SCIENCE IN A SOCIAL CONTEXT

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by

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VICE-CHANCELLOR, REGISTRAR, LADIES AND GENTLEMEN:

SINCE the establishment of the Chemistry Department as one of the foundation departments of the University in 1948, there have been seven Professors apart from myself but only one of them has delivered an inaugural lecture, and that more than 20 years ago. Although I first set foot in Nigeria on Independence Day, 1960, twelve years after the establishment of the Department, I have worked as a colleague with six of these Professors, first as a lecturer and finally as a Professor since 1969. Between them these Professors have given 95 years service to the Department. In accepting the invitation to give this lecture, and in choosing the topic for it, I was very conscious of the beneficial influence of these distinguished colleagues on my own development. There was no doubt in my mind that I wanted to talk on a subject which would reflect in some way the ever-present concern of the Department that its various activities should reflect the needs of the country, and that it should be in the vanguard of those in the University who seek to plan for the needs of the future. As Head of Department for the past three years, the problems of university planning in a national environment undergoing extremely rapid development have been in the forefront of my mind and I chose the topic: "Chemistry in a Social Context" rather than one related to my own research interests. However, as I put pen to paper it became clear that much of what I wanted to say had relevance for science as a whole. Hence "Science in a Social Context."

In order to bring order to my remarks I shall take as my theme, the activities of university scientists in a social context, but in so doing, I shall touch on many topics that are broader in scope than this theme, but which are embraced by the title of my lecture. If in so doing I provoke discussion and debate where none existed before I shall have achieved my major purpose of convincing you that there are controversial but crucially important issues concerning the relationship between what we do as university scientists and the social contexts in which we do it.

How do we as chemists spend our time? Our activities fall quite clearly into four categories: Firstly, teaching undergraduates; secondly, carrying out or directing research work; thirdly, administration — a necessary duty but in most of its aspects an unproductive occupation and I shall not mention it further; fourthly, acting as advisers or consultants for government or industry. Very little of this last activity goes on in Nigeria and an increase would in my view benefit the university far beyond any possible financial gain. I shall confine most of my comments to the first two activities: teaching and research, but first I shall digress to examine more closely what I mean by the social contexts in which we work.

First let me assert that I believe there are, or ought to be three interacting but essentially separate contexts in which we work. The first and obvious context is the national one. We should not, cannot, and indeed do not work in isolation from the national economic and social environment. The government pays the piper and hence calls the tune, and we as university professors and lecturers can but be the choreographers of the students dancing to this tune in a style determined by their aspirations and motivations which are largely conditioned by the society in which we all live and work. If we agree, as I am sure most of us do, that, to a large extent, the government should, by overall control of the source of funds, have a pervasive influence on the priorities in university development, and if in addition we accept as given the aspirations and motivations of our students, then our role is the severely restricted and largely uncreative role of a shop floor manager in a factory for high level manpower. It is doubtful if a university professorship would retain the prestige accorded to it if this were the only social context in which we work, and of course it is not.

We are, in addition, members of a community of scholars working in a social context, the university, which allows and encourages scientific and scholarly enquiry into all aspects of man, his society, and his environment. This social context extends both in space and time, beyond the boundaries of any individual university because it is embeded in a tradition that goes back 700-800 years, and which has cut across national and geographic boundaries to foster and sustain universities throughout the entire world. If we are to be active and creative, as opposed to passive and pedestrian, choreograhers of the educational dance to an economically and socially determined tune; if we are to be composers of new tunes as yet unheard of by he who pays and calls the tune; if we wish to influence, widen, and enrich our students' motivations, aspirations and intellectual horizons beyond those they hold when they enter the university, we must recognize, cleave to, and make our contribution to modifying this cosmopolitan tradition of universities.

For scientists and those scholars whose discipline encompasses a scientific component, there is a third and vital social context which is partly coextensive with, but which in some ways is wider than, and has a different origin from that of the community of universities. The study of science, the science of science as it has sometimes been called, from the philosophical and sociological point of view has developed into a highly technical subject of which most scientists are unaware, and if they are aware, consider it with considerable justification to be irrelevant to their day-to-day activities as scientists. Nevertheless, one theme that has been emphasized by some writers, notably Ziman, is the crucial position in scientific activity of the need for a concensus and the procedure by which it is attained. It is in the very nature of science itself that this concensus is sought among the widest possible community of scientists capable of assessing the quality of one another's work. I quote from Ziman:

> The manifest internationalism of Science is not a bourgeois or communist conspiracy; it is not mere sentimentality about the Brotherhood of Man; it is inherent in the very nature of Science itself, which must always seek to encompass the largest public for the knowledge it aspires to.[1]

