The Rutherford Memorial Lecture, 1968

Some problems of the growth and spread of science into developing countries†

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For a physicist brought up in New Zealand, it is a special honour, and especially humbling, to be asked to speak in memory of Ernest Rutherford. Whatever we may think of the subsequent applications of nuclear physics, we cannot fail to class him among the immortals for his brilliant voyages of discovery in that vast and unsuspected realm of Nature. How could I present my own little scholarly pastime—that agreeable game called the theory of the solid state—under the shadow of his achievement. He dominated the scientific life of his day; and there are too many distinguished scholars who remember him personally for me to dare to say anything about him at second hand.

The story of the poor farmer’s son, from the backblocks of distant little New Zealand, who won a scholarship to Cambridge and became world famous, is still one of the inspirational epics of the life of the mind. Many another young man, from many another far-off land, has taken it as his model, and gone to seek his fortune thus, in the great intellectual metropolises of Europe and North America. The urge to contend and make one’s mark in the great game of ‘high science’ is as strong as ever among the ideals and ambitions of gifted youth.

Yet times have changed; from the point of view of the patriotic New Zealander, Rutherford’s permanent migration to England might now be seen as a serious case of ‘brain drainage’. In speaking in his memory nearly a century after his birth, it seems appropriate to discuss the converse phenomenon—the growth and spread of basic science, from its original nuclei in the industrial countries of western Europe, into all corners of the Earth. We hold it almost as self-evident that currents of knowledge, skill, attitudes and techniques should diffuse the culture of scientific research throughout the world, so that eventually the conditions for the pursuit of mysterious Truth may be provided anywhere, from Timbuctoo to Tahiti, from Kamchatka to Kathmandu.

Now, of course, for a developing country, struggling desperately to provide a better life for millions of ordinary men, women and children, the problem of giving

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an appropriate priority to this essentially exotic activity is not trivial. I am not speaking now of the techniques that pure science has uncovered and created—improved methods of agriculture, the control of birth and disease, rapid communication and transport, and so on—but of research without any more conscious aim than the understanding of how things are, or were, or might be. When money and men are scarce, it is by no means self-evident that an institute of theoretical physics, or molecular biology, or archaeology, should take precedence over a tractor factory, a hospital, or a school of civil engineering. Even in Britain, which is already as rich in material comforts as any sane man would wish, we argue about these priorities; in a country such as Indonesia, or Brazil, or the Congo, a very strong case indeed must be made for the diversion of any financial and human resources at all into such unproductive channels (cf. Blackett 1967).

As a first approximation this argument is compelling. Those of us, in advanced countries, who have the good fortune to be allowed to make our living solving the splendid puzzles of Natural Philosophy may be well aware that what we are doing is not likely to be very useful; but we can comfort ourselves with the thought that we live in a society where most of the basic human needs are adequately satisfied, and that we are no more parasitic than such admirable citizens as estate agents, jockeys, bar tenders or advertising executives. But this irresponsible attitude is not permissible in less fortunate societies; in a country such as India or Pakistan, I can see no justification for, shall we say, the neglect of practical animal husbandry for the sake of research in pure physiology, or for the study of the theory of superconductivity in the absence of good schools of electrical engineering. I want to say this at the beginning in case I seem to be yet another pure scientist who has his nose so deep in his silly little field of research that he cannot look out at the tragic real world around him. Indeed, I suspect that the solution of technological problems, which come already loaded with human cares and consequences, is more satisfying than finding the answers to those abstract impersonal puzzles which we dream up for ourselves in our ivory towers; and I simply cannot understand the intellectual snobbery of those silly people who give more credits for the discovery of another meson than for the design of a suspension bridge, and who cannot see that a zip fastener is a far more beautiful idea than a zeta function. Who knows: Rutherford might easily have earned his immortality for developing radio and radar rather than for splitting the atom.

But the utilitarian argument against basic research can be carried too far. In an interesting series of controversial articles Professor Harry Johnson (1965, 1966, 1967) has attempted to show quantitatively that pure science is essentially an extravagance whose applicable results may be bought from abroad far more cheaply than they can be produced at home. This follows from the rationale of free trade and the international division of labour. The developing country benefits more by the export of a few clever people, and by the subsequent import of technical know-how, than it would by attempting to expand its own very inefficient and unproductive local research industry.
But as others have countered (Kidd 1968; Toulmin 1966) mere economic accountancy is not compelling in such a complex of subtle circumstances. A certain amount of fundamental research must be sponsored in a developing country, for a number of excellent reasons which have been clearly stated by Moravcsik (1964).

In the first place the education of technical experts—engineers, doctors, agricultural advisers, even government administrators—cannot be left entirely in the hands of technologists of their own practical kind. Modern engineering, for example, requires the exercise of skills, and the application of knowledge, acquired from pure physics, chemistry and mathematics. The rapid development of new techniques can only be exploited if the practitioners are adequately trained in these fundamental disciplines, by teachers who are themselves in close contact with the latest theoretical principles. If we have learnt anything in the past century about scientific education, it is that only those who are actively engaged in research can truly absorb and retransmit these new principles as they arise. The research work of university teachers is not an extravagant irrelevance to their professional task; it is absolutely essential to their efficiency in passing on useful, applicable knowledge to their pupils.

From a purely practical point of view also, it is impossible to import technical know-how, and to apply it successfully, if one does not have available, locally, a corps of learned men to whom one can appeal for guidance on matters of pure scientific principle. Suppose, as an imaginary example, that we have set up a factory for the production of simple radio sets. The beautiful machinery for making the transistors, imported from America or Japan, does not work properly because, perhaps, there is too much copper in the water supply. The engineers study the problem and eventually unearth a paper in the *British Journal of Applied Physics* referring to 'the diffusion of transition metal ions along dislocations in covalent semiconductors'. Their training is adequate to suggest the relevance of this paper to their practical problem—but only an 'academic' physicist, with a profound knowledge of the science of materials, could really tell them just what it meant, how much it could be relied on and whether it contained the answer to their questions. All our experience tells us that such queries cannot be dealt with by correspondence at a distance. As Crawford (1966) has pointed out, local experts can see more clearly the needs and wants of the country. Science-based technology is not a lusty crop of rules of thumb, perfected by evolution over long periods; it is a delicate plant, which thrives only when tended by mixed teams of experts, including those impractical specialists to whom the buck can be passed when fundamental principles are at stake.

