PONTIFICIAE ACADEMIAE SCIENTIARVM SCRIPTA VARIA

49

SCIENCE AND THE MODERN WORLD

PART II

Proceedings of the Symposium
on « Science and the Modern World »
Plenary Session
11 - 13 October 1978

PONTIFICIA
ACADEMIA
SCIENTIARVM

EX AEDIBVS ACADEMICIS IN CIVITATE VATICANA

MCMLXXXIII

FOREWORD

The Pontifical Academy of Sciences presents in this booklet the discussion on "Science and the Modern World" which was held in its Plenary Session of October 1978. The impact of science on our society has such manifold aspects that it cannot be treated in a simple way. The subject has been discussed in many meetings and has been the object of study in numerous books by important authors. Our publication is simply an addition to the current bibliography on the subject, hut I think it will be useful. Though published belatedly it has the merit of bringing together the feelings of scientists working in different fields and belonging to different countries.

In reviewing the papers and discussions now published, I felt that they have not lost their timeliness. On the contrary, the central theme which was treated needs every day more reflection in order that the moral and spiritual values that upgrade human dignity will not be submerged by the technical advances resulting from the stupendous progress made in recent years in the fields of basic science. This submersion may occur as a consequence of economic and political pressures which may arise from national egotism or group interests. We must face and defy those pressures.

Humanity must, can and will survive in conditions where the quality of life makes living a lovable situation, only if it can override the abuses of power in a civilization where men and women see the destruction of tbeir human and physical environment by the inventions which the human mind has created. Thus any word, especially from some of those who have created modern science, is a blessing which can strengthen, and sometimes renew, the hope with which we all should and do envisage and admire the progress of science and of its practical uses.

It is interesting to note that in treating the different topics of the meeting every participant drew from his scientific life, his own cultural background and his human experience a word of constructive criticism and of encouragement, which shows that science can still be one of the tools for acquiring the knowledge necessary to improve the human condition and defend the dignity of men and women.

I am very thankful to all of my colleagues who so willingly and generously contributed to the meeting and gave to it the benefit of their talent and good will.

I wish especially to thank Professor Marini-Bettòlo for the exhaustive work he has undertaken, without which this booklet would not have been published, and my collaborators, Father di Rovasenda, Mrs. Porcelli, Mrs. Massa and Silvio Devoto for their constant help.

January 1983

CARLOS CHAGAS
President of the
Pontifical Academy of Sciences

I

SCIENTIFIC RESEARCH AND SCIENTIFIC POLICY

SCIENTIFIC RESEARCH PLANNING

G.B. MARINI-BETTÒLO
Pontifical Academician
University of Rome
Roma

To speak of planning in scientific research is to go right to the heart of a series of problems which challenge the world of Science today.

My words are intended as an introduction to a discussion which, in its aims should focus the problem, and therefore besides stating the actual terms of the problem, I shall have to propose a number of questions, which can stimulate a discussion and thus guide us to formulate our point of view.

The first observation I would make is that the planning of scientific research might seem a contradiction of terms inasmuch as scientific research, by definition, is the search for truth, the unhampered expression of human intelligence, of the capacity to observe. It is scientific curiosity, experimental ability, hard criticism of the results; it may lead from well known premises, through man's work to results which often are quite different from those reasonably expected.

Scientific research, whether in the theoretical or in the experimental field, is born spontaneously from a personal need of the researcher.

It may be said that Renaissance criticism, which with Descartes, Galileo and Copernicus led to the experimental method in Science — as opposed to the medieval "ipse dixit" that is the authority of Aristotelian texts, which summarized all the existing knowledge at the time they were written — created the figure of the modern scientist who discovers the laws of the universe, studies the phenomena of the biological world and creates new mathematical systems.

Yet Science was for almost two centuries cultivated as a side line by famous scholars whose chief aim was to teach, or by other scholars who carried out research along with their other main occupations.

Very seldom do we find before 1800 a scientist who devoted all his time to research. Among the first group I might mention Galileo, Linnaeus, Volta, and among the latter Lavoisier, who was the director of the Tax Office of Paris, Avogadro who was a lawyer, Lazare and Sadi Carnot both military men.

During the 19th century there appears the figure of the professor scientist; the University becomes the center for the diffusion of the most advanced scientific discoveries. Thus is born the great generation of scientists of the last century, who with their scientific discoveries are the direct promoters of the industrial revolution which took place in the western world at the close of the century.

The results of science, through technology, give origin to new industrial processes: thus new materials are found, and new work cycles and procedures. From the laboratory, data go rapidly through a long chain of processing to extraordinary results: from the observation of Faraday to the industrial electrolysis of salts, from the experiments of Ampère to the power motor, from the electromagnetic waves of Maxwell and Hertz to their use by Marconi in the field of telecommunications.

If we consider these examples, we can ask ourselves whether the achievements of Faraday, Maxwell, Ampère and Hertz, who revealed fundamental laws of Nature and discovered new properties of matter, were due to something other than scientific curiosity; and yet their discoveries have made possihle an extraordinary development in many applied fields; indeed they have significantly modified our society and our whole way of life.

We can find many other examples of this in the history of mankind — we need mention only the studies on the atom, or on the metabolism of lower fungi, or the synthesis of organic molecules, to show how scientific research has furnished the theoretical and experimental basis for the development of all modern technology. Moreover an evolution has taken place in the method of facing scientific problems. Science has become an extremely important thing and Bacon's claim that *Scientia est potentia*, Science is power, has become real.

The scientist is no longer a man who takes time out of his leisure hours to work in the laboratory or on charts; the researcher must dedicate himself continuously to his work. The industries created by the findings of scientists of the last century have now organized their own research centers to solve their problems, and everywhere large research laboratories are being created outside the Universities to study particular problems, such as the Institut Pasteur in Paris, the N.I.H. in the United States, the units of the Research Council in Great Britain, the laboratories of the C.N.R.S. in France, the I.V.I.C. in Venezuela, the many laboratories of the Academies of Sciences in eastern Europe.

And thus, while the 19th century has seen practically all of scientific research concentrated in the University, our century sees a proliferation of scientific research centers in the industries, in the state agencies and everywhere Science is needed.

The professor scientist in many cases is being replaced by the researcher, whose only function is to produce scientific results.

With this system the scientist can still be the enthusiastic investigator of nature who pursues truth or what he believes to be the truth, but he can also become a research employee, paid to produce data and results. The latter may be an honest scholar who widens many fields with his methodical work, but rarely will he become the innovator who makes Science advance. While it may be useful to carry on research in both fields, the danger of planning is that it can tilt the equilibrium in favour of bureaucratic researchers.

Another problem arises: the first scientists of the 1600-1700s usually required very modest means to carry on their research. The economic aspect of the apparatus and of the structures was negligible in comparison to the dedication and personal creativity.

In the nineteenth century the question became more complicated and in order to carry on advanced research it was necessary to resort to financing by government and private enterprises; this however does not create unsolvable problems in view of the still modest amounts involved.

Whoever has, like myself, lived about fifty years ago in laboratories remembers the tragic lack of funds in the Universities and the enormous expenditure of energy to overcome the difficulties of ordinary situations. Even industrial research laboratories, although oriented towards practical solutions and thus towards a quick return of investments, required financing that was still within the limits of the average budget.

Today the laboratories, in order to achieve a minimum of functionality, require significant means to meet expenses of sophisticated apparatus and equipment, tools and costly materials.

Also the mathematician who formerly could work with just pen and paper, today requires computers and well stacked libraries. No private individual today is in a position to provide researchers with these means. Therefore, one must appeal mainly to the State, which can distribute funds through its various channels.

Meanwhile Science in our time has become more and more not only an instrument of power but even an economic necessity. Ever since Science has promoted productive processes through the realization of new technologies, it has become an indispensable factor in the development of modern Society, not to mention the support it gives to the technologies of destruction: atomic weapons, electronic war, etc.

National support for Science today may be measured by figures representing the percentage of gross national product invested in research, which represent the indexes of development of a nation.

The funds invested in scientific research are, at present, both public and private and represent huge sums. Now the key to all our problems is here. Whoever makes the funds available naturally wants them to give some return, that is they want results, but not just *any result*, which may even be quite important, but *useful results*.

This implies that there are priorities in the distribution of funds and grants, depending on the field which one wants to study. This means, in other words, the *planning of scientific research*. It means developing and adapting research plans to a certain investment policy, based on definite criteria; it means necessarily that Science must be oriented towards certain fields rather than others.

The result is that the need of a considerable economic investment indirectly subjects the projects to the will of the financing agencies.

If we analyze this aspect more closely in its details, we see that for certain fields of Science the cost of research is rather moderate, while in other fields, such as for example that of high energies in physics, of nuclear physics and space research, huge financial resources are required such as can be provided only by a group of Nations, as we have in the case of the CERN in Geneva. A corrective to choice made by the financing agencies is given by the general directives of scientific policy, which in general establish a distribution of the funds between the various types of research.

In general, national research planning is divided into basic, applied and development research.

Basic research is that research which is carried on to extend human knowledge and in order to create new researchers. This research can be free when the researcher is given the widest choice of tbe field and subjects on which to operate, or it can be oriented when the field and the objectives of the research are established.

Applied research tends to the transfer of the results into practical applications: therefore it is limited to a particular objective. The *Development* research represents the last stage of applied research and mainly consists in the industrial applications of the results obtained in the previous stages.

As we can see, the structure of research planning lies within very broad lines, where many gaps occur that permit the researcher a certain initiative.

In the policy of research in order to establish priorities the following criteria are usually adopted:

1) the choice of the fields and areas of activity to be encouraged are established on a political basis and constitute one aspect of national planning;

2) the development of certain areas of research is dependent on the investments to be made in that very area (e.g. industry, transportation, energy);

3) it is desirable that each nation for its own progress utilize the results already achieved by the research carried on in the whole world. Naturally, for the scientific and technological transfer of these results it is necessary to develop a national research program to make this transfer valid;

4) the proportion of gross national product to be dedicated to research is conditioned by national politics and based on socio-economic evaluations; in particular the sacrifices to be accepted in the present in order to achieve a better future, standard of life, etc;

5) in certain fields of production and in social services it is necessary to promote research which produces also scientific and technical personnel who can then adapt to the conditions of their own country the results obtained elsewhere;

6) the promotion of basic research in every field of production in which the country intends to operate.

These principles lead to some general rules for scientific planning, which include the percentage of the national gross product to be assigned to research, how to subdivide this investment among the various programs and services and types of research, and finally, the determination of the public and private agencies to which to assign the responsibility for promoting and fulfilling the research itself.

In spite of the difficulties of correct planning and of the negative effects, in certain sectors at least of the planning itself, the present tendency is to subdivide expenses for research into categories, already mentioned, i.e. basic, applied and development research.

An analysis of the percentages of total expenses for research in these three fundamental areas can be of great help.

The ratio, in certain countries, between the expenses for basic research and the global expenses for research is usually higher for the less industrialized countries. Let us not be deceived by this figure; it is not that highly industrialized countries invest less in basic research, but in these countries the investments for development research are much higher, so that in proportion, but not in absolute figures, the investments in basic research are lower.

Today the distribution of the investments is considered an average when 10 to 18% is assigned to basic research, 30 to 40% to applied research and 40 to 60% to development research, where the lower figures for basic and applied research are those of the more industrialized countries whereas the lower figures for development research are those of the less industrialized countries.

On the basis of these criteria we may now draw some conclusions.

First, who decides the amount of money to be spent for research as well as its distribution among the three categories above mentioned? Since public expenditure is involved in most countries, this is decided by a political agency or a political body such as the Ministry of Science and Technology, usually based on the recognition of certain needs which have been ascertained by agencies now existing in all countries under various names (Research Councils, Academies, or special scientific institutions, e.g. N.I.H., N.S.F. etc.). The Chief executive, represented in most cases by the coordinator of scientific activities or by a Minister of Research, decides every year on the sums to be allotted and on the distribution criteria.

The proposal of the Executive is then submitted to the legislative body, which can discuss the proposals, if necessary make amendments and, finally, give its approval.

This system has acquired a certain importance in practically all industrialized countries and has been suggested to developing countries, especially with the creation of bureaucratic centers.

Now, although in the industrialized countries there exists a certain number of scientists who may collaborate on the selection of topics and the objective of national research, this does not always occur in developing countries, where decisions may be left to a limited oligarchy of scientists, or even worse, to the arbitrary decisions of functionaries who are influenced by models which are unattainable in the present conditions, or by committees of foreign experts who rarely have the capacity to understand the real needs of the host country and who wish to translate the plans of an industrialized nation into those of a developing country without making the necessary corrections.

The major problem encountered by developing countries is the lack of a large enough class of researchers, capable of offering to their country not only the scientific results of their investigations, but also an opinion and advice on many questions of scientific and industrial policy.

The transfer of technologies does not mean simply the transfer to developing countries of modern industries built up and often also operated by foreign experts; it means also the formation and training of the researchers and technicians and making them participate in the selection of the plant, in its design in accordance with local needs — which may be also ecological — in its construction and finally in its management.

For this it is indispensable in the developing countries to encourage basic and fundamental research even though tbe apparently more immediate solution of encouraging oriented and applied research may seem more attractive.

A discussion on tbis point sbould be opened among us with a view to clearing our opinion on this important problem.

In effect it is possible that basic Science, for political reasons, i.e. for lack of sufficient investments for all branches, may be confined to only certain fields, thus excluding some typical aspects of the so called "Big Science" such as high energies, electronic machines, space research etc.

It will also be necessary to establish what are the most valid criteria for promoting basic science in the developing countries.

The solutions may be many and more or less valid, but it is necessary that they be studied and thoroughly evaluated.

These may go anywhere from the education of the researchers abroad, who on their return can estahlish study centers, to the invitation of scientists from abroad to educate young researchers in their own country, or even a combination of the two systems.

Research oriented towards practical ends can facilitate financing; but basic research, which is apparently without any immediate return or benefit, should nonetheless be encouraged everywhere because its aim is the formation of the science worker and especially of a critical conscience and a creative capacity which can be put to good use by the country in many circumstances.

Thus one comes to distinguish a division between "Big Science" and "Small Science", which terms relate to the need for infrastructures adequate or suitable for the aims pursued. This division has a negative influence on the creativity of young people, "Big Science" requiring for its structure a very complex organization articulated in teams composed of many scientists. This results in limited freedom on the part of the younger workers, who find themselves obliged to work within the group with very little possibility for taking initiative or making decisions.

An example of this is seen in the vast program in physics using large machines and employing dozens of operators. One reaction to this group setup can also be appreciated in the tendency of young physics researchers to turn to theoretical study, where individualism can succeed in establishing itself on a personal basis. "Small Science", on the other hand, does not always succeed in obtaining adequate financing.

There is still another danger in modern research. The scientist, when he has available a machine or an instrument or any complicated tool, can become so dependent on this and orient all his activity towards the utilization of this machine. When for some reason the machine does not work — which often happens — the researcher will be paralysed in most of his activity. This, however, will not happen if the scientist has acquired so much knowledge that he can himself plan or construct a machine, which may even be better than the first. Also it is important to bear in mind the factor of innovation and originality in research: that is, not only to use the machine but also to improve it. Otherwise the role of the researcher would be only to submit a problem to the machine and wait for a solution.

Today practically everywhere, in order to tackle a particular problem, or only to do a research, it is necessary "a priori" to set up a program, to justify expenditure, to foresee or anticipate results and submit an application to a scientific agency in charge of the distribution of funds. The agency will seek the advice of one or more experts and will grant or refuse the financial support. Even if generally the program is studied so as to leave open every possible road, yet there is always planning involved, which, while it is necessary, yet limits the initiative of the true scientist and favors instead the secure, repetitive follow-up work of the professional researcher.

This is also complicated by the fact that it may happen that the evaluation of a program is not always correct, especially when the scientist programs something which seems feasible but which differs too much from conventional designs. In this case planification itself becomes at times an obstacle to innovation.

I recall, as an example, the failure of the application of the program of young Mössbauer presented to Euratom, not approved by this body, and then promptly carried out in North-American universities, which led to substantial innovation and to a remarkable scientific progress, distinguished by a Nobel award.

The problem remains: once the principle is accepted that scientific planning has to be carried out in each country, what direction and what percentage is to be given to basic science?

As I have already mentioned, everywhere in the world in order to obtain financing the individual researcher, or group, must present a program. At this point he himself has made a choice and set up a program on a personal basis; in the case of the group by agreement between its components.

Then comes the second phase: the committee granting the funds must decide which programs are to be carried out based on their feasibility and interest, as well as the capability of the scientist making the application. It is the duty of these Committees to evaluate not only the quality of the programs and their possible implications, but also the capacity and scientific background of the proponent or proponents to guarantee a maximum space for free initiative and for unbiased research. That is, these Committees must thus take the place of the generosity of the Maecenas of the Renaissance who like Federico Cesi provided funds for the Academies, where scientists could discuss and develop their ideas, or provide for individuals who wanted to devote themselves to Science the means to do so.

And yet as soon as the disinterested munificence on behalf of the scientist was replaced by "planning", difficulties arose. I would like to quote a passage from a letter — still unpublished — written by the great astronomer Father Angelo Secchi (President of the Academy of the Nuovi Lincei, forerunner of the Academia Pontificia Scientiarum) in 1857, to the Secretary of the Società Italiana delle Scienze, detta dei XL: "too much is demanded today, it is required that everyone make discoveries or instist that they be made. To achieve more, two other things are necessary, but no one wants to hear about these: *personnel and sophisticated machines*, that is to say a lot of money. But here 'haeret aqua' (here the water doesn't run anymore)".

The situation has not changed since then for the administrators and planners of Science all over the world, who are bound by the same necessities of limited budgets, political choices, and by the fact that, erroneously, basic Science is considered a luxury.

Today it is more and more necessary, if we want real "innovation" and progress, that the researcher should be free to proceed objectively in search of truth. No clue that might indicate something unforeseen should be overlooked in order to develop his critical sense towards originality and innovation, and even if the objective obtained is not what was planned in the program, it will have the true merit of having made progress on the scientific front.

These problems leave us with a number of uncertainties which give rise to many questions. As a conclusion to this presentation and to introduce the discussion, I would like to draw your attention to the following points:

Is the present model of research planning in the industrialized countries and the developing countries still valid today?

How can we overcome the structural difficulties regarding creativity and individuality of young researchers caused by "Big Science"?

How can we assure for the researcher the freedom of initiative and research within the lines of the planning, and how is it possible to stimulate his innovating capacity and his creativity?

Your contribution to this discussion will surely be of great help for the future policy of Science.

LES AVANTAGES ET LES DANGERS DE LA POLITIQUE SCIENTIFIQUE

M. LORA-TAMAYO
Académicien Pontifical
Consejo Superiore de Investigaciones Cientificas
Madrid

1. La politique scientifique qui a dû son origine aux plans de guerre et qui s'est imposée de ce fait aux hommes d'état des grandes puissances, s'est réaffirmée vers la fin de mil neuf cent soixante et un au cours du premier Conseil Ministériel de l'OCDE, célébré à Paris, pour discuter des politiques économiques offertes à la vaste communauté des pays membres. On y fixa comme but collectif une croissance du cinquante pour cent de la productivité nationale pour l'ensemble des vingt pays, qui, à cette fin, devraient adapter leurs politiques économiques et les coordonner au moyen de consultations et d'une coopération internationale.

Conformément à cet accord, deux points fondamentaux devaient être considérés: *a*) Situer dans le cadre de la politique générale de chaque pays l'estimation des ressources de la science et promouvoir l'innovation technologique de manière à atteindre les objectifs nationaux et *b*) utiliser, de la façon la plus efficace, les ressources scientifiques des pays membres, grâce à une coopération scientifique entre nations.

Ces aspects définissent l'idée de politique scientifique comme un élément de la politique générale qui se situe au point où convergent les trois principaux champs d'action de tout gouvernement: l'éducation, l'économie et les relations extérieures.

2. La politique scientifique présente une dimension différente d'un pays à l'autre. On dit qu'elle est essentielle à la scène nationale dès le moment où les ressources destinées à la science dépassent le seuil de l'un pour cent du produit national brut. De toutes manières, il faut établir une différence entre ce que l'on entend par politique *pour* la science et ce qui constitue une politique *par* la science.

La première, indispensable dans des pays modestes qui n'atteignent pas ce niveau, poursuit le but de procurer aux scientifiques, indépendamment de leur sujet de travail, les moyens nécessaires pour le mener à bien. Elle donnerait lieu, dans l'immédiat, à une recherche de prestige et serait, à plus long terme, la base d'une politique *par* la science. Celle-ci se propose de faire de la science un instrument pour le développement ou la défense du pays. Ce fut celle des grandes puissances pendant la guerre et c'est celle qui s'est poursuivie, avec plus ou moins de rigueur, postérieurement.

3. Il a coulé beaucoup d'encre sur la philosophie des politiques scientifiques et, notre réunion de ces jours-ci peut être plus efficace, si au motif qui la réunit: « Les avantages et les dangers des politiques de la Science » nous apportons les fruits de notre expérience sur trois plans: *l'enseignement supérieur, la recherche, la coopération internationale.*

4. *L'enseignement supérieur* doit s'attacher à obtenir en qualité et en quantité, les moyens humains que le projet scientifique exige; c'est pourquoi son programme doit être établi en rapport avec les perspectives offertes par le développement que le pays peut avoir, qu'il s'agisse d'une politique *par* ou *pour* la science.

Obliger à une relative précision dans ces prospections, est un avantage indubitable des politiques scientifiques. C'est de celles-ci que dérivent l'installation adéquate et l'équipement des Centres, la valorisation de l'augmentation et de la qualité du professorat et les moyens matériels et moraux pour qu'ils accomplissent dignement leur fonction.

Si les organismes auxquels on confie l'étude sont solvables et dignes de respect, les gouvernements doivent prendre l'engagement d'en prendre soin.

Pour être efficace, ce programme ne doit pas tenir compte seulement de ce qui pourrait être projeté vers l'économie, mais il doit laisser dans le nombre des diplômés prévisibles, la marge convenable pour la science elle-même et pour la libre compétence professionnelle.

Il existe deux dangers certains lorsque l'on fixe la politique de l'enseignement supérieur. L'un d'eux est constitué par le décalage existant entre le développement prévu et le nombre de diplômés qu'il requiert, surtout si, pour d'impondérables raisons politiques, économiques ou sociales, il n'atteint pas le niveau prévu. Il s'ensuit alors un excès de gradués, privés d'occupation, qui provoque, là où cela arrive, une cause grave de chômage et de frustrations. La valeur d'un programme éducatif doit tendre par lui-même à ce que cela ne se produise pas, en attirant l'attention sur le risque d'un manque d'adaptation entre la politique éducative et de recherche d'une part, et le développement technologique et industriel d'autre part.

Un second danger qui conduirait au même résultat de surproduction de personnel est celui qui peut être causé par un désaccord sur des critères doctrinaux à propos de la fixation de limitations dans l'admission des étudiants. Au critère de l'Université pour tous, s'oppose celui d'un prudent « numerus clausus » qui sélectionne.

Du choix de l'un ou l'autre dépendra, en outre, le fruit de la programmation.

La politique d'enseignement supérieur, qui se définit comme prioritaire dans les politiques de la science peut être, en effet, avantageuse, dans la mesure où elle engage l'Administration par delà les discontinuités politiques. Bien méditée dans ses orientations et servie par des équipes enseignantes qui en même temps l'inspirent avec savoir et expérience, elle est rentable par la qualité et le contrôle des diplômes qu'elle forme.

5. *La recherche* est le point-clé d'une politique de la Science. Son encouragement, sa programmation, le contrôle des résultats revêtent une importance singulière.

Les Instituts universitaires cultivent fondamentalement la recherche pure. Il est évident que prétendre soumettre à des plans, la création de science vers laquelle cette recherche tend, serait une erreur. La programmation générale de la politique ne peut faire plus que d'apporter les moyens matériels et humains pour qu'elle se développe sans aucune médiatisation. Nous nous trouvons ainsi devant un aspect de la politique *pour* la science qui donne du prestige à un pays.

Même aux Instituts de Recherche extra-académiques, qui cultivent des secteurs d'application technologique, comme l'Institut de Fer ou des Métaux, les Instituts du Charbon, de la Graisse, des Plastiques, et coetera, qui sont soumis à une planification en vue d'objectifs marqués dans une politique par la science, il convient de concéder une marge de liberté en recherche fondamentale de libre initiative, même sans utilité immédiatement prévisible.

Qu'il me soit permis de rappeler à ce propos, une anecdote biographique de Ziegler, du Kohlenforschung Institut de Mülheim, que plusieurs d'entre vous connaissent sans doute. Prix Nobel de Chimie, c'est à lui que l'on doit la grande projection industrielle de la Chimie macromoléculaire, grâce à la découverte de l'importance des catalyseurs organo-métalliques mixtes dans la polymérisation à pression normale de l'éthylène, du propylène et d'autres oléfines. Mais c'était un chimiste organicien pur, dont les recherches initiales étaient dirigées vers les composés organolithiques; lorsqu'il fut présenté par le Kohlenforschung Institut, il établit carrément sa liberté de recherche. Il le relate ainsi: « J'ai stipulé, comme condition de mon transfert à Mülheim, que je devrais y jouir d'une entière liberté d'action dans le champ de la chimie des composés du carbone, qu'il y ait ou non une relation directe avec les recherches sur le charbon ».

« Etant donné mon intérêt pour les composés organo-métalliques, dit-il encore, si j'avais suivi les travaux de « pain et beurre » de la chimie du charbon à laquelle la majorité de mes collègues attribuait la raison de mon engagement, j'aurais rompu le fil invisible que j'avais dans les mains et qui a pu conduire à des résultats d'une telle importance, précisément aussi pour le bassin de la Ruhr ».

Quelle que puisse être une programmation dans le cadre de la politique par la science, il serait dangereux de dédaigner « a priori » de raisonnables hypothèses de travail qui ne sembleraient pas susceptibles d'avoir du succès.

6. Une politique *par* la science est celle qui est impliquée dans les plans de développement d'une politique générale. Il s'agit de mettre la recherche au service de l'action.

Cela exige une infrastructure dans laquelle s'intègrent harmonieusement à côté des scientifiques, des représentations de techniciens, d'économistes et même de politiciens. Il est facile et simple d'établir de grandes étiquettes: Energie solaire, Satellites, Biologie moléculaire, Santé publique, Microélectronique...; mais cela ne l'est pas autant quand il s'agit de préciser les objectifs d'intérêt national. A cause d'un déséquilibre entre ces représentations qui sont, selon leurs origines, très inégalement intéressés, il arrive souvent que lorsque les équipes de travail sollicitent auprès de ces organismes de conseil une concrétisation d'objectifs, ou bien on ne leur répond pas ou bien on le fait d'une manière peu réfléchie.

