Papers and Originals

Administration and the Structure of Scientific Knowledge*

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British Medical Journal, 1969, 4, 517-522

It is probable that when future historians come to write the history of the first half of this century they will see it as that period in which scientific knowledge emerged from its adolescence and attained adult stature as a determining factor in human affairs. It is equally probable that when they come to the history of the second half they will, with the benefit of hindsight, judge our generation by the ability we have shown in adjusting our traditional views to the situation that confronted us.

I, personally, have no doubt that scientific development, with its executive arm, scientific research, is now at one of the turning points in its history. I have equally no doubt that sooner or later, and over the world as a whole, it will attain its fulfilment. Whether or not this will be sooner rather than later, whether it will come in one country rather than another, is, however, still unclear. We should, nevertheless, hardly be wasting our time at this present juncture if we tried to make an objective appraisal of the situation and to see the part that we, as scientists, will need to play in the future of scientific development.

If today we were to look back at the state of scientific research and its position in public estimation fifty years ago, we should see that, in general, it was still largely regarded as a scholarly pursuit, essentially personal in nature, but one that could occasionally, though somewhat unpredictably, yield results of great practical importance. Of course, this was not the universal view. Increasingly, farsighted men were coming to appreciate the trend of the times, and the experiences of the first world war had not been entirely without effect on the attitudes of national governments. In general, however, it required the experiences of the second world war for the public as a whole to become convinced that their own future, or even their very survival, was bound up with the development of scientific knowledge. For this we have largely to thank our colleagues in the physical sciences who, by producing the atomic bomb in the incredibly short space of four years, demonstrated unequivocally not only the power of their knowledge but also that, in some cases at least, such knowledge could be developed predictably and with purpose. After Hiroshima and Nagasaki there were many senses in which the world could never be quite the same again.

But it is not only in regard to nuclear physics that the revolution in public attitude has occurred. In other branches of the physical sciences, in biology, in agriculture, and in medicine there has been similar, if less shattering, evidence that scientific development is now becoming a decisive factor in human affairs. Even, therefore, without the inevitable tendency of unskilled opinion to extrapolate beyond the evidence, there are ample grounds for the new emphasis in public viewpoint. In consequence, in all developed countries support for scientific research has escalated, scientific development has become

a major matter of national concern, and expectation rather than hope has become the public measure of scientific achievement.

This social revolution has occurred in our time. It is difficult, however, to avoid the impression that both the scientific and the administrative communities have been somewhat disconcerted by the scale and speed of these changes and even more so by their implications. Naturally the increased support for scientific research has been welcomed by scientists, even if its adequacy has not gone entirely unquestioned. But there has also been a background of pervading uneasiness. Increased support from public funds inevitably entails increased accountability to public authority, and this has understandably aroused fears among scientists of strictures being imposed on scientific activities that, however justifiable in more routine spheres, would be destructive in that of creative work. Equally to the administrator, raised in the tradition that the role of the expert is advisory and the belief that there are always two sides to every question, the realization that scientific considerations can now override all others is similarly unsettling to his views on social organization.

It is no wonder, therefore, that in recent years both the scientific and the administrative communities have, from their different points of view, become deeply concerned with the problem of national organization for scientific development and its integration into the structure of government. It is this problem that I intend to consider in this lecture.

Material Realities

It is a cardinal principle of administration that for any human organization to be effective it must continue to satisfy both of two basic requirements (Bryce, 1968). The first of these is that it shall be in conformity with the material realities of that which it seeks to organize; the second that it shall be equally in conformity with the deeply held sentiments of those who have to make it work and thus productive of the necessary loyalty and initiative. Leaving aside for the moment the second of these requirements, let us look at the first and ask ourselves the question, "What is the material reality to which any effective organization for developing scientific knowledge must conform?"

Put this way there can surely be no doubt about the answer. It is to the way the scientific knowledge develops. Obviously if we have no valid appreciation of the structure of scientific knowledge and the way that it grows, we have no rational basis on which to construct any organization for its development; and, in default of this, our devices can be no more than expedients to cope with the changing pressures and distractions of the moment. It seems to me, therefore, that the first task that confronts the scientific community today is to examine closely the ideas they hold on the structure and development of their particular knowledge. Without this as a basis an effective start

* Foundation Lecture given to the Royal College of Pathologists on 12 November 1969.

cannot even be made on the organizational problems that now face our civilization in respect of its science-based activities.

