nature of the Gestalt Transformation

Let us try to compare and contrast the intellectual developments of a typical good Western student studying science in a good Western institute, and of a typical good non-Western student studying science in a good institute in his own country. Let us name these students *W* and *N* respectively. We further assume that neither of them is an extraordinary genius, but that both are highly intelligent persons with the potential for becoming competent working scientists.

First consider the Western student, *W*. If he is going to a good school, from the very beginning of his college lessons he is associated with a community practising total science. He has the opportunity of coming across faculty members who are distinguished scientists. He may have a few courses from them, and, in some cases, he may even get a chance to work on a research project with one of them while still an undergraduate: but, whatever is the situation, he can always see what kind of human beings these practising scientists are, and it is unlikely that he will ever entertain some false myth about what a scientist is. If he is keen on becoming a scientist himself, he may consider some of his professors as his heroes, and gradually his impressionable young mind builds up his own private dreamworld in which the activities of these professors assume a living reality. Thus the two different processes of (1) acquiring scientific knowledge and (2) undergoing the gestalt transformation progress, in his case, simultaneously and side by side. Sometimes they may progress in such harmonious unison that *W* may not even be aware that these are two different processes. Thus he undergoes his gestalt transformation without any sudden shock, very often even without conscious knowledge; and, by the time he joins his graduate school, he roughly has an idea of what is expected of him. Even if, in a particular case, *W* may not have the proper gestalt before starting research, he acquires it quickly as soon as he begins research under the aegis of an active group.

But now consider the extremely interesting case of the non-Western student, *N*. For the sake of fair comparison, let us assume that he is going to one of the best institutes in his country. There he comes across very brilliant professors capable of giving highly stimulating courses, and he has to compete with other classmates of outstanding calibre. But, in spite of everything, *N* is in a community practising partial science only. Consequently he, most probably, does not have an opportunity of knowing a person who is carrying out good research successfully. In his textbooks, however, he reads about the worlds of scientists of other countries, and he naturally starts wondering what sort of human beings these scientists are. If first-hand experience is totally lacking, he tries to reconstruct the personality of a typical scientist in his mind on the basis of the clues available to him. What are his available clues? *N* knows that there are two main ways of showing his excellence: by doing well in his tests, and by giving evidence that he understands the fundamental concepts of his discipline. He may come to think that the supreme test of a scientist also lies in his possession of those particular qualities which are necessary for showing real excellence in these two ways. *N* also knows that his best professors are not very well-known scientists; so he thinks that a scientist doing worthwhile research has to be much more brilliant than his best professors, and better equipped to show his excellence in the two standard ways. Now how does *N* show excellence in his tests? In India, we mostly had to take in-class tests. To do well in such a test, *N* has to be thoroughly prepared before he walks in to take it. If he finds that he can solve some of the problems on the basis of the knowledge he already possesses, he solves them. On the other hand, if he finds that his current knowledge is inadequate to solve some problems, he has to leave them unsolved. In the absence of any other data, it is not at all unusual for *N* to think that scientific research must simply be a more magnified version of this test. Since he believes that a successful scientist must be smarter than his best professors, he naturally imagines that this successful scientist must possess a much more complete knowledge of the discipline than his professors, so that, whenever this scientist encounters a problem, he can just sit down and solve it, in the way a college student solves his test problems. In India, a very bright classmate of mine believed that if a person wants to be a theoretical physicist, then before he can hope to start any research, he should have such a command of all the branches of theoretical physics that if somebody stops him in the middle of a street and asks him to solve one of Jackson's problems,[21] he should be able to do it immediately. Another bright classmate of mine was extremely surprised when he read in a book that Heisenberg was almost ignorant about matrices when he started having the first ideas of matrix mechanics, and had to learn more about matrices before he could pursue his ideas any further.[22] According to that classmate, a rational reconstruction of Heisenberg's discovery of matrix mechanics should be like this: Heisenberg must have been an extraordinary genius, who was completely well-equipped with all the technical apparatus that may be necessary to develop matrix mechanics, even before he started any research, and while pondering over the deep mysteries of the quantum world, as soon as he got his ideas, like a diligent schoolboy he sat down to work them out immediately.

There is another factor which contributes to this unfortunate myth of a true scientist, developing in the mind of *N*. Though he is under the impression that his best professors are not *on a par* with the scientist about whom he reads in his textbooks, he still feels intellectually stimulated by them and hero-worships them. But very often these brilliant professors are themselves frustrated individuals, who, very clearly, perceive that they are failing to produce research works which truly reflect their abilities, and who feel that the best thing they can do now is to submit to their fate in a gradual, stoical manner. Many of them find it exciting to give good courses which may inspire gifted students, and they usually feel inhibited in talking about their unsatisfactory research activities to their bright students. On the other hand, there are also professors who are not that brilliant, and who may not be able to impress the students with their intellectual calibre. Often, some of these professors try to do whatever research they can and talk to the students about their research projects. Now what does *N* conclude from all these? The professors, who are his intellectual heroes, talk about fundamental issues only, and never about their research;

whereas the non-heroes vainly try to impress him by talking to him about petty research projects. *N* is led to the conclusion that it is a noble venture to contemplate the fundamental problems of science, whereas there is something almost inglorious about getting too preoccupied with particular problems of lesser magnitude.[23]

We can now summarize what *N*'s conception of the scientific enterprise is:

> A good scientist must be a genius, intellectually much superior to *N*'s best professors. He is fully equipped with all the technical tools which may possibly be necessary for any kind of research he may wish to undertake. He usually spends his time pondering over the fundamental issues of his discipline and when this divinely inspired individual happens to have a brilliant idea, he works it out in a straightforward way without much trouble, like a smart schoolboy solving his test problems.