Dans le cadre de cette coopération nécessaire, il faut insister — pour éviter un plus grand mal — sur le fait que la politique scientifique ne peut pas être un élément de la politique économique ni cependant en être envisagé séparément; au contraire un dialogue ouvert est nécessaire entre les chercheurs scientifiques et les autres éléments représentatifs suffisamment informés, pour qu'il ne s'agisse pas d'un dialogue de sourds qui ne mène à rien de positif, ce qui discréditerait les premiers et provoquerait l'indifférence des politiciens.

Si l'on fixe rationnellement des objectifs clairs avec une marge relative d'action, les ressources humaines et matérielles qui constituent le potentiel scientifique du pays, pourront se développer au milieu de la compréhension claire de leur nécessité, et de nouvelles connaissances, de nouveaux produits et de nouveaux matériaux seront le fruit d'un effort commun. C'est seulement de cette manière qu'une politique scientifique *par* la science est positive.

Une fois que les programmateurs d'une politique scientifique et ceux qui sont chargés de la mener à bien sont arrivés à une compréhension réciproque, les avantages sont grands pour une connaissance mutuelle et une mutuelle estime. Il existe dans bien des pays, surtout là où la recherche est modeste, une sous-estimation du chercheur de la part des équipes techniques et de gestion des industries et, réciproquement, une indifférence du chercheur envers le travail de ces derniers, et enfin les uns et les autres se méfient des politiciens interventionnistes. Il est certain qu'il y a bien souvent un excès d'audace et de légèreté dans les opinions de ceux-ci quand ils arrivent, par ignorance, à prétendre soumettre les scientifiques aux objectifs politiques, mais, de toute façon, il s'agit là de différends qu'il faut vaincre pour la meilleure réussite d'une politique scientifique.

7. Il faut faire mention à part du cas où l'on recherche la collaboration de l'industrie pour le développement d'un programme scientifique, ainsi que le cas où c'est l'industrie qui s'adresse à l'Administration pour concerter avec elle un projet de recherche. Le premier cas est fréquent dans des situations d'urgence ou presque toujours au moment du passage vers une opération de développement et là, le rapprochement entre ceux qui font la recherche et ceux qui l'appliquent est une condition « sine qua non » pour atteindre un objectif.

La tactique qui accepte des plans concertés de recherches dans l'industrie elle-même, que ce soit communautairement par des Associations de recherche ou bien à titre individuel, est, en principe, un système avantageux d'élargir la base de la coopération dans l'aire nationale grâce à sa répercussion favorable sur l'économie, mais elle offre des risques vérifiés. Ceux-ci peuvent provenir d'un système de contrôle déficient au cours de la poursuite du travail et pour l'appréciation des conclusions qui peuvent ne pas offrir les conditions de compétence et d'honnêteté requises. C'est ici que l'infrastructure doit servir à une juste application des ressources qui sont attribuées, en évitant l'habitude picaresque de fausser les rapports périodiques ou de masquer les résultats. Il n'est pas rare qu'il ne surgisse des intérêts obscurs qui nuisent au critère ouvert de ce système de subvention contractuelle, très avantageuse dans sa conception.

8. La politique scientifique d'un pays tire bénéfice dans tous les cas de la *coopération internationale*, mais celle-ci offre des caractères très différents suivant les possibilités du pays. Dans les pays d'économie réduite, les relations internationales sont effectives au niveau des Universités, des Instituts, des chercheurs eux-mêmes et de leur présence dans des Congrès et des Symposiums, ainsi que par l'échange de publications surtout quand il y a affinité thématique, mais elles requièrent une aide pour des bourses et des déplacements non seulement de la part des budgets nationaux (en égard à leurs politiques scientifiques) mais aussi des institutions d'autres pays qui puissent la fournir, en permettant un séjour dans leurs Centres de Recherches, en régime, soit de concession gratuite, soit d'échange entre institutions. Toujours à ce même niveau de moindres possibilités, les organisations internationales à vocation économique, par exemple la C.E.E., favoriseraient considérablement les politiques de chaque pays

au moyen de programmes de recherches technologiques dans des branches specialisées de l'industrie, ou en patronnant des recherches sur la santé et l'alimentation (comme le font discrètement l'O.M.S. et la F.A.O.) qui inclueraient des fournitures de matériel et d'équipement expérimentaux.

Les avantages de ces différents cas sont indubitables, si, en définissant les priorités d'une politique, on conclut des accords en toute responsabilité.

9. Il y a un ordre plus particulier de coopération qui est rendu nécessaire pour des branches géographiques et climatiques: les recherches en océanographie, en hydrologie, en géophysique entre autres, se mènent ohligatoirement en régime de collaboration internationale. Elles doivent figurer comme des aspects de la politique scientifique du pays, dans les limites où ses possibilités le permettent, mais leurs avantages de rang mondial, par ce caractère justement, n'atteindraient un juste rendement que par le canal d'organisations comme l'UNESCO, qui appuieraient ces recherches qui concernent le milieu humain.

10. Il reste à considérer un autre aspect important, dans l'ordre de ce que l'on entend comme macroscience on mégascience. Une politique internationale à ce sujet ne peut pas s'établir sur des politiques nationales déficientes. La coopération internationale ne profite qu'aux nations, qui, pratiquant une politique responsable de développement par la science, veulent surmonter l'obstacle d'une insuffisance de ressources à l'égard de la dimension qu'elle exige et qui s'unissent à des fin spécifiques dans le cadre d'une même vision politique globale. Ici, en tout cas s'impose toujours à niveau pragmatique comparable, un plus grand désintéressement des plus puissants en faveurs de ceux qui n'atteignent pas leurs possibilités et un effort maximum dans les pays intéressés, parce qu'ils disposent d'équipes humaines qui peuvent suivre l'évolution scientifique du secteur et qui, sans arriver à une participation directe, soient préparés pour une éventuelle collaboration. Les succès remportés dans des cas comme la recherche spatiale, donnent créance au système.

THE IMPORTANCE AND NEEDS OF CANADIAN RESEARCH IN SCIENCE

G. HERZBERG
Pontifical Academician
National Research Council of Canada
Ottawa

We Canadians sometimes try to belittle our own accomplishments. A few years ago in a report on "Research Policy in the Universities of Canada" the statement was made "Canada will never be able to identify many great researchers". The same sort of statement presumably could also be made about art, literature and music. Such statements are obviously not a good starting point for encouraging the intellectual and cultural growth of this country. It seems clear to me that the number of intellectually outstanding people in a nation or country is a nearly "constant" fraction of the population. Obviously Canada, with one-tenth of the population of the United States, cannot produce as many outstanding people as our neighbour to the south.

There are countries in which, for ideological reasons or because of a peculiar educational system, excellence of a few is not encouraged and as a result such countries will fail to take advantage of the "constant" fraction of outstanding people. They engage in the de-emphasis of excellence at their own peril. They will not be remembered a hundred or a thousand years from now for their contributions to human heritage and even at the present time they will not profit by scientific discoveries in their technological development.

Canada has had in the past, and I believe has now, a considerable number of outstanding research scientists. The whole development of nuclear physics started when Rutherford spent 8 years at McGill University. He received the Nobel Prize (in Chemistry) one year after leaving Canada for Manchester, England, for the work that he had done in Canada; his extraordinary pioneering contribution can therefore be clearly claimed by Canada. Rutherford did not believe in the possibility of practical applications of nuclear energy (he considered those envisaging this possibility as "talking moonshine"). Nevertheless the practical use of nuclear energy was a consequence of Rutherford's work. The development of the heavy water reactor by the Chalk River Laboratories under W.B. Lewis is another example of outstanding research produced in Canada. The heavy water reactor is considered by many experts in Canada, the United States and elsewhere as the best solution of the nuclear power problem.

There are many other unique contributions of Canada's physical scientists to the world pool of knowledge which readily come to mind even without studying the history of Canadian science in detail. I shall not mention any specific names but should like to refer to the early recognition of the structure of our galaxy, which was accomplished in Canada, and to more recent Canadian work on intermolecular forces, on various aspects of laser phenomena and on energy storage, all of which have attracted worldwide attention. Even outside the physical sciences, where I have obviously only very indirect information, one immediately thinks of the discovery of insulin, of the development of rust-resistant wheat, of the discovery of sex chromatin, and of the development of the cobalt treatment of cancer. Thus it is evident that Canada, when given the opportunity, is quite capable of making a significant contribution to world science.

I believe that almost all scientists, including most of those working on applied problems, are well aware of the need for basic research in order to maintain the flow of new ideas and discoveries for the development of new innovations in technology. Even politicians have come around to acknowledging the need for basic research. There are some exceptions. For example, Mr. Drury, the former Minister of State for Science and Technology, has been quoted as saying that we can leave to other nations the advancement of knowledge and simply use their results. The fallacy of this method I think is obvious to all scientists. Because of the complexity of modern science only those who are themselves creatively involved in research can fully appreciate the nature of the advances made elsewhere and the possibilities of their applications.

In this connection it is perhaps appropriate to point to the development of science and technology in Japan. Since the Japanese early in this century did not have a proper base in basic and even applied research they simply imitated the western models in various industries. They soon found that they could achieve a far higher level of their technology if they also developed basic research. To-day the Japanese have arrived at a level in both basic research and technology which is close to that of the most advanced countries at a time when the latter are cutting hack. Even to-day the Japanese government is funding new institutes in basic research and there is no question that they will reap the benefits in rich measure.

Quite apart from the economic need to support basic science in Canada there is also the need to support science as an intellectual and creative effort of the highest order. Surely as the second or third richest country in the world Canada can not abdicate, in financially difficult times, its obligation towards pursuing the high aims of mankind to try to find what is the nature of man and of the world in which we live, even if this activity would not gain us material rewards. (In fact of course it always does).

The past ten years have seen, especially in Canada, an endless number of reports on science policy. Some people, like Senator Lamontagne, do advocate support for basic science but they qualify their support by the demand that the main effort in basic research should be in fields that are relevant to possible applications. The historical fact is that in many instances even the discoverers of some new phenomenon were unable to foresee the practical consequences of their discoveries: it is not possible to make a reliable prediction of the "relevance" of a given basic research project.

Senator Lamontagne and people with similar views seem to have in the back of their minds the idea that science, including basic science, can be centrally organized and that a "coherent science policy" can be established. They do not realize that science does not work that way. Science, especially basic science, flourishes under conditions where there is a minimum of organization. The great discoveries of the past can, almost without exception, be attributed to the genius of individual scientists who were able to pursue their ideas in an atmosphere free from the restraints of excessive organization. A certain amount of organization is, of course, necessary at the operating level (as Steacie said "There must be light, water and power, people must be paid, there must be technicians and workshops, the better people must have assistants, the less experienced people must have some guidance, and so on") but the attempt to plan and organize science at the administrative level will lead at the best to a mediocre routine science deprived of the inspiration that leads to the outstanding discoveries.

At this point I must quote again, as I have on a number of previous occasions, a remark by Michael Polanyi which illuminates the situation.

> "Any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science. ... You can kill or mutilate the advance of science, you cannot shape it. For it can advance only by essentially unpredictable steps, pursuing problems of its own, and the practical benefits of these advances will be incidental and hence doubly unpredictable".

In this day and age we talk a lot about teamwork: in certain fields teamwork is certainly very important but nevertheless the real advance is made by some bright idea in the mind of one individual. Faraday when he discovered electromagnetic induction (the basis for all electric power production to-day) was working alone, so was Roentgen when he discovered X-rays and Einstein when he developed relativity theory and discovered the equivalence of mass and energy. None of these scientists was motivated by practical problems, by the wish to improve the standard of living or to help the survival of mankind. They were motivated by the thought (as expressed so beautifully by the famous mathematician Jacobi more than a hundred years ago) that "the sole aim of science is the glory of the human spirit".

The only workable concept of science policy that I have been able to find is the one given in the First Report of the British Council for Scientific Policy:

> "Science policy does not direct the advance of scientific knowledge, though it may well be concerned to encourage or to direct the application of the results of scientific advances. The tasks of science policy are of another kind: to maintain the environment necessary for scientific discovery; to ensure the provision of a sufficient share of the total national resources; to ensure that there is balance between fields and that others are not avoidably neglected; to provide opportunities for inter-fertilization between fields, and between the scientific programmes of nations".

In the 1950's and early 60's we had in Canada, and especially at NRC, the "environment necessary for scientific discovery". But then the government of the time appointed the late Mr. Glassco, an accountant, as the Chairman of a Royal Commission on Government Organization. The Glassco Commission was not interested in ensuring that the environment necessary for scientific discovery was maintained. Rather it was interested in good accounting. I said in my Convocation Address at York University in 1969 "The Glassco Commission considered the National Research Council in the same way as the Post Office or the Justice Department". In quoting this part of my address in his book "The Chaining of Prometheus" Ronald Hayes remarks that I "might have added that the application of Glassco precepts to the Post Office has also been baneful". This is even more true to-day than at the time when Hayes was writing his book.

It is interesting to note that while Mr. Glassco and his Royal Commission were appointed by the Diefenbaker government their report was completed under Mr. Pearson and implemented at an accelerated pace during the Trudeau administration, suggesting that accountants stand much higher than scientists on the totem pole of politicians no matter what party they belong to.

In one of his speeches Dr. Steacie aptly described the method of Glassco and his successors by saying "An efficient organization is one in which the accounting department knows the exact cost of every useless administrative procedure which they themselves have initiated". It was I believe at the instigation of Glassco that the Treasury Board introduced PPB (Program Planning and Budgeting) to the Public Service and applied it indiscriminately to all agencies even though its strongest advocates had questioned its application to the management of scientific activities. As faith in its usefulness has decreased, PPB has been followed by a number of other management techniques all distinguished by acronymic designations and all equally inept as applied to research. The trouble is that in estimating the advantages of these procedures no account is taken of the time wasted by scientists in filling in the endless forms and in producing what can only be unreliable forecasts. In their preoccupation with organizational details and management procedures the politicians and the bureaucrats quite overlook that, for a scientist, the only thing that counts is the quality of his research. They are consistently led astray by what Bertrand Russell has called the "administrator's fallacy", that is, the error of mistaking means for ends. The implementation of management procedures becomes more important to them than the completion of outstanding research or the production of innovative techniques. This attitude is nicely illustrated by the recent reorganization of the grants system. We have now a nice pyramid of Councils which must be a joy to every bureaucrat but whether it will work as well as the old system is a big, big question. Even if it does the added bureaucracy will swallow a sizeable fraction of the available funds.

The needs for science in Canada and basic research in particular are not reorganization but simply support by adequate funds with as few strings attached to them as possible. No scientific advance has ever been made by reorganization. We should stop the production of more reports on science in Canada — reports that are quite costly. We should rather spend the money used for such reports on the direct support of research.

The most important need in our support of science, and especially basic science, is to single out research workers of high purpose and ability and to give them funds to do the research that *they* consider as most promising without all sorts of bureaucratic rules. In Canada and elsewhere our politicians seem to be so concerned about the possibility that one in a hundred scientists might abuse the freedom that the Canadian scientist had in the 1960's, without noting that in all human activities there are people who do not pull their weight. In the peer system of selection of grant recipients the best possible guarantee for a minimum of failures is given. Everyone, including the politicians, is agreed that the top people should get what they need for their research. The real problem is to judge the people a little below the top who will eventually replace the top people. We should not be too stingy in the support of the very good but not yet excellent workers.

Support of this kind is needed for the future of Canadian science and of Canadian technology, indeed for the future of Canada.

DISCUSSION

RICH

I think the problems discussed here are of central importance. It is odd that the scientist understands the issue and also the extent to which science is molding the future, and it is remarkable to what extent that understanding is not shared by the general public and particularly by the politicians. There are many strategies that one could use in this regard. What is appropriate for one country is not necessarily appropriate for another. Very often the question of scale is important. In the United States we have been lucky up to now. The budget of the country as a whole is very large, but the budget for science in general was rather small and politicians have not looked at it. So it grew rather well. Unfortunately that point has changed: suddenly the science budget has gained visibility and now it is being subjected to a great deal of analysis in the sense of Dr. Herzberg's excellent paper much to its detriment. We have a few positive features: we actually have an agency whose charter assigns to it as its prime responsibility the furtherance of fundamental research in the country; that is, the charter of the National Science Foundation; which is now some 25 years old. Actually it laid down at the doorstep of this agency the responsibility for fundamental research in the country as a whole. This is very important because in a sense it represents our shield, it is an agency that has this responsibility. It now has a budget approaching a billion dollars a year and most of that money is well spent. One of the strengths in our system, and one that I think could well be copied elsewhere, is that the support of science is often decentralized; that is, we do not have a "Science Department". Our science funding is pluralistic: it means that scientists can get money from many different agencies in the government as well as from private sources. Now that is of great importance in certain disciplines and of lesser importance in others. For a discipline that needs, say, an enormous machine costing many millions of dollars, it is not very relevant, but for science — so-called small science — involving modest expenditures, it becomes quite critical because there one can apply to a number of different agencies.

Thus, I think that multicentric funding is quite useful. It provides a way of balancing the kind of biases and prejudices that one often sees in administrators of different agencies. I think we have to address the central problem and I am afraid there is no easy solution. In a way the problem is in part due to a failure on the part of ourselves to have educated the public in general and the politicians more effectively. The number of politicians in the United States who really understand the role of basic research and the phase lag between scientific discoveries and their implementation is rather small. If we try to patiently explain to them that there is often a 10- or 20-year phase lag between a discovery and the way that it may modify either the development of a new industry (and therefore tax revenues for the government, or the development of new medical treatments, or the modification of health costs, etc.) we may be able to describe this phase lag, but since in our government very few officials are elected for terms of ten or twenty years, it often does not have practical consequences; so one has to take a different route. I think in the long term what one needs to have is an educated electorate that sees the development in science as something positive. And one of the features here which is, sad to say, of growing importance is an anti-science attitude, in which the negative aspects of technological progress are laid entirely at the door of science, the positive aspects are completely ignored and the key role that science must play, let us say, in rectifying the negative effects of technological advance is not understood at all.

WEISSKOPF

I would like, first of all, to congratulate Mr. Herzberg, who in so strong and impressive words described the difficulties which we face in so many nations, namely I would say the conquest of science by bureaucracy. Therefore, although I agree with a lot of what Dr. Rich has said before, I am a little worried in spite of the fact that our system in the United States varied and you get different support. The National Science Foundation is a very good institution; I still feel very strongly, and so many colleagues do too, that the amount of paper work which one has to carry out these days for really unnecessary accounting purposes (and it was so good that Dr. Herzberg emphasized the accounting nature of the enemy) is growing to a tremendous extent, which not only prevents the development of science but also takes money away because one needs so many employees simply to write and to work out the different allotments and every single cent has to be justified; the freedom of experimentation is reduced. Of course one must agree that things could be worse, they always could be worse, and science is still flourishing to a good extent in the United States and elsewhere.

Indeed, to change my subject slightly, I would like to come back to a question that our first speaker has mentioned and also Dr. Rich was talking about: the emphasis on application. Surely one has to educate the public and the Government and Congress, for example, to understand that it takes a long time before basic science is applied. But I think there is more to it — it may be applied at all and still be terrifically worthwhile; and actually I must say I am pleased to see that in spite of all these arguments, astronomy for example is well supported. Astronomy of the researches into the nature of the clusters of galaxies, as we heard from Professor Dirac, the expansion of the universe, quasars, etc., that are almost guaranteed non-practical applications, are still supported, and indeed I take some hope for mankind that they do it. The same is probably true to a lesser extent for particle physics.

Herzberg

I would like to report here how this problem is envisaged in China. When I went to China, I said to my Chinese colleagues: "*I do not quite understand why you want to build accelerators*. The philosophy in China, I have heard, is always towards the practical, science for the people; and particle physics, may have an application, but probably not, and it will take a long time". Then they said: "we are glad you asked; we have thought about it and we have three reasons — as usual in China — three reasons for basic science. Namely, 1) that it is a tenet of Marx and Engels that science will always have applications at some time; 2) that such science as particle physics is training the engineers, mathematicians and physicists to some extent in the challenges which they have to face for training and for learning; and 3) that every civilization which is true to itself must be interested in the fundamental structure of matter, the universe and life, and that is why we support particle physics, astronomy and biology".

Now I have myself often used the first two reasons to convince our government and European governments to spend money on basic science, but the third reason is usually not very impressive, and the strange thing is that I do not even believe that I think there is a very deep awareness, in spite of the fact that it is not expressed, in the people as well as in the government, that basic science has a value by itself. One never has enough of it, that is true, but I do believe that it is there, and I remind you of another quotation (about particle physics again, since I have worked in this field so much). When Robert Wilson, the Director of the laboratory in Chicago, was asking for support before Congress, a Congressman asked him: "*Will particle physics ever be useful as a weapon for the defense of America?*" and he said "*No, I am convinced it never will be, but it will make the country more worth defending*". I think he has here a good point — of course it goes for all mankind — and the point of the dignity of man that is involved in doing basic science must never be forgotten. I think people could understand it, and we as scientists have a responsibility to point this out, a responsibility, by the way, which we do not fulfill very well because most of the modern scientists have no contact with the general population and we must learn how to explain the greatness of science to the public.

Now let me just make one last remark, and that is the problem that Marini-Bettòlo has mentioned, about the developing countries. I really see here a very great problem and I do not know how one can solve it. Surely Japan has introduced basic science on a large scale and done extremely well, and certainly has had tremendous advantage from it — an advantage which we cannot judge yet today for the future. But if one goes to smaller countries, it is for me always a problem whether it makes sense to educate the people in sciences of a very fundamental nature, very far from application, because of the brain drain. Many of them do not return because they have no way of continuing the research at home; they stay in the country where they have learned it and that is very bad for their own country. So the question of the brain drain of big science in developing countries is always a problem for me, and it would be very interesting to hear more comments on that topic.

Siddiqui

At the last Plenary Session I presented a paper bearing the title "Imperatives of Research and Development in the Developing Countries". Many of the points to which reference has been made in two presentations today were in one way or another dealt with in that paper. Now I am most happy that in both papers the greatest stress has been laid on the vital importance of basic research in the developing countries as well. That is something which is absolutely necessary for creating the atmosphere in which research of any kind can take root and prosper. Reference has been made also to the excessive planning — planning of science of all categories — and this categorization in itself can be only a broad categorization, because basic and applied research cannot be so completely kept in two different compartments. It is a sort of ping-pong game between basic and applied research, one doing the problems for the other; and this is something which has definitely got to be realized.

I might for instance give one example if you permit me. It happened after the presentation of my paper at the last meeting of the Academy. A well known plant *Peganum harmala* was studied since 1840 and the two main alkaloids isolated from it, namely *harmine and harmaline*, have been the subject of chemical studies over a long period, even today in many countries. The procedure was: powdering the seeds — they are very tiny seeds — extracting them with alcohol or acidulated water, and then separating the bases and studying their structure, etc. We also worked on *Peganum harmala* alkaloids and used the same procedure, but in a somewhat basic aspect of research in this field, namely the germination metabolites of the seeds? In the course of this study we found that the whole of the alkaloid is located in the husk of the seed and that the kernel contains 20% of oil which is edible in character as against 17% which we get in Pakistan from cotton seeds. Now, *Peganum harmala* grows wild in arid areas, not only in Pakistan but also in Iran, in central Asia and in Morocco. This would be an asset for the production of an edible oil, insofar as the large-scale plantation of *Peganum harmala* would not compete with the cash crops. The lag between findings of basic research and their applications, to which reference has been made, is 20 years — in some cases it may be less, in others longer — but what developing countries mostly suffer from is this organization of applications of scientific research in technological directions.

DÖBEREINER

I think that we owe Prof. Weisskopf some comments on the situation of applied and basic science in developing countries and this perhaps should be explored a little more. Perhaps one point which is to a certain extent independent of government policies of support and non-support is the following question: which are the subjects where the people working in developing countries can do really relevant basic research? Since everything is more difficult in most cases, even if the funds are available, if we start a large program on molecular biology, when we are one step further, in developed countries they are probably already ten steps ahead, so we never will catch up with these people. So perhaps one could specify better that there should be basic research but on problems which can be solved only in developing countries. Most of these countries are in tropical regions, and perhaps in some way preference should be given in financing or supporting types of research or experiments on research lines — ecology studies or others — which are linked to an environment where tropical conditions predominate and which cannot be solved by the large majority of scientist who are living in developed countries.

UBBELOHDE

I want to echo remarks made by a great number of the speakers, but instead of merely being an echo I want to pick out a few vital academic points — I mean points which belong to our Academy. Now the first one that strikes me is that we must not appear to be wooden headed about opposing government accountancy where large sums of government money are spent. This just does not make sense. If the sums are very large, they are in competition with national resources in other directions, and therefore we must rationally accept some forms of accountancy control. That is the first statement. The second, of quite the same importance, is that we are determined to protect free scientific curiosity acting in an effective way on a broad field of undisciplined — or disciplined if you like — but not planned science. So you want to combine two apparently antithetical situations, both of which are important. One is if you have very large expenditure of national resources, you must accept forms of accountancy control. On the other hand, if you want to protect — as well all want to protect — free scientific curiosity in the most unplanned directions, we must also protect that. How do we do it?

Well, I think there are two doctrines that have already been mentioned which I am going to emphasize. One is and I agree with Dr. Rich — that it is essential to encourage plurality of support. The moment you have unitary support, monolithic support, you have lost the battle, because in this case the national requirement of reasonable administration of resources is bound to be predominant. But if, as some countries have achieved, you have plurality of resources, you can so arrange things that your free curiosity research — and note I do not say basic, I am an engineer, so I use the word "unplanned" — is not planned beforehand, but equally of course that large expenditures are controlled by planning. Therefore I want to see more support by academicians of national movements to have more money given to free bodies. In Britain I do not think that Dr. Brück has been very lucky because in fact we have had quite a few small but very effective sources of curiosity supporting financial bodies, and their budgets, though not enormous, are very, very useful because they are completely flexible. I have been associated myself with administering one or two of them, but we also have of course enormous areas, as he said, of planned research. I myself have been a member of a Research Council for a number of years and I know the problem always present in any form of planned research is that you are not only planning the expenditure of resources but you are planning the life cycle of human beings.

Now this is a factor which has not been mentioned yet and which perhaps we shall mention even more tomorrow because if you have a large team of trained scientists working in planned research, their directives are measured by two-year, five year, ten-year success and then changed, stopped, blocked, start something else because this is our plan, whereas the curiosity scientists are free to pursue a line of investigation which very often, as we all know, moves in new directions as the very result of the work being done, it is free to evolve and go in new ways. This we also want to protect. How do we do this? Well, the answer is that in encouraging more funds to be channeled into uncovenanted, free research, by taxation privileges in particular, we must remember that the tenure problem of young scientists should not be too easy in free establishments, because it is a matter of experience that young scientists do not all remain at the same stage of prolific productivity for curiosity research. This is a very important question in the middle kind of establishment — what happens to the 40-year-old scientist if he is expected to do original research? He is excellent at trained work but he may not be very good any longer at really original research. Some of the people remain green until 80 of course, but it is not common; and therefore if you are thinking of natural resources, whereas you can perfectly plan the life structure, the career structure of your scientists and your engineers in planned establishments, you have to watch out that your non-planned or university or other charitable bodies do not hold on to young folks too long.