Quite apart from utilitarian arguments, there are spiritual considerations. The pursuit of scientific knowledge for its own sake has become one of the major artistic enterprises of humanity. Even though we would deplore the supersession of all other forms of art, sport or religion by such a specialized activity, we must surely recognize that the opportunity to undertake scientific research is one of the
important attributes of the Good Society. I cannot imagine any goal towards which a human society might claim that it was ‘developing’ that did not nowadays give some room for at least a few of its members to participate in this great, world-wide movement. The attempt to understand the genetic code may promise no immediate practical dividends, and may perhaps give direct pleasure to only a small proportion of the population. Nevertheless, like a ballet, a poem, a park or a stained glass window, it embellishes the society that has commissioned it, both for its own sake as a thing of beauty and as a symbol of the time to come when bread will not be our immediate lack. Failure to give at least token support to pure science would be as philistine as banning the public performance of orchestral music, or ploughing up Regent’s Park to grow potatoes.

There is, therefore, a very good case indeed for modest encouragement and financial support for basic research in any country on the threshold of industrial development. On the other hand, these arguments should not be carried too far. They do not, for example, justify leaving the choice of basic sciences entirely to the accident of the availability of trained men, and the directions into which their enthusiasms happen to lead them. There are occasions when deliberate decisions of policy can be taken at a relatively general level. Shall we foster ecology or physiology? Should we strengthen plasma physics or materials science? Do we need an institute of molecular biology, or a department of radio astronomy? Surely one should then take account of the relevance of the subject to the economic, geographical, meteorological or social background of one’s country. Even within pure science there are branches that are more natural, so to speak, to one region of the world than another, and which are therefore more likely to thrive in the appropriate context. In Alaska, for example, a physicist might find glaciology congenial—while the study of thunderstorms could be left to the fortunate inhabitants of the tropics: in Western Australia, with its vast new mining industry, he ought to become interested in mineralogy—while the Japanese steel industry might encourage him to study the mechanical properties of metals—and so on. This chameleon behaviour is not a matter for formal legislation, but quite apart from its technical consequences it is the sort of shrewd and practical policy that can counteract the brain drain. It gives the local scientist a better reason for staying where he is than mere patriotism. If, like one of the first real scientists I ever knew, you specialize in the physiology of the mammary gland, and if cow’s milk is the life blood of New Zealand, then where better to live and work than beside the Waikato River! On the other hand, what could be more demoralizing than attempting to construct theories of superconductivity in a laboratory without low temperature facilities, or more frustrating than trying to look through a large astronomical telescope in the climate of the British Isles!

It would seem sensible, also, to put the main emphasis on what I should describe as ‘potentially applicable’ science. Of course we do not know in advance what may come of any particular scientific discovery. It is a trite argument, often used to justify the most esoteric activities, to point out that nobody could have known the
practical uses of radioactivity, or electromagnetism, when they were first discovered... and therefore, gentlemen, you must of course give me another million dollars in case I discover something equally useful up there in the stratosphere' etc., etc. I am afraid I do not see the necessity. Let us not stand in the way of utterly pure mathematics, astronomy, palaeontology, and other apparently useless disciplines; but do not let's fall into the trap of equating uselessness with snob value. There are numerous excellent, difficult, and rewarding branches of pure science which at least attempt some understanding of humanity and the everyday human environment and which therefore promise, some day, to be of real use. Although I have pretended to laugh at my own little game of solid state physics, I could easily trace the connexions between the basic problems we study and such practical activities as the design of radio sets or of bridges.

Indeed, the distinction that is here being made between 'applied' and 'basic', 'fundamental' or 'pure' science is not at all precise, and the attempt to make it sharp and clear is bound to fail (Reagan 1967). I am using a rough and ready conventional classification, which I should not want to defend in detail. The sooner we all face up to the fact that theory and practice are indissoluble, and that there is no contradiction between the qualities of usefulness and beauty, the better. If there is one feature of American science that we should all imitate—I speak for Britain, Europe, and most of the rest of the world—it is their willingness to have both fundamental and applied research going on together under the same roof. Their universities do not despise technological development work, while their industrial laboratories are well equipped with basic research groups. In a developing country, where short-term problems have much the higher priority, the conventional practice of keeping such work out of the universities is quite indefensible; as I have already said, the main function of long-term research without specific goals is to provide the best intellectual and educational background for well-informed technical innovation.

From these relatively sober considerations, it follows that any developing country, once it has emerged from the most primitive poverty, should foster a small amount of basic science. Of course this can be overdone; a research reactor can become as extravagant a 'status symbol' for a small country as its own international airline. But I do not wish to dwell upon the difficult problem of relative priorities and the criteria for choice between technology, applied science and fundamental research. For the remainder of this lecture I shall try to express some opinions concerning the establishment and maintenance of basic scientific work under these difficult circumstances. Although the obstacles are all too evident for those actually involved, very little attention seems to have been given to this problem in all the mass of writing and talking on industrial and technical development. In particular, I want to emphasize the intellectual and psychological aspects, which are neglected in the struggle for material resources.

At the outset, one must have a clear idea of the nature of science itself. It is all too easy to adopt an uncritical, imitative attitude. 'Science is what they do at the
Cavendish Laboratory, so we must try to build a laboratory that is just the same. But any attempt to make a carbon copy of a social institution, under another sky, is bound to end in disappointment. If we are to foster new institutions that really work—productive and creative Indian science, or Pakistani science or Nigerian science, or Patagonian science—then we must have a proper understanding of the long-term goals and short-term functioning of the parent institution on which they are moulded. To give a ridiculous example: putting on a white coat and peering knowingly down a microscope no more makes a biologist than chanting metrical psalms makes a presbyterian. It is the inner spirit that counts, not the ritual.