I shall call this the 'schoolboy conception of science'. The textbooks also conspire to give a similar wrong impression of science: since they discuss only the most outstanding achievements of a scientist, they necessarily tend to project an image of research as a discontinuous process. Take, for example, the case of the great physicist Max Planck. Though he was a vigorously active scientist, an undergraduate usually comes to know of only one discovery of the first order made by him — the discovery of quantum theory. If the student has a scholarly bent, then he may know something about the series of extraordinary physics textbooks written by Planck. Altogether, the image of Planck formed by the student is that of a great scholar and a profound thinker, who engaged himself in research only when he had ideas of fundamental importance.

If one looks at the Indian scene, one will occasionally find a very gifted student who has undergone the gestalt transformation spontaneously, and has a mature conception of the nature of the scientific enterprise. But these are very rare cases. For total science to flourish in a community, it is absolutely essential that a large number of intelligent members of the community have the proper psychological gestalt. Out of my own personal experience, I can vouch that most Indian students who have intrinsic potential to be fairly good, though not outstanding, practitioners of total science, but who have never been to a Western country, continue to have either the schoolboy conception of science, or no conception at all. Nowadays, I feel at a loss to understand how I, along with nearly all my classmates, entertained such absurd views only a few years ago.

Since these views afterwards seem so incredible, even to persons who once held them, it is no wonder that they will puzzle persons who never held them at all. Whenever I have discussed this problem of proper psychological gestalt with my Indian friends here, we never had any difficulty in mutual communication. We had all grown up scientifically in more or less the same way, and could understand each other perfectly. But very often, when I have tried to explain this problem to my American friends, at first they could not even understand what I was talking about. Similarly, if somebody had described the true nature of the scientific enterprise to me when I was still in India, I would probably have had difficulty in comprehending that description. I read Thomas Kuhn's *The Structure of Scientific Revolutions* after coming to the United States, and, though I could not agree with Kuhn in many places, was deeply impressed by it. Had I read this book in India, I would probably have been unable to appreciate many parts of it — especially the chapters containing Kuhn's brilliant analysis of 'normal science'.[24]

We now have to consider *N*'s reactions when he comes to a Western institute. Suppose he comes to do his graduate work at an outstanding American university, which is nowadays very usual for bright Indian, Chinese and Japanese students. During the first few months, his experience may well be one of utter shock, which destroys a large part of his mental edifice. He finds that most of the successful scientists he meets now, other than the very top ones, are no intellectual matches at all for the brilliant professors whom he had previously. At first he is almost dismayed to realize that many of his new professors, who are well-known scientists, often lack a deep conceptual understanding of the fundamental issues. Then it occurs to *N* that these people are so successful in research not because of any superior intrinsic calibre, but just because of a different mental buildup and a different approach to science. On his exposure to research, he can often also become troubled in another way. At least in the case of physics students, I know that many of them choose their careers as a result of being attracted by the aesthetic beauty of different branches of theoretical physics. But this particular sort of aesthetic beauty arises mainly from the magnificent structures of these branches, and is possible only for a finished product. A building in construction is not always an aesthetically pleasing sight. *N* finds that a living research field, in which a grand structure has not yet emerged, and different parts just make up a formless mess, is in principle incapable of offering the kind of aesthetic pleasure which can be obtained from a finished product. *N* realizes that the treacherous charm of his first love was actually the mysterious, cold beauty of a dead body. A living science often moves in a rather ungainly and awkward fashion. I believe that a similar case can also be made for works of art. A finished piece of work by a great master may appear so fresh and spontaneous that we tend to forget how the master had toiled and sweated over it for many uninspiring hours.

Thus we see that though the gestalt transformation for a Western student is a rather smooth process, the belated transformation of *N* on coming to the West is full of sudden surprises, and can often be quite painful.[25] Hence, in contrast to his Western colleague, the non-Western student is usually much more conscious of this transformation. At least, that is the clear impression I have obtained from my conversations with a large number of both Western and non-Western scientist friends. The non-Western student, who comes to a Western institute for training, at least eventually experiences the transformation described above. But those non-Western students, who decide to do research in their own countries, may continue to be handicapped.

A Normative Structure of Partial Science?

The last section analyzed the process by which a scientist may come to possess the proper psychological gestalt. However, I want to emphasize that this gestalt is not just passive knowledge of how to do research; it is an active disposition of the mind, which is possible only after one has undergone the transformation described above. A scientist with the appropriate gestalt thinks of himself as a person who is just obliged to create science. It often happens that a student from an underdeveloped country acquires the gestalt while studying in a Western university, but, on coming back to his own country, quickly loses the active mental disposition for research due to very commonplace things in everyday life. Suppose a competent person with proper scientific training is placed in a community practising partial science. If nobody else from the community goes to the lab in the evenings and he does so, then his wife may begin wondering why her husband is behaving so differently from his other colleagues. If the scientist is lucky to have a reputation as a genius, then he may attract general reverence. But if he just has the reputation of being a competent professional (and that, too, amongst scientists in foreign countries), his wife, for instance, may very well consider him weird and deviant. His colleagues and neighbours may start passing remarks on his mental stability. By contrast, while those in an American scientist's immediate social circle may not have the slightest understanding of what he is doing, they can at least see that it does not differ greatly from his colleagues' work. A community of partial science almost seems to have a system of social control which swings into action as soon as somebody behaves in a way that deviates from the usual norms of that community.