This is absolutely vital. It is quite easy because you can arrange that quite a lot of the charity money is tenable for five years, or eight years, or whatever, but not indefinitely. This is one of the pragmatic factors which differentiate curiosity research from planned research far more than the fundamental difference. I have been associated with a lot of planned research, as well as curiosity research, and I have always found that if the director of an establishment of planned research cuts off all curiosity research from his flock, it is deadening, and so those of us who have to advise from outside on the budgets of planned establishments usually make a rule that a simple figure, about 15% of the budget may be at the director's complete volition and evolution regardless of the general plan, but of course using the general facilities. This means that the young men, even within an establishment of fully planned research, do not lose contact with their outside fellows. Otherwise, if you make the distinction so great, you get the most harmful segregation between the planned people, who are machine serfs and so forth, and the free people who go to seed at the age of 38...

GARNHAM

I would like just to comment very briefly on three points: The first one is the true vocational research, which is irrelevant to external influences and is based on the personal curiosity which Professor Ubbelohde has just referred to. A single person is concerned in such research, although he may be helped by a few colleagues or pupils. The home of this research is essentially the university; and combined with the research there is nearly always an element of teaching. The second type of research as I see it is the sponsored or government-directed research, very often initiated as the result of public demand like cancer research, and that of course is based on expert teams, and the home of that research is in fact the big research institutes, in which there is no teaching combined with the function of the individuals — and moreover it is more directed research than the previous one. And thirdly is the applied or field research, and as a person who worked for many years in tropical Africa and saw immediate important pressing problems around, I was forced to undertake applied research. I could never see any real distinction between the basic and applied as so many people have said. It seems to me the essential criterion of either field or applied research and basic research is that there must be no certainty of the result. It is a venture into the unknown, and the home of that type of research is what I might call the field. I just want to end by quoting a little saying of a great research worker of the past, Charles Nicolle: « La recherche est un jeu, mais chaque jeu a ses règles. Bien sûr on doit suivre les règles ». The other great worker who lived at the same time more or less as Nicolle was Ramón y Cajal, who also gave tremendously good and useful advice to any research worker or person concerned in this subject.

SELA

I understand that at this stage we are discussing mainly the interference of finances or budget, and I as a scientist am certainly as aware as anybody of the budgetary difficulties. It is true that academic freedom does not necessarily mean budgetary freedom. I think sometimes there is a confusion in the sense that every scientist is free to do what he wants, but this does not mean that he is free to get whatever budgets he wants. Nevertheless, the point I wanted to make here is that it would be oversimplification to assume that the problems we are discussing today are really mainly budgetary problems. I think sometimes beyond the discussion I have heard, that if we could only explain things to the politicians, things might change; but I am afraid that most of the politicians are not what some of us would want them to be: namely, leaders, who are leading the population. I think that very often politicians are what they should be in a democratic society: namely, they represent, or I should say they follow the feeling of the population. I think that the budgetary difficulties may be part of the plight of science today. I think of the anti-intellectualist spree which swept the western world, starting from the wealthy west in Berkeley and going through America, making it stronger after its move, reaching Europe. But certainly this cannot be explained all in terms of finances, of budgets. I think that this is just another very important component.

A third component is something which was just mentioned by Dr. Ubbelohde, i.e., not giving tenure easily, and I could not agree more with this. Nevertheless, I have heen faced by this problem a lot in recent years and I want to say explicitly what I am referring to. I helieve that giving tenure very quickly is bad, because the only thing that science cannot make compromises with is the striving for excellence. I am not saying excellence, but the striving for excellence. On the other hand, it is also forbidden to say we are a free society. We can explain to a Ph.D. student coming to us that he does not have a chance to work after he finishes because we are not one of those heavily structured societies, like the communist society, where you are told when to study, what to study, where to study, and then when to work and where to work. So we are a free society, it is beautiful, everybody is free. Well, I do not believe in it any more. I believe that when we have our students and our younger colleagues and not only because they are our friends and our colleagues and we are close to them, we have a definite duty to see that once they are in the world of science they have satisfactory work. And if they are not as excellent as we would want in our universities or institutes, we must make sure that we find for them somewhere else work of satisfaction because otherwise, to put it in simple language, we are plugging the pipes, and we must not be surprised if the next generation — the scientific student generation — does not go into science.

Now the way I see it, there is a very clear difference between what I would call the political West, the political East and China. The political West is still tops in science and technology — no comparison with the other two — but I am afraid that it is going slowly down. Behind the iron curtain still I think that to be a scientist or to try to be a scientist is one of the most prestigious vocations and purposes, and no matter from what lower level they are, from this moment they are going up. And when we hear the recent announcements and pronouncements from China, it is clear that if they say that the scientist does not have to study ideology because he has to have his head free for science, they will be very easily capable of moving; they may be at the lowest point now but they will go way up. So I see a certain point which should be of concern to the western world, namely — and somebody has already mentioned this, it is one of those problems which cannot be solved in one electoral period, in any parliament or senate. Therefore politicians are not so keen on it. But on the other hand, if in 15 or 20 years there will be fewer good scientists and technologists (fewer usually means not only a smaller pool but the quality of the pool and the average becomes decreased), then you cannot reverse this in one shot. I mean it is not a situation where if you are lacking truck drivers you train textile workers to become truck drivers in six months. If the pool for the scientific leadership in the next generation will not be large enough and of a high enough quality, then we will feel it, not today but in a certain time, and I think one should think about this.

So, in summarizing I want to say: many of the problems, including the problems of the various types of science and not only budgetary problems, are rather psychological problems to a certain extent — i.e. it is what people think, how people feel that has to be changed. And maybe to refer to just one other point, there are many, many definitions of research: pure or basic, or fundamental, or applied. That is a problem of semantics. I personally do not believe there can be really relevant basic research because semantically the two terms clash, even though I am sure that what you are referring to, Professor Döbereiner, is research which may be, many, many steps removed from an application but which is still pointed in this direction. I find that science, besides a clear expression, is only good and bad science, nothing else, and in almost every single field (and there may be exceptions in mathematics and physics) it is only a question of how many times you ask the question "why?" because in some cases you ask it once or twice and you have already the applied answer; and in other cases you ask it five times, but by the time you ask it for the fifth time of a scientist, "then why do you plan or do you wish to do this", you always end up with something which is of interest and might be applied. Certainly, in anything that has to do with bio-sciences, I see nothing that would be completely irrelevant.

BALTIMORE

Sela and Döbereiner, have raised a different level of question about the organization of science. One of those questions is the fact that in most industrialized countries that have had a science establishment for many years we are seeing a movement into a steady state of available positions in science, such that the training programs are no longer looking forward to expanding possibilities for scientific activities unless they change their nature unless, for instance, in biology there becomes a much larger pool of relevant positions in industry for which we can train people. In fact there are not the same kinds of positions available in universities in the numbers that there were before. So the question really is: what do you do in the steady state; where do you find outlets for people in a scientific style which is based so heavily on students and on training? Actually the largest part of the production of science is really run by students and post-doctoral fellows, and they then have to go on to a career some place that is meaningful. That is one question.

The second question is related to the issue of science in under-developed countries or in developing countries. You made the suggestion that in developing countries it is more appropriate to do a kind of science which is more based on the natural life of the country and use it as an example of the kinds of bacteria that you have discovered or for instance Dr. Pavan's discussion of *Rhinosiara*. What bothers me about that is the idea that you are leaving out the ability to use the newly generated technologies for instance in molecular biology. If it is in fact possible for molecular biology to play a role in the solution of problems like the problem of parasites in tropical countries where we have to understand the immune system and we have to find ways around the immune system in order to try to develop the body's defenses against these; there modern biology can play a role. The question is: will there be people available with an immediate sense of the problem? Unless a program develops, for instance, in Brazil, to train indigenous people in Brazil, this will not be possible. I would make the suggestion, that the countries of Latin America should be making an effort to get together to develop an institute of molecular biology that can have a critical mass in order to bring to Latin America a large pool of people trained in the most modern areas of biology, conversant with everything that is going on, so that the attacks that can be made on indigenous problems can take advantage of the whole range of possibilities instead of being limited to whatever happens to be indigenous. So those are two quite separate questions.

DÖBEREINER

I think you are right, and perhaps I did not take a very good example in talking ahout molecular biology, because this is a tool and not a purpose. So if you maintain methodology as a tool, we definitely will need this. I agree with you. I rather proposed these ideas because I think the research prohlems should be directed in terms of problems which are somehow linked to the environment or somehow linked to the country, but it will remain a difficult problem, because you need to have enough people to have a critical mass. So I agree with you that we will need molecular biology but need it as a tool, not as a purpose of research.

SELA

Your question concerning the steady status is of course a very difficult one, but I would say first of all it depends on what kind of a steady state are we talking about? If you talk about a situation in twenty years, I would not be afraid of the steady state which was the result of the fantastic expansion of fifteen years ago, because by then there will be so many people retiring that even in a steady state you will have lots of new positions and if things go the way they are now you will worry how to find the people to replace them. So the great problem is not steady state, the great problem is the timing of the steady state. What do you do between now and fifteen years from now? You already suggested part of the solution, namely, there is a certain place and it could be very useful, for the good of industry and agriculture, etc., if there would be more Ph D's in the industrial and agricultural R & D. Now I do not know in numbers whether this would fill all the needs. I would go one step further and say that there would be ultimately no harm in our society if a small percent (one or two percent) of high school teachers in biology or chemistry would actually have an experience in Ph D before it. So I feel that there are lots of places for people that have had some scientific training to do useful constructive work for society. The great problem is how to change the idea which gives a black spot to any such young scientist tbat would leave a basic research career to go into industry or into teaching in a non-academic institution. Now I am not at all sure that what I am saying is really the correct answer — other approaches may be more nohle — but what I want to say is simply that I think the academic atmosphere, the studies and the teaching of students, must continue, and you must have the right outlet in a way, maybe in some kind of rotation of people who can spend some years here in research and some doing either teaching or industrial work. But I think that there is still room of expansion; we are not at the limit of where such acquired competence might still be useful.

UBBELOHDE

I just want to make a statement of fact, which is that we have experimented in England with both PhD's for schoolmasters with academic sponsors of their work in part-time or time off, and with industrial PhD's or PhD's with people with research establishments, research institutes but nevertheless with sponsors in the universities. They do not work badly — in either case one has to be a rather dedicated young man or young woman to be able to stand the strain of this double harness — but the difficulty always is this tremendous phase of disinterested pursuit of science, which is experienced at its best when one is young. I used to find that my engineering recruits always were very much more sensitive to relevance. They had to be sometimes needled into becoming disinterested, whereas the chemical physicists could really pursue science, being young and enthusiastic, in a disinterested way. And it is that aspect of innovative science that I fear most, of the things you have suggested, which we have tried and are trying have limits. That is the reason, you see. You cannot become disinterested unless you are young and protected nowadays. It is getting harder and harder to achieve this — not to achieve being young, but to achieve protection.

WEISSKOPF

Just one more word on the problems, the really grave problems, that Baltimore has raised. Regarding the first one, I think the great difficulty is the transition from one steady state to another. I would not say steady state but from the growing state to a steady state. The real difficulty in my mind is the following: that whatever you do, the average age of the scientist is going to grow until we reach the stage where they die. And the trouble then is: how will science survive those twenty years, because the great success of past science was that in spite of the fact there were old people there, there was always a larger number of young people. That is the essence of scientific programming. I think that in the next twenty years science is bound to be less productive than it was.

The second remark I would like to make is: I would really second very strongly Baltimore's call for international institutions in those regions where basic research is less developed. CERN was a wonderful example; in that field Europe was indeed an underdeveloped country. Now the idea of having international or regional laboratories in basic science, I think, has never really been done, except in Europe. And I wonder whether Dr. Döbereiner did not refer to this: the possibility of having an international molecular biology or a South American molecular biology laboratory or a South American laboratory in solid state physics or in any other field. I do believe that this would be a tremendous thing and I do not see this actually going on. The same is true of course in other parts of the world; and it may be that even this Academy, which is after all the only international academy, could really help in producing those regional international laboratories.

CHAGAS

The protection of basic science, of curiosity, of innovative science, is a preoccupation which was both in Ubbelohde's and in our mind and has been highly expressed here. Coming to the fact of academic freedom, I had the opportunity some time ago to be at a meeting where it was very easy to speak of academic freedom in the field of basic or pure research; but it became difficult — one had to explain what really academic freedom means — when the field was applied research. And my idea is that this corresponds really to the fact that one needs to keep the path of communication, the freedom of communication, the freedom of discussion and the freedom of criticism alive, and this is what I would consider to be academic freedom in the field of basic or applied science, and this is not a frequent case.

On the other hand, I agree completely with the dangers of centralization which were here expressed and brought up by Dr. Rich. In speaking of that, I would say that for developing countries and even for undeveloped ones, one of the myths which should be very seriously considered is the establishment of ministries of science. In developing countries it is quite clear that the experience of establishing ministries of science has has been a failure up to now.

Answering the question of international institutes in America, I come back to a very broad concept I have. It applies very well to Brazil, and I suppose also to other countries. The fact is that science is not advanced because we have still the colonial structure in the country, and the higher economic level strata of the population are still living in what I call the attitude of the temporary colonizer — which means that the colonizer comes to Latin America, gets money as fast as possible and goes back to Europe. Now I think that one important problem, which was brought up here by Professor Weisskopf, is the question of brain drain. In the many discussions about brain drain, I never saw brought up the most important thing which is the fundamental reason for brain drain. That is, first of all, that brain drain is a national responsibility and not an international responsibility, in the sense that if you give working conditions and if there is political stability in a country, the brain drain lowers down to a minimum. This minimum we cannot avoid, because it corresponds to the freedom of communication and of the ability to move around the world. I say, from my own experience, during my tenure at the institute I am working for, we have had only two cases of brain drain.

But the main question can be expressed in these words: why basic science in developing countries? Well, I think there are many reasons, and I was a bit disturbed by what Johanna Döbereiner said yesterday — and I think that she has now put it in a very precise way — I think that when we see science pervading culture, as it is now, if we do not prepare our younger scientists in the methods of science, including modern methods of science, we are in great danger of having a single universal culture and we are going to lose what corresponds to the precise character of a nation. This pervasion is very important. We have to consider that basic science in our country is done not to compete with basic science in other countries, but to form a basic culture. I think that the example given by Baltimore is a very good one: many of the problems of tropical science can be solved only by what he called modern biology. I see, for instance, that much progress in our knowledge of Chagas' disease has come at the present time from people working on cellular and molecular biology — that means modern biology — so that it corresponds to what was expressed here by Johanna Döbereiner. If we do not undertake basic science in developing countries we will have a second-rate culture very soon. I do not think that for many Latin American countries this problem is so serious financially as some governments would say. The expenditure for military weapons and airplanes and so forth, is so great that if we had at least a small part of it devoted to the development of science we could reach a much better state of scientific development, what we see, for instance, in Latin America, just some isolated islands of good research here and there making a sort of archipelago.

Now there is another point which I think is very important when we consider basic science in developing countries, and that is the fact that someone who has undertaken a research in basic science and has to apply it should do it himself, or with his group, or directing a group, and not leave it to others who have not done the work from the beginning. I think this is an interesting point. I would also like to point out that what hampers science in our countries is, and I will repeat it, the overgrowth of administration in science, and I think that Dr. Herzberg's paper applies very much to many countries, and particularly to my country. An example is that we had a very efficient Research Council and then we developed it so much that the great part of its budget now is expended on administration. I think that this point regarding basic science in the developing countries, which was defended by Marini-Bettòlo and by many of our colleagues, is so important that it should be stressed. That is why I insist on it, and it is the only way to protect us against a very bad form of colonialism, scientific and technological colonialism, which is really a part of the economic colonialism from which developing countries are suffering. On the other hand, one should not expect that our scientific development should come only from the generosity of the missionary spirit of some scientists of the developed countries who can afford to give lectures or who will devote some of their time to our problems. What I am saying is that we should really undertake a task which is *our* task: the task of developing science, and the development of science in an underdeveloped country begins with the teaching of science in secondary schools. This teaching is done via basic science and the most advanced and modern techniques. This does not mean that we should put aside many problems of applied science, or even in many instances the use of the old type of technology, which may be important.

To conclude, I would like to say that one point that was not explored enough here is the fact that for developing countries what we need in many fields for the application of already existing knowledge to the solution of their problems, is what should be called operational research, meaning the way by which knowledge acquired can be most efficiently used. I am speaking mostly of the fields of public bealth and sanitation.

In these fields we have a tendency to neglect the old knowledge which exists in some countries and which would be very interesting to use. It is in this sense I think that some of the experience which has been gained in China — not in science but in the human aspects of applying science in a modest way, in a way which can also be beautiful, to quote from a very recent publication — is something which should be taken into consideration. Those are the small remarks I wanted to present and I thank all the speakers for their wonderful presentations.

II

SCIENCE AND THE CONTEMPORARY WORLD

LA SCIENCE ET LE MONDE CONTEMPORAIN

INTRODUCTION AU DEUXIÈME SOUS-THÈME DE LA DISCUSSION

PIERRE LEPINE
Académicien Pontifical
Paris

Si nous cherchons à définir les traits par lesquels s'affirme et se reconnaît une civilisation, qu'il s'agisse de la situer parmi les plus primitives ou de la ranger parmi les plus évoluées, trois ordres de mobiles dans la quête de l'homme vers la croyance, la beauté et la connaissance nous apparaissent essentiels. Ce sont: la religion, l'art et la science. C'est-à-dire la philosophie de l'être et ses rapports avec l'essence divine, la conception et l'expression d'une esthétique et enfin l'affirmation de lois gouvernant la matière et leur aboutissement à une maîtrise technique.

Mais si la religion, et à un moindre degré l'art, ont été de tous temps un recours à la portée des masses, la connaissance scientifique a longtemps été l'apanage exclusif d'un petit nombre d'initiés.

Notre civilisation moderne a eu pour effet d'ouvrir potentiellement à toute l'humanité non seulement les bienfaits matériels de la technologie, mais les voies d'accès à la science par l'acquisition et la mise en oeuvre de la méthode scientifique.

Il ne s'agit pas là seulement d'une aspiration élémentaire à la satisfaction de besoins matériels, mais du droit donné à quiconque qui en manifeste le goût ou le désir d'appréhender le domaine intellectuel de la recherche dans les diverses branches de la science.

L'extraordinaire développement des connaissances scientifiques auquel nous assistons depuis un siècle et le retentissement généralisé par le progrès technique sur tous les aspects de notre vie quotidienne ont mis l'accent sur la part croissante prise par la science dans l'évolution de la civilisation moderne. A tel point que c'est sur le degré d'acquisition de la technique scientifique et de ses applications que se fonde la division du monde en pays « développés » ou « en voie de développement ».

Il peut être jugé regrettable que ce soit aujourd'hui à peu près exclusivement le niveau scientifique d'une nation qui permette de lui attribuer une place dans l'échelle des valeurs, mais le fait est là: sans culture scientifique il n'y a pas de progrès technologique, et sans technologie de pointe, il n'y a pas de puissance, cette puissance pouvant être intellectuelle, industrielle ou militaire, mais relevant généralement des trois termes confondus.

Il en résulte que pour toutes les nations, quoique à un degré divers, le problème du développement de la recherche et de la formation du personnel scientifique est devenu un aspect fondamental des choix à opérer dans l'immédiat et des plans à projeter dans le futur.

Les conditions dans lesquelles s'opère la recherche scientifique se sont profondément modifiées depuis un siècle. Jusqu'à l'essor explosif de la science contemporaine, « souvenons-nous, disait Einstein, au Président F.D. Roosevelt, que de tous les savants qui ont fait la science, les neuf dixième sont vivants », le chercheur était un isolé, un *amateur* au sens élevé du mot. Souvent il ne consacrait à la recherche que ses loisirs: Cavendish avait hérité une belle fortune, Lavoisier était fermier général, Mendel, Supérieur d'un couvent. Le matériel scientifique était simple: Archimède traçait ses figures sur le sable de Syracuse, Galilée faisait rouler des boules sur une planche inclinée, Newton regardait tomber une pomme, une bonne loupe a suffit à Pasteur pour découvrir la dissymétrie moléculaire. Aujourd'hui la science est tributaire d'installations qui coûtent des millions, le chercheur est membre d'une équipe, dépend d'une institution, d'une université ou de l'Etat.

L'esprit demeure libre, mais qu'on le veuille ou non, le travail du laboratoire est lié à des facteurs matériels avec lesquels il faut compter. Les investissements de plus en plus considérables qu'impose la recherche, ont, par nécessité introduit une notion de rendement des résultats qui risque d'être contraignante.

Bien sûr, lorsqu'en février 1896, Henri Becquerel impressionnait avec un fragment d'uranium une plaque photographique enfermée dans du papier noir, il n'entrevoyait pas la bombe atomique. Sans doute, Max Planck, Niels Bohr, Paul Dirac, Otto Hahn et Lisa Meitner, Enrico Fermi, Frédéric et Irène Joliot-Curie et tant d'autres, ont-ils abordé en théoriciens et dans la soif de la connaissance le problème de l'énergie nucléaire. Mais les découvertes eussent-elles marché aussi vite, les moyens matériels eussent-ils été mis aussi libéralement à leur disposition, si l'étude des phénomènes atomiques n'avait pas postulé la conquête d'une forme nouvelle d'énergie dont les effets destructeurs étaient parmi les plus sûrement escomptés?

Aujourd'hui même la conquête de l'espace par les deux plus grandes puissances mondiales serait-elle menée avec un tel luxe de moyens si elle ne sous-entendait pas des applications militaires terrifiantes?

Nous ne pouvons donc plus séparer le fait scientifique de ses applications. Aujourd'hui, c'est dans le laboratoire même où s'élabore la théorie que la pratique voit le jour. La recherche scientifique est en principe désintéressée: l'application ne l'est pas. Pour des raisons inexorables qui tiennent à la complexité croissante de la science, à son prix de revient parallèlement croissant, la distinction que nous établissions entre la science académique et les applications qu'on en tire tend chaque jour à s'effacer.

Ainsi le développement de la recherche scientifique ne représente-t-il plus seulement une valeur culturelle: il est devenu un élément essentiel de la structure nationale et un facteur important de son prestige comme de sa prospérité.

Ajoutons enfin, et ce point n'est pas négligeable, que la recherche scientifique représente pour l'individu une chance de promotion sociale en lui permettant par l'acquisition des connaissances et leur exploitation de se hisser dans l'échelle des valeurs de la société.

On voit par là l'importance majeure que revêt tant pour l'individu que pour l'Etat la formation des cadres scientifiques et leur adéquation à une carrière dans la recherche.

La culture scientifique ayant pour point de départ la culture générale, il paraît normal de rechercher dans l'Université le terrain propre à favoriser l'apparition du chercheur.

Sans doute connaissons-nous des exemples, et il en est d'illustres, de savants de génie, qui n'ont point, à l'origine au-moins, passé par les filières universitaires. Mais ce sont là des exceptions pour notables qu'elles soient, et en règle générale les chercheurs sont issus de la formation universitaire.

Celle-ci est-elle pleinement adaptée à la tâche de former des chercheurs?

Du point de vue de l'individu, c'est-à-dire de l'étudiant qui va devenir un chercheur scientifique, l'engagement passe par une série d'étapes qui sont le choix, l'orientation, la sélection.

Le choix repose sur la liberté laissé à l'étudiant au sortir des études secondaires de se diriger librement vers une carrière dont l'accès lui est théoriquement ouvert, soit par l'acquisition des diplômes, soit par le barrage d'un concours d'entrée: c'est le cas des grandes écoles.

L'orientation se fera au cours des études par l'attrait ou l'opportunité qu'offrira une branche de celles-ci. Encore faut-il qu'elle soit de nature à éveiller le goût de la recherche si celui-ci n'est pas dès l'origine l'effet d'une vocation affirmée. Il faut donc que l'enseignement reçu ait de ce point de vue un caractère incitatif.

Enfin *la sélection* se fera au stade terminal où entrent en concurrence les différents individus ayant choisi la même branche d'orientation par l'accession à un poste, généralement temporaire à ce niveau, dans un laboratoire de recherche débouchant sur une carrière hiérarchisée. Le déroulement de celle-ci dépendra des différentes appréciations de valeur émises par les chefs hiérarchiques, parmi lesquelles l'originalité du chercheur ne tiendra pas nécessairement la première place.

A l'échelle de la nation, les problèmes de la recherche scientifique et de l'adéquation du personnel comme du matériel au but proposé constituent un « système à états » au sens donné par David Easton à ce terme. C'est-à-dire, et ceci du point de vue national, qu'en fonction du degré de réalisation de l'objectif — la sortie, en terme d'informatique — l'état des entrées, c'est-à-dire le personnel à engager, le matériel mis en jeu, le budget, nous donnent l'image du degré d'avancement du progrès et par là de la politique à suivre pour l'amener à bonne fin.

En d'autres termes, il appartient à l'Etat de placer l'entreprise — qu'elle soit politique, industrielle ou scientifique — à un point d'équilibre entre les entrées et les sorties à l'intérieur d'une fourchette de variables essentielles pour faire converger ses résultats sur le but poursuivi, un peu de la même manière qu'une fusée, dirigée tantôt par autorégulation et tantôt par action commandée, finit par atteindre la cible qui lui a été assignée.

Mais la politique scientifique d'un pays faisant partie d'un tout qui est fonction de nombreux facteurs, parmi lesquels la taille et la population, son développement industriel, son revenu national, son système éducatif sont des éléments majeurs, le degré de liberté dans le choix des sujets de recherche à retenir comme dans le nombre des savants à former est nécessairement limité par l'optique réaliste des gouvernants.

Du point de vue gouvernemental, le « système à états » que constitue la science est par nécessité partie intégrante d'un ensemble de systèmes dont la cohérence est destinée à équilibrer les besoins de la nation dans des domaines divers mais dépendants: marché du travail, possibilités d'emploi, besoins de main-d'oeuvre dans telle ou telle branche, structure économique, satisfaction des aspirations des différentes catégories socio-professionnelles.

L'ensemble repose sur un schéma d'évolution qui réclame une juste proportionalité dans la répartition des individus entre métiers et entre spécialités. Ce qui implique que les besoins en qualification soient dans une mesure fixés à l'avance pour pouvoir s'intégrer au schéma d'évolution.

L'Université, les grandes écoles, doivent en principe et pour les qualifications hautement spécialisées, dont la recherche scientifique est un exemple, satisfaire les besoins répondant au schéma d'évolution, c'est-à-dire dans une perspective répondant à un horizon temporel d'environ une génération, soit la trentaine d'années à venir, pour remplir les postes offerts tout en maintenant le plein emploi.

Or, il est possible, sous cet angle de vue, de mettre en question le rôle de l'Université, en tant que formation des élites qui auront à occuper les postes ouverts au schéma national d'évolution.

Le système universitaire repose en effet sur le libre choix de l'étudiant. Or, l'Université est toujours apparue, en tant que corps constitué, plus comme un facteur de stabilité que comme un facteur d'évolution dans la structure de la société.