To do this, however, is no light matter. As Francis Bacon himself said, at a previous turning-point in scientific history, "things in themselves new will yet be apprehended with reference to what is old" (Bacon, 1960). The views on the structure of scientific knowledge and its development to which we have been traditionally conditioned were formed in a far simpler age and under the influence of philosophies that have now lost much of their authority. It would be surprising if today, after three centuries of scientific progress, these views were still entirely adequate. Yet, by now, though we may have relinquished the philosophies from which they were derived, the attitudes of mind to which these gave rise may still be deeply ingrained in our modes of thought. It would seem prudent, therefore, if, in approaching this problem, we started not with ideas but with data.

Sequences of Knowledge

Let us start by looking at the way understanding has developed in the field with which we are most familiar, that of biomedicine, and consider, as a first example, how our knowledge of the existence and role of the vitamins has come about. As a particular instance, take the sequence of developments that started from the identification of the disease beriberi and stretched to the discovery of thiamine (aneurine) and its role in intracellular metabolism.

Clinical observation identified, and morbid anatomy further defined, the varieties of this disease. Epidemiological studies established that beriberi developed when polished rice but not unpolished rice was a staple of the diet. The way was thereby opened to experimental pathology. Diets in which the staple was polished rice were then shown to produce an analogous condition in animals, and extracts from the rice polishings to prevent or cure this. The biochemist and later the organic chemist could now enter the field. The active principle in rice polishings was isolated, its composition was determined, and ultimately it was synthesized. At all stages within this sequence research took place within a developing framework of knowledge by which the intellectual relevance and validity of each further step could be established. The revolutionary concept of deficiency diseases had become inescapable, and it was not long before the investigations were extended into the role of the accessory food factors, or vitamins, in intracellular metabolism.

As a second example consider the development of our knowledge of endocrinology. In practically every case this started with Nature's own experiments in producing disease by either the overfunctioning or the underfunctioning of particular endocrine glands. Identification of the clinical syndromes and their definition by morbid anatomy were the first steps in our understanding. Once this was done, and the gland concerned particularized, the way was open to the experimental pathologist. Ablation of particular organs produced states of hypofunction; organ extracts sooner or later produced states of hyperfunction. Biochemistry and organic chemistry could now come to grips with these problems. Hormones were isolated, and many have been synthesized. The way is now clear to the next development which, through intracellular metabolism, will carry us to the molecular level.

And as a third example consider the sequences of researches that now need to be mounted in a less developed field, that of cancer research.

Cancer research starts with observations on disease in sick persons and animals. On this basis its varieties are distinguished and by morbid anatomy characterized more precisely. In the course of clinical observation associations between the development of particular kinds of cancer and particular factors in the environment are glimpsed. By epidemiological tech-

niques these are either established or rejected. If established, the whole endeavour broadens and the experimental approach becomes possible. The production of cancer by chemicals, radiations, viruses, and so on is now a feasibility. Biochemistry, immunology, genetics, and virology are inevitably drawn in. Now we can profitably ask such questions as "What is the essential difference between the metabolism of malignant and normal tissue?" or, more important, "What is it that keeps a cell normal?" Chemicals and ionizing radiations produce changes in chromosomes; viruses are packets of nucleic acid. Intracellular control mechanisms come into the forefront of consideration—in short, we approach what is now known as molecular biology.

We could continue to multiply examples of such sequences in the biomedical field, and doubtless many such will readily occur to you. I believe, however, that I have said enough for my purpose. Looking at these sequences in general, it is evident that there is a logical connexion between the range of activities within each. Each forms a single continuum of ideas from the specialized, or mission-orientated, studies at the clinical extreme to studies in unspecialized, or basic, biology at the other. Within such sequences there is no break, either intellectual or practical. All the knowledges in each are necessary, all mutually sustain each other. All are in context.

Intellectual Unity

But now, having looked at the field so to speak under the low magnification, let us, like good pathologists, look at a segment of it under the higher. It cannot have escaped your attention that in the three sequences I chose biochemistry occupied a place in each. In one it was linked to nutritional, in another to endocrine, and in a third to malignant problems. But it was, nevertheless, an identifiable subject of knowledge, distinguishable from the clinical and pathological problems on the one hand and from the general body of chemistry on the other. We might well ask, therefore, what is it that gives biochemistry its title to separate identity?