This brings us to the question of whether we can postulate a normative structure for partial science. Since I am not a sociologist, I will only attempt a very short discussion of this highly provocative issue. In the hands of Robert Merton and his followers, the functionalist approach has become one of the central approaches in sociology of science.[26] Though several sociologists have pointed out difficulties with this approach,[27] we can still regard it as a first order approximation. It seems that if one studies a community of partial science within the framework of a functionalist approach, then one can obtain a remarkable pattern of normative behaviour. Max Weber has pointed out that the life-style of the Continental textile industry in the pre-capitalist era was rather idyllic:

> The number of business hours was very moderate, perhaps five to six a day, sometimes considerably less; in rush season, where there was one, more. Earnings were moderate; enough to lead a respectable life and in good times to put away a little. On the whole, relations among competitors were relatively good, with a large degree of agreement on the fundamentals of business. A long daily visit to the tavern, with often plenty to drink, and a congenial circle of friends, made life comfortable and leisurely.[28]

When I read this picturesque passage for the first time, the vivid image of a community practising partial science leapt to my mind. There is a kind of idyllic aspect in the life-style of such a community also. Professors give intellectually stimulating courses, and students discuss fundamental issues with them. Since the tremendous pressure of producing research is absent, members of the community do not have to spend too much time on hard work in one day. There is ample time to enjoy other things of life, to cultivate broad intellectual interests in diverse subjects, and to have pleasant social chats with friends and family members.[29] Just as the capitalist entrepreneurs, who disrupted the comfortable life of the textile industry community, faced tremendous resistance, similarly a newcomer, who has been trained in a Western centre of science and who attempts to instil the spirit of research in a partial science community, faces strong resistance. Remarks are passed: 'Does he consider himself to be an Einstein?' Sometimes, however, we see the emergence in such communities of extraordinary individuals with great personal charisma. Occasionally such charismatic pioneers are able to inspire temporary bursts of vigorous research; but, after that, the partial science community again lapses into its usual state.

Since the role of the normal research scientist is absent in such a community, and there is no standardized system of allocating honours on the basis of scientific creativity, there develops a hierarchy of social stratification in which scientists who hold important bureaucratic positions may have very high social prestige. Many second-rank Indian scientists, who achieved some recognition as associates of somebody like Saha or Raman or by working with some scientist abroad, afterwards showed excessive eagerness to take full-time bureaucratic jobs as chairpersons of different committees or vice-chancellors of universities or directors of research institutes. Since the independence of India, there has been a mushroom-like growth of all kinds of commissions and boards in order to promote science in the country, and the chairpersons of these organizations often possess a considerable amount of political power. Because of the nature of such jobs, some of these bureaucrat-scientists occasionally become controversial public figures. Some of them have even been accused of doing damage to the cause of Indian science by such means as personal favouritism, and sometimes their own scientific credentials also have been questioned. It is often extremely difficult to make an objective assessment of the roles these men have played in the scientific development of India. When I was an undergraduate in Presidency College (where the two Bengali physicists Bose and Saha studied), I, in collaboration with two classmates, undertook the project of writing the first history of the physics department of the college.[30] We had to start from scratch, and interviewed about 25 people who had been associated with the department at various times. During many of these interviews, we had occasion to discuss some of these public scientific figures who were influential enough to control the destinies of scientists in the next generation. We quite often found that one man's Messiah was another man's Mephistopheles, and sometimes it was hard to reconcile two different accounts about the same person.

I would like to suggest some general features of a partial science community. They are, however, rather broad trends, and are sometimes observed even amongst non-scientific intellectuals of underdeveloped countries. Some of these trends have been analyzed by various authors, though sometimes in quite different contexts; but their relevance to the present discussion should be obvious. Here I shall just touch on these points, citing some appropriate references.

Partial science communities usually lack self-confidence. They are not only 'provinces' of the true 'metropolises' of science in the West, but they come to regard themselves so much in that way that they often cannot have any faith in their own judgement.[31] All their standards are set by the scientific communities abroad.[32] But when a member of the community is able to make a contribution outstanding enough to create a big impact in the West, he is suddenly elevated to the status of a mythical hero. He becomes a symbol of hope for the whole community, and exaggerated claims are advanced on his behalf.[33] Another prevalent feeling in such communities is that of being cut off from the people.[34] It is repeated again and again, in a ritualistic fashion, that one should not just try to copy the West, but instead develop an indigenous scientific tradition incorporating the philosophical and cultural traditions of the society in some way.[35] Those clear-sighted persons in the community who have strong faith in the internationalism of science, and who believe that modern science has to be done (at least in the near future) in the way Western scientists do it, are frequently accused of being brain-washed by overtly Westernized conceptions of science and rationality.[36]

There is an interesting linguistic practice still prevalent in India, which I consider to be a highly significant clue to the attitude of Indian society towards science. At present I am practising science in the United States, and here I always describe myself as a 'scientist'. But if I describe myself in a similar way when I am visiting India, many of my friends and relatives will be rather upset, and will probably consider me to be a most proud and conceited person. In India, it is generally thought that the word 'scientist' should be applied only to persons who can claim spectacular achievements in science. It is a word reserved for persons like Bose and Saha, and is not to be applied to people like me who are just practising science as a profession. Whether the label 'scientist' should be given to an individual is determined by his past singular achievements, and *not by his current professional occupation*. I leave it to the reader to consider the implications of this curious linguistic practice, and its possible relations to a schoolboy conception of science.