For this purpose, it is essential to begin the analysis at the most general level; indeed, one must first try to define science itself. As I have argued at length, in a recently published book (Ziman 1968) the most useful definition comprehends the social dimension of science. The role of the pure scientist, however eccentric, imaginative, speculative or opinionated, is to make a contribution to ‘public knowledge’, to help construct a rational consensus. Many familiar dichotomies—experiment and theory, creation and criticism, accidental observation and deliberate search, logic and intuition, master and pupil, freedom and authority—can live together without contradiction in this definition. And, as I hope to show in this lecture, one can deduce from it some quite powerful guiding principles for the practical management of scholarly affairs.

This point of view implies, conversely, that there is no very special ‘philosophy of science’ that must already be latent in the culture of a developing country before a scientific community can be established. It is sometimes argued (Odhiambo 1967; Basalla 1967) that the philosophical, religious or cultural climate in certain regions of the world is so hostile to the scientific attitude that research could never thrive there without a complete psychological reorientation. ‘In Jub-Jub Land’, this argument runs, ‘the study of botany would be quite impossible, for everybody is brought up to believe that oak trees spend the hours of darkness flying to the Moon’, or ‘How could you teach Euclid to a Zen Buddist, whose every utterance is deliberately irrational?’

We are not entitled to reject such assertions just because they are uttered with a hint of cultural superiority. Yet I do not think we need take them seriously into account, for several reasons. As Levi-Strauss has demonstrated, the supposed credulity and irrationality of primitive peoples is often over-emphasized in anthropological writings. Primitive systems of thought are not illogical. They respect such general principles as ‘seeing is believing’; they know the difference between cause and effect; and they make regular categorizations of natural objects. From our point of view, the weakness of primitive thinking is the incorporation of incongruous and (to us) irrelevant elements in the chain of deduction—totem animals, ancestral spirits, ghosts and such like—which can, of course, lead eventually to the most bizarre consequences. But then many Western scientists seem not to be hampered by belief in the Virgin Birth, the infallibility of the Pope, the prophetic foresight of Karl Marx, the unwholesomeness of the flesh of the pig, or the
superiority of the American Way of Life. I do not see the harm in a little totemism, white magic, or contemplation of Nirvana—provided it is out of office hours. The initial premises and modes of thought of science are essentially those of matter-of-fact, everyday life, and not those of the sort of metaphysician who feels that he has to prove that the tree is still in the Quad when nobody is about to see it. The primitive savage—and the plain man in the streets of London, Paris, Tokyo or Delhi—has many non-scientific beliefs, which another lifetime of education could scarcely correct; but he is, none the less, a naïve realist at heart. Only a very small proportion of the population, even of the most civilized societies, is susceptible to a genuinely anti-scientific, genuinely irrational, metaphysic. It takes a good dose of neurosis, or an altogether too literary education, to arrive at such a decadent intellectual impasse.

We must be careful here not to confuse the conditions required for the growth and spread of science from those required for it to appear spontaneously. However much we may admire the technical achievements of various other civilizations, we must concede that the science about which we are here talking came to birth in Western Europe in the seventeenth century, and has not had the occasion to appear independently at another time and place. One of the grand problems of cultural history is just why it did appear there; what were the origins and causes of this unique event. Of course, the whole thing is very well documented; but it still remains almost as baffling as the corresponding biological problem of the origin of life. This, indeed, is a good analogy, for the conditions envisaged for the original spontaneous generation of the very first living organisms are entirely different from those which could easily be colonized in the later stages of biological evolution. The sophisticated science of today could certainly not now be expected to appear all of a sudden out of the forests of the Amazon or the aboriginal wastes of Australia; but we know that it can be taught quite well, and can thus reproduce itself, in almost any civilized cultural environment.

But although the basic metaphysical foundations of science are relatively naïve, we all know what an immense superstructure of actual knowledge they carry. The unity of science—the fact that it is the product of innumerable hands, cooperating over the centuries—and the unique power of the dialectical processes of imaginative creation and critical reappraisal, make it by far the most elaborate intellectual construction that the world has ever seen. If one is to contribute to this vast system, one must become the master of what is already known, at least in some limited aspect. Occasionally an ignorant scientist may stumble accidentally upon an important new discovery but such serendipity must not become an excuse for lack of education as a policy. Self-taught geniuses, like the incomparable Srinivasa Ramanujan, are entirely exceptional and prove nothing against the necessity for the deliberate teaching of scientific knowledge.

This is, of course, fully recognized, and the first condition for the growth of science in a developing country is the establishment of centres of higher education. It is here that the child brought up in an advanced society has his major advantage.
Up to the age of ten or twelve he is still no better than a primitive savage, with only a material, instrumental, familiarity with the products of modern technology to offset his natural beliefs in magic and mystery. But the ordinary curriculum of secondary education introduces him rapidly and efficiently to the general principles of scientific thought, and his talents are guided by experienced teachers into the higher realms of knowledge. If he works hard, then he may have learnt enough by his early twenties to begin to do research on his own account.

For a child born into a developing country this process is far more erratic and uncertain. His whole environment may be entirely traditional, materially and intellectually, and it may only be a stroke of fortune that carries him beyond mere literacy. He will be lucky if he encounters good secondary teachers to give him the necessary grounding in scientific subjects, and it is more than likely that most of his university education will be at the feet of scholars who have not, themselves, fully mastered their subjects, and who can convey only the outward inessentials to their pupils. As I have already emphasized, university science teachers who are not in contact with research find great difficulty in keeping their knowledge fresh and up-to-date. The most serious obstacle to the creation of a genuine scientific community in a developing country is often the prior existence of a self-perpetuating academic system of low quality.

This problem is not confined to poor and technologically backward countries where higher education itself is relatively new. In a number of European countries where universities have existed for centuries, a tradition of uncritical book-learning, taught by lazy professors to a mass of students who are only seeking a nominal qualification for a white collar technical job, is the greatest impediment to scientific progress. It is instructive to notice that the best basic research workers in such countries often come through the 'Technical High Schools' or 'Institutes of Technology' which are supposed to be for the training of practical engineers. The reason is that these schools have developed outside of the old medieval university system, and as their qualifications have become more highly regarded they have built up very high standards of competitive entry and rigorous professional training. They make the most of the traditional 4 to 5 years course and give that thorough grounding in the basic sciences which is the hallmark of Continental European scholarship at its best. Whether or not it makes sense to copy such institutions in other countries with different cultural traditions, the value of a few such Centres of Excellence cannot be denied. The recent Indian decision in favour of such a policy can only be welcomed.