Thus the university scientist has a loyalty and allegiance to three social contexts: the national society in which he lives and works, the cosmopolitan university tradition, and the international community of scientists in his own discipline and speciality. There is no logical requirement for these loyalties and allegiances to the different social contexts to be always compatible. The relative importance that would be accorded to the contexts and the extent to which the allegiances are seen to be complementary or incompatible will vary from university to university, from country to country, and it will inevitably depend on whether the assessment is made from inside or outside the university. Such disagreements are inevitable and indeed can be productive if a dialogue leads to a greater understanding, but if the divergence of opinion, both within and outside the university, on the relative importance of these social contexts is very wide, and if the striking of belligerant postures is substituted for dialogue, then I contend that the contribution of the university to both knowledge and national development will be adversely affected. However, it is beyond my purpose today to delineate in detail these various social contexts and the relationship between them, and I return to my theme of examining what we as university scientists spend our time doing.

First let us look at the activity of undergraduate teaching. Whom do we teach, what and how do we teach, with what objectives in view, and what happens to our graduates when they leave? In the language of industry: what are our raw materials, in what way do we add value to these raw materials and by what processes, and who are the consumers of our product? Let me start by examining the last question first: Who employs our graduates? The table shows the distribution in various categories of employment of single honours graduates from the Department of Chemistry in the period 1955-73, based on information gathered by the Department over the last three years. Of the 353 graduates from these years we know the type of employment of 93 per cent. The percentages shown in the last column refer to those actually in employment, that is to say we have excluded from the calculation those for whom we have no information and those who are postgraduate students not yet in the employment market. The following points are noteworthy: 55 percent are employed as teachers in schools, polytechnics or universities. If we recognize that most of the chemistry staff of the polytechnics are teaching the equivalent of 'A' level, then 33 percent are engaged in teaching at the secondary evel. 21 percent are employed in industry and 19 percent by the government in administration and as professional chemists in laboratories and research institutes. Assuming that those in the universities and polytechnics are, or should be, engaged in research, 52 percent of the graduates are active as professional chemists at the laboratory bench or supervising others.

I would suggest that projections of such figures as these are likely to prove as reliable a base for future planning as any centrally produced global estimates of relative requirements for high level manpower in various sectors, and I should like to take this

UNIVERSITY OF IBADAN, DEPARTMENT OF CHEMISTRY

EMPLOYMENT OF SINGLE HONOURS GRADUATES

(1955-73)

AS AT JUNE 1975

	No	%
Universities	61	22
Polytechnics	31	11
School Teaching	61	22
Civil Service Administration	18	6
Government Laboratories	36	13
Industry — Management	41	15
Industry — Laboratories	17	6
Sundry	13	5
Postgraduate Students	42	
Deceased	8	
No Information	25	
Total	353	

opportunity of urging all departments to collect and publish comparable statistics.

That 52 percent of our graduates are employed as professional chemists has implications for the content of our curriculum. It must expose our students to the latest concepts and techniques, for they will require them as working chemists. There is a small but growing literature which advocates the adoption by university chemistry departments in developing countries of a more "relevant" content of their degree with the implication that topics such as NMR spectroscopy are irrelevant. I have used the much abused and usually ill-defined word "relevant" here. Whatever it may mean in some countries that are smaller, poorer and less educationally developed than Nigeria, it cannot mean for most of our graduates anything but a training that equips them to work as chemists anywhere in the world; atomic absorption spectroscopy is the same, and just as useful, whether used in Cambridge, Massachusetts, Cambridge, England, or Ibadan, Nigeria. The universities have a responsibility for anticipating future instrumental needs and there is no doubt in my mind that for a large proportion of our graduates a sophisticated in-depth exposure to contemporary concepts and techniques is essential.

However, 21 percent of the graduates are employed in industry; a far smaller percentage than is common in industrialized countries. This is a reflection not only of the low level of industrialization in the country, but also of the lack of research and development sections in such industries as do exist. The 6 percent that do work in industrial laboratories are employed predominantly in quality control. I shall return to the specific problem of the lack of research and development in industry later but I note here an important consequence of this deficiency. One of the major routes to mangerial positions in industry for science graduates in industrialized countries has been through the research and development sections, and I agree with Graham Jones when says in his book, *The Role of Science and Technology in Developing Countries* that:

> Management is one of the most important single factors in modern industry, and has a vital part to play in the creative and efficient use of available resources including technologists. The experience of industrialized countries indicates the importance of encouraging scientists and technologists to move into

industry, agriculture, administration and management generally rather than take an exclusive interest in research.[2]

In the absence of research and development sections in Nigerian industries, entry into management through this door is blocked and there is the danger that Nigerian science based industries will be managed by non-science graduates. Some companies have made efforts to recruit science graduates into trainee management positions, the civil service hardly at all.