The Unbroken Circle

It is really remarkable that a society can be fixated at the level of partial science for a very long time. I have already hinted that, if we consider the sociological and organizational developments of science in this century, then we can roughly think of two different periods. The first period, which ended around the time of World War II, was the period during which science thrived somewhat like a cottage industry — an era which almost seems romantically nostalgic to us nowadays. Then, after World War II, the second period gradually started — the age of Big Science. The problems of practising science in the non-Western countries during these two periods were very different.

Though, in the present age of Big Science, most good Western universities offer a large number of assistantships and fellowships accessible to bright students from anywhere, the situation was not like that in the age of Little Science. Of course, Indian boys from very rich families always used to be sent to Oxford and Cambridge. The Indian physicist Bhabha, who came from the wealthy Parsi community, and who was later to be the founder of a major Indian research institute (the Tata Institute in Bombay), was promptly sent to England at the tender age of sixteen. But the other bright Indian boys, who were not lucky enough to be born with silver spoons, continued to learn science as authorized knowledge to be gleaned from textbooks written by foreign experts. They never saw science in the process of being created, never acquired the proper psychological gestalt — and hence many of them, who must have possessed the intrinsic ability to create science, never became aware of their potential. There were, of course, exceptions. Raman and Saha, two completely self-trained Indian physicists, learned how to dance to the right tune on their own, and were able to guide successful research teams, at least for limited periods of time. But even among the very best Indian physicists, not everyone had the right psychological gestalt for research. The physicist Bose,[37] whose formulation of quantum statistical mechanics is probably the most fundamental contribution to physics made by any Indian to this day, apparently continued to have a schoolboy approach to science through the most important years of his life.

It is instructive to compare the personalities of Saha and Bose, the two stalwarts of physics in the Bengal renaissance. They also happened to be classmates at Presidency College of Calcutta, probably the most famous undergraduate college in the whole of India. Saha was born into an extremely poor village family. Since his family was financially dependent on him, at first he aimed at getting some well-paid, secure government job. Only when such jobs were denied to him because of his political affiliations,[38] was Saha virtually forced into a scientific career. Fighting against all kinds of odds, he was able, at the age of 27, to solve some of the frontier problems of astrophysics by formulating his Saha Equation and applying it to give the first satisfactory explanation of stellar spectra. After this work he won a scholarship to study in England and Germany for one year. But the young scientist, who had already written papers which were regarded as epoch-making contributions to astrophysics, found that his scholarship was not sufficient for him to stay in Cambridge, the glamorous centre of astrophysics where Saha had wished to stay and work.[39] However, he had fruitful interactions with many scientists in several places, and after returning to India, he continued his research for some time. But he had eaten the fruit of knowledge, now that he had seen conditions in Western universities, and knew how uneven the competition was for him. With his youthful enthusiasm gradually fading out, the story of his later life is the story of a rugged personality being engulfed by increasingly deeper cynicism and bitterness, and finally giving up science altogether for politics.

S. N. Bose, in contrast, always seemed to be more interested in the structure of theoretical physics, rather than in current research problems.[40] Even while Bose was a green undergraduate student, he had a fabulous reputation of extraordinary intellectual powers. Initially he collaborated with his classmate Saha in one or two small projects, and the two of them together prepared the first English translation of Einstein's papers on relativity. But, after that, in the sixteen years between the ages of 26 and 42, Bose published just two very short papers — the two papers which created quantum statistical mechanics! The behind-the-scene history of this work is really striking.[41] Apparently, Bose's attention was drawn to this problem by Saha, who had already attained international recognition and had visited Europe. Since this problem was of a fundamental nature, baffling some of the world's greatest physicists (including Einstein), it caught Bose's fancy, and within a few days he had a brilliant idea for looking at the problem in a new way. Once the idea was there, the subsequent calculations were fairly straightforward, and Bose, whose formidable skill for mathematical manipulations was legendary, must have worked out the problem like a smart schoolboy solving his homework. Then Bose sent his small paper (written in English) of six typed pages to a complete stranger, Albert Einstein, asking his opinion whether the paper was worth publishing in a German journal which was among the world's top physics journals at that time. Einstein was so struck by this strange paper from an unknown scientist that he himself translated it into German and had it published in that journal. Afterwards Einstein wrote papers developing Bose's method (now known as 'Bose-Einstein Statistics') in a more general way, and the papers by Fermi and Dirac on the complementary aspects of the problem followed in quick succession. But Bose himself lapsed into a long silence. He took an opportunity to visit Europe for two years, but, unlike Saha, he seemed to have failed to interact with Western scientists in any fruitful way. After his long silence, he finally produced a stream of completely amateurish papers on such diverse topics as thermoluminescence, ionospheric electricity, reaction of sulphonazides with pyridine and extraction of germanium from Nepal! What are we to make out of all these? Was the man a genius or madman or a crook or just a lazy bum? All this time Bose was being worshipped by his devotees as a semigod, and was being teased by his enemies for not getting the Nobel prize. Bose's last work was a series of five highly mathematical papers on unified field theory in the mid-fifties. Competent judges considered them to display amazing technical virtuosity, but this work, probably Bose's closest approach to normal scientific research, was a wasted effort, since the unified field theory failed as a viable research programme. The entire output of Bose's long life consisted of about twenty papers on widely different topics and of widely varying qualities — providing a classic example of the schoolboy approach to science. Bose always refused to work for a PhD degree in his youth, and, in later life, he was particularly fond of telling people that he did not have a formal doctoral degree.