But a good basic education in the established principles of mathematics, physics, chemistry or biology is only half the training of a proper scientist. The longer this is drawn out, the more difficult it becomes to learn the other half—how to do research.

The harshness of the psychological transition from being a student to being a researcher is not appreciated in the world at large. Even in scientific circles—especially in the older Oxbridge tradition—it is sometimes assumed that a man
who has shown that he has a 'first class mind' by answering the stereotyped riddles of the Tripos can be put down in a laboratory and safely left to get on with his research on his very own, with no more assistance than is appropriate to a junior colleague. That is what happened in Rutherford’s day, so why should we act differently now.

Well, of course, Rutherford was a genius, and had enormous reserves of spiritual energy. The policy of ‘throwing ’em into the deep end and larning ’em to swim’ is not inappropriate to a strongly independent ‘inner-directed’ personality. Although I would not go all the way with Feuer (1963) in his attempt to show that modern science was founded by ‘hedonist libertarians’, and I am certainly not convinced that there is a direct intellectual link with the rise of capitalism or of Protestant theology, I am sure that this facet of the scientific attitude comes relatively easily and naturally to persons whose psychology has been moulded by the Puritan culture of northern Europe and the United States. The professional scientist must be able to shut off his ego from the opinions of other people, and ‘press on regardless’ with his own ideas (Merton 1968). In an age when bureaucratic conformism is said to have totally infected the human spirit, he must still assert the stubborn ‘It still moves’ of Galileo, which echoes Luther’s ‘I cannot do otherwise’. That same psychological type also arises from another more ancient variety of non-conformism—the ghetto. At its worst, the orthodox Jewish personality is pig-headed, intolerant and argumentative; but these are traits which can become the virtues of strong-mindedness, scepticism and intellectuality!

Unfortunately, these qualities are certainly not those that are encouraged by a system of mass education geared to the passing of book-work examinations (see, for example, Karve 1963). Even in the best of circumstances, one may be caught in a psychological trap. To become a scientist one must first master the current consensus, which demands many years of accepting the arguments and opinions of one’s teachers without serious question; one must at the same time preserve, or acquire, the self-confidence to reject some of those arguments when, in due course, one uncovers evidence against them. There is no simple way of avoiding this dilemma; we must plunge into the educational system and endeavour to struggle out of it again without too much loss of impetus. In a strange way, some of the most brilliant scientific intellects have escaped the trap by never falling into it in the first place; either arrogantly, or in a dream, they simply opted out of the competition for early academic success, and preserved themselves for their imaginative, critical, creative role as research workers.

Again, we cannot legislate for the Einsteins and Darwins of this world. In the past, when science absorbed an infinitesimal proportion of the manpower and gross national product even of the most advanced countries, one could safely leave training in research to the rough and ready competition of ‘sink or swim’. Now we need to organize and plan this stage in the creation of efficient scientific cadres with care and forethought.

By convention, this has now become the function of what the Americans have
taught us to call a ‘graduate school’. Unfortunately the very name—school—tends to put the emphasis on the quality of the advanced courses of instruction offered by the faculty. These are, of course, very important, especially in countries where the ordinary undergraduate curriculum is of a low standard, or too heavily weighted by old-fashioned ‘classical’ topics. In the ordinary way of things, advanced lectures both by the resident staff and by visitors from abroad, are the means by which living scientific attitudes and issues are communicated to the students. This is information that cannot be picked up by reading. The textbooks are inevitably a few years out of date, and review articles usually demand too much expertise for the beginner. Although the time of the student should not be overloaded with such courses, in the vain hope of covering all possible aspects of some very large subject, they are absolutely essential, both to those who listen to them and to those who must assemble the material to deliver them.

But a good graduate school is far more important as a training ground in research. This is not just a matter of picking up the practical techniques of the particular science—electronics, glass blowing, computing, plotting data and drawing logical conclusions: much depends upon the quality of the supervisors. For example, one of the attributes of the successful leader of research is his ability to set problems that will yield good doctoral theses for his students and teach them the art of research while making a positive contribution to the advancement of knowledge (Merton 1968). It is no good telling a young man to go away and solve the Riddle of the Universe, or find the Philosopher’s Stone; unless he is a genius, he will fail, and become hopelessly discouraged; or even go off his head. On the other hand, putting him to work as a mere technical assistant, turning the knobs, reading the meters, and plotting the points in yet another set of measurements with the departmental, white elephant apparatus, will never make him ‘self winding’ as a scholar. One of the real disadvantages (besides its enormous cost!) of the very elaborate equipment now deemed essential to research is that a whole team of students and assistants is sometimes needed to mount an experiment. In neutron physics, in high energy physics, in space science and other sophisticated fields, the individual student may get an excellent training in advanced technology, but he is seldom called upon to defeat a real live problem of natural philosophy. Scientists in developing countries may not always be wise to deplore the poverty that cuts them off from these beautiful but extravagant playthings.

Out of this necessity one can scrape a little virtue; the experience of successfully conducting a modest investigation with limited apparatus may well be of greater value to the student than being one of a team of a hundred Ph.Ds manipulating a billion dollar ‘facility’ under the distant direction of powerful boss. Let me give a simple example. Following the lead of several other British universities, we have been experimenting recently in Bristol with research ‘projects’, in places of set piece laboratory work in the final undergraduate year. A little problem is set: could the sulphur dioxide emitted by a factory chimney be detected optically from a distance; design an electronic organ capable of imitating any type of musical
instrument; what is the actual mechanism by which a steel wire passes through a block of ice? Under the supervision of a member of the staff, two students work together planning experiments, designing apparatus, and interpreting the results. Very little of this work is at all profound, but it has the very important characteristic that nobody—not even the supervisor—knows the answer in advance. In this respect, therefore, these little projects are true to the spirit of science, and provide a most valuable psychological introduction to professional research. The old tradition of a sharp transition from undergraduate course work to postgraduate research may have been quite wrong. The changeover should perhaps be quite gradual, over a period of years; research projects introduced into the undergraduate curriculum are, so to speak, a counterbalance to the further formal instruction now required at the graduate level. In this way, also, we, and our students early discover their natural inclination—or disinclination—towards a scientific career, and they can move away into more useful and practical professions without loss of face.