In a technological age a country cannot afford to be entirely administered and governed by those who have had little or no scientific or technological training. However, I do not intend to make a detailed case in this lecture for the active recruitment of science graduates into industrial management and to the civil service administration. Rather I assume that those responsible for recruitment into these sectors will, in time, follow such a policy, and I anticipate the question that will then arise as to whether the present single subject honours degree is the best science education, as opposed to professional training, that the university can give to a potential administrator. Should we not consider the possibility of an alternative degree programme to run in parallel with our single honours degree programme; a programme in which the specialized technical content of the present programme would be replaced by courses designed to make the student aware of the social, economic and technological environment in which he, as an administrator, would be expected to work.

This brings me back to asking, what and how do we teach, and with what objectives in view? Too rarely do we reflect on these questions in the daily rush of teaching, research, and administration with the periodic crisis thrown in for good measure, but I suggest that most of us rely on one or other of two, usually tacit, assumptions. First we may assume that the single honours degree is the best recipe for training a professionally competent scientist in a particular discipline. We could validate this opinion by referring to the necessity for, and relevance of the training to the needs of the society, that is by appealing to the first of my social contexts, the national one. Secondly we may assume that an education in depth in one subject is the right, and even perhaps the only proper thing for a university of standing to concern itself with. To validate this opinion we would appeal to the second of my social contexts, the universitv tradition. In Ibadan this appeal is usually phrased in terms of "Ibadan standards." For example, of a proposed new degree programme the members of the appropriate university committee might ask: is it in accord with "Ibadan standards"? This is right and proper because universities do, and should, have their own internal criteria by which they judge the propriety of engaging in this or that activity. Nevertheless, too rarely is it explicitly recognized that the single honours degree is not an ineluctable and logical outcome of 800 years of university tradition. In general its roots can be traced to the late nineteenth century, and specifically, in the case of the University of Ibadan, to its special relationship with the University of London.

When the appeals to the national social context and to the university tradition are in agreement, few problems arise. As I described earlier, I am convinced that a single honours degree in chemistry can be justified in terms of the type of employment taken up by 52 percent of our graduates. This type of programme is also consistent with the traditions of the University of Ibadan, and a few question whether it is right and proper for us to structure our teaching in this way. But is this type of degree the most suitable for a science graduate who is a potential administrator in industry or government? Is it necessary for such a man to be familiar with the details of metal carbonyl chemistry, the refinements of relativity theory or analogous specialities in other disciplines? Surely there is a need for a man who is interested in, and can understand, the world around him in scientific terms, and who is also aware of the problems associated with developing science-based industry in the country; a man who is aware not only of the type of chemistry which allows the petrochemical industry to produce his nylon shirt (which it must be emphasized is quite distinct from the chemistry likely to be of use to most chemists employed professionally), but who is familiar with the economics and politics of the international oil and petrochemical industries; a man who not only understands the theory of and problems associated with the transmission of electric power from Kainji to his home, but who can also offer an educated opinion on the so called world wide energy crisis; a man who can tell you whether he is standing on granite or sandstone and who in addition can contribute to a discussion of the future prospects for tin mining in the country; a man who is aware of the ecological changes brought about by the Kainji Dam, and who is also familiar with the difficulties of controlling parasitic diseases; above all a man who is aware of the political, economic and administrative problems associated with science in the service of technological and economic development. An idealized man perhaps, but a scientifically educated one; one whom we could not perhaps produce in three years, but one who could develop by continued self education once given a sense of direction by the University. I see it as a challenge to the scientists in the university to devise and teach an alternative honours degree course whose merits would lie in its breadth and scope rather than its depth of penetration into one subject. But unless my words today are more persuasive than I anticipate, such a proposal, although desirable in relation to the national context, would be found wanting when judged against the customary university requirement for an honours degree of study in depth of one or two subjects. I might add in passing that since such a degree would not have as its aim the production of professional scientists, the amount of practical laboratory experience could be less than is customary for a single honours degree and on this account alone might be suitable for the external degree programme. If it is argued that the employment policies of government and industry give overwhelming emphasis to the single honours degree as showing evidence of intellectual attainment, then I assert that here is a case for the university to convince he who pays the piper that there are alternative worthwhile tunes to play.