These short sketches of the careers of Saha and Bose illustrate many of my points. The vigorously energetic, self-trained Saha carried on successful scientific investigations for a few years in a completely singlehanded way, but as the era of Little Science gradually changed into the era of Big Science, he realized with increasing frustration that the competition was becoming more and more uneven for him; whereas Bose, though himself a genius, still typified in many ways the mental confusion of an average Indian scientist in that age. Now I would like to point out a most remarkable fact. Just as Bose and Saha, the two most eminent Bengali physicists, were classmates in Presidency College, the two most eminent Japanese theoretical physicists of that time, Yukawa and Tomonaga, were also classmates in Kyoto University.[42] *Was this just double coincidence?* A very provocative hypothesis readily springs to mind. In a remote place outside the scientific mainstream, if there is one single person of outstanding intrinsic calibre, maybe he never realizes his own potential: but if there happen to be two such persons in the same place, then they can have mutual intellectual interactions, and hence increase the chance that their talents can flower. More data are necessary: I merely offer the conjecture for further investigation.

So far in this section, I have concentrated more on problems in the age of Little Science, because they are conceptually more subtle and intellectually more fascinating. In the age of Big Science, the main problems arise out of the overriding importance of money and communication — problems of such gross and obvious nature that whatever I say will probably be something the reader has already thought of. Nowadays it has become rather easy for bright non-Western students to come to the United States for graduate studies, get a right psychological gestalt, and do good research. When these students go back to their countries, they quite often find that they cannot maintain the standards of their research due to the lack of the two necessary preconditions, money and communication; they then tend to lose their mental disposition for research, as a result of various social factors. When these people become the teachers for the next generation of students, they are not only unable to communicate a proper psychological gestalt, but, what is worse, they inadvertently encourage their students to develop a schoolboy conception of science. These students may then come to the West and achieve a belated gestalt switch: but, in their turn, when they go back and become teachers for the next generation, the same process repeats itself. The unbroken circle goes on. It is no wonder that many ambitious Asian scientists still feel that they have to settle in the West if they want to be successful in science. It is worthwhile to remember that scientists are human beings in the first place, and not just robots for producing research. When an Asian scientist decides to settle in the West, he usually faces a host of complex human problems arising out of his attachments to his home country and out of the possibilities of maladjustment and social alienation in a new home.[43] I know of scientifically successful Asian scientists, otherwise willing to settle in the United States, but hesitating because they think that they may not be able to find suitable spouses or sexual partners in this country. Alas, a scientist cannot deny life — that sublime mystery which science is yet to comprehend!

Conclusion

My aim has been to show that societies which are at advanced stages of scientific development but have not yet become truly creative centres of science, are prone to give rise to partial science communities. Since such communities possess normative structures and, consequently, are very much resistant to changes, such a state may persist for a long period; this is what makes it so hard to take the final step in building a new scientific tradition. Even after the community has successfully implanted literary and artistic traditions from other communities, and even after it has had a full exposure to modern science, the role of the research scientist may fail to emerge. I have argued here that the emergence of such a role is contingent upon the existence of social conditions that enable scientists to sustain a proper psychological gestalt, after acquiring it through specific kinds of learning processes.

This paper has discussed a complex human predicament without much reference to the problem of its evolution in time. It is beyond the scope of this paper to analyze those deep forces, working in such communities, which may eventually disrupt the structure of partial science, leading the community to realize its ultimate goal of total science. As the issues of money and communication have been discussed quite often, I have not tried to treat them here; but this does not imply that I wish to deny their importance in any way. My point is that we need to consider some additional parameters in analyzing the problem of the implantation of science in non-Western societies. With the change of background socio-economic conditions, a society may become more and more suitable to nurture a scientific tradition. In addition, if a large number of scientists trained in the scientific metropolises go back to the partial science community and together try to instil the proper spirit of research, the resistance arising from the normative pattern of partial science may finally give way. I know from personal experience that there has recently been a general feeling of optimism among many intelligent and well-informed Indian scientists working in India. A recent survey of the Indian scientific community led to the conclusion that there 'are few technical communities elsewhere that enjoy the sense now common in India that circumstances have recently improved and are still improving'.[44] I would not venture to guess whether India is really on the threshold of a scientific golden age, or whether we are just being fooled by the rays of a false dawn. This is an issue on which it is really difficult for me to think objectively and dispassionately; and, after all, the sociology of science has not yet reached that stage of sophistication which would allow it to provide definitive answers to such profoundly challenging questions.