Biochemistry has been defined as that subject in which biological phenomena are analysed in terms of chemistry. In this context the operative word is *analyse*. In other words, it is biology that poses the questions and chemistry that provides the intellectual and technical tools. Of course this is not to deny for one moment that in another context, that of the study of the properties of materials in their own right, chemistry is not an identifiable subject itself. My point, however, is this. To describe biochemistry as an applied subject is to misconceive the whole position. If biochemistry were to sever its links with biology, it would lose the essential source of its inspiration. It would cease to exist as a separate discipline and become merely the study of the chemistry of biological compounds—a very different thing. Clearly, biochemistry could not come into existence until biology had broken down its material to the stage at which it could make contact with the concepts of chemistry. Then, after the first gropings of what was called physiological chemistry, the new subject arose (Himsworth, 1951).

This is an evolutionary process that we find permeating the whole course of scientific development. Exactly similar considerations apply to the genesis of the subject of biophysics or that of molecular biology. It applies also at the more mission-orientated end of the biomedical sequences. Take those diseases that are the manifestations of some alteration in metabolism. What is the essential purpose of research here? Surely it is to analyse clinical phenomena in terms of biochemistry. In this case it is clinical medicine that provides the questions and biochemistry the tools. Here again if the link between the more specialized and the less specialized knowledge is broken the subject disappears.

It would seem, therefore, that if we take any individual scientific subject we shall find that this depends for its development not only on the elaboration of its own particular knowledge but also on the continued inflow of questions from contiguous subjects further towards the more mission-orientated end of the sequence on the one hand, and of intellectual and practical methodology from those towards the more unspecialized or basic end on the other. In considering how scientific knowledge develops it is therefore quite unrealistic to treat of individual subjects divorced from their context. We need to think in much larger units. Although he recoiled from following the evidence through into its implications for our traditional ideas on scientific knowledge, Herbert Spencer (1854) came very near to doing so when he wrote, over a hundred years ago: "A more general science as much owes its progress to the presentation of new problems by a more special science, as a more special science owes its progress to the solutions that the more general science is thus led to attempt."

But this is not the only inference to be drawn from the preceding considerations. Starting as they do at separate points and in different special problems, these sequences as they develop to progressively deeper levels of inquiry come to have increasingly more interests in common. Thus, as we have seen in the three examples considered, all tended together at the level of biochemistry. Had we been able to follow them farther we should have seen that they coalesced even more closely in molecular biology. It is possible, therefore, to conceive of biomedical knowledge as made up of a series of such converging sequences which, so to speak, collectively form a single province of similar natural knowledge. This is the basis of the concept that I wish to put before you. But before pursuing this matter further and considering its implications for our traditional assumptions on the structure of scientific knowledge, we must ask ourselves whether such sequences are a peculiarity of the biomedical province, or are they a feature of scientific knowledge in all its provinces?

Other Provinces

To consider the question adequately would require far more time than I have now at my disposal. Elsewhere I have attempted to do this (Himsworth, 1970). On this occasion I must, however, perforce content myself with merely indicating the way that knowledge has developed in other provinces and leave you to draw your conclusions.

In the province of our nearest neighbour, agriculture, I direct your attention to the sequence of inquiries that has led down from the problems of soil fertility to soil chemistry and soil microbiology, and to those that have led from the problems of plant and animal breeding to our common interest in genetics. In the province of materials, I would point to the sequences that have led from the extraction of ores and the refining of metals, those that have led from the production of dyestuffs, the elaboration of drugs, and the problems of biology to the vast mass of information that, on systemization, we call chemistry. In the province of energy I would indicate the sequences that started with the invention of gunpowder and cannons, and prompted the study of ballistics, force, and inertia; those that started with the "suction" pump and led via the steam engine to the elaboration of the subject of thermodynamics; those that started with the mariner's compass and carried us to our own knowledge of electromagnetism. And in the province of the earth sciences I would look at the sequences that started with our need for an annual calendar and developed into the study of astronomy, and those that arose from the problems of our own planet and led us to geophysics. These are but cursory indications. I hope, however, that they will suffice to answer, at least tentatively, the question I posed, to the effect that sequences such as I sought to distinguish in the

biomedical province are, indeed, features of scientific development in general. On the assumption that this is so, I propose to turn now to the problem of the structure of knowledge, the natural reality that must necessarily inform all our attempts at organizing for its future development.