To recapitulate, I have examined the first of our activities, teaching undergraduates, from the point of view of who we teach, how and with what objectives we teach, and by whom our graduates are employed. I turn now to the second of our activities, carrying out research and directing the research work of postgraduate students. In Ibadan it is taken for granted that research and teaching should go hand in hand. I shall not question this assumption, for, from my own personal experience, involvement in research has played an essential part in my development as a university teacher, and conversely my involvement in teaching has without doubt contributed to the quality of my research. Any further discussion of this point would be merely academic and I unashamedly generalize from my own experience and assume that for a university to provide an undergraduate training of hihg quality and lasting value, its staff must be actively involved in research, with the implication that both time and facilities must be provided to allow this; but what research?

What should be the nature of the research work carried out in the University of Ibadan in 1975, what benefits might be expected to arise from it; how should it be financed and what should be its relationship to the national research effort? Contrary to the impression created by the facile rhetoric and commendations of the occasional newspaper pundit, these are not straightforward questions; they are part of a complex matrix of problems increasingly faced by all countries, whether industrially developed or undeveloped. To the question, how should national priorities of scientific and technological research effort be determined, and how should the effort be financed, supported and administered, there have been as many answers as there are countries. To examine these questions in depth would take a large monograph, not a 50-minute lecture, and my intention for the remainder of the lecture is to draw attention to some facets of the problem, with particular reference to university scientific research. In so doing, I may at times appear as a gloomy Casandra prophesying doom, but I have intentionally adopted this stance to emphasize what I consider to be a greater danger than insufficient emphasis on science and technology; the danger which can arise from a blind faith in the efficacy of scientific research in promoting economic and social development without a corresponding cool and calculating appraisal of how the efficacy is to be assured. I have no glib political, financial or administrative recipe to give you; rather my main message is that unless such a recipe is found by the constant efforts of those in the universities, government and industry, the present unquestioning faith in the socially beneficial role of scientific research may be replaced by a profound disillusionment.

Throughout the development of modern science there have always been those who held the view that science could, and should be useful. Sir Francis Bacon, the English politician, lawyer and scientist in discussing and condemning the apparent barrenness of the intellectual activities of his day wrote in 1592:

> Shall we not thereby be able to produce worthy effects, and to endow the life of a man with infinite commodities?[3]

In 1610, Galileo, when seeking the patronage of the Duke of Florence (the seventeenth century equivalent of applying for a research grant) was not above mentioning the significance of his type of work for:

> the practice of fortification, ordinance, assaults, sieges, estimation of distances, artillery matters, the uses of various instruments and so on.[4]

Bacon's heartfelt advocacy of the use of science for solving man's problems and Galileo's tactical emphasis on the possible utilitarian significance of his work when applying for financial support are themes that persist to the present day. Furthermore, as Merton[5] has shown, there is no doubt that much of the scientific research in Britain in the seventeenth century could be related to the economic and social needs of the country, albeit in an indirect way. Throughout the nineteenth century the social and economic consequences of scientific research and technological development became increasingly manifest but the relationship between them was an extremely complex one, scientific research more frequently arising from prior technological development than visa versa.

However, it is also true to say that governments remained largely aloof and indifferent to the activities of scientists and scientific activities. At the height of a major war between Britain and France at the beginning of the nineteenth century, the eminent British scientist Sir Humphey Davy visited Paris to meet French scientists. His visit caused no alarm on either side, not because it had some cladestine mutually acceptable political purpose, but because it was thought, if it was thought of at all, to be irrelevant.[6] Thus at a time when Britain was the most highly industrialized country in the world, the activity of scientists was considered by the government to be of little consequence. The following anectode suggests that the situation was little different a century later. J.B. Conant, the distinguished U.S. chemist relates the story that during the First World War the American Chemical Society contacted the Secretary for War to ask what they, the American chemists, could do to help the war effort. The reply expressed polite appreciation, but thank you very much, the War Department already had a chemist[7]; that is, in 1916 the greatest industrial power in the world, when involved in a world-wide war, was satisfied that it had a chemist. For the next twenty years governments in Europe and the U.S. remained aloof from extensive organization and financing of scientific research. I note in passing that the expenditure on research by the United Kingdom in the late 1930s has been estimated to be 0.1 percent of the G.N.P. , which is to be compared with the current figure of about 0.3 percent in Nigeria. But in 1939 we come to the publication of a book that exerted an enormous influence on some of the most able young scientists at that time: Bernal's, *The Social Function of Science* subtitled: *What science is; what science could do.*[8]

This was a prophetic book and the novelty of its main thesis was emphasized by a comment made by Bernal twenty-five years later as to why he had omitted any reference in his book to the practical aspects of nuclear fission, both peaceful and the possibility of an atomic bomb:

> The omission of any reference to this in its constructive or destructive aspects may seem surprising but it was deliberate. I knew very well about nuclear fission but I was warned by my friends in the Cavendish that any references to its practical application would prevent reputable physicists from taking my book seriously.[9]