But a graduate school is more than a stock pile of scientific learning, technical advice, solvable problems and material equipment. The psychological transition from student to researcher is not merely a matter of strengthening one’s personality and throwing away the crutches of bookish knowledge. Paradoxically, one must also learn to be ‘other directed’ in a special way: one must acquire the habits and conventions of a responsible member of the scientific community.

For example, the graduate student must learn to write scientific papers in the peculiar impersonal style which is now customary. He must learn to accept criticism without personal offence, and to offer it without rancour. He must learn to give due credit to other people for their prior discoveries, and not to claim too much for himself. He must learn to scour the literature for references and to keep abreast of all relevant work in his own subject. Above all, he must somehow acquire high standards of scientific accuracy, honesty, and judgement, so as to distinguish quickly between the true and the false, the meaningful and the trivial.

Now it often seems to be taken for granted that these desirable habits of mind and art will come quite naturally to any sufficiently clever young man who is set down to a good scientific problem in a well-equipped laboratory. Nothing could be further from the truth. They do not come from some inner source of soul, but are acquired by imitation of the current standards and conventions of the particular institution where he is trained. They cannot be mugged up from a textbook—which in any case, would be about as useful as an instruction leaflet on skiing or horse-riding. It is rightly assumed that the difficult art of research is learnt by apprenticeship, by direct personal contact with an experienced practising master.

This is of the very greatest consequence for the whole theme of my lecture. I have emphasized the world-wide unity of pure science. That unity is not achieved and maintained by such feeble strands as the publication of learned journals, nor by corporate junketings at conferences and congresses; it exists because modern science stems historically from a single source in Renaissance Europe, and has
spread outwards by the natural process of an apostolic succession—by the fact that almost every practising scientist has at one time or another been the personal pupil of a scholar of a previous generation. In our own day we have institutionalized this tradition; the regulations for the Ph.D. now practically ensure that one cannot get registered as a professional research worker unless one has been supervised by someone who already has a Ph.D. As one who is often called upon to act as external examiner for Ph.Ds of other universities, I must admit that this ritual is becoming a little like a secular version of the ‘laying on of hands’ by which a priest is catechized and ordained by a Bishop.

The spread of science throughout the world has not been the haphazard distribution of wind blown seeds, encapsulated within the dry covers of reprints of scientific papers, but by runners and tendrils reaching out from the original institutions and establishing themselves in new soil. Ideas, standards and traditions travel around inside people, and are only transferred from one to another by prolonged contact.

Of course the major developing nations now have self-sustaining scientific communities where this tradition is preserved and retransmitted. But this is quite a recent development, even in some of the most advanced industrial societies (Basalla 1967). It is interesting to recall, for example, that the St Petersburg Academy, founded by Peter the Great in 1724, was largely staffed by non-Russian scholars until late in the nineteenth century (Vucinich 1963) while even the United States was dependent upon German graduate schools for the training of research workers in many scientific fields until a similar date. The stage of ‘take-off’ in pure science has been reached only in the past few years in so rich a country as Australia; while small nations, such as New Zealand, or Finland, still find very great difficulties in becoming self-sufficient in graduate studies over a wide range of the sciences.

It is easy enough, at this stage in the argument, to echo the points expressed very properly and concisely by Moravcsik (1966). Graduate schools, he says, should be established in developing countries because of the cost of sending people overseas, because of shortages of places in Western institutions, because graduate students are valuable technical assistants in research, and because once you have sent a man overseas for training it becomes difficult for him to re-adjust to more primitive conditions on his return. All these are valid reasons, and the various suggestions for greater technical assistance by way of buildings, equipment, and visiting experts would fulfil genuine needs, which should be given the highest priority.

It is also generally agreed (Kidd 1968; Maheshwari 1964; Blackett 1964) that one should try to give a future research worker the fullest possible preparation in his own country before sending him to an institution for advanced training overseas. There are many factors, of cost and morale, in favour of such a policy.

Yet there are real dangers in forcing the premature growth of graduate schools before sufficient human resources are available to man them. Only a policy of rigorous selectivity, by the encouragement of really first rate scholars, makes sense.
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I know how difficult it is to achieve such a policy, against all the academic political arts of log-rolling, me-tooism, appeals to fair play, etc; but the consequences of flabbiness are all too sadly evident in all quarters of the globe—the proliferation of third-rate research which is just as expensive of money and materials as the best, but does not really satisfy those who carry it out, and adds nothing at all to the world’s stock of useful or useless knowledge. The old adage should be stood on its head. Of applied science, it can reasonably be said ‘If a thing is worth doing, it is even worth doing badly’; of pure science I am tempted to say ‘If a thing is not worth doing, it is only worth doing well!’

The difficulty is that one cannot create good research groups just by setting up graduate schools: the two types of institution are strongly interacting, and come into existence simultaneously. An indigenous school of research is the culmination of the growth of pure science in a particular country, not a mere subsidiary agent.

We have got beyond the historical phase where Henry Cavendish or Michael Faraday could work away in his own little laboratory, make his own apparatus, carry out his own experiments and report his own interpretations and theories in communications to the Philosophical Transactions of the Royal Society. We find ourselves now with groups of professional scientists, organized in teams, departments, divisions, etc., working collectively. If such a group is to live for more than a few years, it must have a balanced composition, with a range of ages and experience and mechanisms for acquiring new members as the older ones retire or become administrators. These are the makings of a graduate school; the problem of tacking graduate students on at the bottom, and arranging for their supervision and formal instruction, is quite trivial by comparison with the problem of creating such a research group in the first place. All active scientific institutions are schools of apprenticeship for their junior members, whether we call them graduate students, research students, ‘post-docs’ or assistant scientific officers. The responsibility for producing well-formed research workers out of raw graduates should be the most serious burden on any senior scientist or laboratory head. These are the seed corn of the next crop, and the whole quality and success of the scientific activity of one’s country depends on how they are treated in these vital years.