Bien que le choix de l'étudiant soit certainement influencé, autant que par les goûts personnels, par des ouvertures de carrière et des perspectives d'emploi, trop d'orientations sont finalement dominées par des facteurs secondaires, tels que la facilité, le dilettantisme ou la vogue passagère d'une spécialité.

Si le principe du maintien du libre choix est souhaitable et bénéfique, préférable à une ségrégation par voie autoritaire, il ne doit néanmoins pas aboutir à une inflation de diplômes inutilisables, à un afflux de chômeurs en cols blancs, à un prolétariat de pseudo-cadres, sous-employés parce qu'engagés dans des voies sans débouchés. Combien n'avons-nous pas vu déboucher sur le marché du travail de psychologues, d'ethnologues, d'économistes dont la qualification illusoire dissimulait mal la fragile base scientifique.

Il ressort à l'évidence des remarques qui précèdent que dans les conditions actuelles de l'économie mondiale, il ne saurait y avoir de développement scientifique d'un pays donné, quelle que soit sa taille, sans une certaine planification fixant les buts à atteindre, en fonction des objectifs retenus, des moyens appliqués à la recherche et de la sélection d'une masse critique de chercheurs. Il y a donc au départ un triple choix.

Le choix des objectifs, qu'il dépende directement de l'autorité gouvernementale ou de commissions ayant reçu une délégation de pouvoirs, est avant tout un choix politique au sens large du mot. C'est dire qu'il repose sur des données qui ne peuvent pas être exclusivement scientifiques: il doit répondre aux impératifs nationaux en même temps qu'au bon développement de la science.

Le choix des moyens qui seront appliqués à la poursuite de l'objectif est essentiellement économique. Il dépend à la fois du niveau du revenu national et de la proportion de ce revenu qui sera consacré à la recherche. En fait le pourcentage attribué à la recherche, qui ne pourra l'être qu'au détriment d'autres articles du budget, sera toujours jugé insuffisant par le corps scientifique, mais le chiffre budgétaire finalement retenu le sera après des marchandages qui auront pour effet de favoriser certaines branches de la recherche et d'en éliminer d'autres. Aucun pays, si riche fut-il, ne peut se permettre de développer au maximum toutes les sciences et certains sujets, en raison même des moyens qu'ils exigent; ils devront être épaulés par une coopération internationale dont nous connaissons déjà des exemples.

Le choix, enfin, des cadres et du personnel scientifique, représente la part la plus délicate de la planification scientifique. Il comporte la définition d'une masse critique, c'est-à-dire le nombre des chercheurs à orienter dans telle discipline, ce nombre devant être suffisant pour obtenir le résultat escompté et cependant limité par les moyens économiques appliqués au programme. Il impose une vue futurologique du développement de ce programme, la formation des chercheurs correspondant à un délai de dix à quinze ans selon la qualification, c'est-à-dire qu'il faut prévoir l'état des effectifs à l'horizon temporel d'une demi-génération.

Il est bien évident que même si le nombre des postes à pourvoir dans la recherche pouvait être défini avec certitude dans ce champ de prévision, le nombre des candidats présents au départ se trouvera sérieusement amenuisé à l'arrivée du fait de la sélection qui s'exerce à tous les degrés de l'échelle à gravir.

Il faut donc pouvoir estimer l'inévitable déchet annuel résultant des abandons, des changements d'orientation, ou de l'impact des facteurs économiques.

Si on a pris soin de suffisamment garnir les rangs au départ du cursus de la recherche il faut compter à l'arrivée sur un excédent de candidats au poste disponible. C'est à ce stade que jouera la sélection finale en fonction des qualités déployées jusque-là dans le secteur de la recherche. Les postes d'enseignement universitaire pourront recueillir ceux que la sélection aura laissés pour compte, car selon la formule cruelle mais juste de Bernard Shaw: « Those who can, do, those who can't, teach ».

Il est bien entendu qu'au départ, les goûts personnels, les affinités intellectuelles ou les opportunités de carrière auront contribué à mettre en liste les futurs chercheurs. Mais comment distinguer la véritable vocation d'un engouement passager? Nous aurons à discuter de la valeur des tests de sélection qui ont été proposés. Personnellement je pense que la qualité du dossier d'études du postulant et surtout le contact quotidien dans la pratique du laboratoire donnent de meilleurs éléments d'appréciation d'un candidat que les tests psychologiques ou les épreuves d'un concours. C'est au cours du travail que se manifestent les qualités d'originalité, de décision, de méthode qui seront les vrais critères du choix d'un chercheur.

Il peut aussi arriver qu'au cours de ses stages de formation un futur chercheur voit se développer une vocation pour une branche des sciences, un type de recherche pour lequel l'environnement national n'offre aucune possibilité de développement. Il est alors du devoir des maîtres qui patronnent l'élève de l'orienter vers le pays étranger où les possibilités de recherche répondront le mieux aux dispositions qu'il manifeste. Nombreux sont ainsi les chercheurs qui ont su décrocher en-dehors de leur pays natal une notoriété ou un Prix Nobel auquel ils n'auraient pu prétendre sans s'expatrier. La véritable science ignore les frontières et les nationalismes.

Comment faire le meilleur choix pour orienter les jeunes vers la science, pour définir les programmes, pour fixer et sélectionner la masse critique des chercheurs, pour assurer à ces derniers la meilleure qualification, ce sont là les questions que nous nous posons tous avec acuité.

Une bonne planification de la science ne peut s'accommoder ni d'un rigorisme autoritaire, ni d'un laxisme anarchique.

Certes, il est nécessaire d'arrêter des programmes pour en prévoir les ressources, mais en dernière analyse c'est sur le choix des chercheurs que repose la plus grande responsabilité de ceux qui ont pour tâche de promouvoir et d'orienter la recherche.

Car la science est en fin de compte oeuvre humaine, et l'homme ne vaut que par cette étincelle divine qui le pousse à se sublimer dans les domaines de la foi, de l'art ou de la science.

Quelles que puissent être par ailleurs nos convictions, notre foi dans la science repose sur l'idée que des lois d'une simplicité mathématique régissent la nature.

Mais il n'est pas donné à tous d'accéder aux lois qui sont la clé des phénomènes naturels. Comme l'a écrit Albert Einstein: « quelle foi profonde dans la rationalité de la structure du monde, quel ardent désir de comprendre, ne fût-ce qu'un infime rayon de la raison révélée dans le monde, il dut y avoir chez un Képler ou un Newton ».

C'est cette valeur de l'individu qu'il nous faut savoir déceler car, je cite encore Einstein: « fixer les buts les plus fondamentaux et l'appréciation des valeurs essentielles, les établir fermement dans la vie de l'individu, me semble être la plus importante fonction de la religion dans la vie sociale de l'homme ».

Si nous savons nous inspirer de cette éthique alors nous pourrons résoudre les problèmes de la science et du monde contemporain.

BIBLIOGRAPHIE

D. Easton, *A systems analysis of political life,* New-York 1965, (cité par Jacques Lesourne, Acad. des Sciences, Paris, 3 octobre 1977).

A. Einstein, *Mein Weltbild,* Frankfurt a.M. 1955.

DISCUSSION

LEPRINCE-RINGUET

Permettez-moi de faire quelques remarques après l'exposé du Professeur Lépine et les remarques de plusieurs collègues puisque naturellement nous avons tous réfléchi sur notre existence de rechercheur et de patron de recherche. Il y a une remarque qui me paraît très importante; c'est que, en général, les certitudes budgétaires pour la recherche, pour un laboratoire, pour un équipment, sont annuelles. Or, la formation de chercheurs, la possibilité d'utiliser un nombre un peu plus grand ou un peu plus petit de chercheurs dans certains domaines n'est pas annuelle, et nous sommes toujours devant le problème — j'ai été toute ma vie devant ce problème — est-ce que nous pouvons engager des chercheurs cette année, les payer comme nous devrons peut-être, mais que nous sommes sûrs ou à peu près sûrs d'avoir un budget de recherche qui soit, je dirais, à peu près défini ou avec un certain croissant défini pour sur quatre ou cinq ans? Si nous commençons à engager de jeunes chercheurs et que le budget n'est pas stable, n'est pas défini pour un temps suffisamment long, c'est une perte complète et on perd tout — on perd la face, on perd des chercheurs. Des plans de cinq ans, ne sont jamais une assurance. Or, c'est une affaire à long terme la formation de chercheurs et le développement d'une équipe. Par conséquent, c'est un problème qui se pose qui me paraît très important.

Deuxième remarque: quand on va choisir les chercheurs qui doivent être promus, comme par exemple dans les cadres du C.N.R.S., est-ce qu'on doit prendre ceux qui sont bons, ou ceux qui sont vieux, qu'il faut faire avancer? Nous avons ce problème constamment, et pour la solution de ce problème il y a naturellement dans les commissions plusieurs tendances. Il y a une tendance qui consiste à dire: « Voilà quelqu'un de remarquable, il faut lui donner des possibilités, par conséquent une promotion », et il y a ceux qui disent: « Voilà quelqu'un qui a bien travaillé honnêtement, qui arrive à la fin de son mandat, on ne peut plus le renouveler, alors il faut le promouvoir ». En plus je dois dire qu'il y a une troisième intervention qui se fait jour depuis quelques années, c'est l'intervention syndicale, voire l'appartenance syndicale. C'est une considération qui malheureusement, je dirai, commence à être prise en compte un peu trop. Je dirai que pas dans les recrutements mais au moins dans les avancements, si l'on appartient à un tel syndicat qui a beaucoup de représentants à la commission, on a plus de chance d'avancer, naturellement: On voit qu'il y a des problèmes qui ne sont pas faciles à résoudre.

Je voudrais faire une troisième remarque. C'est une remarque qui concerne le professeur Weisskopf. Nous savons très bien que lorsque on est dans une période de vaches maigres, de restrictions, bien, on a toujours tendance dans un pays à réduire les crédits de la science fondamentale et à essayer d'orienter davantage la science vers les disciplines dont on peut prévoir des applications utiles à l'économie du pays. Je dirai que celà dépend aussi un peu de la structure de la recherche. Si le Ministère qui s'occupe de la recherche est lié au Ministère qui s'occupe de l'industrie, si c'est le Ministère de l'Industrie et de la Recherche, comme ça a été le cas de temps en temps, on est absolument certain que la part de crédit recherche sera réduite en faveur des laboratoires industriels qui ne vont pas bien, qui sont malades, et que par conséquent il n'y aura pas du tout de garantie ou d'assurance pour la poursuite de recherche fondamentale, ou du moins il y aura peu de garantie. Si au contraire il y a un Ministère de la Recherche indépendant du Ministère de l'Industrie, et bien, on pourra se battre davantage et il n'y aura pas, je dirai, presque automatiquement prise de l'argent dans une part d'un ministère vers une autre part. Alors, est-ce que actuellement les recherches fondamentales sans application visible sont possibles dans une période de restriction, dans une période difficile?

Analysons un peu l'exemple du CERN. Le CERN déploie la recherche fondamentale, très onéreuse — le dernier synchroton a coûté l'équivalent de 400 milliards de lires. Donc ce sont des équipements qui ont une certaine valeur, qui sont compliqués. Alors on dit il y a des retombés, naturellement en électronique, en informatique, on a fait travailler des usines, on a fait travailler des centres industriels; pour avoir de la bonne informatique on a peut-être même aidé le développement de la supraconductivité, par exemple, les aimants supraconducteurs. Oui, il y a naturellement des retombés de ce genre-là, mais les découvertes que l'on fait au CERN n'ont aucune application pratique depuis 1959. Or, voilà donc une installation qui est très onéreuse, qui coûte au gouvernement très cher et qui ne sert à rien sur le plan pratique, sur le plan économique, sur le plan industriel. Et pourtant ça continue, et jusqu'à présent les gouvernements sont un peu réticents, naturellement, mais on arrive à tenir un budget, on arrive même à la maintenir, à l'accroître un peu, enfin il n'y a pas eu de catastrophes budgétaires pour le CERN. On est même capable actuellement de faire des projets d'avenir et de penser que ces projets d'avenir pourront être réalisés, même sur le budget courant, avec une petite réduction dans certains domaines. Alors, comment peut-on analyser celà? Il me semble que lorsqu'on a construit CERN et on a cherché de l'argent, il y a eu un véritable siège de tous les ministres intéressés par tous les scientifiques. C'est-à-dire que moi j'ai été voir le Ministre dell'Industrie que je connaissais, Grégori a été voir un autre ministre, un troisième a été voir un autre, etc., le Président du Conseil; finalement, on avait fait le siège; et voilà pourquoi celà a réussi. Donc, le CERN, a démarré, a déclenché. Alors après je suis persuadé que s'il n'y avait pas, je dirais, un symbole d'union internationale, avec toute sa signification, signification politique, humaine, etc., le CERN, ou du moins la physique des particules que l'on peut faire dans un pays, cette physique chère, ne continuerait pas facilement. J'ai l'impression que le grand soutien du CERN c'est que c'est la seule institution scientifique européenne qui ait bien réussie. Je ne sais pas si Victor Weisskopf est de mon avis. C'est lui qui a dirigé, lancé, animé, le CERN, qui lui a donné une forme de pensée qui est vraiment très internationale, très scientifique et très européenne aussi, et bien, je crois vraiment que s'il n'avait pas eu ce caractère, à la fois ce symbole, à la foi cette réussite, et bien on n'aurait pas actuellement les crédits pour une recherche onéreuse et fondamentale sans application dans une période de vaches très maigres. Donc je voudrais demander à Weisskopf ce qu'il en pense.

Il y a une dernière remarque: il y a la formation de chercheurs mais il y a aussi la réorientation de chercheurs, qui est un problème, un gros problème. Il me semble qu'il faut donner sa chance à beaucoup de chercheurs, les former scolairement à l'université peut-être, mais surtout au départ, en leur donnant une chance, dans des équipes qui travaillent bien, dans de grandes équipes bien actives. La formation d'un chercheur est très différente s'il est dans une petite équipe qui traîne, qui ronronne, ou bien dans une grande équipe. Dans une équipe qui travaille très dur — sa formation à ce moment-là devient excellente. Et... après? Et bien, il est certain que lorsqu'on a sa thèse de doctorat, un chercheur ne sera plus nécessairement très valable — c'est pas sûr — dans le domaine de la recherche fondamentale, et que l'orientation vers l'enseignement, l'orientation vers l'industrie sont possibles et souhaitables pour le pays. Alors c'est un problème, car quand on a goûté la recherche fondamentale, on a bien de la peine, même avec un salaire supérieur, à accepter des contraintes de l'industrie, être présent à huit heures du matin, n'avoir que trois semaines de vacances. Ce sont des choses qui sont inconcevables pour un chercheur déjà un peu invétéré, ou presque. C'est aussi un problème, et je crois que ce problème est difficile à résoudre, et celà dépend de la situation économique du pays, ça dépend de beaucoup de questions, ça dépend également de la saturation de l'université, qui chez nous par exemple est saturée actuellement et ne peut plus prendre un chercheur comme professeur. Ce sont des grands problèmes, mais il me semble qu'ils doivent être posés pour la compréhension de la formation du chercheur, l'évolution du chercheur, et de la place de la recherche fondamentale dans un pays.

WEISSKOPF

I am very glad that Leprince-Ringuet brought up the example of CERN. He essentially said everything that should be said, but I would like to add, or only emphasize three points. First, it is true CERN has a very large budget, but a recent study made at CERN about the advantages of the member states from having CERN turns out to be such that most countries actually get more money in than they spend. Why? Not because of the results of CERN, which are purely pure science in the purest sense of the word — the applications are far away — but because of the experience of the industries the investigation was made. The member states contributing to the building of the apparatus, the superconducting magnets, or electronic devices, have made additional experiences which they can use in other productions. This was studied rather objectively, indeed; and it turned out that the additional business that those results could make in the member states is larger than the contribution to CERN. That is one point which is a purely material point and has nothing to do with the greatness of fundamental research, which is of course very near to my heart.

The second reason mentioned by Leprince-Ringuet is the international nature of CERN, which gives it — what shall I say? — an appeal to governments that institutions within a single nation do not have. And I would like to emphasize the point again, that when we talked before about the support of basic science in regions where it is not enough developed, the mere fact of having an international institution helps. It gives it a special prestige and it is easier, from the practical point of view, to get money for it, but from the intellectual point of view of course it is especially valuable to have this concentration of intellectual force from different countries. And indeed I would say that the regional international laboratories have a special advantage. For example, I do not think CERN would be as good if it were a world organization because people are too different around the world, fortunately; and therefore these regional international laboratories, I think, have a special promise because of almost all reasons one can think of: budgetary, intellectual, cross-fertilization, and then the appeal for international activities which is very noticeable there.

The third point I want to mention is that one reason why CERN could develop relatively well is because only a relatively small part are permanent employees. They are too many in my view, but it is still only perhaps 30 or 40% of those who work there. And therefore there is a constant stream of younger people from the European universities who work there and bring their ideas and their life and their new impulses into it; and I believe that especially in Europe the situation in the national laboratories there is much more difficult because everybody wants a permanent position when he is 24 or 25 years old. This is of course quite impossible in science, because people are not productive from 25 to 70, and therefore in a way the success of CERN should be a warning to the other European national institutes and to the governments to change this policy. To put it brutally, one must be able to fire scientists, to get them away from the laboratory to make room for the younger people. And just today, when the number has kept constant — we do not get more money — it is even more important to be able to get rid of people who are no longer productive. The development, however, goes in the other direction, particularly in Europe, and I think that is one of the great dangers for basic science — by the way, not only for basic science, but for science and technology in many countries.

LORA-TAMAYO

Je pense que le problème de la formation de chercheurs c'est peut-être le plus intéressant de tout ce dont nous pouvons parler en cette occasion. Il faut stimuler un bon chercheur, c'est-à-dire celui qui a une vocation, de bons professeurs et de bons patrons depuis le moment qu'il entre dans un laboratoire de recherche, c'est-à-dire depuis le moment qu'il commence à préparer sa thèse de docteur. Le patron c'est pour moi le point principal dans cette période de formation. Le patron, je dis, doit vivre près de ses novices pour les encourager et éviter le désarroi auquel les jeunes chercheurs peuvent arriver fréquemment. Le contact intime entre le patron et le jeune chercheur est indispensable. Il faut que le jeune chercheur voie toujours un esprit ouvert de la part du patron pour faciliter l'ascension de ses disciples et collaborateurs. La science exige un climat et il faut le vivre de cette manière. C'est une bonne récompense pour le maître d'avoir des disciples qui le supèrent. Il ne faut pas oublier que la générosité est une condition essentielle du patron.

CHAGAS

Before I give the floor again, I would like to say that this question is of very great importance. Yesterday we had discussed a problem which is of enormous importance to what we are discussing now and which was brought up again by Professor Weisskopf; and that is the question of the tenure, which is a problem that becomes very different from one country to another. It depends very much on the cultural system existing in each country, and it differs very much from the United States, for instance, or England, where there is a much bigger market for research workers than in other countries, like the Latin countries. This is a problem with which we have had some interesting experience which I want just to refer to. We had an old institution in Brazil which decayed very much, and suddenly it was completely reshaped, priorities were given, and so forth, and many scientists were dropped; some because they were useless, they had no more interest in science, but others because they were not involved in priority given to this institute; and the interesting thing is that this involved a wave of protest from the scientific community itself. So the question is a very delicate one, as much as it is a very important one. It is easy to understand the mobility of the scientist in the United States for instance; it is very difficult to understand it in other countries. Permanent tenure is an ideal that the young man who comes to science, if he is not first-class, has a tendency to acquire very easily.

The second point I believe to be also important regards the mobility inside science of a scientist: the capacity to change. I think that one of the good examples of this capacity was given when Baltimore presented the evolution of his life. This mobility depends first on the personal capacity of the scientist, of his ludic interest for science; but it depends also on the fact that the average younger scientist in general terms becomes too soon a specialist, or too much attached to a machine, as was said here by Marini-Bettòlo yesterday; so much attached that he is unable to recognize that he is finished in this field and also to change fields or to drop out of the one he is working. This is a fault in the form of preparation, due in most countries to the "patrons", the teachers, the masters, who are "using" — let us underline the word — the graduate students very much as a cheap labor type of assistance, much more than giving them the personal relationship which was characteristic of science before its present organization. Another point: there is something wrong with the system of appraisal of a young scientist in many countries in a general way, as we base his evaluation on the number of publications. This depends very much on the system, and we are seeing that the system is deteriorating so much in many countries that even political affiliation has an influence in a career.

TUPPY

I was deeply impressed by Professor Lépine's exposé because it took into account not only the needs of scientific excellence but also the needs of human beings engaged in science, whether they actually reach this excellence or not, whether they stay in pure science or not; and he also took into account the needs of society and nations, the whole being an intricate and delicate system. Freedom of choice of a subject to study in the university, freedom of choice of one's own subject, the object to do research upon, academic freedom, curiosity, orientation, and all that is fine, but it can only apply to a certain number of people in a society, or at least it can only apply for a certain time of the career of people and not, as I said, to all people. I wonder whether our universities really find the right way to train people so as to, on the one hand, enable them to find their own way and, on the other hand, to orient themselves to the needs of society, to social needs, and so on. We have a kind of — at least I would like to say it for myself and many of my colleagues — we have a kind of ideology of pure research, curiosity-oriented research, and all that, and we hand it over to our students, and even to the teachers in the schools who are trained in the universities; and many of our students find it very difficult then really to adapt to the needs in the society. I would like to come back to what has been said: we should not only orient our students in one direction, but give them opportunity for reorientation — we should not only give them a basic training but opportunities and intellectual orientation, to be balanced by later freedom of using people for different purposes; and we should really think of better means in our institution for attaining this goal.

BLANC-LAPIERRE

Je voudrais revenir sur le point du mouvement du chercheur dont Monsieur Lépine a parlé. M. Leprince Ringuet a insisté — je crois que c'est un aspect qui est extrêmement important. Je suis un petit peu moins optimiste que ne l'a été le Professeur Lépine lorsqu'il a dit: ceux qui savent faire, ceux-là savent enseigner, parce qu'on sait faire avec plus ou moins de succès. L'expérience que j'ai de ce problème montre que, même si l'on n'est pas dans une période de restriction de postes universitaires, il n'est pas évident que, quittant le laboratoire, on est immédiatement un bon professeur. De même il n'est pas évident que quittant le laboratoire, même avec de gros appareillages, qui est orienté vers la recherche fondamentale, on devient bon chercheur ou ingénieur dans l'industrie. En fait, les problèmes de l'industrie sont des problèmes qui sont beaucoup moins spécialisés et je rejoins la dernière intervention: il est certain qu'il n'est pas évident qu'un chercheur a la flexibilité qu'il faut pour prendre dans son intégralité un objectif industriel. Alors je crois que c'est une grande responsabilité pour les directeurs de laboratoires de préparer leurs chercheurs, ou un certain nombre de leurs chercheurs, à cette éventualité. Et je pense qu'il faut les mettre en contact avec le monde industriel, peut-être par des contrats de recherche sur les problèmes plus techniques, peut-être par des stages qui seront ennuyeux mais qui garantissent cette mobilité, parce que je pense que le réveil vers 35 ans s'il faut passer d'un laboratoire de physique fondamentale, au monde industriel, est très difficile et doit être un souci permanent des directeurs des laboratoires.

Je voudrais maintenant dire un mot sur les critères de choix au début de la carrière. Naturellement, on prendra des critères universitaires.

Je pense qu'il faut porter une grande attention au point suivant: les études universitaires sont assez compartimentées en disciplines distinctes, et énormément d'objectifs de recherche sont pluri-disciplinaires, et par conséquent ils mettent en jeu la volonté tenace du chercheur de faire aboutir un projet en allant chercher les moyens de solution où ils sont, dans deux, dans trois disciplines, entre deux disciplines, et je crois qu'il faut dans le travail en équipe, qui est un travail très formateur, donner assez de liberté pour que la personnalité de chacun, presque son mauvais caractère vis-à-vis de ce qui est admis dans l'équipe, puisse s'affirmer. Ceci lui permettra des reconversions ultérieures.

O'CONNELL

A practical point was raised by Professor Lépine toward the end of his discourse, which I should like to endorse and emphasize. Indeed it was touched on also by Monsieur Leprince-Ringuet. And that is that in order to judge the value of a young scientist, the best way is to have him working with a senior scientist for a time in order to judge his capacity for originality and his enthusiasm; and no tests or examinations can possibly take the place of that.

HÖRSTADIUS

We are discussing science and the contemporary world. I should like to talk about something as contemporary as one year old, and this is to deal with the influence of politicians on university life. Before entering upon that, I should like to mention that in Sweden nowadays everything is democracy. The newspapers are full of discussions on problems... to be solved in a democratic way. Now we have had a Socialist government since the last World War — more than thirty years. In old times the universities were rather self-governing. We only had one man above universities, the so-called Chancellor of the Swedish universities, and he was elected by the universities. Recently a Council was put above the university. The Council consists of only a couple of professors, the other ones being selected from ordinary life, politicians, some representatives of industry, and so on. Decisions used to be taken by the faculty and the Senate, but now for instance, the selection of a new rector of the university is done hy electors which have been first elected. One group of these comes from the professors and all those in the university service who have doctor's degrees or higher up. The other group originates from the personnel, from the technicians, down to all those who are cleaning up in the laboratories and rooms, etc. At the last election the representatives from the universities, the university professor and so forth, and those of the trade unions have exactly the same number of votes. Then of course it may be possible somewhere in the future that also the rector can be really selected only by the trade unions. Now they have new ways of accepting students to start working at the university. There used to be freedom in that way — all those who had passed through the higher school up to what we call "students' examinations" at the age of about nineteen, were always accepted, except in some of the subjects where we bave laboratory work and where you cannot receive everyone. Now this has been changed in this way: it is not necessary anymore to have this "students' examination". You can get points to enter in two other ways. One is if you have some experience of the working life, for instance if the applicant has been standing five years on the floor of a factory, that will give you a lot of points. If you have been a member of a council, of an association, whatever it is, that also gives you some points. In this way many of the good students cannot get in. The other ones are above them. It has been found that people with these five years of life experience often fall out after a short time because they really cannot follow the courses as they bave no special knowledge of English, or mathematics and so on. You may understand what it means for the national economy.

PAVAN

I am pleased with all these discussions that we are having here, but my point is that I think the problem is much more complex. If the problems we are discussing here would depend only on us, we would be pretty sure that many of them could be solved, but since they do not depend on us but on other people, scientists, politicians, administrators and so on, then I see the difficulty in convincing these people. For instance, yesterday we were discussing the planning of science. This is something very difficult to deal with. I do not see how we can develop science without some planning. For instance, in Brazil every time we try to make some plannings, we have among the scientists arguments against this: don't do this because if you do it, the politicians put their hand on you and then you will never get free of it. But if we do not have planning, we will not have enough money. On the other hand the "good ones" are few and have to be used for multiple purposes. They may be taken out of the lab to do some other work than science and orientation of their students. Also when we send a student to foreign countries, we have to be very careful about his return in order to assure him the possibility to continue his work in bis own country. This becomes more acute in the fields of advancing sciences and still more acute when we take a student from small universities and send him abroad. He gets his Ph.D. sometimes very brillantly but back in Brazil in his own university, he finds just nothing to do there and becomes unhappy. We should plan how to send the people abroad. We have to plan their stay in order, as Dr. Lépine said here, to give to the students the right preparation.