The Second World War broke out nine months after the *Social Function of Science* was published and Bernal's prophecy quickly became a reality. It became apparent that the systematic application of science, and the organization of scientists could contribute immensely to the attainment of national goals. However scientifically disreputable it may seem in retrospect, the single minded pursuit of the national objective of the construction of an atomic bomb which was the Manhattan Project in the U.S. remains the most successful example of the harnessing of the combined scientific and technical expertese of government, industry and the universities for the attainment of a national objective in any country. I do not intend here to describe the multiferous ways in which the involvement of scientists in the prosecution of the second world war led to the general acceptance by governments that, far from being irrelevant, scientific research can be immensely profitable, and how after the war this led to at least partial acceptance of the principle, first enunciated by P.M.S. Blackett, that:

> allocation of money to science should be made in the measure of what a competent scientist can usefully spend and not according to what he can just manage on.[10]

The study of the development of the acceptance of this principle would be interesting only as history, for in the industrially developed countries this age is past. The burgeoning of funds for scientific research at a rate far exceeding the rate of growth of the gross national product in most developed countries between 1950 and 1970 has come to an end, as indeed it had to on purely economic grounds, and with the slowing down of the rate of growth of funding comes the inevitable reappraisal in these countries of the justification for this massive expenditure. Along with the reappraisal comes the questions to which there are few uncontroversial answers that receive widespread acceptance even in one country let alone internationally: how should scientific research be financed, where should it take place, how should it be administered, how should priorities be determined and by whom, and a host other related questions.

The central problem is that, in the words of Zuckerman, one time Chief Scientific Adviser to the U.K. government, writing in 1967,

> ...in spite of any amount of literature on the subject,we still know very little about the true significance of research and development to economic growth. Of course, we all accept that if there were no new scientific knowledge to be exploited, the processes and products of manufacturing industries would soon become fixed in their present mold. Equally we know that broad international comparisons of research and development expenditures tell us very little that is significant about economic growth.[11]

I might add by way of emphasis that while few deny that the economic gap between the rich and poor countries cannot be meaningfully discussed without reference to the gap in scientific and technological expertese, little is known in specific terms about how investment in scientific research can contribute to the reduction of this economic gap.

Thus as a broad although simplified generalization we may say that in the short period of thirty years the attitude of the governments of developed countries to scientific research has metamorphosed from a state of indifference, through unquestioned financial support, to the present state of an increasingly vociferious questioning by legislatures of the uses to which government money is put in the name of science, and of reappraisal by administrations of how and to what extent the national research effort should be controlled and administered. I do not have time to venture into the broad field often referred to as science policy, and I shall restrict myself to an examination of those options open to the University. However, in passing I should like to take this opportunity of suggesting that it would not be out of place for the University of Ibadan to begin, perhaps initially in an informal way, to provide a focus outside government of informed opinion on matters of science policy. Far too often one hears opinions expressed that confuse scientific research with technological innovation; that show an implicit and unquestioned belief in the linear model for the effect of scientific research on economic growth; that ignore the fact that massive government investment in research in economically developed countries followed rather than preceded industrialization; that not only make the correct assumption that investment in scientific research leads to wealth but also the invalid assumption that scientific research is predictable and hence all research programmes could and should lead to demonstrable economic consequences. We all want it to be demonstrable in Nigeria today that in specific cases scientific research can lead to desirable social consequences, but sloppy ill-informed thinking about how this might be brought about does not help us; and a university contribution to the national research effort can only be effective if there is an explicitly formulated government policy on scientific research and technological development.

What then are the options open to university scientists? In framing my comments I have had the physical sciences uppermost in my mind but with slight changes of emphasis they are applicable to all the sciences. I must start by saying that I reject the classification pure and applied research. First, I reject it because along with this distinction has grown up in the public mind an incorrect view that something called applied research is good because it supposedly always leads to results useful to society, and that something called pure research, if not bad, is a luxury since it does not lead to anything manifestly useful. Second, I reject it because within parts of the scientific community, there has grown up the feeling that in some way pure research is superior to applied research. I prefer the classification, "curiosity-oriented" research and "mission-oriented research. Using Weinberg's terminology,[12] mission-oriented research is to be judged primarily by the external criteria of technological and social merit, and in practice it is most frequently carried out in industrial or government laboratories with specific terms of reference, or in university laboratories that have been given a grant by a government agency to carry out research work with a prescribed end in view. Curiosity-oriented research is just what it says, research in fields dictated by the scientific curiosity of the scientist carrying it out. The value of such research is determined, in the short term, by criteria internal to science itself. But let me emphasize that mission-oriented research frequently produces results of no social or technological value and results of curiosity-oriented research are sometimes, albeit with a long lead time in many cases, the basis for entirely new fields of technology. Furthermore, even where the funding of research project would lead to the label mission-oriented research being attached to it, good curiosity-oriented research is frequently inextricably entwined with it.