Until that happens, there is no substitute for post-graduate and post-doctoral study abroad by the most promising young scientists. Whatever the disadvantages, costs and difficulties of such a policy, they must be borne if a developing country is ever to acquire a worthwhile scientific establishment.

In any case, the notion that a country can ever become so self-sufficient scientifically that individual research workers need not travel is a gross fallacy, only pardonable in a Chancellor of the Exchequer, a cost accountant or a Commissar of Culture. The whole tradition of science feeds on the sacrifices of pilgrimage and exile. Even in Europe, we are learning that polite letters and occasional conferences do not constitute a network of communications: we must actually go and work for a while with our colleagues across the border.

The reasons for this are more fundamental than that ‘travel broadens the mind’.
or that ‘one needs the stimulus of a new environment’ or even that one can only
do good work under the ideal climatic conditions of California—presumably with
a good fat Californian income to keep the wolf from the door. The universality of
science is at stake.

Consider, for example, the choice of a research problem, and the strategy and
tactics of one’s attack upon it. The aim is to produce a piece of original, publishable
research. What this really means is that it must be the sort of research that is of
interest to others; in other words, it must seem to contribute to the great body of
‘consensible’ knowledge.

There is always a stimulating philosophical debate going on concerning the
‘importance’ or ‘significance’ of various branches of science, and of particular
discoveries in particular fields. It is doubtful whether such questions of taste can
be resolved by argument; but it is certain that the vast majority of scientific
workers have only a single simple definition of publishable research: it should look
as much like other published research as possible, with a few new features to make
it seem adequately original.

This remark is not meant to be altogether sarcastic. The great power and philo­sophical authority of scientific knowledge stems from its cooperative character;
each of us builds on and into the work of his predecessors and contemporaries.
The rare geniuses are master masons, projecting tremendous extensions of the
fabric, but the stones are put in place, one by one, by the numerous journeymen.
A piece of research that does, indeed, add just a little to the work of others is more
valuable than a disconnected, fruitless, speculation, on however grand a theme.

But this means that the topics and techniques of current research are determined
by the actions and opinions of the whole scientific community, throughout the
world. By its very nature, pure science is completely supernational, for it is essen­tial that every new discovery should be communicated to every scientist whom it
might interest, and that it be subject to the critical eye of every competent authority,
whether he lives in Tobolsk, Medicine Hat, or the legendary hamlet of Waikika­mukau. Everyone knows that science, like love, knows no frontiers; but I do not
think it is adequately realized that this is of its very essence. To cut a scholar off
from his potential audience (however critical) by administrative, financial, and
political curtains and walls is to imprison his mind.

International art fashions or international political movements are signs either of
impoverishment of imagination or crude powerseeking. It is sad and unnecessary, for
example, that Japanese motor cars should look just like Italian ones, and that Mexi­can communists should use the same debased Leninist terminology as their Malayan
companions. But there is nothing out of the ordinary in my personal observation that
the Mössbauer effect is being studied in Helsinki, and in Western Australia, as well
as in its native Germany. Even in China, whose scholars are so unhealthily cut off
from direct personal contact with foreign intellectuals, current scientific work is
still recognizably international in its appeal (Orleans 1967). While there may be
charming variations of style in research from country to country, the idea of a
distinctive American, Russian, or Brazilian science, with a different intellectual content, obtaining different results, is a contradiction in terms: When, as sometimes happens in wartime, the usual channels of scientific communication and criticism between two countries are blocked, and divergences of opinion develop on purely scientific questions, the very first task of the scholars in both countries is to organize meetings at which the breach can be mended—not by force but by the presentation of the most convincing arguments and evidence until a minimum consensus is re-established. It is the analogy of such meetings that lies behind the Pugwash movement, which has tried to extend the range of consensible topics to include more serious questions of peace, war and politics.

Science in a developing country cannot, therefore, cut itself off from competition, and protect itself by tariffs like an infant manufacturing industry. If it is to be genuine science at all, it must, from the very beginning, be able to stand on its own feet. The international scientific community is not a very lenient examiner, and does not award even one mark out of ten for just attempting the question. Work that is not up to the standard set on the world market of ideas will simply be ignored; it will either not get published, or will appear in an obscure local journal which is not read outside its own country.

Now there is nothing more disheartening and debilitating to a scholar than to know that his work goes unrecognized and unread. His whole professional activity is directed towards the production of some contribution to knowledge; his labour is utterly wasted if that contribution is eventually judged to be negligible. If pure science is to exist at all, in any country, it must be adequate in quality, by this criterion. But only by keeping in close contact with his foreign contemporaries by word of mouth, hand-waving, and sketches on the backs of envelopes or lunch table napkins, can the active research worker maintain his critical standards. He must actually meet the authors of the papers he reads; he must have the opportunity to persuade them, face to face, over periods of weeks and months, that he has something of his own to contribute to the discussion. Without such intercourse, his work is liable to drift into a backwater, where he engages in mock debates with himself, quoting only his own papers and ignoring the progress made by other people. Until the day of the universal, free, intercontinental videophone, there is no alternative to carrying the body around when the mind needs to travel.

Even within the range of publishable science with an international audience there is a great deal of choice. The mistake that is often made by those who are a little out of touch is to follow fashion—not even current fashion but the styles of a few years back. This is natural enough, I suppose. Topics and techniques that are heavily emphasized in the literature have a high visibility, even from a distant continent. Imperfect communications will make them seem the only important topics for research; while the peculiar frenzy that excites some ambitious scientists when they see a problem half way to solution can seem the collective wisdom of the community at large. The phenomenon of fashion in science is of great interest and significance (see, for example, Hagstrom 1965) but its saddest consequence is the
tail of ill-equipped research groups who have not quite succeeded in jumping on that gaudy band-wagon as it flashed by. The fact, of course, is that fashionable subjects are those in which the competition is fiercest, and often where too many good people are chasing too few ideas. It does not seem very wise for a small research group, with limited equipment and many distractions and difficulties, to take on the Bell Telephone Laboratories at their own game!

