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The National Science Council of Sri Lanka

G. C. N. JAYASURIYA AND MARINA DE SILVA
National Science Council — Sri Lanka

Historical Background

Concepts of planning for science, are not new, although it was only in the last quarter century that state-recognized organizations were set up, specifically for the planning of science.

Agitation for the establishment of such an organization in Sri Lanka was begun as early as 1948. A memorandum on the establishment of a Council for Scientific and Industrial Research in Ceylon was prepared by the Ceylon Association for the Advancement of Science (CAAS) and presented to the Prime Minister, Mr. D. S. Senanayake. This proposal was well taken and a draft bill on lines similar to that establishing the Indian Council of Industrial Research was prepared. Although this issue was pursued vigorously, the Government did not take any further positive steps towards establishing this Council.

In 1953, sn IBRD team, appointed to report on proposals for the Economic Development of Ceylon, recommended the establishment of an Institute for Scientific and Industrial Research. The original proposals went askew from there on, and though the Ceylon Institute of Scientific and Industrial Research was established on 1955, a National organization for science in Ceylon had still got no more than a sympathetic hearing.

The need for some central organization for the planning and direction of scientific research in Ceylon was still keenly felt. Again the CAAS stepped in, and in an attempt to fullfil this need, even to a limited extent, the CAAS created within its own organization a General Research Committee (GRC).

Planning of science then came within the purview of this body of scientists. However, such a voluntary organization could do little other than advise the Government on some of its policies relating to scientific development. Lack of funds and non-availability of a full-time secretariat and the inaccessibility to data held in Government documents and files all contributed to the little impac this committee of voluntary officers could make.

Therefore in 1960, the GRC of the CAAS itself recognized this inadequacy and re-examined the original proposals for establishing a National body, which by statute would have overall responsibility for the planning and direction of the country's scientific development effort.

Ten years after the original proposals, the proposed National Research Council was yet not a reality. In the period 1961—1963, efforts for promoting this proposal were intensified, but final success was only to be achieved much later.

The sustained efforts of a few scientists led mainly by Professor N. G. Baptist, who pursued this issue in the face of much delay and frustration, merit very high commendation and their personal contribution toward the scientific development of the country should by no means be underrated.

The first official acceptance from Government of the need for a body like the National Research Council and the place of science in National affairs came in 1963, when the Prime Minister Mrs. Sirimavo Bandaranaike addressed the Nineteenth Annual Sessions of the CAAS. She said " I assume that your invitation to me, as Prime Minister, to open this Conference is your method of expressing your view that Science and Scientific Research is of primary significance to our country. My acceptance of your invitation will, in itself, indicate to you the fact that I rate your work as of the greatest importance."

On this same occasion, the Prime Minister in conclusion said, " For its part, the Government has accepted the need for the formation of a National Body with the necessary authority to guide scientific activity....I am glad to announce on this occasion that the Government holds the view that a body such as the National Research Council, which has been proposed for Ceylon by your Association, is most essential, and the necessary steps will be taken to constitute such a body as early as possible."

The Prime Minister following this took immediate action and a Committee, with the Permanent Secretary, Ministry of Education as Chairman, was appointed to work out details of an Act to constitute a National Research Council. However, with the change of Government in 1965, the establishment of this Council was delayed for a further period.

In November 1967, after almost two decades of agitation a bill to provide for " the establishment of the National Science Council of Ceylon and for matters connected therewith " was presented to Parliament by the Prime Minister, Mr. Dudley Senanayake, in his capacity as Minister of Planning and Economic Affairs.

The Act for the setting up of the National Science Council of Ceylon was approved by Parliament early in 1968. Although the CAAS had agitated for the Council to be directly under the Prime Minister, the Government created a new Ministry of Scientific Research and Housing and the Council came within the purview of this Ministry.

With the change of Government in 1970, the scope of the Ministry was expanded and a Ministry of Industries and Scientific Affairs under whose purview the Council presently functions, was created.

II The National Science Council : Its Structure and Organization

Set up by Act of Parliament No. 9 of 1968, the National Science Council became a reality in April 1968. The Council comprised twenty one members appointed by the Minister " from persons who had been closely associated with the pursuit, promotion or application of science ". Selection was also based on the representation of six areas of research. viz. Physical Science Research, Biological Science Research, Agricultural Research, Industrial Research, Medical Research and Social Sciences Research.

The only full-time member of the Council was the Secretary-General, who in terms of the Act could be selected from among the members of the Council. On a later amendment to the Act, however, the Secretary-General ceased to be a member of the Council and was appointed as a full-time officer of the Council on terms and conditions determined by the Minister in consultation with the Minister of Finance.