• *NOTES*

It is a great pleasure to thank Robert Richards, who had the patience to go through several versions of this article, and suggested improvements. In fact, while attending his course on 'Historiography of Science' at the University of Chicago (Fall 1983), I had the first idea of writing the article. Several conversations with Joseph Ben-David were crucial in shaping my conception of the sociology of science, and in making me able to see my work in a wider perspective. My friend Timothy Woodward was responsible for kindling philosophical and sociological interests in a dry scientist like me. Suggestions from David Edge, Robert Anderson and three anonymous referees were very helpful in revising the initial draft. I am indebted to several other persons, who read earlier versions of this article and discussed various aspects of the problem with me. Especially I would like to thank Subrahmanyan Chandrasekhar, Sumit Das, Nicholas Mascarenhas, Palas Pal, Patrick Palmer, Eugene Parker, Amal Raychaudhuri, Shyamal Sengupta, Edward Shils, A. P. Shukla and Noel Swerdlow. Last but not the least, I heartily thank Charles Gillispie and Robert Merton for encouraging me with long letters of critical comments on my manuscripts.

1. It is true that in many technological areas, Japan is now competing with the most advanced of the Western nations. But on the basis of my conversations with several Japanese physicists, I have the impression that Japan has not yet completely solved the problem of establishing a tradition of fundamental research in basic sciences, and hence I believe that some of the issues discussed in this paper still hold for fundamental sciences in Japan. This view is also supported by James Bartholomew, 'Japanese Culture and the Problems of Modern Science', in Arnold Thackray and Everett Mendelsohn (eds), *Science and Values* (New York: Humanities Press, 1974), 109–55.

2. Amongst these friends, I would specially like to mention the names of Sumit Das, Subir Sarkar and Amlan Raychaudhuri.

3. The classic work on this subject is Joseph Ben-David, *The Scientist's Role in Society* (Chicago: The University of Chicago Press, 2nd edn, 1984).

4. In his well-known investigations, Derek de Solla Price argues that science approximately doubles itself in little more than a decade: see Price, *Science since Babylon* (New Haven, Conn.: Yale University Press, enlarged edn, 1975), Chapter 8,

and *Little Science, Big Science* (New York: Columbia University Press, 1963).

5. See, for example, David Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976).

6. To readers who are interested in getting an idea of the quality of Bengali literature during the renaissance, I would recommend the incomparable short stories of Rabindranath Tagore as a first start (many of them are available in English translations).

7. M. N. Saha (1893–1956), in 'Ionisation in the Solar Chromosphere', *Philosophical Magazine*, Vol. 40 (1920), 472–88 and a series of subsequent papers, developed the theory of thermal ionization, which is of fundamental importance in understanding the spectra of stars. The 'Saha Equation', named after him, is a landmark in modern astrophysics.

8. S. N. Bose (1893–1974), 'Plancks Gesetz und Lichtquantenhypothese', *Zeitschrift für Physik*, Vol. 26 (1924), 178–81; 'Wärmegleichgewicht in Strahlungsfeld bei Anwesenheit von Materie', ibid., Vol. 27 (1924), 384–93. These were the first papers on Bose-Einstein Statistics, and created a new branch of physics — quantum statistical mechanics.

9. Let me give two illustrations. Out of the twelve physics students in my Indian Institute of Technology class, six sat for the GRE Advanced test: five of them scored above 90%, with myself and two others scoring 99% — the highest possible score in the test. And here at the University of Chicago, the physics department gives a prize for the most outstanding performance in the compulsory candidacy examination for graduate students: during the last ten years, this Telegdi Prize has been won five times by Indian students (I happen to be one of the five).

10. Michael Polanyi, *The Logic of Liberty* (London: Routledge and Kegan Paul, 1951), 56.

11. The literature on this subject is scattered in various languages and in various publication formats: books, journal articles, reports of government committees, manifestos of private organizations, and so on. The major bulk of this literature is, however, nothing more than tedious repetitions of standard clichés in very loose, monotonous prose. It is not an easy task to sort out islands of worthwhile research in this dull sea of undistinguished writing. One of the easily accessible sources of competent writing in this subject is the journal *Minerva*, which has continued to publish papers on implantation of science in Asian, African and Latin American countries. If one is interested in more extensive explorations of the Indian scene, then two outstanding pieces of research can be cited: Robert S. Anderson, *Building Scientific Institutions in India: Saha and Bhabha* (Montreal: Centre for Developing-Area Studies, 1975) and Ashis Nandy, *Alternative Sciences: Creativity and Authenticity in two Indian Scientists* (New Delhi: Allied Publishers, 1980). Anderson's excellent book deals almost exclusively with the problem of organizing scientific institutes, and not with the more philosophical and psychological problems associated with scientific creativity which are my main concern here. Nandy's monograph is a brilliant portrait of two unusual Indian scientists, who had rather pathological scientific careers, and, thus, does not throw much light on the process of creativity in a normal Indian scientist who aspires for professional reputation in the customary way. A. Rahman is another scholar who has written on various aspects of the Indian situation over the years. Some of his important articles have been collected in the volume, A. Rahman, *Trimurti: Science, Technology and Society* (New Delhi: People's Publishing House, 1972). However, so far I have failed to locate any work analyzing the socio-psychological

basis of scientific creativity which is the central idea of this paper. This is rather surprising, since I know from personal experience that similar ideas come up very often in private conversations amongst students from underdeveloped countries studying in the United States. My only claim to originality is that I have tried to arrange these ideas in a (hopefully) coherent and systematic pattern.