The most important thing in developing countries is to know how to influence government in order to obtain funds, but this happens also in developed countries. A good device is to insist that the funds you are asking for are important for the future and to solve national problems, and that of these the most significant one is to prepare personnel in basic research, be it genetics or molecular biology or particle physics. But my question here is the following one: How could we, as a group, have influence in the government of different type of countries? To use what we are saying here, what kind of activity shall we take so that our time will not stay all on paper? We have really a point, a solution of a problem, not only to raise more problems but how can we help tbe problems to be solved? This is my main question.

LÉPINE

Je voudrais seulement ajouter quelques brèves remarques à nos discussions. Les remarques faites par Leprince-Ringuet étaient tout-à-fait de bon sens et j'approuve tout ce qu'il a dit. Je ne ferais qu'une petite restriction. Il a signalé le grave inconvénient d'avoir des crédits annuels pour la recherche, et c'est vrai. Je dois dire qu'au moins dans notre pays on commence à comprendre l'inconvénience de ce système et que maintenant le crédit qu'on obtient en général s'étend sur plus d'une année. Les collaborateurs techniques qui sont très loués, les crédits de recherche généralement portent sur plus d'une année et, soit sous forme de contrat, soit sous forme d'une allocation plus continue, permettent de mieux prévoir l'extension donnée à un sujet; c'est donc sur ce petit point un progrès. Leprince-Ringuet surtout et Weisskopf ont insisté sur l'importance de la collaboration internationale et cité un exemple: le CERN, qui est en effet un excellent exemple. Je suis profondément convaincu des avantages de cette collaboration internationale: pour les pays qui participent, il y a là une question de prestige qui les incite à continuer, et pour les chercheurs qui sont impliqués dans ces travaux il peuvent travailler avec un matériel dans des conditions qu'ils ne trouveraient généralement pas dans leur propre pays. Le Professeur Weisskopf a insisté sur l'importance d'une collaboration régionale, et il a raison, car il y a des similitudes de conception, des rapports plus proches qui rendent plus facile cette collaboration à l'échelle régionale. Mais elle peut aussi se faire à distance, entre des instituts qui ont des objectifs communs; comme exemple je donnerais la collaboration qui s'est instituée récemment entre l'Institut Weizmann à Rehovot en Israël et l'Institut Pasteur, dont les résultats sont excellents, parce qu'ils poursuivent un même but et peuvent concentrer leurs moyens sur le but qui a été choisi. Je suis tout à fait d'accord avec Mr. Blanc-Lapierre sur le fait qu'il n'est pas toujours facile de reclasser un chercheur qui n'est pas à sa place dans le domaine de la recherche et qui veut s'orienter ailleurs. Néanmoins au cours de la carrière assez longue que j'ai eue en tant que patron, je suis toujours arrivé à replacer ceux qui n'étaient pas faits vraiment pour la recherche. Je n'ai pas parlé de l'industrie, où la situation n'est pas toujours commode — et rappelons-nous que l'industrie ne comporte pas toujours de situations stables et permanentes. Il y a encore des carrières administratives — je pense par exemple au domaine de la santé ou au domaine de l'hygiène où on arrive à remplacer des collaborateurs. Il y a une question à laquelle le Président Chagas a fait une brève allusion et qui à mon avis est importante. La science est un domaine mouvant — les hommes vieillissent mais les instituts aussi vieillissent — et nous connaissons tous des exemples d'instituts qui, ayant vu leur structure se scléroser, ont dû au bout d'un certain temps être renouvelés. Il faut donc garder en mémoire que l'un des rôles de ceux qui font la science devrait être de veiller à ce que l'institution avec laquelle ils collaborent ne vieillisse pas et ne se sclérose pas; c'est très important car on voit immédiatement baisser le rendement de la recherche dans des instituts qui n'ont pas sû se maintenir dans l'équipe de pointe des travaux de recherche.

Pour terminer, je dirai qu'au fond nous sommes tous d'accord sur les problèmes de la recherche. L'inconvénient c'est en parlant entre convaincus et qu'il faudrait que nous disions « ici aille vers ceux qui sont responsables de l'attribution des crédits — là le Dr. Pavan a parfaitement raison — il faut trouver le moyen de faire entendre en dehors de cette enceinte, il faudrait que ce soit les gouvernements et les politiciens qui soient persuadés de la conviction que nous avons que dans la voie que nous traçons se trouvent l'avenir et le salut de la recherche. Et c'est là justement que nous avons envisagé ensemble les mesures à prendre.

ROCHE

Pratiquement tout ce que je voulais dire a été dit admirablement bien par des orateurs qui m'ont précédé. Je voulais simplement souligner un petit fait. Le Professeur Lépine cite Albert Einstein, qui disait au Président Roosevelt que de tous les savants qui ont fait la science neuf-dixièmes sont vivants. Je crois que c'est Price qui a fait le commentaire suivant: Etant donné que la science a un accroissement explorentiel depuis le 17ème siècle, au moins depuis que l'Académie des Sciences en France et la Royal Society en Angleterre ont été créées, cette affirmation d'Einstein est-elle vraie? Il paraît qu'à n'importe quel moment dans les derniers 200 ans on pouvait dire que 90% des savants qui ont vécu vivaient, étaient vivants à ce moment-là; c'est-à-dire que ce fait — qui est vrai et qui est frappant — a été vrai depuis au moins 200 ans.

SIDDIQUI

A number of questions have been raised in the course of the discussion and I wanted to express some of my observations on the basis of my long experience, with both the organization of science and active research. One of the problems we had, to which reference has been made, is the tenure of service, the permanence in our service as against contract service. It will not be necessary to send away any scientists when we have become unproductive if the appointments are made more or less after years of research carried out by younger scientists on the basis of contracts. It may be, and we have arranged in our Institute, the Post-Graduate Institute of Chemistry in Karachi University, that those who have done their doctorate and post-doctoral research experience, can be appointed as senior fellows for a period of five years, after which their work and their productivity is to be evaluated, and for this evaluation their research publications, lists of research publications, reprints of publications are sent to three foreign scientists in their specific field; and it is on their recommendation that the appointments may in that case be made or their contract further extended. With regard to the problem of the exploitation of the young scientist — which has also come up for discussion — the practice followed is that those who have showed demonstrable capability of carrying out independent research, can follow their own ideas, join up with some of these scientists through mutual agreement, without the Director of the Institute having anything to do with it. That cuts out the possibility of using younger scientists as helpers, scientific helpers, not allowing them any freedom to work along their own ideas. Even if the Director feels that they are barking up the wrong tree, they should have the freedom to do just that and return after some sad experience. Then eligibility for appointments as against seniority, we have the problem of merit. One has to make many enemies guarding the interests of young scientists on the basis of merit and there is no particular merit in being either young or old — it all depends what one has done with one's life, what he has produced. But this is generally not accepted, and there are such heavy pressures from professors, who generally insist on considerations of seniority. In spite of that I have had a young man — it has been possible for me — get him appointed as director although he is only 35 years of age, even if there were 20 seniors, he has been appointed over their heads. This creates so many difficult, human problems in a university, and one advantage that we have is that the department, which has 600 students and about 20, 30 teachers with doctor's degrees and so on, has a separate status. It was very difficult to fight for such a separate status, but I found that it was the only way of establishing a post-graduate center of research where doctoral and diploma work can be carried out on the basis of research. These are the various difficulties which I am sure many of the developing and underdeveloped countries would be facing and it is not an easy problem — one has to fight very hard to carry one's point through, with the government and with the university.

RICH

I am responding with a thought about the question raised concerning a concrete thing that this Academy might do. We have had rather an interesting experience in Europe with the development of the European Molecular Biology Organization. It was established in a sense because compared to the United States Europe was much less developed in this area, somewhat analogous to the way that CERN was established to rectify a situation in physics. EMBO, as it is called, has been a great success. There are a number of European organizations which belong to it. It works in a variety of ways, and the thought comes through my mind whether this Academy might not act as a catalyst for the establishment, not of the EMBO but of the LAMBO — the Latin American Molecular Biology Organization. The reason why I think it might be practical is that this Academy has prestige in Latin American countries, and an initiative coming from this source would be such that the proposal would be listened to very carefully. The organization, as EMBO was set up, was really set up in two stages. In the first stage — and here again one could rely rather heavily on the European experience and try to follow lines which are rather parallel and which have proven successful — the first stage is one in which essentially an administrative or consultative body is established, with money which is supplied by the member nations. This money is used to carry out a number of activities. These include so-called long-term and short-term fellowships, allowing investigators to go from one laboratory to another to carry out some collaborative experience. These fellowship in EMBO are not only within the EMBO nations but also allow the recipients to go abroad to the United States if necessary. They also set up a variety of EMBO conferences, in which specific themes were developed, speakers were invited from all over the world for these conferences and these meetings were successful in facilitating and stimulating the development of this area of science. Now, in the initial stages, this organization was not a bricks and mortar organization but rather one which facilitated interchange, and it also had laboratory courses where people would come, learn new techniques and then go back. Because of this, EMBO persisted for several years in this stage. Then things worked out successfully so that it became apparent that there were advantages to having a central site and out of this has grown the European Molecular Biology Laboratory, which is set up in Heidelberg. It was just recently formed and has been built and is now operating, so it is too early to make an assessment about it. But I wonder if one could not use this as a pattern or try to stimulate a similar development in Latin America, and could perhaps this Academy, which has a fair representation in that field of science, take an initiative to catalyze its development. One would have to do a great deal of thinking about the way this could be organized, but basically what one needs is a continuing commitment from the government to carry this out. In the initial stages it would be in the nature of an experiment to see how it works. If it works well, then one could continue, but since you are not in first stages establishing a fixed laboratory, it is the kind of investment or involvement that many of the nations might very well go into. So, I will put this on the floor as a proposal: namely, to think about the question of using this organization to catalyze the formation of a Latin-American Molecular Biology Organization, following the pattern which has been so successful in Europe.

Chagas

Thank you, Dr. Rich. I think we will come back to this question because this is in part an answer to Dr. Pavan's question.

Garnham

I should like to make a few comments on the question of international participation in research. This has been referred to by various speakers, and I particularly wanted to draw attention to the fact that this does not necessarily involve big and complex organizations but can arise from the interests and experience of individual workers. I would like to just take two examples. One refers to an institute which was named after your own late brother Evandro Chagas, the Evandro Chagas Institute in Belem, which is a very good example to my mind of where a group of foreign workers have integrated for a number of years with the Brazilian scientists. The workers comprising the Rockefeller teams are from yellow fever work, British teams with three or four people concerned with parasitology, who have in fact integrated so well that two at least of them have taken Brazilian wives, and the third, people coming from neighboring territories participating in the work in that institute. Then to jump from Brazil to Europe, we have at present a good example again of individuals interested in a certain subject, of different nationalities, and we have in fact formed what is called the "équipe franco-britanique" in the University of Montpellier, where the organization and people concerned are people from my own institute, the Liverpool School of Tropical Medicine and the laboratory of ecology in Montpellier. Now these are simple international research groups which seem to me to be one way of helping cooperation between different countries.

UBBELOHDE

I wanted to make a rather technical remark about the career development of young graduates. For a number of years we have been monitoring this as far as it is possible to keep track of what happens to graduates for about ten years after they have graduated, and our object is to see what proportion of them follow different lines extrapolating from the first degree. For example, we encourage students with the right kind of gifts to take further studies, for example in law or business management or in economics, in the hope that they might become politicians or at least big business men; and we encourage others of course to go into industry and management, and others that do not need any encouragement have a real vocation for long-term research. And my point is a rather technical one. If one can collect at least 80% information for about ten years, one gets a very important guide to the educational functions of any particular scientific discipline; because in Britain one of our weaknesses I think is that very often the people who eventuate as Members of Parliament, for example, or as top senior civil servants have had very little scientific impact in their education, just the way our educational system is very divisive, and we want to encourage more scientists to become MP's and if possible Ministers in Parliament and so forth, and I am quite sure in different ways this problem in other countries also arises. Our educational channels are very divided, and therefore we want to encourage programs being crossed after graduation. A technical point therefore is that if we have monitor information of this kind, this could be very useful in the course of years in order to exchange informed opinions, not just guesses, as to what is happening. In other words, we are educating in science, we are not just training in science.

LECOMTE

Permettez-moi, Monsieur le Président, de revenir un peu sur la discussion d'hier. Je voudrais répondre à Monsieur Roche sur la question qu'il avait posée: « Qu'est-ce que la science fondamentale? » et je pense que la réponse avait été donnée partiellement par Monsieur Herzberg disant que c'était pour la gloire de l'esprit humain. Mais il me semble que nous avons aussi une réponse négative dont nous voudrions tirer bénéfice. C'est celle que donnait Aldous Huxley dans « Brave New World »; et effectivement l'administrateur de « Brave New World » explique qu'on ne fait plus de recherche fondamentale, pour une raison très simple: c'est qu'elle ouvre sur l'absolu et que l'absolu est redoutable pour une civilisation qui ne tend que vers le bonheur physiologique de ses sujets. Ceci est très important, car Aldous Huxley voit toujours très bien les choses. S'il existe à l'heure actuelle dans certains pays une certaine peur de la recherche fondamentale, c'est parce qu'elle débouche sur l'absolu, et elle correspond à un besoin fondamental de l'être humain, qui est la vraie gloire de son esprit, c'est-à-dire l'admiration devant les merveilles de la création. Et en entendant discuter Monsieur Dirac ou Monsieur Colombo, il me semblait que ce qui nous touchait c'était justement que nous pouvions accéder à cette admiration, qui est, je crois, le sommet de l'esprit humain quand il s'agit de la science. Et c'est peut-être celà la meilleure définition de la science fondamentale: c'est celle qui admire la création et, si j'ose dire, l'ingéniosité du Créateur.

CHAGAS

I believe that this concept of the presence of admiration in scientists for other works is a very important one and I was sorry that we did not hear, when we spoke about the formation of young scientists and so forth, something which I think is very important for young people: the joyous sense of research, which I think is a fundamental element in research. I always tell my students: when you lose this sense, you are also lost to research.

If there are no other interventions on the subject, then I think we should discuss a bit what Dr. Pavan asked: what will be the role of the Academy? What is the role of the Academy? I think that I would come back to what Dr. Lépine said in his last intervention. The fact is that we are speaking here for people who have the same ideas — and this is really the core of Pavan's question: how can we get outside of the walls — and this is an important question. I always remember, in '56 or '57, when I gave the inaugural lecture at the session of the Brazilian Society for the Advancement of Science — this was held in a very beautiful small town in Brazil — this time the meetings were quite easy to attend, we had only 200 or 250 people, and I made a speech about the place of science and what should be the role of science and how it should develop in Brazil. At the end, when I looked at the audience, I saw that I was speaking to people who were absolutely convinced about what I was saying, so that my talk had really no significance at all, I was speaking to people who were absolutely sure of what I was saying. But how to reach the politicians, the government, and the society in general? I think the question of Dr. Pavan is extremely well placed. We have had here a precise question, a proposition of Dr. Rich. The only thing on which I would correct him is that if we translate it to Portuguese and Spanish, it would make OLABIM and not LAMBO: Organización Latino Americana de Biología Molecular. Well, the fact I think is that for all developing countries exchange of scientists within the countries and the workshops held in the countries are very important and should be increased as much as possible. I think this sort of initiative is going on and should be increased as much as possible.

Secondly, we have heard from Professor Garnham the wonderful experience which has been the sort of scientific venture at the E. Chagas Institute in Belem. It has been a wonderful experience on the basis of what we might call a scientific cooperation in which the participants of both sides had something to do. I think that many other examples like that could be found about Latin America, about certain countries of Asia. Most unhappily, much less in African countries, but even so, in Africa it was possible to reach a superb organization, which is the Institute for Plant Physiology and Ecology, in Kenya. This was an outcome, of the discussions I held between T. Odhiambo who was the forceful man behind it (one has always to have a national person, a person from the region who pushes it) and Carol Williams, at a meeting of the Committee for the

Application of Science and Technology to Development, which was held in '65 or '66 in Ethiopia. I must say that I think the idea of regional institutes is a very interesting one, mostly as Dr. Rich said, that it has the physical basis, it has to be established after an experience of interchange and courses, and so forth. I think that in Latin America many workshops are held under the auspices of international organizations or with the patronage of the host country also. I have also seen ideas which were never developed. For instance, in '46 Professor Szent Györgyi proposed an international institute for brain research; during a lecture he gave at the Collège de France when we were all meeting just after the War. I think on the occasion of the 50th anniversary of Pasteur's death. Szent Györgyi gave a wonderful lecture and he proposed exactly at this time the lines on which CERN was afterwards produced. The maximum we reached in the international field for brain research is the International Brain Research Organization (IBRO). On the contrary, the sister organization, International Cell Research Organization (ICRO), has been much more successful, making it possible that our meetings could really indicate not only our ideas but also our wishes. I think that in the field of international collaboration one should always quote what has been done in astronomy. I think the two big observatories which are in the Andes in Chile, are a good example of international collaboration, where that is really collaboration and not simply the use of the particular opportunity. This has not been done in Bolivia where a lot of research was done by outsiders, where national talents were developed.

LEPRINCE-RINGUET

Qu'est-ce que nous allons faire de toute cette discussion et de tout cet échange d'idées — je dirai entre personnalités de toutes nations? C'est assez rare quand-même — c'est une chose assez exceptionnelle, et je dirai que pour une fois nous avons un sujet sur lequel tous ont à dire quelque chose, tous ont à parler. Alors, est-ce qu'il ne serait pas très utile de la faire connaître pour le prestige de l'Académie Pontificale d'une part, mais surtout pour la formation des personnalités scientifiques, et des personnalités politiques des différents pays? Est-ce qu'il ne serait pas très utile actuellement, en particulier dans cette période très difficile, cette période de crise, d'affirmer un certain nombre de choses sur la recherche fondamentale, sur la formation des chercheurs, sur le budget, sur la transformation et sur la collaboration internationale? On peut faire deux choses: faire des comptes-rendus de tous nos débats — c'est une chose qui peut se faire, qui se fera probablement je pense, mais qui ne touchera pas énormément de monde — ce sera un document, un document historique peut-être. Ou bien alors on peut faire également un petit fascicule qui ne devra pas avoir plus de quinze ou vingt pages au maximum et qui reprendra, à l'occasion de nos réunions de ces jours-ci, un certain nombre de données, de données fondamentales sur la recherche de base, sur son indépendance, enfin, sur tous les problèmes qui ont été abordés et qui seront encore abordés ce soir. Un document qui, répandu largement en tous les pays du monde, avec la signature des membres de l'Académie, pourrait avoir une influence dans les cercles internationaux et aussi auprès des gouvernants ou des responsables des différentes nations. Je crois que ça serait très utile, de faire une oeuvre efficace à un moment comme le moment actuel.

SELA

The EMBO started on the proposal of a group of biologists who presented a list of 100 names. These people until now never met at EMBO. EMBO never spent one penny on convoking its membership and discussing. Everything has always been organized by postal ballot. It is only when they define their wishes and their desires and what they are for that they are a group that wants actually to go ahead and move things and not just to be a learned society. They have some very precise goals, they were at that stage very concerned that the stream of funding money from the United States to Europe would stop. They tried to think about it six years ahead of time. Now, one of its original purposes, which was funding of research by the way, never materialized. They approached some non-governmental agencies and, with the exception of one government that gave them some money, they had to begin with money from the Volkswagen Foundation, and this permitted them to start and show what is going on. Within a couple of years, using the model of CERN, the Swiss Government invited other governments, and that is how it started. What I wanted to say is, it was pragmatic from the beginning and was made possible by the initiative of several good scientists and a minimum of organization and bureaucracy.

KHORANA

Since we are going more and more deeply into this question, I think I would like to try to reclarify what seems to me the basic issues that we have been discussing. The questions that I am going to enumerate, I guess, start with Baltimore and I would like to bring in Döbereiner's thoughts and also Pavan's thoughts. Myself, having started out as one of the frustrated students that Professor Siddiqui would certainly know about, I like to try to think that we should discuss. The first problem is: how can the excellence of universities in developing countries be improved and what measures can be recommended for that purpose? Now we have discussed, in regard to that, for example exchange of professors, scholars and courses in the different countries; and the next idea was the development of international laboratories. I think these are all fine, but I still think that the next most important question in this is: how do the developing countries train their young people, and what becomes of them when they come back? And finally this is to be somehow interwoven with the question as to what kind of research they are going to do when they come back, because the kind of research they do in the frontier areas they obviously cannot continue — not very easily and not in most cases — and this actually does lead to frustration, because any field that is active and developing very rapidly is also very competitive. Therefore the people, students that go back, really have no chance of trying to compete. The only important kind of thing that they have learned is the basic way of looking at problems, as in molecular biology, or techniques; and then the question is: what other questions or problems they could work on, and here I think I do like to support naturally Dr. Döbereiner's thought that somehow this basic science should be able to involve young people for the problems that actually are more specialized — agricultural problems for example, and so on. Natural products I think in India for example has been a very fertile field for young people. Anyway, those are the questions I thought I would like to at least clarify for my own purpose.

CHAGAS

Dr. Khorana, I think that the prevailing idea is that basic science is mostly a tool for the formation of younger scientists, as Döbereiner said, and I would say that if we are steps behind developed countries we can nevertheless use those tools when the applications in the developed countries are fruitful. This is particularly the case I would say in modern biology. I think of the example I gave of progress made in Chagas' disease, for instance, by the use of methods of modern biology. It is quite clear that the tools are not discovered by young people of Brazil but they are using them with very interesting results.

DÖBEREINER

Maybe we should clarify better that we are talking about two things, I think, and we should separate them. We feel that there is a strong interest in having all these discussions brought to the politicians and to the other people, and I think we should make a summary and perhaps we could suggest forwarding the summary of the conclusions which include all the discussions, especially (what we all felt so strongly about) on the ratio of money spent for research against money spent for administration. These questions I think all of us feel should be summarized and then sent to the Academies and to different research councils and to universities, perhaps to a larger number of people responsible for research in the various countries. This would be one point on which I think our President could take the initiative and which should be separated from the point (on which we still do not have a complete proposal) regarding the creation of a central institute which could help to create the critical mass of people we need in order to start or to intensify basic research. If this institute is located in Latin America, it could of course be better oriented to Latin American problems and to tropical problems of Latin American and African countries. Such organization could be very useful, but we need someone to take the initiative to start because it is really important.

PAVAN

About the proposal of Dr. Rich I think it is very good and I like it very very much. I think it is easily feasible and I would not call it a laboratory or an institute or something like that; I would say an organization like EMBO, and I would not centralize it anywhere in Latin America. My suggestion would be to indicate those laboratories which already have some people at a certain level and which could be used for centers of preparation of new people with the participation of visiting professors from abroad. I am very much concerned about what Dr. Khorana said in relation to what a student, who goes to learn and comes back to his country, is going to do there. This is one of our problems, and I think the Rockefeller Foundation in that relation was perfect because what they did was to send a young man abroad, and when he went back it gave him at least the minimum necessary to continue the work he was doing. Of course they were very sensible not to send a man to a place where there was a big computer saying — "Now, I will give you a computer, in Latin America". That is not the case; but anyway they would give to the people when they returned at least the minimum necessary to do the work. Unfortunately, this is not what our Government is doing. The people get very enthusiastic about the idea of sending people abroad, and they think they are doing all. Well, they did only the beginning, and we tell them: "Look, that is not the way to do it". But I would say that if we do have this kind of organization, it would be very, very important, not only for my country but for all the Latin American and any other countries that would be involved in this program.

I would say that one thing that we could do — and there is a book published hy Virginia White, called *Grants*, and in this book she mentions many thousands of foundations that are involved in giving grants. Perhaps what we could do is just search for some of these foundations to help us start to build up this type of organization... Let us take as an example: one experience would be molecular biology, to help us to start the thing. And I would say it would be not difficult at all if we have the names of the people in this Academy signing a paper saying: "Look, this is important, at least we think this kind of thing would be important", I am sure we would find easily some foundation to help us to organize it. I would say that for this type, you do not need a lot of money at all.

The question for us in Brazil, and I would say in Latin America, the important point is contacts. We are very much isolated. And you see we are isolated not only in relation to people, but we are isolated in relation to libraries — we have very few good libraries there, and when we receive the journals, often they are six months late or a year late. That does not make much difference for us, but of course for young people who are enthusiastic and want to know not what happened yesterday, but what will happen tomorrow already, then this would them certain disadvantages and they lose interest.

BALTIMORE

It seems to me the next step pretty well has to be the identification of a group of people from Latin America (and I guess we have by default focussed in that area) who are interested in carrying us forward, because although Dr. Pavan's very wonderful enthusiasm might carry *him* to make necessary contacts, I am not sure that it is always true. A procedure that would guarantee if anything is going to happen out of all this, that it does happen, would be to identify a group of people who would find this an attractive idea. I would be curious to hear from the other people here who represent Latin America. There are, I notice, seven members of this Academy from five different South American countries, although not all present here. That represents already a significant although small group (something like 100 sounds like what one needs) of people who at least express an interest. There are certainly a fair number of people who were born and brought up in South America who are now resident in the United States. Many of those might find this an attractive way to involve themselves in their parent countries.

So what one needs I think is some way to bring together energies. The couple of times I have talked about proposals of this sort were with people from South America and from Central America. I have always had an enormously positive response but this same sense of "but where do you start?" and I have never met any of these people before. I do not know what you are talking about. So it takes some people with a large scale international connection within Central and South America who can identify and get people in touch with each other to start generating a little enthusiasm, and I think in a period of six months or a year you could hope to generate enough enthusiasm to develop a proposal of some specific sort. I would say as a guess that developing the necessary funds for something, or at least the beginnings of something would be a relatively easy job; if nothing else, the Organization of American States would probably be more than happy to provide the money because this is just the kind of thing they talk about at least.