> Bernal has recommended a feudal method for operating laboratories. The research worker ploughs the Lord's land for half the time and can plough his own for the remainder. Bell Telephones follow the same policy with a success which makes them outstanding in commercial research.[13]

To illustrate the possibilities of interaction of curiosity and mission-oriented research in a university, let me give an example from the U.S. Apollo moon landing mission.

Irrespective of whether we think it sensible or misguided, the Apollo mission was a clearly stated national objective of the U.S.A. crucial requirement for the successful outcome of this project was to return the astronauts safely through the earth's atmosphere. This in turn led to the requirement for a nose cone that would protect the spacecraft from the heating effect of the atmosphere during reentry. Eventually, as one of the thousands of research programmes funded by the Apollo mission project, a university scientist interested in the physics and chemistry of surfaces studied the surface cracking of ceramics at high temperatures. The chemist was following his own curiosity-oriented research interest in the broader context of a large nationally determined mission-oriented research programme. Can we use this as an example of what we in Ibadan might be doing? In principle yes, but in practice we find that notwithstanding the establishment of the National Council for Science and Technology and its associated research councils, it is significant that we are yet to see in Nigeria the development of the complex formal and informal, and frequently ill-defined, mechanisms by which a broad but clearly stated national objective is translated into specific and precisely defined scientific research projects supported by funds justified by the political acceptance of the national objective. Until they do develop the prospect for the widespread establishment of this type of mission oriented research in the universities is bleak.

Having clarified my views on the appropriate terminology for the classification of research projects, I return to the options for research open to the university. What could we and should we be doing? I shall look at this from the point of view of the three social contexts I defined earlier: the national, the university, and the international scientific contexts. First the perspective as seen from the university context: I suggest that our prime responsibility in this context is to attempt to pass on to our research students the intellectual and technical challenge, exitement, and satisfaction to be found in scientific research and to introduce them as members of the international community of scientists in a particular discipline. Too rarely is it recognized that the highest hurdle in the race to establish a viable productive research unit is not the availability of funds for equipment or the availability of a sufficient number of Ph.Ds. and associated technical staff. Rather it is the task of creating the intangible and, at best, vibrant atmosphere in which creative scientific work is carried out; an atmosphere which is particularly necessary for the training and encouragement of young scientists whose motivations are internalized, and who will singlemindedly throw themselves into a research problem because they find it a fascinating and personal challenge which contains its own satisfactions rather than working because they are paid to do it or to gain promotion. Naira are necessary but not sufficient to create this atmosphere, and it cannot be created overnight. Government laboratories the world over are not noted for their ability to create and sustain this atmosphere and they are not likely to change as long as civil service administrative procedures dominate their organization.

The universities are the institutions that must pass on from one generation of scientists to the next the ethos in which good scientific work can be carried out. In this social context the decision as to whether or not a certain type of research work is carried out should be determined primarily by whether the department and the members of staff concerned are competent to engender this ethos; the particular nature of the research is of less significance. The achievement of the University of Ibadan in establishing and expanding its postgraduate programmes is too often assessed solely as a contribution to the supply of high-level manpower. As significant is the achievement of establishing from scratch the necessary intellectual and social milieu for good scientific work to flourish. The psychological and sociological achievement that this represents is too little recognized. There are those who, often speaking from hindsight and without personal experience, have criticized as irrelevant or inappropriate some of the research carried out at this university. In so doing they take our achievements too much for granted. In the formative years of the University of Ibadan, any research activities which helped to prepare a fertile soil in which future postgraduate scientific education could grow made a valuable contribution, whatever the precise nature of the research topic. I might add that there is a justification for allowing the most able Nigerian scientists to pursue, for part of the time at least, research interests dictated solely by their scientific curiosity and judgement. If anyone doubts the validity of this let them consider whether a Nobel prize awarded to a Nigerian scientist would be more or less prestigeous than Olympic gold medals for some Nigerian athletes, or a victory for Nigeria in the world football cup, both of which activities require more financial support than would be required by the prospective Nobel prize winner over his lifetime.