12. After the first draft of the paper was written, Professor Chandrasekhar pointed out to me that his first important work on the mass limit of white dwarf stars was done on his way to England, before any actual exposure to the West. Afterwards, however, following a brief stay in Cambridge, he has been on the faculty of the University of Chicago for almost half a century.

13. Sir Arthur Eddington, *The Nature of the Physical World* (Cambridge: Cambridge University Press, 1929), Introduction.

14. E. T. Bell, *Men of Mathematics* (New York: Simon and Schuster, 1937), 9.

15. Ben-David, op. cit. note 3, 16–20, 24–31. Ben-David has argued that a society can come to have a tradition of scientific research only when the 'role' of the scientist emerges in that society.

16. Perhaps it is worth emphasizing here that the discussion in this paper is completely limited to the basic sciences, which usually do not lead to immediate practical applications. Very often, when one talks of building scientific traditions in underdeveloped countries, much confusion arises from a failure to distinguish basic science from applied science and technology. The problems of building traditions of applied science and technology are very different in character. For example, an agricultural scientist in India may not beat his counterpart in the United States, but if he is just able to make an improvement over the existing conditions of agriculture in India, then he has a feeling of accomplishment. The demand for an applied science in the local community can make the motivation for research qualitatively different from that of basic science.

17. Max Weber, trans. Talcott Parsons, *The Protestant Ethic and the Spirit of Capitalism* (New York: Charles Scribner's Sons, 1958), 71.

18. One great scientist of our time has given a fascinating account of the emotional involvements of scientists with their work: James D. Watson, *The Double Helix* (New York: Atheneum, 1968).

19. Sir Karl Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959).

20. P. A. M. Dirac, *The Development of Quantum Theory* (New York: Gordon and Breach, 1971), 24.

21. J. D. Jackson, *Classical Electrodynamics* (New York: John Wiley and Sons, 2nd edn, 1975), is a standard graduate textbook containing a large number of not-too-easy problems.

22. In *The Born-Einstein Letters* (New York: Walker and Co., 1971), 166, Max Born writes to Einstein, 'Heisenberg did not even know what a matrix was in those days (he was my assistant, that is how I know).

23. It should be noted that the situation of a good Western student going to a somewhat mediocre school is completely different from the situation of my hypothetical student *N*. For such a Western student the gestalt transformation may get delayed, but he is not in a community with brilliant members practising partial science only. Thus he does not have the danger of developing misconceptions in the way *N* does.

24. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The

University of Chicago Press, 2nd edn, 1970), Chapters II, III, IV.

25. I want to emphasize here that *N*'s misconception of science is much deeper than just a lack of knowledge of the appropriate techniques of research. In fact, even some of the most distinguished Western scientists have mentioned that they did not have the proper notion of research until they actually started research. This is what we learn from the personal account of G. H. Hardy, *A Mathematician's Apology* (Cambridge: Cambridge University Press, 1967), 144–47. Heisenberg also mentions that he was initially interested only in the philosophical ideas underlying physics, and did not care for details: see Werner Heisenberg, trans. A. J. Pomerans, *Physics and Beyond* (New York: Harper & Row, 1971), 17. However, to Western students, trained in active centres of science, there is nothing mysterious about science. But in the case of the non-Western student in a partial science community, creative science is something very remote, and famous scientists are mythical figures. This is what lies at the root of the misconception about science. Ignorance of proper research techniques is just part of the story.

26. The *locus classicus* of the functionalist approach in sociology of science is a tersely-written paper by Robert K. Merton, 'Science and Technology in a Democratic Order', *Journal of Legal and Political Sociology*, Vol. 1 (1942), 115–26, reprinted as Chapter 13 of Merton, *The Sociology of Science* (Chicago: The University of Chicago Press, 1973). Several other papers in this volume are also classic statements of the functionalist approach.

27. Michael Mulkay, 'Some Aspects of Cultural Growth in the Natural Sciences', *Social Research*, Vol. 36 (1969), 22–53; S. B. Barnes and R. G. A. Dolby, 'The Scientific Ethos: A Deviant Viewpoint', *European Journal of Sociology*, Vol. 11 (1970), 3–25.

28. Weber, op. cit. note 17, 66–67.

29. Nothing can be further from my intention than to suggest that all educational institutions in the underdeveloped countries are exemplary sites of idyllic life-style. But I do know from personal experience that there exist outstanding centres of partial science to which my description is applicable.

30. Arnab Rai Choudhuri, Subrata Sen and Subinay Dasgupta, 'A History of the Physics Department of Presidency College' (written in Bengali), in Choudhuri (ed.), *Re-union Commemoration Volume, Physics Department, Presidency College* (Calcutta: Presidency College, 1977), 9–28. In those days it was not usual for a college student in Calcutta to own a personal tape recorder, unless he was from a rich family. I still regret that none of the authors had a tape recorder at that time, and we could not record some of the memorable interviews. I especially remember a very touching interview with a classmate of Bose and Saha, a grandfatherly person then in his eighties, who contributed a charming little reminiscence to our volume.