CROXATTO

One problem which worries me very much, looking at the problem we have in Latin American countries, is dealing with a vicious cycle. We have not a very well developed science because we are a developing country, and we are a developing country because science and technology are not developed. How to break this up, that is the main point. I consider that one of the most difficult things to solve, at the present time, is the critical mass of scientific people working in our countries. I would say that 90% or more of the scientific people in Latin America are working in the universities. There are only a few people who are doing important scientific work in private institutions. Society is supporting universities and I think universities also have something to do with development. I am troubled because the critical mass of scientists we bave in South America is too low and most of the people doing research are doing also teaching and administrative work and do not care very much about the problems of development. There is practically no national program for development. We do not know priorities, what to do in our countries in order to increase our economic development. I think it is a task for the scientists to take care of the problems dealing with development. My feeling is that it would be very difficult to expect from the government initiative for the development of the university and to increase the amount of scientists in the university. For this reason I think everything that can be done in order to accelerate the role of this critical mass will be of great benefit for science

I was suggesting to my colleagues to do something in order to participate in a program of economic development. I think it is a great responsibility of the scientist in these undeveloped countries to take care of this problem: we do not grow scientifically because we have no economic support. In our country, where there are still so many people who have no food, there are so many important problems to solve, there is really only a small chance that the government can provide much money for a rapid development of our scientific level. That is for me a very important point and I suggest that any initiative which can help the scientists of the university to participate in some program of national development will be very helpful. In some countries in South America, I know they have tried to organize big programs, but the scientists were not consulted, only bureaucrats and politicians were involved. I think it is a very important responsibility for the scientist to take care of the problems of human development. A document delivered by this Academy evidently can help a great deal. I think it would be of very great importance just to elaborate some document, or at least to prepare in a summary way a document which can be delivered to authorities which are dealing with development, with economic and social development in our undeveloped countries.

Roche

Mr. President, I want strongly to support the suggestion of Dr. Rich regarding a Latin American molecular biology organization, as commented upon by Dr. Pavan.

Ubbelohde

I think we want to separate issues, as Dr. Döbereiner says, very carefully here, because we are speaking as an Academy, perhaps the only international Academy but one which comprises a great number of scientific disciplines, and one must be very careful to separate issues which are general to science and which are therefore perfectly proper to engage the Academy as a whole, from issues which are exciting and captivating, but which certainly only lie within the competence of a very few of us here; at least I speak for myself. Therefore we must clearly separate — we must not be found, as it were, supporting a very particular suggestion, attractive though it is, such as that made by Rich and others. We would encourage a small group of our members to go ahead privately, and the form of this encouragement, it seems to me, would follow what Dr. Leprince-Ringuet suggested, that is that we draw up a short résumé of the broad issues of principle which are the outcome of our deliberation. If you like, we can at the most illustrate these principles but this must be kept quite distinct, because some of us have a lot of experience with the politicians, and they are only too keen to laugh at incompletely documented activities, and this really defeats an objective rather than strengthens it. I repeat: we are an Academy, an international Academy covering the whole of science and it would be very proper to have a résumé of broad principles of the kind we have been discussing this week. I think it would be very effective — quite short, though — and these broad principles could be used; but as for our getting more than a general blessing for specific activities, however exciting they are, I would prefer a contract, if I may say so, but I am sure all of us would feel like this. I think Dr. Döbereiner is quite right: we have to separate special issues from general issues. Otherwise we are defeating our own objectives.

ROCHE

I am reminded that many of the points which we have now discussed here were discussed at length at the United Nations Conference on the Application of Science for the Benefit of Developing Countries, held in Geneva in 1963. Now it so happens that in the follow-up of that conference, in which the participants were scientists, the United Nations is arranging a Conference on Science and Technology for Development in 1979. The participants in this would be just politicians and government delegates from the various countries to which we want to send the summary of the recommendations. I wonder if it would not be possible to send this summary as a sort of memorandum for the consideration of the United Nations Conference. I might also say here that the Pugwash organization decided, at the conference held in Munich last year, that they should diligently work out a summary of recommendations as an input for the consideration of the 1979 U.N. conference, and they actually held in January last year in Delhi a Pugwash meeting to draw up the recommendations. So I was wondering whether something like this could be done by the Pontifical Academy of Sciences.

III

THE LIMITS OF SCIENCE

THE LIMITS OF SCIENCE

VICTOR F. WEISSKOPF
Pontifical Academician
Massachusetts Intitute of Technology
Cambridge, Mass., U.S.A.

In the last decade, the limitations of scientific inquiry have been in the center of discussion. In the past, say before 1950, there was no question in the mind of the great majority of the public that the progress of natural science and of technology is for the good of mankind. The more we know, the better off we are, the more we can do with nature, the more we can increase the comfort, safety and length of our lives.

The doubts have started, not because of the use of science and technology during World War II. There would have been good reasons such as the mass bombings, carried out with modern airplanes, electronic guidance equipment, and, of course, the use of the nuclear bomb, a result of the application of the most advanced achievements of nuclear physics. No, the doubts about the desirability of scientific and technical progress developed from different sources. It is hard to recognize clearly what precipitated the feelings against it. Part of it was a certain saturation of the desires for material comfort in the developed parts of the world. Anyway, suddenly people became aware of the negative sides of the development, such as air and water pollution, ruthless exploitation of raw materials such as timber, oil and minerals, and the destruction of the natural landscape by growing industry and construction.

We restrict our discussion to the questions regarding the limitation of scientific inquiry and do not discuss the equally serious problems of limitations of technology. One can distinguish three different sides of the problem.

A) The first reason to put restrictions to scientific research is the fact that certain methods used in scientific research can become dangerous, not only to the active scientist but also to other people. Trivial examples are work with explosive chemicals, or work with radioactive substances. Clearly the scientist must restrict his experimentation so that no harm is done to other people; for example, he cannot throw radioactive by-products out of the window. These are obvious housekeeping tasks which every scientist is aware of. There is no reason why potentially dangerous experiments cannot be performed if adequate safety measures are taken. Here a problem arises: What authority decides the adequacy of the measures? The scientific community or some public authority? In the past it was always left to the scientists, but recently the problem became more acute when the biologists hit upon the recombinant DNA, a method to produce new types of bacteria. In this case the problem of housekeeping may become rather difficult, because of the well-known ability of living matter to multiply exponentially from the tiniest quantities that might escape from the laboratory. But it is still a housekeeping problem. In my opinion, it should be left to the scientists who, at least up to now, always have shown utmost prudence in such situations. Indeed the first allusion to the possible dangers of recombinant DNA originated from a group of biologists who proposed and kept a moratorium of such experimentations until more is known and discussed in regard to the situation.

B) The second reason for restrictions is a more complicated one. It may happen that the results of a certain line of research could lead to dangerous consequences, to new types of weapons, to new means of domination over a population, for example, by mind control; the further development of gerontology may lead to too many old people. There are many examples of that type. Somehow the common idea is this: We have already accumulated too much knowledge for our own good, and we have created much damage. Let us restrict further searches, in particular those where the dangers of abuse are evident. But are they ever? There exists hardly any line of basic scientific research of which the results are predictable.

Any successful step in science has brought about unexpected results; indeed the unexpected nature of the results is characteristic for a successful step. If the character of the results could be foreseen, it usually is a routine investigation or a piece of applied science.

Limitations in respect to detrimental applications of science are certainly in order. There are two kinds of harmful applications: intended ones, and non-intended ones. The first kind is the application of science to the development of new weapons. Here limitations certainly are necessary, but past history has shown that the political forces are usually so strong that limitations are hard to maintain. This is a serious political problem. The present arms race between superpowers should be stopped but how does one go about it?

The unintended harmful applications come from human shortsightedness or simply from our imperfect understanding of the processes. Pollution, destruction of the ozone layer, increase of CO_2 in the air are examples. Limitation of this type of abuse would be possible but is also difficult because it interferes with powerful commercial or political vested interests.

Now back to the problem of limitation of basic science. Past experience has shown that it is rarely possible to predict the practical applications of a scientific investigation before it is completed. Therefore it is impossible to establish limits to basic science on the basis of what will result, because we don't know beforehand. Furthermore, even if a discovery can be applied to practical purposes, it could be used for beneficial purposes or abused for detrimental ones. In general the beneficial uses are by far greater than the abuses. However, in the last decade the emphasis has been laid upon the abuses, such as biological warfare, electronic guidance for nuclear missiles, allegedly unsafe technical devices like nuclear power plants and chemical industries. Some of these abuses are deadly serious, in particular the development of nuclear weapons and their delivery; others, as the unsafe devices, could be made safe by improved care and study. Still, we believe that, in balance, the beneficial applications of basic science have been much greater than the abuses, in the past and probably will be in the future. One only needs to remember the improvements in the way of life of a large part of humanity by communication, by the replacement of manual labor with mechanics;

by the development of agriculture, which has made it possible to feed all people adequately in principle, although not in reality; by the results of medicine that doubled the life span and succeeded in many, but not all, respects to abolish pain. These breathtaking developments caused such a change in the patterns of life, that the social institutions suffered terrible shocks. They were initiated in a different age and humankind has not yet adjusted to the new era, which developed too fast: we had not enough time to arrange our social relations accordingly; we were not intelligent enough to foresee many detrimental consequences of this development.

On all these grounds it is very doubtful whether limitation of basic science makes any sense, except under the assumption that today we know enough anyway, and any possible beneficial results of future research are no longer as urgent as the necessity of avoiding the abuses. Such a conclusion, however, overlooks the fact that much more applied science will be necessary to protect humankind from the unintended harms that previous applications have produced. We need to know much more in order to avoid the mistakes of the past and correct the damages.

C) We now come to the third kind of limitations of science. Here it is not a matter of imposing limitations, but of determining the actual limits of scientific inquiry. Are there parts of human experience that are outside of any possible scientific explanation or recognition?

In view of the tremendous success of natural science in all its branches one may come to the conclusion that there are no limits to the scientific understanding of any phenomenon or of any human experience. It can be maintained that, at least potentially science can justifiably claim the ability to understand every observable phenomenon. The term understand should mean a general recognition that the phenomenon fits into the framework of science, that it is "demystified". We are, of course, far from this state of knowledge, but many previously mysterious processes are now understood, and it is reasonable to predict that no observable phenomenon need be considered alien to, or beyond the reach of, science, even though we may never actually achieve that complete degree of understanding. This statement may or may not be correct, but for the following discussion it is useful to assume it to be valid. However, implicit in this claim to completeness is a very important qualification. If we ask the question, can, does, or will scientific insight cover every aspect of human experience? the answer must be negative. To show that this statement does not contradict the completeness claim, let me give a simple example.

A Beethoven sonata is a natural phenomenon that can be analyzed physically by studying the vibrations in tbe air; it also can be analyzed chemically, physiologically, and psychologically by studying the processes at work in the brain of the listener. However, even if these processes are completely understood in scientific terms, this kind of analysis does not touch what we consider relevant and essential in a Beethoven sonata — the immediate and direct impression of the music. In the same way, we can understand a sunset or the stars in the night sky in a scientific way, but there is sometbing about experiencing these phenomena that lies outside science.

We face a similar situation with respect to problems of ethics and personal relations. There cannot be a scientific definition of ethical right and wrong, of good and evil, of dignity and humiliation, or of concepts like the quality of life or happiness. While it is certainly possible to analyze the nervous and psychological reactions that occur during the process of experiencing such ideas, there remains an important part of the experience that is not touched by this analysis. In the same way, one can scientifically analyze love and hate, human aggression, or intellectual abilities of different races, but the scientific results may not be the most relevant ones for human social problems and may even be counterproductive for the solution of these problems.

As always in problems of this kind, the actual situation is not as clear-cut as the way it is expressed for the sake of clarity. I do not want to deny a certain continuity of thought across the borders from the scientific to what is often called the humanistic. There are many ways of speaking about human reactions to a sonata or to an ethical problem. The language and content of such statements may appear fragile and indefinite when judged by the peculiar requirements of scientific intercourse. Nevertheless such statements may be most lucid and concise when judged by their own intrinsic scale of values.

In a work entitled "Tractatus Logicus Philosophicus", the positivist philosopher Wittgenstein presents a logical system for understanding the conclusions and structure of scientific and mathematical analysis. The seventh chapter of the book contains only one sentence: "What we cannot speak about, we must pass over in silence". In another context, he made a similar statement: "There are indeed things that cannot be put into words. They make themselves manifest". Such experiences cannot be delineated in scientific terms — this is his meaning of "words" — but they can be portrayed in music, literature, and art. The same idea was expressed by Beaumarchais in a more cynical vein: "Whatever is too stupid to be said can always be sung". Especially in human relations, a piece of art or a well-written novel could be much more revealing than any scientific study. In many respects, "Madame Bovary" is a piece of sociology — in fact, better sociology than much of what is done by aping the techniques and language of the natural sciences.

In other words, although science can study and may be able to explain every human experience, it does not always illuminate those aspects that are considered most relevant. There are limits to scientific understanding; to say that science is complete is not to say that it is all-embracing. Let me point to an important analogy in atomic physics — Niels Bohr's description and the quantum properties of an atom. In the classical sense, the atom is a small planetary system of electrons revolving around the nucleus in well-defined orbits. This view cannot be disproved by experiment; it is possible to determine the actual location of the electrons in the atom by using the finest means of observation, but the very act of observation will destroy the quantum state that is essential for the atomic properties.

Just as the quantum state is destroyed when observed with some sharp instrument, so too the significance of certain experiences, especially those relating to art, ethics, and human relations may yet be lost when subjected to scientific analysis. In mythology, one finds numerous references to such complementarity. In German mythology, the god Wotan asks Erda, the goddess of the Earth, for the gift of infinite wisdom; in return he must sacrifice his eye. To know everything costs something.

There is a certain resistance in the human mind to the recognition of different and complementary aspects of experience. It was aptly expressed by Marcus Fierz, the Swiss physicist-philosopher: "The scientific insights of our age shed such glaring light on certain aspects of the experience that they leave the rest in even greater darkness". This is a serious danger; whenever one way of thinking is developed with great force and success, other ways are unduly neglected. There is a certain superiority claim, rooted in the human desire for clearcut and universally valid answers, that tends to exclude other ways of approaching a subject. We have seen this in areas other than science: in the year 1054, during the height of religious belief in Europe, a supernova appeared, brighter than any planet. It lasted for three or four months, yet not a single chronicle in Europe mentioned this phenomenon. The appearance of a bright star in the Middle Ages, when the religious approach was overwhelmingly strong, was not considered a relevant fact worth registering.

On the other hand, the one-sided religious emphasis of the Middle Ages, and the equally one-sided scientific emphasis of our time, have released creative forces of tremendous power. Think of the medieval creations of art, architecture, and moral philosophy, and also of the development of science, natural philosophy, and technology in our era. At the same time, however, as one-sided approaches, both have also produced serious abuses. In the Middle Ages, one must point to the Crusades and to the complete neglect of corporal suffering; in our time one finds a serious neglect of human values with respect to the quality of life and in political decisions and an excessive concern with the value of material goods.

As usual in the history of mankind, each emphasis has been distorted and used as means and reason for wholesale murder and destruction. Think of atomic bombs and electronic warfare today — and of the reply of the papal legate Abbot Arnoud de Citeaux when he was asked what to do with the population of the town of Bezier after it was successfully occupied in the year 1205: "Kill them all, God will select those who should go to heaven and those who should go to hell"!

It must be pointed out that science itself has its roots and origins outside its own rational realm of thinking. In essence, there seems to exist a "Goedel Theorem of Science", which holds that science is possible only within a larger framework of nonscientific issues and concerns. The mathematician Goedel proved that a system of axioms can never be based on itself: in order to prove its validity, statements from outside the system must be used. In a similar manner, the activity of science is necessarily embedded in a much wider realm of human experience. Science itself must have a non-scientific base: it is the conviction of every scientist and of society as a whole that scientific truth is relevant and essential.

This emotional and social embedding of science is the pre-condition of the quest for scientific truth. There are also emotional and nonrational aspects present within the scientific enterprise it-self. Intuition and irrational reasoning play an important role in research; everyone who has worked in science knows the joy of insight, the tremendous emotion, the deep awe he has felt upon discovering a unifying law that contributes to the understanding of the scientific edifice. Keats wrote in one of his poems: "There was an awful rainbow once in heaven / We know her woof, her texture / She is given in the dull catalogue of common things". No true scientist could agree with this view. On the contrary, the scientist would claim that knowledge of the nature of a rainbow strengthens rather than diminishes the emotional impact of the phenomenon.

The intrinsic value of science would be enhanced if both scientists and nonscientists were more aware of the other ways of dealing with human experience, such as art, poetry, literature, and other forms of expression or impression, some of them analogous to religion and mythology. If this awareness were fostered, the pre-judice against science and technology would lose much of its force. To a great extent the prejudice is based upon a halfconscious resis-tance against an implicit claim that only the scientific approach is legitimate and reasonable. The recognition of the validity of other modes of thinking would preclude the blossoming of such pseudo-sciences as astrology and ESP, which are the result of suppressed natural urges that assume perverse forms because at the present time the scientific approach is considered to be the only "serious" way of dealing with the world around us.

Human experience encompasses much more than any given system of thought can express within its own framework of concepts. We must be receptive to the varied, different, and apparently contradictory ways of the mind when we are faced with the reality of nature, of our imaginations, and of human relations. There are many ways of thinking and feeling: each of them contains some parcel of what we may consider the truth. The recognition of the multifaceted character of our relations to each other and to the rest of nature is a necessary step toward groping with the problems of life and toward fathoming the potential greatness of human existence. Science and technology comprise some of the most powerful tools for deeper insight and for solving the problems we face — some of these problems, indeed, were created by the thoughtless applications of those very tools. But science and technology are only one of the avenues toward reality: others are equally needed to comprehend the full significance of our existence. We will need all approaches to deal with the predicaments of humanity that prevent so many of our fellow beings from having a life worth living.

DISCUSSION

Leprince-Ringuet

Je dirai que j'ai été très heureux d'entendre cette présentation de Victor Weisskopf, mais je voudrais — je ne dis pas la compléter — mais donner quelques variations, car ce sont des problèmes auxquels naturellement j'ai pas mal réfléchi, ayant été pendant assez longtemps Président de l'Union Catholique des Scientifiques Français. Par conséquent le problème: Est-ce que la science résoudra tout? Est-ce qu'on peut vivre une vie avec une pensée religieuse en étant en même temps bon scientifique? C'est un problème qui s'est posé tout au long de mon existence. Alors je voudrais d'abord dire à Victor Weisskopf que je suis pleinement d'accord avec son analyse.

Je pense que le domaine de la science est immense, tout ce que la science peut appréhender, elle l'appréhende, ou elle l'appréhendera; il y a une éthique de la science, il y a aussi une pensée d'ensemble des scientifiques, à savoir que la science avec son langage universel, indépendent des ethnies, indépendent des pensées philosophiques, est vraiment un langage qui unit les hommes, si bien que beaucoup de scientifiques pensent que la science doit unir les hommes alors que les religions les divisent. La science a pour base la méthode rationnelle; elle n'a pas que la méthode rationnelle pour base, puisque, comme Weisskopf l'a dit très bien, il faut aussi de l'imagination, de l'intuition, qui ne sont pas du domaine du raisonnement rationnel. Mais la base est vraiment une formation de l'esprit à une certaine logique, une formation rationnelle, et il est certain que la science s'est développée dans les pays dans lesquels cette formation rationnelle existait, et dans certaines régions du monde où il n'y a pas une méthode rationnelle, je dirai inscrite encore au programme; et bien, la science ne se développe pas et les techniques subséquentes ne se développent pas non plus. Donc cette méthode rationnelle est-ce qu'elle embrasse toutes nos activités? Bien sûr que non, et Weisskopf l'a fort bien dit, en donnant l'exemple de la Sonate de Beethoven. Je crois que l'on peut dire ceci: c'est que dans notre existence tous les choix que nous faisons — je dirai à toute occasion — le choix d'un parti politique, le choix d'un camarade, d'une fiancée, d'un époux, d'une femme, le choix d'un livre, d'un voyage, le choix syndicale, tous ces choix ne sont pas, n'ont pas comme base, un raisonnement scientifique. Ils manifestent notre personnalité très complexe — je dirai que nous ne sommes pas capables d'analyser, mais ce n'est pas un choix scientifique. C'est-à-dire que dans l'existence il y a beaucoup de réalités qui ne sont pas des réalités dont la base est scientifique. Pour celà je dirai qu'il faut prendre les choses telles qu'elles sont aujourd'bui, actuellement.

On peut dire: mais votre liberté, vous croyez avoir une certaine liberté; en réalité il est possible que vous n'en ayiez pas. Le developpement de la science progressivement va permettre de connaître mieux les mécanismes, vos mécanismes intérieurs, va permettre même de savoir pourquoi finalement vous réagissez à telle Sonate de Beethoven d'une certaine façon et au contraire à tel morceau de Ravel d'une autre façon. C'est possible, mais je ne crois pas que l'on puisse affirmer, au nom de la science, qu'il en sera ainsi. Et l'expérience que peuvent avoir les anciens scientifiques va dans ce sens. Lorsque, par exemple, en 1934 et '35 on supposait que l'on connaissait toutes les particules de la matière — il y avait le proton, le neutron récemment découvert, l'électron, le photon, le neutrino — et avec ça l'on construisait tout. Et bien, plus on a cherché, plus on a expérimenté, et plus la complexité s'est manifestée. C'est-à-dire que plus on cherche — et je crois que tous les scientifiques sont d'accord pour considérer cela — plus la nature découvre des complexités nouvelles, si bien que, actuellement, la physique des particules est infiniment plus compliquée qu'elle ne l'était en 1934. Ce qui était ce que l'on pouvait considérer comme découvrable l'a été, mais on a trouvé beaucoup d'autres choses auxquelles on ne s'attendait pas.

J'ai l'impression que ça doit être aussi la même chose dans l'étude de l'homme, dans l'étude de la biologie, et que, par exemple, le cytoplasme devient maintenant une véritable cité, avec des tas d'usines qui servent à détruire, à construire, à envoyer des messages, etc., alors que autrefois, du temps de ma jeunesse, c'était une sorte de masque confitureuse sans structure, et si on fait des progrès on découvre aussi des choses beaucoup plus complexes. J'ai entendu dire que les possibilités de connexions entre neurons étaient maintenant beaucoup plus compliquées qu'autrefois, et cela n'est pas terminé. Je crois par conséquent que l'on peut dire que l'on fera des progrès, on en connaîtra plus, mais cela ne veut pas dire que l'on déterminera finalement complètement un individu, pas plus que l'on déterminera probablement les constituants fondamentaux de la matière; si bien que si on dit maintenant, vous considérez que votre attitude n'est pas scientifique et que vos réactions ne sont pas scientifiques, mais plus tard on vous montrera que ça l'est, je ne pense pas que l'on puisse affirmer cela sur le plan scientifique, et je crois que, ultérieurement, même toujours probablement, dans la suite des évolutions de l'humanité, je pense que, ultérieurement, il y aura toujours des manifestations de la personnalité qui ne seront pas du domaine rationnel, qui ne correspondront pas à une connaissance scientifique réelle, et je dois dire, heureusement pour nous.

Et alors, ce qui me paraît très important c'est que dans ce domaine, qui n'est pas irrationnel (puisqu'on considère que les choix que l'on fait sont raisonnables), mais qui n'est pas rationnel non plus (donc je dirais non-rationnel et non pas irrationnel) on peut placer, comme l'a dit Victor Weisskopf, l'amour, la haine, et également aussi un désir de donner un sens à son existence: faut-il être bon, faut-il être mauvais? Et toute l'option religieuse intervient dans ce domaine, je dirais dans la mesure où la frange qui peut être en contact avec le donné scientifique ne soit pas en contradiction avec le donné scientifique. Alors tout ceci me semble à moi actuellement, après les réflexions je dirai assez longues sur ce sujet, après des contacts avec des théologiens et des philosophes, me semble valable pour moi — c'est une construction qui me satisfait et qui laisse à la science toute son importance — qui est énorme et considérable, et je dirai qu'il y a pour elle un certain infini en ce sens que tout ce qui est accessible par la science sera pris par la science. Mais je crois qu'il est impossible d'affirmer que tout dans un temps lointain sera du domaine de la science, même notre définition de la liberté et de notre comportement, tout ceci me semble, finalement, être tout à fait en accord avec l'exposé de Victor Weisskopf. Et je le remercie non seulement de la pensée personnelle; c'est-à-dire la personnalité de chacun de nous intervient dans (il y a aussi des instants de vérité qui ne sont pas des instants de vérité scientifique) un regard d'amour d'une mère avec son bébé nouveau-né. Il me semble que ces choses ont une réalité qui n'a rien de scientifique mais qui est aussi forte que la connaissance de l'existence d'un neutron. Voilà donc une première réflexion que je voulais faire sur cette partie, cette troisième partie de ce que Weisskopf a indiqué; et peut-être tout à l'heure si on a le temps on reviendra sur les autres parties.

BALTIMORE

I unfortunately, cannot follow French well enough, so I am not exactly sure where what I am going to say stands in relation to what you just said. But I want to take issue in a sense with the third part of what Weisskopf said, at least for the sake of argument if for nothing else. The assumption that he is making is that — to take three examples — a Beethoven sonata or an ethical problem or a sociological analysis cannot be understood in the way we understand the structure of the atom or a biological system, cellular system if you wish. I doubt that. For instance, I would guess — and this is only a guess to give a flavor of the kind of explanation that could be possible — that there may be kinds of harmonies between nerve circuits in the brain and the music of Beethoven that give it the sense of being so attractive to human beings. After all, it is very hard to figure out why Beethoven is different from Ravel or better than a lesser master, but the fact that it is is hard to figure out. I would be very surprised if some day we do not find a way to understand what it is about music that is attractive to human beings.

The same thing is true about ethical problems; I think most ethical problems can actually be reduced to the point where one can begin to get an idea of where people's feelings, if you wish, come into the discussion, and where logical analysis leaves off. And that is, in a sense, the gut feelings become the axioms of the situation, and then you have to ask: where do people's feelings come from? I think that is something that will one day be understandable: why one person tends to give more importance to one type of activity than another, which is after all what is at the heart of ethical uncertainties and ethical arguments. In terms of sociology, I am quite sure that there are laws of human behavior and laws of animal bebavior, laws of interaction laws may be the wrong word, but at least rules that one can propose, that will make sociological analysis more precise. It is easy, and in fact I have done it myself, to dismiss today's sociology as pseudo-science. But that does not mean that sociology *per se* need always be pseudo-science. I think the fact is that the kinds of information we need we just do not have. If genetics in the middle of the 19th century was pseudo-science, no one knew what the molecules were, no one knew what the elements of the discussion were, and so any proposal for how genetics worked, until very recently, was obviously a proposal made out of thin air and easy to dismiss, because there was no basis for it; that does not mean there won't ever be a basis for it.

So I guess I feel pretty strongly that most of the examples that he gave are things that are understandable. Now, it is important to realize that the fact that we would understand them does not change them as human activities. The fact that we may be able one day to say that a given person has pathological behavior because of different hormonal relations, different brain connections, won't make it any different, but that that person may develop an enormous sway over a large body of people because of the power of his personality, that will go on. Human relations will continue in spite of the fact that we may be able to understand what is going on, because — the probable example that comes to mind most readily is the talk we heard last night about the solar system. The laws of physics that describe the solar system were laid out long ago, and yet the solution as to why the solar system is as the solar system is, is obviously not yet complete, and there are lots of questions that one can ask about it. And so in one sense, it had been in the very nice word that Weisskopf used, "the solar system is demystified as an event of science", but it is neither completely explained nor can we describe all of its behavior in terms of well understood interactions between every individual planet and every other individual planet. That is an awfully good analogy for human affairs — the fact that we could explain the underpinnings of ethics or the underpinnings of the appreciation for music will not in any way completely take away human experience. Just by explaining things we do not turn ourselves into machines and I do not think we should be afraid of that; I think it is one of the most unfortunate aspects of the anti-science strain of thinking, which is always present and seems now to be a little stronger than usual, that people believe that by understanding we take away something essential in human experience.