The optimum strategy for the organization and planning of pure scientific research in a developing country would thus seem to be to concentrate on a few solid scientific problems, not necessarily those that are currently fashionable, and to establish a sound reputation for good if unspectacular work in these particular fields. The free trade in knowledge, which is the key to the so-called scientific method, demands specialization and division of labour. It is trite to remark that no single person can now have the whole of knowledge as his province, that the era of the universal scholar is long past. I would go further, and say that the age of the universal university is over—that even such great institutions as M.I.T. and Imperial College cannot hope to have experts in every field of science on the faculty.

It is much more profitable to emphasize particular lines of research, allowing each research group to reach the critical size for continued viability. I have thrown a spanner at team research which reduces the graduate student to a mere pair of hands; on the other hand, most modern scientists work best if they have a number of close colleagues and contemporaries with whom to discuss problems and from whom to acquire the unwritten expertise of the subject. One cannot lay down an optimum size for such a group, but my own guess is that one needs three or four permanent, established staff, with perhaps a dozen juniors and graduate students, to generate enough intellectual heat, by their mutual interaction, to keep the pot boiling. There are, of course, many forces leading to the fragmentation of research—and there are some gross evils in the building of academic empires—but this degree of concentration and specialization should be the aim of any deliberate policy for the encouragement of pure science, however 'advanced' the country.

Such a policy, however, demands just as much attention to the machinery of communication and travel as trying to follow fashion. It is necessary, for example, to know, and be known to, the few other research groups in the world who are also interested in the same topics. The formal exchange of reprints and pre-prints is not enough (Moravcsik 1966). To ensure coordination, collaboration, and fruitful competition rather than sterile shadow boxing, the leaders need to be on terms of personal friendship, and there must be regular exchanges of persons between the groups. I cannot emphasize too strongly the difference that it makes to one's attitude to a paper when the author is not just an outlandish, unpronounceable name, but that clever young man one met last summer in Uppsala—or, conversely, that long-winded old fogey from Nebraska who was mooning about the Department a couple of years ago. Perhaps such emotional prejudices ought not to be, but they are to be reckoned with in the real world.
I am fully aware that this optimum strategy is a counsel of perfection which is not at all easily followed. The reality of pure scientific research in many developing countries is often tragically wasteful of training and talent. There is a regular pattern of failure, which continually frustrates the well-meant effort to construct viable scientific institutions. Let me draw attention to one typical phenomenon.

Let us assume that a young and able student has taken himself off to a good graduate school, in his native country or abroad, and has got his Ph.D. He has the makings of a competent scientist, and given a decade of experience in active research he might well have become the powerful nucleus of a good new research group. But for perfectly proper reasons—patriotism, availability of jobs, family and cultural ties etc.—he takes a relatively junior position in a small university away from any major scientific centre. What happens to him?

We do not know in detail, because no sociologist has yet had the inspiration to go and find out. But it does not take all the professional technique of sociology to ask a few questions, and to make a few modest enquiries. On this topic I can strongly recommend an excellent article by Amar Kumar Singh (1962), who discusses very perceptively the impact of foreign study on Indian students. He expresses very forcibly the dissatisfaction and disillusionment with such things as (I quote) ‘nepotism and corruption in public organizations and government; poverty and low standards of living; waste and dishonesty; low morality in commerce; red-tape and bureaucratic delay; the discouragement and obstructive attitudes of senior persons in positions of authority; the general inefficiency, lethargy and disorganization permeating all spheres of social and political life, and the absence of social justice and individual dignity’. Stevan Dedijer, in an equally forthright article (1963) adds, for good measure, the lack of any general cultural sympathy for the scientific point of view, and remarks on certain pathological forms of careerism that flourish in this unhealthy soil. I have quoted evidence from India, but much the same would be said about many developing countries by those who know them best. One could even apply some of these remarks to a region such as Sicily, where the dishonesty and moral corruption of the Mafia is a formidable obstacle to the scientific spirit.

It is not surprising, in these circumstances, that many a promising young scholar abandons serious science at this stage. He loses enthusiasm for research, and becomes eventually one of those lazy, do-nothing politicking professors at whom so many brickbats are thrown—or else he resigns his post, and flees to a bigger centre at home or abroad, where he can continue to be a scientist. At the moment when he is trying at last to stand on his own feet, he is bowled over by the irresistible tides of an inimical cultural environment. It is asking too much to expect superhuman fortitude in the face of such circumstances.

I see little point in belabouring such a familiar topic and sermonizing on the ills of other people’s cultures. One might as well address stern admonitions to the weather, and rage, like King Lear, at the elements! But I do believe that there is
a very special factor which particularly affects the professional scientist, and about which something can be done. He becomes intellectually isolated.

As I have emphasized throughout this lecture the romantic picture of the scientist as a lonely hero, on a solitary expedition through a sort of starlit outer space of the mind, is applicable only to a few extraordinary geniuses; whatever we do, they look after themselves. The modern research worker is a highly trained professional, who has been taught to cooperate—and compete—with his contemporaries. The better his training, the more he will have to come to depend upon the face to face contact with his peers, for discussion, stimulation, criticism and technical information. He is no better prepared for doing research on his own, with no one near to whom he can speak about his work, than the inhabitant of a city is equipped for life in the middle of the Sahara. If he is to pass safely between the Scylla of scholarly decay and the Charybdis of the brain drain, then we—the international scientific community—must try to help him. We owe this not merely to our own charitable instincts but to our allegiance to the pursuit of learning, for he is not merely a fellow man but a scientific colleague. What can we do to keep his mind alive?