Structure of Knowledge

When men think conceptually they necessarily think in terms of analogies or, as we in our present rectitude prefer to call them, models. Traditionally in regard to scientific knowledge the analogy or model we are conditioned to use is that of a tree. Like all analogies, that of the tree of knowledge derives from other and quite different fields of experience. In Western civilization it seems first to have been used explicitly in the book of Genesis in regard to moral knowledge. But it was equally consistent with the trend of classical and mediaeval philosophy. It is no wonder, therefore, that when in the sixteenth and early seventeenth centuries the accumulation of natural knowledge became such as to require systemization, men sought to fit their new experience into existing beliefs and saw in the analogy of the tree of knowledge a ready model for this purpose. Thus, in *Of the Advancement of Learning*, Bacon (1966) writes: "but because the distributions and partitions of knowledge are not like several lines that meet in one angle and so touch but in a point; but are like the branches of a tree that meet in a stem which hath a dimension and quantity of entireness and continuance before it comes to discontinue and break itself into arms and boughs"; and from this he goes on to avow his belief in one universal science which is the main and common way to understanding. So at the very outset of the modern scientific era its leading protagonist confirmed men in their previous concepts and left this as a legacy to future generations.

But, it may be asked, does this matter? After all, more than three and a half centuries of scientific progress stand between us and Bacon and his views. Surely we have been able in that period to make any necessary adjustments in our thinking. But attitudes of mind once enshrined in analogy die hard. We have only to recollect how prone we are to talk of "science" in the singular and to use terms like "branches of knowledge"; or to ask ourselves what is the basis for our belief in the unity of scientific knowledge or that the flow of scientific development is essentially from the trunk of "basic" knowledge to the branches of "applied." We have, as I said, only to think of these things to suspect that these early attitudes of mind may be more influential in determining our opinions than we, as scientists, would care to admit.

In my view, however, this traditional concept of the structure of scientific knowledge is entirely mistaken. Elsewhere I have stated the case at some length for opposing to it one that is entirely different (Himsworth, 1970). It is one that finds its analogy in the model of a vast globe of primitive ignorance from separate places on the surface of which penetrations are being driven in towards a common centre. It is these different penetrations of inquiry, which at the surface are engaged with mission-orientated and specialized problems and which, as they progress centrally, become increasingly unspecialized, that I have referred to previously as provinces of natural knowledge. Biomedicine is one such. Bioagriculture, materials, energy, sociology, and the earth sciences are others. I find myself in agreement, therefore, with Karl Pearson (1937) in the view that in science, as in logic, the unity lies alone in its methodology, not in its materials. One has only got to try to compare space research with cancer research, or astronomy with physiology, to see that, scientifically speaking, these are incommensurable. Yet, if the traditional view that science is in fact a unity were correct, we ought, on the basis of purely scientific considerations, to be able to do this. As I see it, therefore, scientific knowledge is not a unity but a confederation of knowledges; or, if you wish

to put it more epigrammatically, "there is no such thing as science, there are only provinces of scientific knowledge."

You may, of course, if you are so inclined, dismiss all this as philosophical speculation, interesting perhaps but of little importance to the practical affairs of life. But let us just look for a moment at some of the implications of these two different views of the structure of our knowledge and see, according to which we hold, where each would lead us in actual practice.

Organizations for Scientific Research

At this present time all developed countries have set up, or are in the process of setting up, central organizations for the support of scientific research. In some this has taken the form of a single organization to cover all kinds of such research. In others, as in this country, separate organizations have been created in regard to each of the major categories of natural phenomena—the biomedical, the bioagricultural, and so on. Which are we to advise is the correct solution? Clearly, if one believes that scientific knowledge is a unity it makes sense to bring all varieties of scientific research under one organization. Equally clearly, if there is no such unity then obviously the unitary scheme is mistaken and the alternative is the right one. In default, however, of a view by the scientific community as to which of the opposing concepts of scientific knowledge is valid, the choice between the alternative schemes of organization will fall to be decided by the pressure of irrelevant interests.

Again, if one holds to the unitary concept that finds its analogy in the tree of knowledge one will be predisposed to the opinion that there is a common or basic kind of knowledge which constitutes as it were a kind of sap, on the upflow of which the different branches of scientific knowledge depend for their development. One has only to look, however, at the history of how the different subjects of scientific knowledge—"basic" as well as "applied"—have developed to see that this is, at best, a half-truth. Yet if one adheres to this view it will effectively determine our distribution of the resources for supporting research and introduce a corresponding bias into our policy for its development. If, on the other hand, we believe that development within a particular province of knowledge depends on the interplay between specialized knowledge coming down progressively from its mission-orientated frontier and unspecialized knowledge infiltrating back towards this, then our policy, and the deployment of our resources, will be correspondingly different.