WEISSKOPF

I am very glad about this intervention because in a way what Baltimore said supports what I said; namely, I have said that science has a claim for completeness, and I am quite sure — not quite sure but I think it will really be there will come a day when you will know why we like Beethoven or what happens in the nervous system when we hear something beautiful or make some ethical conclusions. In other words, that is just what I meant with the justified completeness claim of science. What I have meant, however, is that this may be very relevant in some instances but in others less relevant; and it is really this relevance that I have in mind. In other words, for the Beethoven sonata it is not very relevant. I do not exclude completely relevance. Your last remark was of course a very important one, to which I would like to come back; but essentially I would say that for the problems of a Beethoven sonata the understanding of what is going on in our heads is irrelevant. I think that is to some extent also true for ethical questions. And I think that the example that Leprince-Ringuet used: you see a mother with her child and you observe the phenomenon of love — this phenomenon certainly has very important scientific bases, evolutionary nervous bases, etc. we can show that for the survival of mankind — not even of mankind alone but of the animal world — it is necessary that the mother love her child. But there is an element in this love that is a direct experience, and the direct experience is an important one. I do believe that our culture has suppressed this too much by emphasizing that the only serious way of looking at it is the scientific way.

Now there is one point on which I fully agree with Baltimore — in fact I did not mention it when I talked, but it is in the written paper — namely, that these different avenues to human experience together make up the real human existence. In other words, I said in the paper, and some of you may have read it, I quoted this poem by Keats, where he says that the rainbow is no longer beautiful because we understand its colors. That is of course certainly not so; on the contrary, and this is I think the depth and greatness of human existence, that there are very different contradictory, or as I prefer to say, complementary ways to come to see an object or an emotion, and that these together only make the greatness of human existence. Therefore I do believe, to come back to the Beethoven sonata, that it may be that the fact that we know what is going on in the brain would make it even more beautiful — as by the way I would say and I also wrote in the paper that the beauty of a starry night and the beauty of a sunset are enhanced if I know how big the distances to the stars are, or what has made the red color stronger than the green. I would say this only underscores the point I wanted to make, namely that there are very different things, aspects, and some are relevant for this and some are relevant for that.

LÉPINE

Ce qui'il y a de merveilleux avec les scientifiques c'est que lorsqu'ils discutent entr'eux sur le plus profond de leur discipline ils deviennent des théologiens, alors que quand les théologiens discutent entr'eux ils essaient de devenir scientifiques. C'est peut-être parce qu'il y a un rapport entre les deux choses, et je suis très reconnaissant à la fois a Monsieur Weisskopf et a Monsieur Leprince-Ringuet d'en avoir parlé.

When we speak about science, it is essentially using a logical panthway of our thinking which is purely cortical, that is in the cortex of the brain. But the emotions, the feelings and the sense of guilt, of good, of bad are not in the cortex — they are in the center of the brain. And it is a very curious story in the Bible that at the beginning when Adam committed the first sin it was in fact proposed by a reptilian, and the reptilian brain still exists in us — it is the feeling brain — and what the reptilian proposed to the first man was to split his reptilian brain, the feeling one, from the cortical one, which is the reasoning one. And you have just defined our difficulty in science, that now with that split in our two brains, the heart and the reason, we always make science with the cortex, and we can go so far but sometimes I would say, paraphrasing Pascal, that "la raison a parfois des emportements que le coeur ne peut pas souffrir".

SELA

I wanted to make two points, but one of them was really said so beautifully by Weisskopf himself and by Dr. Lejeune that all I can add is that even if we understand scientifically that sonata of Beethoven, this still is not the sonata — this does not increase — the sonata of Beethoven remains but is not reduced. My other comment was that understanding is not a closed system; if we understand more, it does not mean that less remains to be understood. As a matter of fact, the best example of an open system, the more we understand the more remains to be understood. So it is not the question that if we would understand the sonata and we explain scientifically the love of the mother for the child and so on, we are closer to something that we have to reach — I think the more we understand the more remains to be understood.

PAVAN

If I would be smart, I would be quiet. But I am learning a lot and I am really very happy to hear all these comments. My question is that in principle I am in agreement with David Baltimore in the sense that I do not put any limitation on science. Now, take this as the "truth" of a scientist. When I say I am sure of it, I always have my feet back here because tomorrow I change my mind — I'll be here and say something again and put my feet there. Really, although I recognize the limitation of science now, by my feeling, by by way of life I see no way — even if I cannot know, I try — I will work hard. This brings me to another very important problem, of which I am very much afraid to speak in public, because I do agree entirely with what is in the contents, with some small differences, with Weisskopf, with what Weisskopf put in his paper, which I enjoyed very very much. But I think that one of the greatest limitations that we should put, at least for the public and for the people that are responsible for the development and for the government of the countries, is that there are some things that are being destroyed today, and science should do something about it. We are destroying something, natural resources and many other things that science can substitute in some way but cannot replace. And more than this, in this specific case we are talking not of the destruction of forests, or the pollution of the sea or the losing of billions or trillions of liras, dollars, francs or marks or anything, what we are doing is, we are destroying or avoiding the survival of human life, and life in general. Since the earth is limited, in hundreds of years we will have to limit our population one way or the other, and the number of people that would survive in the future would depend on us today. We should not trust science much to solve this problem. We should be careful and about our responsibilities for the future generations.

COLOMBO

My only comment is that my feeling is that the strongest limitation of science is that we do not know the limitation of science.

SIDDIQUI

First of all I must offer my felicitations to Prof. Weisskopf for this comprehensive and beautiful treatment of the subject matter of his presentation. As I see it, his presentation is actually in two parts: one is placing limits to the pursuit of science, and the limitations of science. In that context he has quoted Wittgenstein: "what we cannot speak about, we must pass in silence".

And as far as the involvement of science with ethical and moral values, is concerned, I think I agree fully with Professor Weisskopf that this is not the province of science. In respect of placing limits on science, scientific pursuits, I am reminded of what Nietzsche said about it over a hundred years ago, that in our love affair with the pursuit of knowledge we do not realize that it is ultimately going to lead to our destruction. But he said something to the effect — I do not remember the exact words — that it would be much better for humanity to go into flames in the pursuit of knowledge than die out in ignorance. It is human destiny, you cannot set limits to the pursuit of science, and so far as I see, actually science in itself is innocent. It is because of the diabolic forces that it releases through technology with all the good it can do and all the harm it can do, that we have difficulties. I do not agree with the date Prof. Weisskopf has given to the change of attitude toward science as 1950; actually it is 1945, with Hiroshima and other cities bombed. I was not all this old then, but I remember evry clearly that I had given up smoking for nearly five months for reasons of health, and on that day I started smoking again because life did not seem worthwhile. I remember in a discussion on much the same topic in the U.K. in the course of Cornwall science conference meetings the observation that reminds me of that line in Macbeth: "Macbeth has murdered sleep; Macbeth shall sleep no more". That is the case of humanity today: we are living under the balance of terror, but we cannot get away from it and we cannot put limits to the pursuit of science. It is a sort of historical chain reaction, we cannot get away from it. All the hopes that one can have for the future of humanity are perhaps in this: that the light of humanity can win out against this challenge. I hope it will.

BLANC-LAPIERRE

Je vais peut-être faire descendre le niveau du débat et je m'en excuse. Je voulais simplement faire part d'une expérience qui a été celle d'un directeur d'une école d'ingénieurs qui pendant dix ans a dû s'adresser à des étudiants, soit individuellement, soit sous forme de grands discours faits de temps en temps aux élèves en assemblée. Il est certain que la jeunesse actuelle est beaucoup plus critique que je ne l'étais lorsque j'étais sur les bancs d'éleves, se pose beaucoup plus de questions et je crois que si on se borne à regarder la situation telle qu'elle est sans chercher à l'expliquer, que la jeunesse accepterait très difficilement qu'on lui dise actuellement que la science va résoudre tous ses problèmes, et il est certain que mes premiers discours quand j'ai dû parler à tous les élèves assemblés, ça a été de leur vanter l'électronique, tout ce qu'on faisait pour des gens qui veulent être électroniciens. On m'a écouté gentiment, car les élèves sont gentils, mais je n'ai pas senti que je faisais vibrer très profondément leur âme; et je n'ai pas hésité, àprès quelques expériences naturellement, à leur expliquer que l'électronique était très importante et que c'est là-dessus qu'on leur donnerait un titre d'ingénieur, mais à leur dire qu'il ne fallait pas qu'ils attendent parce que nous leur enseignions la plénitude de la satisfaction de leur développement d'hommes et à les engager à faire de l'art, de la politique si vous voulez, à s'occuper de choses religieuses, naturellement leur disant qu'il faudrait quand-même apprendre l'électronique. Et je crois que la jeunesse est très sensible à tout ce qui touche à sa sensibilité à côté de ce qui touche à l'intelligence. Et alors ceci m'amène à repenser à notre discussion d'hier soir sur les justifications de la recherche fondamentale indépendamment de toute idée d'application, et je crois que dans une analyse sommaire on peut dire que l'homme a des facultés d'intelligence, de sensibilité, de volonté, il a le droit de mieux connaître la réalité qui l'entoure — ça correspond a son intelligence — je crois qu'il a droit à de l'art, il a le besoin et je me demande si finalement les deux ne se justifient pas de la même façon si on élimine toutes les idées de l'application. Et alors là je ne suis pas tout à fait d'accord avec une citation qui a été faite par notre collègue Monsieur Roche lorsqu'il disait que la recherche fondamentale était ce qui était jugé par les gens qui le faisaient, c'est-à-dire par les pères. Je crois que l'art est jugé par l'humanité, les artistes peuvent discuter entr'eux mais s'ils ne touchent pas l'humanité, ils ont raté une vocation — et je crois que par la recherche fondamentale on doit rendre perceptible à l'humanité quelles sont ses tendances et vers quoi elle tend. Je crois aussi qu'on ne peut pas dire que ça ne sert à rien. Simplement on doit refuser le type de question suivante: "vous proposez telle expérience, dites-moi dans six mois quelles seront les applications", car ça n'a pas de sens.

LÉPINE

My comments will be short. I just want to say how much I appreciate the talk by Professor Weisskopf and also the comments that were made, especially those by Leprince-Ringuet and by Lejeune. I think that finally we shall all agree on this problem of the limitation of science. We all love science, we are devoted to science.

Most of our life has been using science and frankly we know that there are limitations. There are obvious limitations, physically, ethically, and there are also limitations when we are emotionally concerned, as was outlined by Professor Lejeune. Science is when we can explain and we know the truth about it; a phenomenon which contains only a part of truth is not a scientific phenomenon. For instance, unidentified flying objects may have some truth, but as long as we do not know the whole truth they are not scientific subjects. All these descriptions remind me of a short story that was written by Jean Cocteau, a French poet who was also very humorous and witty. It is about a dialogue between man and God. Man asks God questions about life, the future, and everything, and finally ends by: "God, what about the railway catastrophes? How are you going to explain them?" And God, feeling uneasy, answers, "You don't explain, you just feel them". And that is it.

ROCHE

I did not ask for the floor and I did not intend to speak at all, because the subject is an intensely personal one to me and I did not want to share with such a large group personal beliefs and attitudes. To me science is the only source, has been the only source of certain security and certitude that I have been able to have in my life, and I feel that it does give, through its objectivity, its characteristics, a "planche", a site for man to step on, and I agree with Professor Colombo that one of the problems today is that we do not know its limitations and I think it is boundless. We do not know, we will not know in our generation, and man will not know for a long time; this is a story that has just begun and which will take a long time to unfold. This is about what I wanted to say.

LEPRINCE-RINGUET

Sur la première partie de ce que Monsieur Weisskopf a dit il y a dans nos pays occidentaux, une sorte de réaction anti-scientifique de la jeunesse, et je crois qu'il y a plusieurs explications; en particulier Weisskopf en a donné une, qui est une saturation du confort, et il y a aussi la vision d'un monde qui apparaît comme contraignant pour la personnalité plus jeune, en particulier contraignant par les attitudes nouvelles qui sont provoquées par les ampleurs des possibilités du monde nucléaire, les problèmes d'environnement également. Alors ce sont des réactions contre les inconvénients d'une civilisation, de la civilisation actuelle et en particulier, je pense, contre le gigantisme, le gigantisme sous toutes ses formes, que ce soit les grands tours, que ce soit les grandes routes qui sont obstruées par des foules de véhicules en même temps, que ce soit des grandes usines dans lesquelles les choses sont impersonnelles, le gigantisme se pose contre la personnalité de chacun. Et il me semble que la jeunesse actuellement réagit contre ce gigantisme, qui est inhumain en fait et qui réellement est inhumain parce qu'elle désire chercher ensemble son existence et elle désire développer sa personnalité, elle la développe davantage dans des petits groupes d'amis dans lesquels on est quelqu'un plutôt que dans le métropolitain dans lequel on est un objet parmi beaucoup d'autres. Alors je crois que cette réaction de la personnalité est une réaction contre ce genre de civilisation. Il y a aussi un autre phénomène. Les jeunes actuellement se lancent dans l'irrationnel avec volupté — les grandes manifestations, par exemple, dans lesquelles on se sent avec une certaine amitié entre soi ou milite contre quelque chose que l'on ne connaît bien dailleurs, en général, et ce sont des réactions contre le rationnel. Le rationnel c'est l'éducation, la formation; la formation dans les lycées c'est une formation rationnelle, le but c'est de former l'esprit avec une certaine rigueur et une certaine logique, et cette formation rationnelle elle se développe jusque dans les classes de préparation aux grandes écoles et jusque dans les universités ou les centres juridiques également — tout ça c'est rationnel. Et comme cette formation rationnelle est assez dure et les mathématiques sont difficiles, et qu'elle n'est pas absorbée facilement par tout le monde, alors il y a des réactions de rejet, il me semble. Ces réactions de rejet sont des réactions qui poussent les jeunes à se lancer dans n'importe quelle manifestation pourvu qu'elle ne soit pas rationnelle; alors que ce soit l'astrologie, que ce soit les manifestations politiques, que ce soit les manifestations nucléaires etc., tout ça vient, je crois, en partie d'une sorte de réaction contre un enseignement qui chez nous en France est manifestement orienté vers le rationnel abstrait.

CROXATTO

D'abord je voudrais exprimer mes félicitations au Professeur Weisskopf. Je voudrais ajouter que je suis très optimiste sur les possibilités de la science. Malgré cela, je pense qu'il y a un domaine qui reste en dehors de tout. Je crois qu'on arrivera à expliquer tous les phénomènes; c'est tout à fait possible. Les explications qui se donnent, que peut donner la science, c'est l'explication de comment le phénomène peut se développer, mais pas la question pourquoi le phénomène se produit; et je crois que les problèmes en relation avec le pourquoi ne sont pas dans le domaine de la science. Alors, admettant que la science n'a pas de limitations, on arrivera à expliquer tous les phénomènes. Par exemple les phénomènes qui se passent dans le cerveau par les émotions, pourra-t-on arriver à expliquer tous les plus petits phénomènes chimiques, physiques qui se passent dans le cerveau? Quand je pense, quand je souffre, la question pour moi la plus importante encore est de savoir pourquoi cela se passe.

WEISSKOPF

I would like to say a word to what we have just heard. The question of why is of course always a very difficult one, and in most cases science answers the question how, and not why. But you said you are optimistic in respect to science. Let me be even more optimistic. There is a tendency — and I think a very good one — among those people who have to deal with the fundamental laws, to show that the fundamental laws are the only possible ones. To give an example which is not very fundamental, but just to make the idea clear, the Maxwell equations are the only possible linear equations that will describe the electromagnetic field. Now there is all the time a search for Heisenberg's world formula, although I do not believe that this is the solution. However, I cannot exclude that one day one will find that the laws of nature as we observe them are the only possible ones. And this then would be in a certain way an answer to the question why. Now, let me hasten to add that I express it only because there are many people believing it. I am more inclined to think the way Leprince-Ringuet has expressed it, namely that the world is unending and the further we go in almost any direction the more surprises we discover. For example, I mean the structure of the nucleus and then the structure of the nucleon and the quark and now I believe that maybe there is a structure of the quark, and so on. Now, I will not say that I believe it, but it may be. But perhaps more important, whenever we discover a new level of natural behavior, this new level is much more surprising to us than the previous one. In other words, it seems that the deeper we go into the universe, the more surprises we find; and of course this makes the final world formula solution rather improbable, but I cannot exclude it; and there I think that the answer to the question why may even be in science. I just express the possibility.

OORT

There is one aspect to the problem of the relation between science and the world which we perhaps have not quite sufficiently discussed, and this is a thing that has been worrying me from the days when I first entered science — the enormous chasm that exists between the scientists, especially those who are working in most fundamental problems of science, and the man in the street, the general public. After all, for the future of science it is not only the administrators and the ministers whom we have to convince of the value of science; in the long run this depends I think very deeply on the way the man in the street, the people in general consider science, and I believe that in every man there is a bit of this spark of curiosity that Ubbelohde has mentioned, and that it is worth trying to blow on this spark in every man to evoke his interest in the doings of the scientists. In practice this would mean that perhaps more efforts should be made to take science to the people — and many efforts are being made: television, shows and a lot of popular books on science, especially on science fiction, which have a wide circulation in the world, but I do not think this is quite sufficient and I do not think this goes always in the right direction to attain the aim tbat I have in mind. Astronomy in a way is in a fortunate position, I think, in this respect, because there are so many people, from ministers down to the workers in factories, who have an interest in the sky and the stars and do it with some kind of an intuitive interest, which makes it easier to approach them and to popularize at least that part of science which is connected with astronomy. This is a very special task for astronomers in this direction. But I do think the other sciences should also certainly take their part in this. It is an aspect, I think, of the relation between science and society which should certainly not be neglected.

LÉPINE

Je voudrais souligner combien est juste la remarque qu'a faite Leprince-Ringuet sur l'attrait de l'irrationnel non seulement sur la jeunesse mais même sur une partie des adultes actuels. Pour une part il s'agit d'une réaction de rejet qui traduit la déception de ce que la science n'a pas, vis-à-vis de l'homme dans la rue, apporté tout ce qu'on lui avait promis. On constate que le développement de la science et des applications non seulement n'a pas diminué sa production mais qu'elle augmente, que certains risques existent encore, que la vie dans les grandes villes est très oppressante à cause du gigantisme comme on l'a souligné, que la science qui devait tout guérir à l'heure actuelle est incapable de guérir certains états, d'apporter la guérison, et par réaction il y a une position devant la réalité qui se traduit par la faveur croissante que reçoivent à l'heure actuelle les charlatans, les astrologues et toutes les formes de l'exploitation de la croyance, de la naïveté populaire. C'est là qu'une vulgarisation bien faite de la science doit expliquer qu'il y a la science et ses inconvénients et les mauvaises applications. Nous voyons se développer partout des mouvements écologistes. En principe, le mouvement écologiste est sympathique lorsqu'il tente à protéger la nature, à respecter les conditions d'une vie normale — mais on s'aperçoit en fait que parmi les écologistes se sont glissés des anti-scientifiques qui aboutissent, si on suit exactement leurs doctrines, à renier complètement notre civilisation et souvent aussi à rejeter la morale qui est la base de notre civilisation. Par conséquent il y a là une maladie de civilisation dont la science n'est pas responsable mais où la science mal comprise a certainement une part.

WEISSKOPF

I am sorry to speak so much but I would like to support the remarks of Dr. Oort, about the popularization of science. I think one should do very much more than has been done, and the trouble which we face is that the popularization of science is regarded by the scientists, whether they say it or not, as a very low activity: you are supposed to do it only after your laboratory work is finished, and it will not be counted toward our promotion, etc., etc. Actually I like to compare it always with music, where the social standing of the artist who presents the works is almost higher than the social standing of the composer. And why is it not so in science that a very good presenter of science, even if he is not creative in science, should be considered just as high as a creative scientist because it is an art, a very difficult art, and indeed it is much harder to write a popular book than to write a textbook. There is something which really should change, and it is part of the reason the public does not appreciate science. The art of popular demonstration of course in astronomy is easy for some strange reason which Dr. Oort has analyzed, though in any other field it is extremely difficult, but it was actually on a higher level in the old days. Remember the hooks by Jeans that were absolutely excellent, and by Max Born but nowadays people of that stature just do not write books that are understandable by the public; and it is not true that you cannot do it. It can be done, only it is very hard. Let us all try — it is very important.

RICH

Professor Weisskopf is too modest. He should write another excellent book called, I think, "On Understanding Science" which he has written, which is quite good, and it is just for the purpose he cites. I should say that the art of presenting science does have a tradition in some countries — in Britain at the Royal Institution the annual Christmas lectures are very much a popular presentation and they are quite prestigious. We have tried in the States but never actually developed something analogous to that. We are doing something nowadays with the enormous power of television — indeed one could have, one ought to have some super award to really pull people towards the popularization of science for younger people especially, suitable for television; and I think it is too bad that we have a series of awards for rather trivial amusement but we do not have an award structure for science presentation.

LEPRINCE-RINGUET

A l'occasion de cette discussion sur la popularisation de la science et sur la vulgarisation de la science, je voudrais dire deux mots seulement. D'abord, une population qui n'est pas informée suffisamment de la science est sujette je dirais à tous les emballements, à toutes les modes, et peut être entraînée n'importe où sans être capable de réagir d'une façon saine. Cela me semble tout à fait évident, d'où l'importance de la vulgarisation pour éviter qu'une population ne soit prise par n'importe quel courant qui à un certain moment l'entraînera d'une façon tout à fait irresponsable. Et deuxièmement, la vulgarisation est souvent faite par des journalistes, et ce n'est pas mal à condition que ces journalistes soient en contact avec des scientifiques. Mais il est indispensable que des scientifiques se mettent à la vulgarisation d'une façon beaucoup plus intense que ça ne se fait actuellement, parce qu'une vulgarisation mal faite par des journalistes, comme cela arrive souvent, ça peut être aussi pernicieux et mauvais, ça peut être une mauvaise vulgarisation, ça arrive souvent. Il est nécessaire que des scientifiques le puissent faire. C'est difficile comme le disait Weisskopf, avec un langage qui est presque un langage d'un journaliste, pour pouvoir accrocher le public, mais il n'y aura pas de choses absurdes dans ce que les scientifiques diront.

PAVAN

I am glad to hear about the problem of "vulgarization" — "divulgation" of science because we started a movement in our Academy in São Paolo; we talked with the Minister of Education and head of the National Research Council and the head of other institutions in Brazil for their help in science, and tbey all agree, and our Academy is in relation with the Society for Scientific Newspapermen. What we want to do there, what we are trying to do, is to have ten fellowships for students of journalism or science that would be interested to be professionals in the matter. Our idea there would be to form a committee which would tell this group of ten students, ten fellows, whom they should interview. The idea there is that the young men would go to a scientist and they together would prepare an article and the scientists would give the important points, the idea and tbe facts, and the students would put them in a journalistic way. What we have in Brazil today is this: there is a big newspaper which has a weekly number on science, scientists would write popularization of their own field. But the trouble that the people are finding there is that it is very hard to read — everything is put in such a dry way that a person does not read more than ten lines of the article. Then the idea was to appoint a journalist who could put it properly together to make this popularization of science in such a way as to avoid this dryness of the scientists' writing, or this nonsense that will sometimes make a sensation instead of presenting facts. This is what we are trying to do with the help of some of the Brazilian institutions for the promotion of science, and perhaps in a few years we will tell you what has happened there as an experience.

MARINI-BETTÒLO

I think that we have had a most interesting discussion and I should like to come back to some of the problems that have arisen in this general discussion, about the role of science and what we should do with the young people. The young people need an indication from us that we are aware of the importance of their work.

I see that young people sometimes say (and this is not only the question of planning), "Does what we are doing have a certain significance?" The frustration of young scientists is due to the fact that the goals of science are not always clear and surely at present there is some difficulty in finding the right way. I think that these points should be considered to give young people the right understanding of the importance of science.

CHAGAS

Since we are coming to an end, I want to make some remarks myself. I feel myself in the position of Marcel Roche: I cannot imagine life without science, and as I said once, in my inaugural address as President of the Brazilian Academy of Science, using a short verse from a very popular song in France, "J'ai la science dans la peau", because I was born in a scientific milieu with my father and my great dream was always to visit his laboratory at the Osvaldo Cruz Institute. So I have faith in science as Roche said. The only difference between us is that faith in science has not inhibited me from being a mystic, not a "mythic", in a certain sense.

I think that one of the duties of people who have a certain experience is to avoid for society the anti-scientific feeling which is being created. We have seen it many times. I remember even during the 1968 students' revolution in France there was certainly an anti-scientific feeling involved. We feel, as Lépine said quite clearly, that in the ecological movements which are so important there is an anti-scientific feeling, and in a certain way this anti-scientific feeling comes quite naturally to mankind because after the war, with the first advances of technology and the marketing of science in a certain way — if I may use this word — hopes for a definie improvement of mankind were so great that people were disappointed.

The extraordinarily nice paper of Weisskopf brought out one aspect of the limitation which is the exhaustion of a certain scientific discipline, limitation in the sense of exhaustion. I am not sure that this exists because I remember once a bacteriologist telling me in the year 1934 that he was working with bacteriophage and that microbiology was an exhausted discipline. I was in Boston and had a talk about quarks and I was told that in no way could experiments be done to show the existence of quarks because this was a purely hypothetical particle. And the next morning I opened the New York Times and there was some scientist in California claiming that by a very simple experiment they had discovered the quark. Now this is a very disputed fact, but it still shows how difficult it is to make such a strong assertion. On the other hand, one sees for instance that there is a new biology which appeared, not only with molecular biology, but if we think about the second transmitters for instance, cyclic, AMP, is is a completely new field which has appeared. A lot of what was said about Enkephalins for instance shows also that a completely new field has been opened in the last five years in brain neurobiology. So I am convinced that we cannot speak of what I was calling the exhaustion, or the limitation of science by ehaustion. I think there will be naturally the question of financial investment but even more of intellectual investment.

And I come back to something which was said by Marini-Bettòlo in the beginning, which is in my opinion very, very important: that in our present laboratories many times the scientist is tied to a machine, an instrument, and he depends so much on this instrument that he does not allow enough time for reflection; he hecomes really a part of the whole experimental set-up, and I think this is a very bad thing. Now, I was glod to see that we again brought up the question of the diffusion of science; I think it was brought up here considering that this would be a weapon to the limitation of science mostly by what we could call the external forces, which means the society, and because of a small introduction I made in this booklet where I spoke about internal and external forces opposing science. It is interesting that two years ago we had already taken up this topic as a very important one: the popular diffusion of knowledge as an element of scientific development. However, what I want to say is that I am very grateful for your contribution. I believe that we have chosen a very interesting topic which offers something to contribute to a book which will report the discussion held here, for 2½ very strenuous days. I hope to see you very soon.