The Council functions at two levels, firstly, it deliberates on issues of policy, when the full Council meets. Secondly, it functions through a Secretariat, headed by the Secretary-General. It was recognized that a full-time supporting scientific staff was necessary if the Council was to function effectively. The background thinking on the Council's activities, and initiation of new activities comes within the purview of the Secretariat's functions. Although adequate provision for the recruitment of such scientific staff was made in the cadre, a full-time Secretariat only became operative three years after the Council was established.

Early in its tenure of office, the first Council realised that to effectively carry out its major functions, certain changes in the structure of the Council would necessarily have to be made. Its failures were considered to be primarily due to the fact, that the Council as a whole had no direct links with the Ministries under which the major scientific activities of the country were carried out.

In order to achieve greater co-ordination among the sectors of research activity presently being carried out in various departments, universities, and research institutions, it was suggested and accepted that the Ministries having related functions should be grouped together, and that Standing Research Committees should be set up within these Ministerial Groups. The Chairman of these Standing Research Committees were then appointed as ex-officio members of the Council.

The Second Council re-constituted in May 1972 in terms of these proposed changes felt, that even the new structure still posed certain difficulties for successful operation.

The Council was of the opinion that radical alterations to the present legislation constituting the Council would be necessary if the Council were to function more effectively. A draft bill which incorporated changes which the Council felt were necessary, if the objectives outlined in the Act were to be achieved, was prepared and submitted to the Ministry of Industries and Scientific Affairs. The Minister has accepted these proposals and submitted for Cabinet consideration, a memorandum, for new legislation based on the lines suggested by the Council.

III The Objectives of the National Science Council

The mandate given to the National Science Council is wide, even in comparison to that of sister organizations of the economically more advanced countries.

Its functions are not purely advisory, and the Council by statute has been empowered to actively support research. The Council has the power to make grants in aid of specific research projects and to erect, equip or maintain research units or laboratories either independently or in association with any other organization involved in any sphere of scientific activity.

The attainment even in part of the objectives spelled out in the enactment has been the major concern of the Council, in the initiation phase. In its attempt to make the planning of science a concept acceptable to the majority of scientists in the country, the Council has met with a certain degree of criticism. This is necessarily so in any venture dealing with new concepts and is even more so in a developing society. In its initiation phase, the Council is only too aware of the limitations imposed on it, and it has also recognised that it is often only possible to attain not the most desirable objectives, but those which can be attained with the maximum available resources.

In its attempt to strengthen potential areas where progress is possible, the importance of the publication of the results of research must assume high priority. It is towards this end, that the Council has decided to publish in Sri Lanka a journal, the scope of which will extend to all branches of Science and Technology.

The support that the Council has had in this effort is worthy of mention. The contributors to this Inaugural Issue are all scientists of eminence in their special fields, and their contributions are of the highest standards. The Council can therefore regard this as a fulfilment of a due national need. We strongly hope that this effort will be a stimulus to our own scientists, and will generate within Sri Lanka's scientific community the effort towards the publication of results of indigenous research in conformity with internationally accepted standards.

A retrospective assessment of the Council, its history and organization underlines a significant fact. The foresight and planning that generated the first idea of a Science Council, the subsequent efforts towards achieving this goal, the final organization of the Council and now the evaluation of its functions and future, have been due at every stage to the efforts of Sri Lanka's scientists alone. This perhaps, has been one major venture, which was set up endemically without any foreign consultation or aid. The acceleration of Sri Lanka's development effort will bring about new challenges and opportunities to its scientific community. New problems will arise, and old problems persist. Research and development will have to be provided, and planned for and the National Science Council will have to continue its effort towards evolving a national framework for science in Sri Lanka. The effectiveness of the Council will depend in large measure on the close contact which it would necessarily need to establish with the scientific community. There is an increasing need to co-ordinate policies and programmes for research activities within universities, Government institutions and industry, and this cannot be achieved without the collaboration of the scientists within these organizations.

The Council cannot at this stage promulgate an inflexible " Science Policy " for the country, but its efforts are geared towards the development of a broad framework, within which enlightened decision-making in science can operate. This will not merely be a matter of deciding priorities and allocating resources, but more important, the National Science Council will have to endeavour to create a climate in which scientific curiosity and sensitivity can flourish, for without these attributes, the investment on scientific research will show little or no returns.

We would like to end this note with a quotation from John Ziman, in Public Knowledge : an essay concerning the social dimension of science.

> "The true sociology of Science is not concerned with the relationship between Science and Politics, or scientists and politicians, between Science and Industry, or scientists and industrial corporations, but with the social interactions between a scientist and his colleagues."