Clearly, according to which of the alternative concepts of knowledge prevails in a country, so will be the form of its scientific organization and consequently the prospects for the individual scientists within it. I make no apology, therefore, for my earlier statement that the essential preliminary to formulating any policy for scientific development, and ipso facto the organizations necessary to support this, is for the scientific community itself to reappraise the views it holds on the structure of its knowledge. Philosophies are none the less potent even if they remain unacknowledged.

But now, with these thoughts in mind, let us look at the instruments we have elaborated for the advancement of natural knowledge.

Instruments for Advancement of Knowledge

In the course of the evolution of our civilization four instruments have been developed successively for the organization and development of knowledge. The fact that this has occurred, or is in process of occurring, in all countries the world over provides a sound indication that these are the response to an unfolding natural situation and not merely expedients to cope with local and transient circumstances.

The first, in time, of these instruments was the specialized occupation or profession with, sooner or later, its particular association or college. The second was the university. The third was the specialized scientific society or academy. The primary orientation of the specialized occupation or profession is to practice; the primary orientation of the university is to higher education; and that of the specialized scientific society to the promotion of knowledge at the subject level. But all these are concerned in the development of knowledge and as such are instruments for scientific research.

To these, under the pressure of increasing knowledge, has now been added a fourth instrument, the central research organization, or research council. As, however, we are still so close to its evolution in point of time, it would repay us to stand back and assess the significance of this in the total provision for scientific development.

At their outset such organizations are usually conceived simply as a means of satisfying certain special needs which cannot be, or are not being, met by existing agencies. It is soon found, however, that such ad hoc requirements cannot always be met by the simple manipulation of existing knowledge, nor by reliance on the chance that the spontaneous interests of others will produce what is required. Such organizations are, in consequence, driven to employ their own staff and to subsidize others not only to solve specialized problems but also to conduct research in depth when this is lacking. Thus from a very early stage central research organizations find themselves identified with the whole span of sequences of knowledge, from the specialized to the unspecialized extremes. Further, not being restricted by institutional considerations, their operations come to be spread on a nation-wide basis.

The third stage comes with their appreciation that it is no more than enlightened self-interest to broaden their views and to support promising research in any subject relevant to their province and so ensure that over the country as a whole a comprehensive set of interests is in being. The fourth stage has emerged during the last twenty-five years. It has been the direct consequence of the increasing growth and certainty of scientific knowledge and the multiplication of evidence that scientific research can now increasingly proceed with reasonable assurance to meet its objectives. This is the stage that we have now reached; the stage when countries have realized that positive policy for the development of scientific knowledge is becoming increasingly feasible and that organization is required to this end. This is the role for which central research organizations are cast in the era into which we are now moving. Their identification with the whole span of particular provinces of knowledge provides the underlying natural reality on which to construct them. The existence of definite sequences of knowledge within such provinces provides the lines towards which purposive policy can be directed.

But I am far from suggesting that we have yet reached the stage when such sequences cover the whole of any province of scientific knowledge. Over much of the province they are only in the course of formation, fragments or only sporadic points of activity the wider relevances of which are not fully evident. We can therefore only venture to deploy our heavy support selectively if at the same time we insure for the future by keeping in being representative activity throughout the province as a whole.

Nor would I view with anything but apprehension a situation in which the whole of research in a particular province came under one organizational colossus. There are limits to human foresight, and no body of men, not even scientists, could have the temerity to set themselves up as the sole arbiters of development in any field of creative activity. Autonomous universities, autonomous professions, and autonomous scientific societies are the prerequisites for embarking on positive policy for scientific development. In deploying its resources, therefore, a central research organization must do so as need and opportunity arise, irrespective of the agent concerned.

Scientific Morale

We come now to the second of the basic requirements for the success of any human organization; that it shall be in conformity with the deeply held sentiments of those who have to make it work.