In my name and in the name of the Council I thank oll of you from the bottom of my heart. Please extend our best regards to your charming wives and to your families, and I hope that you have a very nice trip home.

IV

NEW TRENDS IN SCIENCE

TRANSFER RNA: THREE-DIMENSIONAL STRUCTURE AND BIOLOGICAL FUNCTION

ALEXANDER RICH
Pontifical Academician
Department of Biology, Massachusetts Institute of Technology
Cambridge, MA 02139, U.S.A.

In biological systems genetic information is stored in the sequence of nucleotides in nucleic acids and it is expressed largely in the sequence of amino acids in proteins. Transfer RNA (tRNA) molecules are important links in the flow of genetic information from messenger RNA (mRNA) into proteins. They act by positioning individual amino acids in a correct sequence during the assembly of proteins inside ribosomes. Because of this fundamental role, it is likely that tRNA molecules were an ancient component of biochemical systems. Their origin probably dates back to the period more than 3 billion years ago when life originated on Earth.

Transfer RNA is a small polynucleotide containing 75 to 90 nucleotides depending on the species. tRNA molecules are found in families or isoacceptor species each of which can be linked to a particular amino acid on an aminoacyl-tRNA synthetase enzyme. There are 20 isoacceptor tRNA families, one for each amino acid and its synthetase enzyme. Three of the nucleotide bases in the tRNA anticodon fix the position of aminoacyl-tRNA on mRNA through codon-anticodon interactions. There are two different components in determining the fidelity of protein synthesis: one involves selection of the correct amino acid to be attached to its tRNA upon aminoacylation; the other involves insertion of the correct aminoacyl-tRNA onto the mRNA in the ribosome.

Cloverleafs and electron density maps

Despite the variation in the number of nucleotides in different tRNA molecules, they have many features in common. The first nucleotide sequence of tRNA was obtained by R. W. Holley and his colleagues in 1965 [1] and they observed that the sequence could be organized into the familiar cloverleaf diagram. Over 100 tRNAs have now been sequenced and a number of significant generalizations have emerged which can be summarized in the cloverleaf diagram of Fig. 1 [2]. The molecule has both constant and variable features. It is significant that the variability in nucleotide number is confined to three different locations; the variable loop and the α and β regions of the dihydrouracil (D) loop. The α and β

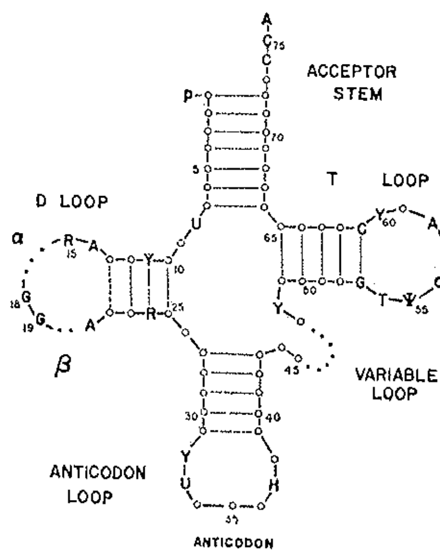

FIG. 1 — A diagram of all tRNA sequences except for initiator tRNAs. The position of invariant and semiinvariant bases is shown. The numbering system is that of yeast $tRNA^{Phe}$. Y stands for pyrimidine, R for purine, H for a hypermodified purine.

regions have from 1 to 3 nucleotides in different sequences, while the variable loop has 4 to 5 nucleotides in most tRNAs, but can have from 13 to 21 nucleotides in some cases. The other component of variability is in nucleotide sequence except where indicated in Fig. 1. Different tRNAs have a great deal in common both in terms of the number of nucleotides in various stem and loop regions as well as a substantial number of nucleotides which are conserved. The commonalities in nucleotide sequences of different tRNAs shown in Fig. 1 clearly suggests the possibility that they may be related to the three-dimensional structure of the molecule.

Determination of the three-dimensional structure of a macromolecule is usually carried out by X-ray diffraction studies of crystals. In 1968, together with several other research groups we discovered that it was possible to crystallize various species of tRNA [3]. Unfortunately, crystals of tRNA are generally disordered. This is expressed by a limit in the resolution of the diffraction pattern. Solution of the three-dimensional structure of a nucleic acid (or a protein) requires a resolution of at least 2 to 3 Å. Solution of the structure at 6 to 8 Å resolution, for example was not likely the information desired regarding the detailed conformation of the molecule.

In 1971, we discovered that the addition of spermine to yeast phenyalanine tRNA made possible the formation of a highly ordered orthorhombic crystal with a resolution of nearly 2 Å [4]. This crystal was then studied by diffusing heavy atoms into the lattice in order to solve the phases in the diffraction pattern. By early 1973, an electron density map was produced at 4 Å resolution which made it possible to trace the folding of the polynucleotide chain [5]. The cain could be seen at that resolution because the electron dense phosphate groups stood out fairly clear. The molecule was found to have an unusual L-shaped conformation in which the acceptor and TΨC (T) stems formed one arm of the L while the D stem and anticodon stem formed the other arm. The 3′-terminal adenosine to which the amino acid was added during aminoacylation was found at one end of the L while the anticodon was found at the other end of the L some 76 Å away. The corner contained a complex coiling of the T and D loops. Further infor-

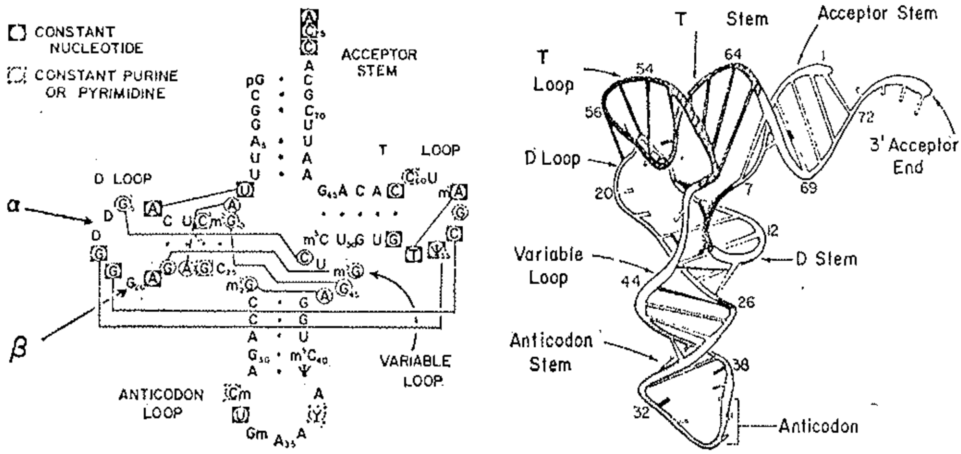

FIG. 2 — (left) The nucleotide sequence of yeast tRNA^{Phe}. Tertiary base-base hydrogen-bonding interactions are shown by solid lines, which indicate one, two, or three hydrogen bonds. (right) A schematic diagram showing a side view of yeast tRNA^{Phe}. The ribose-phosphate backbone is depicted as a coiled tube, and the numbers refer to nucleotide residues in the sequence. Hydrogen-bonding interactions between bases are shown as crossrungs. Tertiary interactions between bases are shown as solid black rungs.

mation was revealed in 1974 by a 3 Å analysis which showed many details of the hydrogen bonding between the tRNA bases [6].

At the same time, a similar study was carried out of the same spermine stabilized yeast tRNA^{Phe} which crystallized in the monoclinic lattice [7]. This revealed substantially the same folding of the molecule even though the packing in the lattice was quite different. This suggested that the molecules have a stable form independent of the manner in which they are packed in the crystalline state. The analysis has now been carried out to a resolution of 2.5 Å with a substantial amount of refinement so that a great deal is now known about the folding of the polynucleotide chain, including the manner in which the nucleotide bases are involved in hydrogen bonding and the manner in which the polynucleotide chain is stabilized by the spermine and magnesium cations [8-10]. The diagram in Fig. 2 shows the base-base hydrogen bonding in yeast $tRNA^{Phe}$, both in the clover leaf form and in the observed three-dimensional folding of the molecule. The tertiary hydrogen bonds outside of the double helical stem regions are drawn in black.

Hydrogen bonds galore

The structure has several clues which suggests that it is a model suitable for understanding the structure of all tRNAs. For example, many of the hydrogen-bonding interactions in the molecule involve the invariant nucleotides.

There are over 100 hydrogen bonds in the molecule. Watson-Crick hydrogen bondings has the effect of stabilizing regular double helical structures. However, they are of less utility in stabilizing the kind of complex coiling of the polynucleotide chain which is found in a globular nucleic acid molecule such as tRNA. Of the nine tertiary base-base hydrogen-bonding interactions, only one is of the Watson-Crick variety and all of the others are a more specialized type.

In general, the three-dimensional structure reveals a specific role played by most of the invariant nucleotides found in tRNA sequences. Hydrogen bonding is the major interaction, but base stacking is also important. A significant exception to the structural role of conserved bases is found in the single stranded CCA sequence at the 3′-end of the polynucleotide chain. They do not have a specific structural role in tRNA, but these bases undoubtedly play a significant role in the various enzymatic steps associated with tRNA aminoacylation as well as in the transfer of the amino acid to the growing polypeptide chain inside the ribosome.

Base stacking and variable sequences

It is well known that the interactions associated with the stacking of the flat bases is one of the major stabilizing features of nucleic acids in a double helix. The same generalization is found even in this globular form of a nucleic acid molecule. The entire molecule is organized into two stacking domains, each of which makes up one limb of the L-shaped molecule. Virtually all of the nucleotides are involved in stacking interactions with a few interesting exceptions. The 3′-terminal adenosine is not stacked, but end effects of this type are quite familiar in study of the nucleic acids. It is interesting to note, however, that all other nucleotides are stacked with the exception of bases in the three regions of the molecule which have dotted lines in Fig. 1, namely those sites which contain variable numbers of nucleotides in different tRNA sequences.

tRNA in solution

The orthorhombic crystals of yeast $tRNA^{Phe}$ contain over 70% water; nonetheless, it is important to determine what happens to the molecule when it is dissolved in a solution under conditions in which it has biological activity. Fortunately, there have been a number of studies which make it possible to correlate the three-dimensional structure in solution with that seen in the crystal lattice [2]. A number of chemical modification experiments have been carried out which can be readily interpreted in terms of the three-dimensional crystal structure. For example, using selective chemical reagents, it is possible to modify nucleotides which are single stranded or in which the bases are not involved in hydrogen bonding. Furthermore, by assuming that other tRNA species have a structure similar to that of yeast $tRNA^{Phe}$, it is possible to interpret their pattern of chemical modification. In other experiments, the binding of oligonucleotides can be understood by inspection of the structure. Nuclear magnetic resonance studies of tRNA in solution have been carried out extensively. The structure not only explains the NMR spectrum of yeast $tRNA^{Phe}$ in detail but it can also be used to help predict the spectra of other tRNA species.

The generalized diagram of Fig. 1 summarizes the sequences of all tRNAs involved in elongation of the polypeptide chain. However, initiator tRNAs, especially in eukaryotic organisms, have some slight modifications in their sequences in the T loop [2]. It is likely that these are associated with some minor changes in the interaction in that region of the molecule. However, the yeast

$tRNA^{Phe}$ structure can be used to interpret chemical modification studies carried out on initiator tRNAs so it is likely that their structure will, in general, be similar to that seen for the yeast $tRNA^{Phe}$.

The conclusion from these varied solution studies is that it is quite likely that there is a significant three-dimensional structural commonality among all tRNA species. This commonality is seen in a symbolic fashion by the use of the cloverleaf diagram for all species.

Mysteries of the anticodon

The stacking interactions of the anticodon stem are extended down the 3′-side of the anticodon loop through all three anticodon bases. The chain then turns sharply just beyond the third anticodon base G_m34 with residues U33 and C_m32 in an extended conformation. This folding is similar to that first suggested by Fuller and Hodgson [12]. The three anticodon bases are stacked on the hypermodified purine, Y37, and all of these bases are in the form of a right-handed helix with approximately 8 residues per turn in the orthorhombic yeast $tRNA^{Phe}$ crystals [8]. Since the anticodon bases are stacked on the modified purine Y37, it is clear that the modification effectively prevents it from hydrogen bonding with mRNA, even though the base is in a position to do so in the absence of modifications. In most sequences, the purine found at the 3′-end of the anticodon is heavily modified to form a variety of different derivatives.

Recent refinement studies have made it possible to visualize the spermine and magnesium ions in yeast $tRNA^{Phe}$ [10]. Two spermine molecules are seen, one of which is wrapped around phosphate 10 where the polynucleotide chain turns a sharp corner. The other spermine is found in the major groove of the anticodon stem, near the region where it joins the D stem. This spermine is in contact with phosphate groups on both sides of the deep groove, and it is probably responsible for the slight bend of 25 between the axes of the anticodon and D stems. The magnesium ions are located almost entirely in the loops and one magnesium ion is found in the anticodon loop. It is likely that the spermine and magnesium ions help to stabilize the conformation of the anticodon end of the molecule, and they may fix it in one configuration. What happens when the aminoacyl tRNA combines with mRNA inside the ribosome? Is there a change in conformation, and could this be brought about by removal of the spermine and magnesium ions which are found in vivo [10]? We do not know the answer to this question but our present knowledge prepares us to carry out experiments in an attempt to find an answer.

tRNA functions in protein synthesis

Structural studies of biological macromolecules are chiefly of interest because they provide important clues to understanding the function of the molecule. As mentioned above, there are two major functions associated with tRNA during protein synthesis. First there is specific aminoacylation of a member of an isoacceptor tRNA family by a particular synthetase. Specificity in the addition of the amino acid is of course a prime requirement in determining the accuracy of protein synthesis. Secondly, there is the involvement of aminoacyl-tRNA within the ribosome where it contributes to the growth of the polypeptide chain.

Many studies have been carried out on the specificity of synthetases. Although these reactions have certain general features, the exact part of the tRNA molecule which is recognized by a particular synthetase enzyme is likely to vary from species to species. It has been suggested that most tRNAs are recognized by interacting with synthetases along the inner surface of the bent molecule [13]. The recognition region may thus include portions of the acceptor stem as well as, in some cases, the D stem or part of the anticodon stem or loop. The detailed solution of the recognition problem will probably come from crystallization of a tRNA bound to a synthetase and determination of its three-dimensional structure. Both tRNA and the synthetase enzyme are capable of undergoing conformation changes when they interact. Thus, firm information can be obtained only by studying their structure while the two molecules are together.

Much more difficulty is associated with understanding the interactions which tRNA has inside the ribosome. The ribosome is a large and complex organelle and it is clear that tRNA undergoes many movements within that structure. Work by Erdmann and his colleagues [14] has led to the suggestion that tRNA undergoes a conformation change in the ribosome in which the D and T loops become disengaged so that they can interact with the ribosomal 5 S RNA, perhaps while the tRNA is being moved. This area of research is quite an active one at present and it is clear that knowledge of the three-dimensional structure of the tRNA molecule in one conformation can be used to design experiments to test for the presence of other conformational states.

Finally, we are left with the question of why Nature has designed the tRNA molecule to have a structure in which a double helix essentially turns a corner to form an L-shaped molecule. Why is the anticodon almost 80 Å away from the amino acid at the other end of the molecule? The answer to this question will probably be related to the detailed movements involved in the transfer of the amino acid to the growing polypeptide chain. However, we may make a general ohservation. During protein synthesis, two tRNAs must act in concert in that at some time they must occupy ajoining codons on the messenger strand. This means the anticodon ends of the molecule must come close together. At the same time, transfer of the growing polypeptide chain to an adjacent aminoacyl-tRNA must occur. Tbis means that the 3′-ends of the molecule, some 80 Å away, must also come close together. Perhaps the intricately engineered L-shape of the molecule is designed to facilitate this transfer; that is, to allow both ends of two adjoining molecules to come close together. It is possible that this may occur by having adjacent codons become unstacked so that the message "turns a corner" as it is being read.

The full implications of our knowledge of the three-dimensional structure of tRNA are yet to be developed and they are likely to be understood in terms of the detailed mechanism of protein synthesis. Knowledge of the structure serves as a useful spur in the design of experiments which may ultimately lead to a full understanding of the dynamics of this process.

Non-ribosomal tRNA functions

In addition to functions associated with protein synthesis, tRNA molecules also have a variety of other biological activities [2]. These include the ability to act as a regulator of transcriptional activity, and the ability to donate amino acids to the N-terminal residues of preformed proteins, to cell walls and to other structures. In addition, tRNA act as a primer for the reverse transcriptase enzyme which makes DNA copies of RNA molecules. These and other non-ribosomal functions probably accumulated during a long evolutionary period, as Nature is often opportunistic in adding additional functions to pre-existing structures. It is possible that these additional functions explain the otherwise bewildering variety of nucleotide variations in the α and β regions of the D loop and in the variable loop. These sequence differences may be important in producing specificity in these additional non-ribosomal functions. The nucleotides in these variable regions are on the surface of the molecule where they can interact with other substances.

In summary, we can say that our present knowledge of the three-dimensional structure of one species of tRNA had provided us with a framework for understanding the structure of all tRNAs. Further, it has left us with the ability to ask very specific questions about the manner in which this small globular polynucleotide structure participates in a large number of essential biological processes.

SUMMARY — The participation of transfer RNA in the expression of genetic information is as universal as the genetic code. Recent work on the three-dimensional structure of one transfer RNA from yeast strongly suggests that the three-dimensional structure of all transfer RNA molecules are very similar to each orher.

REFERENCES

[1] HOLLEY R.W., APGAR J., EVERETT G.A., MADISON J.T., MARQUISSE M., MERRILL S.H., PENWICK J.R. and ZAMIR R., « Science », *147*, 1462-1464 (1965).

[2] RICH A. and RAJBHANDARY U.L., « Annu. Rev. Biochem. », *45*, 805-860 (1976).

[3] CRAMER F., *The early crystallization work is reviewed.* « Prog. Nucleic Acid Res. Mol. Biol. », *11*, 391 (1971).

[4] KIM S.H., QUIGLEY G.J., SUDDATH F.L. and RICH A., « Proc. Natl. Acad. Sci. U.S.A. », *68*, 841-845 (1971).

[5] KIM S.H., QUIGLEY G.J., SUDDATH F.L., MCPHERSON A., SNEDEN D., KIM J.J., WEINZERL J. and RICH A., « Science », *179*, 285-288 (1973).

[6] KIM S.J., SUDDATH F.L., QUIGLEY G.J., MCPHERSON A., SUSSMAN J.L., WANG A., SEEMAN N.C. and RICH A., « Science », *185*, 435 (1974).

[7] ROBERTUS J.D., LADNER J.E., FINCH J.T., RHODES D., BROWN R.S., CLARK B.F.C. and KLUG A., « Nature (London) », *250*, 546-551 (1974).

[8] QUIGLEY G.J. and RICH A., « Science », *194*, 796-806 (1976).

[9] JACK A., LADNER J.E., RHODES D., BROWN R.S. and KLUG A., « J. Mol. Biol. », *111*, 315-328 (1977).

[10] QUIGLEY G.J., TEETER M.M. and RICH A., « Proc. Natl. Acad. Sci. U.S.A. », in press.

[11] KIM S.H., SUSSMAN J.L., SUDDATH F.L., QUIGLEY G.J., MCPHERSON A., WANG A.H.J., SEEMAN N.C. and RICH A., « Proc. Natl. Acad. Sci. U.S.A. », *71*, 4970-4974 (1974).

[12] FULLER W. and HODGSON A., « Nature (London) », *215*, 817-818 (1967).

[13] RICH A. and SCHIMMEL P.R., « Nucleic Acids Research », *4*, 1649-1666 (1977).

[14] ERDMAN V.A., SPRINZL M. and PONGS O., « Biochem. Biophys. Res. Commun. », *54*, 942-946; RICHTER R., ERDMANN V.A. and SPRINZL M., « Proc. Natl. Acad. Sci. U.S.A. », *71*, 3326-3331 (1973).

DISCUSSION

WEISSKOPF

What determines the specificity of the three-base contact, codon and anti-codon?

RICH

We do not know that entirely. A more worrisome question is the following: that process could make lots of errors, but it does not. How does that process prevent itself from making errors? A related question is: when you add an amino acid here, it can also make errors, and one can estimate that the error level — let us say, of going from leucine to isoleucine which consists of simply putting a methyl group in one position or another, should be at the level of 1.10^2 — the actual error level for aminoacylation is one in ten to the fourth or ten to the five. Clearly this enzyme is clever. We think the way this occurs is the following: we know that when the enzyme adds the amino acid, it then leaves go and quickly re-attaches itself again, and it has a checking apparatus which says that everything does not fit in this case it will chop off the amino acid. So the enzyme has not only something for catalyzing the formation of the ester bond but also it has an esterase that releases it. We think that what goes on is that it has a kind of double reading, and if each of these gives us a factor of 1.10^2 it is easy to get up. In the ribosome we do not know how that goes on, it is a more complicated structure, we think something like that could occur — the time for one unit cycle in that diagram is in fact 50 milliseconds — that is a very long-time and you can do lots of things in 50 milliseconds, in terms of making sure everything is right, throwing it out, putting another one in, and so on. But it is clear that ribosome is a rather complex machine and a significant portion of it is related to making sure that errors do not occur. We know that because in certain mutations the proteins involved in making sure that errors do not occur are abnormal; and these things produce proteins with many errors. So we know that there is an error-correcting device — what we do not know is just how it goes on.

LEJEUNE

Je crois que la merveilleuse conférence de Monsieur Rich est une illustration de l'inépuisable ingéniosité de la nature. Je crois que personne au monde aurait imaginé la forme extraordinairement complexe que doit prendre le transfert ARN. En réalité la nature a inventé une machine infiniment très compliquée, et je voudrais poser dès maintenant une question précise à Monsieur Rich: voulez-vous nous dire ce que fait l'une des molécules de ARN?

RICH

The other one simply stabilizes the folding in this region. You see, you have negative charges close to negative charges in an extended way, and the spermine fits right between them, and sort of holds it all together.

LEJEUNE

Now, if you were removing that second one, would not those two extremities come together?

RICH

Yes, they would. There has been a great deal of discussion of the question does this molecule undergo a conformational change? For example, closing down, which it can do by opening up the T loop and the D loop. It should have been obvious but it required this example first, namely that if you neutralize part of the charges in a double helix, it would induce a bending, and one of the places where DNA has bent considerably is in chromatin, where one has nucleosomes and the DNA is actually wrapped around it. And the mechanism for that I believe is that the lysine side chains, the positive side chains of the histones approach one side of the DNA and therefore neutralize it and the thing naturally bends around.

MARINI-BETTÒLO

May I ask you does magnesium ion influence the conformation?

RICH

Well, you can use other ions — magnesium is the one that is found naturally within the cell, hut you can use other ions — you can get the molecule to fold up with only monovalent cations, but you must use more of them. You can also get the molecule to fold up without spermine but you must use a higher magnesium concentration, and in fact you can carry out protein synthesis without spermine — it will work; when you have spermine however, it then goes 50 times faster.

MARINI-BETTÒLO

But is magnesium essential?

RICH

Oh yes, magnesium is essential to the structure and is in fact held very tightly.

WIESNER

Can you determine the exact sequence by X rays?

RICH

No, at 2½ Å resolution you can distinguish a purine from a pyrimidine but not a U from a G, or a U from a C. So in fact we use sequence for this, but the crystals go to 2 Å resolution but even at 2 Å we won't see that. In order to be able to do that, one would have to go down to atomic resolution, and even there it is difficult because if you look at cytosine and uracil, you have to tell the difference between an amino group and an oxygen. Well, that is the same number of electrons, so that cannot be done. But it does not matter; the sequence has been determined quite accurately, and what we do do in carrying out the analysis is carry out a refinement which is of a special sort that introduces restraints and energy constraints in it, and this refinement has allowed us to refine to the point where we have a very good agreement between the observed and calculated data, good enough so that our difference shows all these ions together. I have not shown them to you, but you can see not only the magnesium but all of the water molecules in the hydration shell around it, so we know a great deal, but we must have the sequence — and I should say parenthetically the same is true for most protein structure determinations, where at 2 to 3 Å resolution you can distinguish big side chains from small side chains, but you cannot tell which one is which.

UBBELOHDE

I wanted to ask you about hydrogen bonds. Do you know whether they are long or short?

RICH

Well, I can give you the answer but for another molecule. We have in fact substituted D_2O into the alpha helix and have studied in great detail the change in hydrogen bond and the length of the hydrogen bonds holding the alpha helix together. We have measured of course the bond length — the distribution of the order of about 170 hydrogen bonds, their bond length is normal — 2.7 to 2.8 Å.

If you have a mutation right up here in the D stem, you change the way in which the anti-codon basis hydrogen bonds to the message. That is extraordinary — something happens 30 Å away. What we believe goes on here — in fact there is some evidence suggesting that when this combines with the codon, these bonds up in here are loosened 50 Å away, so what this is is an ordered system, an ordered array, and just changing parts of it either a short way away or a long way away may in fact change the whole manner in which the molecule is held together. So we are just at the stage where we are beginning to try and find out just what the tricks are.

CHAGAS

Now if there are no more questions, I thank Dr. Rich for his wonderful talk and I thank again my colleagues for their presence, their help, and the meeting is closed.

INDEX

C. Chagas: Foreword 5

I

SCIENTIFIC RESEARCH AND SCIENTIFIC POLICY

G. B. Marini-Bettòlo: Scientific research planning . . . 9

M. Lora-Tamayo: Les avantages et les dangers de la politique scientifique 21

G. Herzberg: The importance and needs of Canadian research in science 29

Discussion (Rich, Weisskopf, Herzberg, Siddiqui, Döbereiner, Ubbelohde, Garnham, Sela, Baltimore, Chagas) . . . 37

II

SCIENCE AND THE CONTEMPORARY WORLD

P. Lépine: La science et le monde contemporain 57

Discussion (Leprince-Ringuet, Weisskopf, Lora-Tamayo, Chagas, Tuppy, Blanc-Lapierre, O'Connell, Hörstadius, Pavan, Lépine, Roche, Siddiqui, Rich, Garnham, Ubbelohde, Lecomte, Sela, Khorana, Döbereiner, Baltimore, Croxatto) 67

III

THE LIMITS OF SCIENCE

V. F. Weisskopf: The limits of science 97

Discussion (Leprince-Ringuet, Baltimore, Weisskopf, Lépine, Sela, Pavan, Colombo, Siddiqui, Blanc-Lapierre, Roche, Croxatto, Oort, Rich, Marini-Bettòlo, Chagas) . . . 107

IV

NEW TRENDS IN SCIENCE

A. Rich: Transfer RNA: three-dimensional structure and biological function 129

Discussion (Weisskopf, Rich, Lejeune, Marini-Bettòlo, Wiesner, Ubbelohde, Chagas) 141