Now let us turn to look at the problem from the perspective of my third context: the international community of scientists in any particular discipline. I wish to emphasize one philosophically based reason for such active international participation. In recent years, of those scientists who take any interest in the philosophy of science, most subscribe either to the views of Popper,[14] particularly as popularized by Medawar,[15] or the views of Kuhn as formulated in his book *The Structure of Scientific Revolutions.*[16] I clearly cannot do justice to either of these views in this short lecture but at the risk of gross oversimplification I shall give you a brief statement of the Popperian view by quoting Medawar. He distinguishes what he calls a romantic view of science and an alternative that I call "the nose to the grindstone" view:

> In the romantic conception, truth takes shape in the mind of the observer: it is his imaginative grasp of what might be true that provides the incentive for finding out, so far as he can, what is true. Every advance in science is therefore the outcome of a speculative adventure, an excursion into the unknown. According to the opposite view, truth resides in nature and is to be got at only through the evidence of the senses: apprehension leads by a direct pathway to comprehension, and the scientist's task is essentially one of discernment. This act of discernment can be carried out according to a Method which, though imagination can help it, does not depend on the imagination: the Scientific Method will see him through.
>
> Inasmuch as these two sets of opinions contradict each other flatly in every particular, it seems hardly possible that they should both be true; but anyone who has actually done or reflected deeply upon scientific research knows that there is in fact a great deal of truth in both of them. For a scientist must indeed be freely imaginative and yet sceptical, creative and yet a critic. There is a sense in which he must be free, but another in which his thought must be very precisely regimented; there is poetry in science, but also a lot of book-keeping.
>
> There is no paradox here: it just so happens that what are usually thought of as two alternative and indeed competing accounts of one process of thought are in fact accounts of the two successive and complementary episodes of thought that occur in every advance of scientific understanding.[17]

While agreeing that the Popperian process goes on, Kuhn maintains that it is restricted to certain revolutionary periods in science. As Jevons puts it:

> At first sight, there is much in common between Popper and Kuhn. Neither, for instance, sees scientists as gathering facts uninfluenced by expectations derived from a conceptual framework of some kind. Nor does either of them believe that there are rules for inducing correct theories from facts. They take different views, however, of the ways in which scientific knowledge grows. According to Popper, knowledge grows through criticism; good research consists of making bold conjectures and then ruthlessly criticising them. Kuhn believes that this sort of activity occurs relatively rarely — only in those periods of scientific development which are called revolutions. In 'normal science', according to Kuhn, theories are not so much subjected to tests as used in order to solve puzzles; science, he says, usually operates within the framework of existing theory, which provides it with a powerful puzzle-solving tradition, an effective set of tools and techniques for doing research. Whereas Popper's science is always — or at least should always be — trying to overthrow tradition, Kuhn's is for most of the time exploiting its potentialities........ 'Norman science' is defined by Kuhn as 'research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.' Such achievements are called 'paradigms'. As example of actual scientific practice they are more than just theories; they include the body of accepted theory, together with its successful applications and the appropriate instrumentation.[18]

My own experience as a working scientist suggests that the contrast between normal and revolutionary science is not so clear-cut. The dramatic revolutions do occur, but in addition there is a succession of minor skirmishes, some of which are ignored and some of which lead to modification of , or a shift of emphasis in the current paradigm. Thus there is a constant change in the scientific scenery; the conceptional backdrop against which individual scientists do their work. In most fields of research it is not possible to fully discern this scenery by reading the literature, and more important, the literature does not reveal clearly the way in which the conceptual backdrop is changing. The direction of change and rate of change are often more important than the state of affairs at any particular moment in time and the only way to be fully familiar with the current conceptual and instrumental backdrop is to participate in its modification, that is to play an active part in the international scientific effort. In the words of Graham Jones:

> The results of fundamental research may be universally available but someone must be able to read the language.[19]

It is against this current scientific scenery that a scientist judges what problems he can profitably tackle. In the word of Medawar, science is "the art of the soluble," no scientist he says, "is admired for failing in the attempt to solve problems that are beyond his competence." Thus it is imperative for the university scientist to participate actively in the international scientific context, for if he is to give scientific leadership in his own country he must be fully aware of which problems are soluble in terms of current, and not obsolete, concepts and techniques. To quote Bernal:

> Much so-called applied science is applied obsolete science; the methods of application are even more obsolete than the science they apply.[20]

It is the responsibility of the university scientist to ensure, by appropriate involvement in the international scientific effort, that those involved in mission oriented research in laboratories in the country have the means of viewing the current scientific scenery so that they may solve without the aid of foreign experts today's problems in terms of current techniques and concepts, rather than arrive at obsolete answers to yesterday's questions.

Finally what type of research can be justified by appeal to the national social context. I have already mentioned that the possibility of the participation of University scientists in mission-oriented research projects financed by government departments or by industry is in its early stages of development in Nigeria. The further development of government supported research depends on the clear formulation of national goals, the translation of these into specific research needs and the provision of financial support for which university scientists can apply. The development of industrially supported projects is a complex problem related to the small size of most indigenous industrial companies and to the fact that the larger industries, which are part of international companies, use the central research and development divisions of these organizations rather than establish local laboratories. I should like to suggest in passing that, just as there is government control over the extent to which expatriate staff can be employed in industry, consideration should be given to the possibility of requiring industries larger than a specified size to finance research projects in the universities in lieu of establishing their own research laboratories.