The difficulty of reconciling the requirements for creative work with the demands of practical affairs that are dependent on it has always been a problem for human societies. Now that scientific research has become a major factor in social organization, this problem has emerged in respect of it. On the one hand, there is the scientific investigator, intent on understanding the unknown, who sees at any attempt to channel his interest an infringement of the conditions essential for his work as improper as it is misconceived. On the other, there is the administrator or industrialist harassed by questions urgently demanding an answer and seeing scientists engaged in work that not surprisingly seems to him of less importance if not actually irrelevant. In principle this dilemma is inherent in the situation. It was therefore something of a landmark in the evolution of administrative thought when a workable solution was produced that reconciled these two potentially disruptive forces. The fact that it lies to the credit of this country to have pioneered in this matter accounts in no small measure for the respect in which our opinions on administrative matters are held elsewhere. For this we have largely to thank three men; the lawyer and philosopher Haldane of Cloan, the ex-professor of anatomy and medical dean Christopher Addison, and that brilliantly unorthodox civil servant Robert Morant.

The solution they evolved was that of the independent self-governing research council, composed predominantly of proved scientists, standing half-way between government or industry on the one hand, and research workers and their institutions on the other. Thereby they secured for government and public a continuing source of expert opinion which, because of its independence of other considerations, was demonstrably impartial, while at the same time securing for research workers a type of organization which, being under the control of men like themselves, could command their professional confidence and so enable not only scientific assistance but the creative initiative of the country to be focused.

Scientific Research and Society

It remains now to consider a final, but, in the eyes of the public, probably the most important, aspect of our problem. This can be put in the form of a question: "What considerations should govern the relationship of the research community to the society of which it is a part and to the government of that society in particular: and what consideration should determine the relationship of society and government to scientific research?" The stark basic elements in this problem seem to me to be three.

The purpose of scientific research is to increase our understanding and thereby our control of natural circumstance, and the moral justification for an individual engaging in research is that it is his intention to contribute to this end. The reason why society supports research, and accords special prestige to research workers, is that it believes that this is their intention and that they have now demonstrated that they can to an increasing extent realize the wishes that society, and its government, have in mind. The feasibility of attaining a particular objective and the deployment of resources for scientific advancement can only be a matter for professional scientific judgement, and in seeking the benefits of scientific knowledge it is to such judgement that society must trust.

These are the elements in the case, and to us as medical men they should present little difficulty, for in principle they are the same elements that occur in the essential professional relationships of medicine.

When a physician is called in it is because the patient has some need of which he wishes to be relieved. The obligation on the attendant physician in this situation is that, while sympathetic to the patient's wishes, he shall make an entirely objective assessment of the case and, according to his professional judgement, bring to bear the full resources of his knowledge and skill to realize the course of action he thinks is in the best interests of his patient. Were the physician to allow the wishes of his patient or the patient's relatives to override his professional judgement he would be failing in his duty, and were the patient or his relatives to override the physician's judgement they would only have themselves to thank if untoward results were to follow. Clearly, a physician can discharge his obligation to his patient only if he is in a position of independence in regard to the patient and his relatives and if they on their part accept that he should be in that position.

Scientific Community and Society

The relationship between the scientific community and society, now that scientific research has become effective, is essentially similar. It was the Director-General of the World Health Organisation, Dr. M. G. Candau, who said that the identification of a human need was no indication that effective research was possible. Today the development of scientific knowledge has become a major factor, sometimes a decisive factor, in national policy, and governments are becoming increasingly dependent on the knowledge and skill of experts. But our administrative tradition militates against our appreciating either that for expert judgement to be of value it must be genuinely independent or that the choice of experts is in itself a most expert matter. In this technologically based age which we have now entered, when national welfare or, in times of war, national survival may depend on the quality of the expert knowledge available, no country can afford to leave the selection of its experts, or the interpretation of their findings, to any save those who are themselves experts of the highest class (Snow, 1961; Himsworth, 1970). In the nature of things expert judgements that are politically important are concerned with the frontiers of scientific knowledge where possibilities have been opened but certainty not established. Judgement in these circumstances calls for the highest objectivity and intellectual discipline. Only if consideration can take place in an atmosphere insulated from the pressures of intention or previous commitment can the chances of aberration be minimized.

It was therefore an example of singularly far-sighted wisdom when, fifty years ago, Haldane of Cloan laid down the principle that scientific research must be independent of the organizations concerned with its results; and that, in consequence of this, the new central research organizations, or research councils, should be autonomous and established independently of the industry or the administrative departments of government concerned with their findings. He thus took the first step towards solving what has been said to be the most urgent administrative problem facing our civilization, that of integrating expert knowledge into government. It was a tribute to his prescience that nearly half a century later the Committee of Inquiry into the Organization of Civil Science, under the chairmanship of Sir Burke Trend, unequivocally endorsed this principle.