31. The very useful concepts of 'metropolis' and 'province' in the intellectual community have been developed largely by Edward Shils, *Intellectuals and the Powers and Other Essays* (Chicago: The University of Chicago Press, 1972), 355–71. These concepts have been used throughout Shils's exceptionally insightful monograph, *The Intellectual between Tradition and Modernity: The Indian Situation* (The Hague: Mouton & Co, 1961).

32. Cf. V. Shiva and J. Bandyopadhyay, 'The Large and Fragile Community of Scientists in India', *Minerva*, Vol. 18 (Winter 1980), 575–94.

33. An interesting attempt to relate this mentality to the historical tradition of India has been made by Nandy, op. cit. note 11, 4–8.

34. Cf. Shils, *The Intellectual between Tradition and Modernity*, op. cit. note 31, 67–78.

35. To give an example, in the presidential address to the Indian Science Congress in 1967, the distinguished Indian chemist, T. R. Seshadri, expressed the view that the scope of science in India should be sufficiently broadened to include the 'higher knowledge of Vedanta'. An interesting discussion of this curious case is given by Rahman, op. cit. note 11, 156–67.

36. Perhaps it does not require too much insight to predict that my present paper, most probably, will be a target of such attacks.

37. Here we are talking of S. N. Bose, not to be confused with the earlier Indian physicist-botanist Sir J. C. Bose (1858–1937), one of the most puzzling and colourful figures of the Bengal renaissance. The latter had tremendous prestige as a scientist in contemporary India, somewhat out of proportion to his scientific merit, and though his name is virtually unknown to scientists outside India, he still continues to be idolized by many Indian popularizers of science. He had a compelling ambition to build a personal empire of his own, and had an uncanny insight into how to project a larger-than-life image of himself, both to the lay public and to intellectuals in fields other than science. He provides a fascinating case history for sociologists of science, showing how such figures can dominate the public stage in a country which does not have a tradition of total science. He is one of the two Indian scientists studied by Nandy, op. cit. note 11.

38. At that time India was under British rule. Saha had close friendships with members of a terrorist group and took part in political demonstrations. Cf. Anderson, op. cit. note 11, 7.

39. Kamalesh Ray, *The Life and Work of Meghnad Saha* (New Delhi: National Council of Educational Research & Training, 1968), 19–22. However, this book seems to have given a wrong date for Saha's trip to Europe. Saha sailed from India towards the end of 1920 (after finishing the first three papers on thermal ionization between March and May of that year) and not in 1919.

40. D. M. Bose, 'Foreword', in Santimay Chatterjee (ed.), *Collected Scientific Papers of Meghnad Saha* (New Delhi: Council of Scientific & Industrial Research, 1969), v–vi, gives a description of the interactions of a slightly senior physicist, with Bose and Saha, on his return from Germany after World War I:

> I joined the Physics Department of the University College of Science in 1919 after my enforced five years' stay in Berlin. Two of my young colleagues in the Physics Department, Meghnad Saha and S. N. Bose, had been starved of information about the advancement in Physics which took place in Germany . . . I remember the characteristics of my two colleagues which illustrated their different approach towards scientific research. I gave S. N. Bose two books written by Planck, *Thermodynamik* and *Warmestrahlung*, to read, as they were not available then in this country. S. N. Bose appreciated very much the logical way in which Planck deduced the whole of Thermodynamics from a limited number of postulates. On the other hand, Planck's deduction of his famous formula on spectral distribution of energy in black body radiation was based mainly on Classical Electromagnetism and Thermodynamics to which a single quantum postulate had been added. S. N. Bose noted the inner inconsistency in Planck's exposition and missed in it the clear logical formulation which characterized Planck's *Thermodynamik* . . . Meghnad Saha's approach was more direct; he wanted to learn from me the latest advances

on the frontiers of research in Physics in relation to Quantum Physics and Thermodynamics. From time to time he discussed with me the theory of thermal ionization of gases and its application to the interpretation of stellar spectra . . .'

41. Santimay Chatterjee and Enakshi Chatterjee, *Satyendra Nath Bose* (New Delhi: National Book Trust, 1976), 38–43.

42. Mentioned in Yukawa's recently translated autobiography: Hideki Yukawa, trans. L. Brown and R. Yoshida, *Tabibito (The Traveler)* (Singapore: World Scientific, 1982), 149–50.

43. Academics working in foreign countries constitute a very interesting social group. Some of the human problems, specifically faced by Indians, have been analyzed by Amar Kumar Singh, *Indian Students in Britain* (Bombay: Asia Publishing House, 1963).

44. John Maddox and Vera Rich, 'Science in India: Excellence in the Midst of Poverty', *Nature*, Vol. 308 (12 April 1984), 581–600, quote at 581.

Arnab Rai Choudhuri is currently a graduate in theoretical physics at the University of Chicago. He is nearly at the end of his graduate work and will be joining the National Center for Atmospheric Research at Boulder as a visiting scientist in September 1985. He has written papers on magnetohydrodynamics and its applications to astrophysics. *Author's present address:* Laboratory for Astrophysics and Space Research, The University of Chicago, 933 East 56th Street, Chicago, Illinois 60637, USA. *Author's address after September:* High Altitude Observatory, National Center for Atmospheric Research, P.O. Box 3000, Boulder, Colorado 80307, USA.