Thus I am advocating an increase in the amount of mission-oriented research in the university. Since a large proportion of the able scientists in the country work in the universities there is a clear social benefit to be derived from this. However, in addition I maintain that of equal importance is the awareness to problems of immediate social significance that this type of research gives to university scientists. This awareness is one of the factors which can influence his choice of curiosity-oriented research projects that have potential long term strategic importance in the national context. Such curiosity-oriented research projects I call strategic research.

I consider four types of strategic research. First, an example of what I call strategic research based on technological extrapolation. Within the next few years a petrochemical industry will be established in the country on the basis of imported technology. The general chemical basis for this technology is heterogeneous catalysis, but the details of the processes involved are the result of vast industrial research and development programmes and frequently are closely guarded industrial secrets. The initial problems of establishing the industry will be the training of technological and managerial staff to run the plant. Nevertheless, one can envisage that a time will come when it will be considered desirable, necessary and realistic to establish an intensive Nigerian research and development effort into the processes of the petrochemical industry. If reliance on foreign experts and the inevitable, and usually unsatisfactory, crash programme is to be avoided, a start must be made now to establish a research group in this area within one of the universities that when required would be able to provide not only the leadership in establishing such a research and development programme, but also be able to train the necessary additional scientific personnel. In this type of programme there is a long lead time, for it takes at least ten years for a fresh graduate to become a mature chemist capable of giving strong leadership to a large research group. The pay-off from such a research group will not be this year or next but in ten years time, and in the meantime the research will more closely resemble curiosity rather than mission oriented research.

As an example of a different type I choose molecular biology. Many have argued that the relatively new discipline of molecular biology should not be given priority in a developing tropical country since most of the answers to many of the important biological problems can be sought in terms of classical biology. That may or may not be true but there is no doubt that developments over the past two or three years suggest that entirely new chemical and pharmaceutical industries may be based on microorganisms genetically engineered using the techniques developed in molecular biology. It is too early yet to be certain but it is late enough to discern that this is a horse worth backing, and early enough for Nigeria to ensure that its experts develop in parallel with those in economically developed countries rather than lagging behind as is common in so many industrially significant fields. I would call this an example of "watching brief" strategic research.

Thirdly, a type of research that I call instrumental expertese strategic research. As an example I take natural products chemistry. This field of research, in which new complex compounds are extracted from living material and chemically characterized, is commonly justified on the grounds of the potential pharmacological significance of the new compounds. I would suggest that of equal importance is the contribution which research activities in this field can make to the growth of technical expertese in a variety of instrumental techniques. The enormous progress in natural products chemistry over the past twenty years has depended primarily on the development and use by chemists of a wide variety of physical techniques such as mass spectrometry and infra-red and NMR spectroscopy. The usefulness of these techniques has proved so spectacular that they are now considered essential tools in many areas of chemistry and its applications. It is the active participation over the past twenty years in natural products chemistry using the latest techniques that has put the

Department of Chemistry in Ibadan in the position of being able to give specialist training in these various techniques to chemists from various laboratories around the country. It is the responsibility of the universities to anticipate the need for such instrumental expertese.

Lastly, a type I call "long odds" strategic research. By this I mean research in fields which for some reason or other have lain fallow for some years but which if developed might lead to radically new processes in industry. The chances of success in choosing such a field are of course small but the rewards correspondingly great. I leave my audience with the challenge of seeking fame and fortune by betting on such long odds and winning.

In conclusion then, our choice of research projects must be governed by the criteria imposed by the social contexts in which we work and it is clear that strategic curiosity oriented research is consistent with the demands of all three contexts. This, leavened with a measure of sponsored mission-oriented research is my prescription for the type of research the university should be doing. There will no doubt be those, particularly outside the university, who will disagree with me and demand more obvious and immediate contributions by the university to the technological and economic development of the country. Quite apart from the fact that this is easier said than done, it must be emphasized that while buildings can be constructed in months and equipment purchased in weeks the intangible qualities that are essential for first class university departments take many years to produce and nurture, need constant efforts to sustain, and at the same time are fragile and are easily damaged beyond rapid repair. To those critics who advocate dramatic and supposedly quick-acting reforms I say the following: it may be inappropriate for a goose to lay golden eggs of little apparent nutritional value, but it would be folly to kill the goose rather than attempt to gradually change its diet so that it produces a greater proportion of edible ones.

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