Conclusion

This then, as I see it, is the situation that faces us as we enter into the new scientific and technological age that lies before us.

Scientific knowledge has now shown beyond question its power and its relevance to human societies. We, under the

pressure of developing knowledge, have successively devised four complementary instruments for its development—the specialized occupations and professions, the universities, the scientific academies and societies, and now the central research organizations or research councils. Each of these is primarily orientated to a different aspect of natural need, though all participate in the advancement of the knowledge itself. The specialized occupations or professions are primarily orientated to practice, the universities to higher education, the specialized societies to intellectual development at the subject level, the central research organizations to the comprehensive development of the province of knowledge for which they are responsible. These are all expert functions, and it is only in so far as the organizations expressing these are independent and under the control of experts that they can fulfil their purpose in society. It remains now for us, as scientists, to develop and use these instruments; for it is only to the extent that we are prepared to assume responsibility for them that we can

hope to promote our knowledge or meet the expectations that we have aroused.

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Use of Antilymphocyte Globulin after Cadaveric Renal Transplantation

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British Medical Journal, 1969, 4, 522–525

Summary: Antilymphocyte globulin (A.L.G.) was prepared by injecting fresh frozen splenic cells subcutaneously into horses. The IgG fraction of the serum was concentrated by a batch technique using diethylaminoethanol-Sephadex. Fourteen patients given this material by intramuscular injection after cadaveric renal transplants, in addition to azathioprine and prednisone, had less evidence of rejection compared with patients previously treated with azathioprine and prednisone only, despite a reduction of the mean daily prednisone dose from 65 to 45 mg. Toxicity, especially local reaction, fever, and hypotension, limited the amount of A.L.G. that was given.

Introduction

Antilymphocyte globulin (A.L.G.) has been shown in many instances to possess immunosuppressive effects against cellular and, to a less extent, against humoral immunity (James, 1967). The most effective preparation described is that of Lance and Medawar (1968), who used mouse thymocytes as antigen to immunize rabbits; this A.L.G., used as the sole immunosuppressive agent, has allowed this survival of skin xenografts on mice. Other A.L.G. preparations have shown less dramatic immunosuppression, but in most cases have demonstrated prolongation of homograft survival.

Starzl *et al.* (1968), using horse antihuman A.L.G. prepared with viable human spleen cells as antigen, claimed immunosuppressive properties for this preparation in patients receiving renal transplants from live related donors and reported increased survival, with a decrease in the dose of prednisone required to maintain good transplant function.

A cadaveric renal transplant programme has been in operation in Auckland since May 1966. Seventy-four kidneys have been transplanted into 62 recipients. Antihuman A.L.G. was prepared in horses in a manner similar to that of Iwasaki *et al.* (1967). The main differences were the use, as antigen, of fresh frozen spleen cells rather than viable cells, and of diethylaminoethanol-Sephadex separation of the IgG fraction of serum instead of ammonium sulphate precipitation.

This preparation was given as part of their immunosuppressive therapy to 18 recipients of cadaveric renal transplants. The clinical details of these cases are compared and contrasted with those of 20 patients treated without A.L.G. during the previous 12 months.

Preparation of A.L.G.

Human spleens were obtained at operation or within 30 minutes of death, perfused with normal saline, and stored at -30°C . Cell suspensions were prepared by sieving splenic tissue just before being injected into the horse. Two horses received weekly injections of $1-2 \times 10^{10}$ splenic cells given subcutaneously into multiple sites for four weeks. A further 15 injections were given at intervals over the next six months. Leucoagglutinin titres increased to 1:1,280 and 1:2,560 in each horse within three months and remained in this range for the next four months.

Thymic material was obtained at open-heart surgery or at neonatal necropsy and stored at -30°C . until cell suspensions were prepared before injection. A third horse received 0.5×10^6 thymic cells intravenously weekly for four weeks and subcutaneously and intradermally on six occasions in the next two months. Leucoagglutinin titres reached 1:1,024.

Seven litres of blood was removed from each horse on several occasions after high levels of leucoagglutinins were observed. The serum was absorbed against pooled A, B, and O red cells and against group O inactivated serum.

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