Here again, the only cure is to provide some means of personal contact with other scientists in the same field of study. The admirable practice of sending experts out on lecture tours from the major centres to the provinces, or from advanced countries to less developed regions, helps a little; but it has necessarily only a transitory effect that cannot be repeated often enough, or for long enough on each occasion. It is important to remember that we are not so much concerned with particular pieces of knowledge—the latest formula for the elixir of life, or where to look for quarks, or what would have been found at the bottom of a Mohole—but with recreating an intellectual environment. The visiting expert may convey a great deal of technical information in a brief visit and by criticism or encouragement may decisively alter the direction of research in particular cases. But he cannot transport the atmosphere of a whole institution.

In one of his sympathetic and realistic articles on this whole subject, Moravcsik (1964) has made much of what I should describe as the missionary approach to the problem—the sending out of young but competent scientists from the advanced countries to spend several years or more in the newer institutions to help them set high standards of teaching and research. This too, is an admirable activity, but it is not really cheap in money and manpower. It demands, indeed, a missionary spirit of charity and humility which is not always to be found in psychological combination with scientific ability, and it carries with it just a hint of the anxious condescension that is so damaging to the whole missionary enterprise. As I have already remarked, the exchange of scholars for substantial periods is far and away the best means of linking the institutions for which they work, but it needs to be justified on its own merits, and as between near equals.

Another valuable development is the organization of local, national and regional conferences, summer schools, and other meetings. One of the lamentable consequences of intellectual isolation is isolationism, the unconscious fear of exposing the
inadequacy of one's achievement to one's fellow scientists. In the struggle to get any work done at all, the effort to create time, funds, and organization for such meetings begins to seem not worth the possible benefits.

This is a mistake that we have long been making even in Britain, and in Europe generally. The importance of a scientific meeting does not lie solely in the formal communication of the results of research; it is an opportunity for a scientific community to become conscious of itself, and for individual scientists to become aware of one another's difficulties and common needs. Meetings cost money, and they demand public-spirited initiative from those who arrange them, but they could be the saving of many sound scholars.

The above remedies for the chronic malady of intellectual isolation attack the cause, but the disease is so deep-rooted that more drastic cures are needed. A day's visit from a travelling expert, a week at a conference, a fortnight at a summer school—these are only occasional episodes that do not really change the pattern of one's life. Somehow we must make it possible for young scientists in developing countries to maintain prolonged contact with their fellows, over periods of months and years.

The most ambitious project with this goal is Abdus Salam's International Centre for Theoretical Physics, at Trieste. This was created in 1962 as a place to which active scientists from developing countries might go from time to time, for periods ranging up to a year, to bring themselves into contact with current research and with their contemporaries from both developing and advanced nations. One might say that it was conceived as a permanent meeting place for the members of the Invisible College of this particular scientific subject, where they might return again and again to recharge their intellectual batteries. By arranging lengthy advanced courses of study, and a programme of seminars by distinguished visiting scientists, Salam has made it an institution of the very highest standing, permeated with and radiating the best scientific traditions.

The work of the Trieste Centre is so very relevant to the whole theme of this lecture that I wish I had time to say more about it. I should have liked, for example to speak of the experience of directing one of these advanced courses, which was attended by young research workers from about the most diversified list of countries that you could imagine, and which nevertheless demonstrated the complete universality of the scientific attitude in this particular field. This experience was, indeed, so moving and revealing that it is the real basis for my temerity in giving this lecture; and much of what I am saying here is merely a reflection of their own feelings about their situation.

The International Centre for Theoretical Physics can deal only with a part of the problems on a very small sector of the whole front line of modern science. I am by no means sure that it is in all respects an ideal solution, nor that the same thing could be done in all other fields. But it has produced one entirely new social device—the scheme of Associateships (Salam 1966; Editorial in Science, 1968)

Briefly, this is a scheme by which an individual scientist acquires the means to travel to Trieste and to spend up to three months there, once a year, over a period
of several years. It is like a travelling scholarship, or post-doctoral fellowship, but broken up into small pieces, and spread over a much longer time. Travel and subsistence costs for each visit are paid for, but the associate must, for his part, continue his ordinary duties as a lecturer, professor, or scientific officer in his home country in the intervening periods.

The effectiveness of such an arrangement rests upon its being so well geared to the true needs and time scale of the isolated scientist. A few months at a time is as much as he can be spared from his official and domestic responsibilities, and yet is long enough to make significant progress on a new scientific problem. The annual repetition of this privilege over, say, 5 years, allows his work to continue and grow, with opportunities for critical evaluation each year. It stabilizes him in his native environment and yet gives him a seat at the international 'high table'. By assigning an appropriate interval of time to each of those conflicting responsibilities that tend to pull him apart, he can largely reconcile them, and serve each with honour.

But there is no reason at all why such a scheme should be arranged solely in connexion with an international centre of the Trieste type. Every good university is an international centre of learning. The cure for isolation is to come back, from time to time, to any active research centre in one's particular field. The very strength of the scientific community is that it is cross-linked in all directions, and does not depend upon just a few key 'centres'. It is still true, however, that ideas tend to flow outwards, from the most successful and active institutions (not always in advanced countries) to the isolated workers on the periphery. Let the means be found by which the latter can travel regularly to the places whence their ideas come, so that they may share a little in the making of them, and carry them home inside their own heads. The associate thus becomes, so to speak, a foreign member of an active research group, familiar as a person through his regular visits, and a party to its collective wisdom. On the scale on which money is lavished on the support of science, the cost of such a scheme would be negligible by comparison with its benefits. Here is the most practical means by which the scientists of advanced countries can give aid to their colleagues in developing countries—aid that is more precious than shiploads of books and apparatus.

As I come to the end of this lecture, I realize how much more there is to say. I am aware, too, that I have made an elementary error of scientific strategy; I have tackled an insoluble problem, and merely recapitulated all the familiar, trite, non-answers to it. But the problems of the growth of pure science in developing countries are not scientific problems, which by definition always have 'answers'. They are by no means always technological or economic problems—although these are large factors in the equations. They are historical, political, cultural, and psychological problems—that is, they belong in the realms where there are never 'solutions' but only influences. They demand more than knowledge; they demand wisdom, charity, and heroic strength. I bow to those men and women, in all quarters of the globe, who have shown by their scientific achievements under the most adverse circumstances, that these virtues are still attainable.
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