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A World Federation of Institutes of Advanced Study

ABDUS SALAM

Director, International Centre for Theoretical Physics—Trieste

A number of groups have been working independently towards the project of setting up one or more world universities. That this is of importance in the context of the international future of mankind goes without saying. That at least one university did not come into existence at the same time as the United Nations organization did in 1945 is something of which the world's academic community cannot feel proud. Recognising this, at its twenty-fourth session, in 1969, the General Assembly of the United Nations adopted the resolution 2573 (XXIV) inviting the Secretary-General to undertake a comprehensive expert study on the feasibility of an international university. In introducing this widely sponsored resolution, it was stated that " the establishment of an international university would satisfy the aspirations which were becoming apparent in all parts of the world and it would fulfil an obvious need ".

There are at least four reasons for this universal interest in the setting up of one or more international world universities :

1. *The idealistic reason—international understanding*

   There is no instrument more potent in bringing an appreciation of different—at present national—points of view than the atmosphere of an international university.

2. *Global studies*

   Within the context of such a university there is the possibility of growth of international studies on global subjects—like international development, international economics, global environment, disarmament and the like.

3. *Contacts of scholars*

   Human knowledge transcends national boundaries. To a scholar interested even in his narrow speciality, there is nothing more valuable than the possibility of free contact with his peers from *all* countries. A well-constituted world university may resolve the present political difficulties in achieving such contacts.

4. *Access to specialized knowledge for scholars from developing countries*

In the past, when scholars and scientists have worried about international contacts, they have tended to feel concern about East and West contacts only. One tends to forget the needs for contact of students and scholars from developing countries with their peers from developed countries. Opportunities for such contacts do not exist—not for political reasons, but because of economic factors. A world university, representing East, West *and the third world*, is less likely to forget the needs of these students and scholars and more likely to afford them access to academic, scientific and technical areas at present the exclusive preserve of the richer countries. The developing countries fully recognize that a truly international university—preferably under the UN auspices—is the one real guarantee for their scholars to receive their share—as of right—of the facilities and resources of the international institutions to be created.

In response to the General Assembly resolution a study has been carried out on behalf of the Secretary-General. This study suggests the setting up of a set of postgraduate international institutions within the United Nations family—to be called UN International Universities—with two objectives :

(*a*) " To enable scholars from all parts of the world jointly to study research and reflect on the principles, moral imperatives, objectives, purposes, perspectives and needs of the UN system in the light of its fundamental laws and developing accords, declarations, resolutions and programmes".

(*b*) " Secondly, to undertake a continuing and widely-based international scholarly effort of study and research, directed in consonance with Charter obligations towards social, economic and cultural progress through co-operation among nations and peoples. The universities would achieve these ends through emphasizing.........relevant international studies, largely inter-disciplinary, of wide and generally global significance".

It is clear that the objectives of this particular response to the General Assembly resolution are limited to the special global studies related to global problems. This is not going to be a traditional university pursuing the traditional range of subjects, but a specialized institute or set of institutes.

Commendable as this response is, it falls short of the aspirations of at least two of the communities which have supported world university projects. By and large both these communities have had in mind the traditional range of academic disciplines, *in addition to global studies.* The two communities are :

(i) Academic scholars and scientists in East and West who desire in their traditional disciplines more contacts with each other.

(ii) Developing countries who look upon the world university idea as the one way by which they can secure entry for their students and scholars into the privileged intellectual, scientific and technological club on terms of equality. Notwithstanding the fact that no stated bar operates against anyone from a developing country pursuing advanced studies and research at any of the world's great institutes, in practice the economic and other factors do operate in such a manner that at least the scientific and technological gap between the poor and the rich countries grows ever wider. The developing countries look upon the world university project as a means to bridge this gap.

From this it would seem that nothing short of one or more full-fledged world universities in traditional disciplines—*at least for post-graduate scientific and technological studies*—will satisfy these two groups.

Unfortunately, to develop full fledged universities—and particularly under UN sponsorship—is not all that easy. One does not have to recount the difficulties which are likely to be met. Since the sums of money involved are large, it is out of the question that the United Nations Organization—even with the generous support of the World Bank—could finance such a venture. It is also unclear if one could get a number of the richer countries passionately interested in a project of this type and ready to back it. There are too many casualties among proposed international or regional institutions in the academic field already to give one great hopes of success, unless one proceeded in a gradual manner. Further, the choice of location of such a world university in one country in preference to another will always present difficulties. Even the choice of faculties to develop first is not going to be all that plain sailing.

One way to circumvent the difficulty of creating new institutes, and yet to achieve at least partly some of the objectives listed above, is to take advantage of existing centres of excellence and quality which would like to discharge international functions and to link such centres with the UN institutions for global studies proposed by the Secretary General, the whole making up the beginning of a world university.

This note then is concerned with a world university idea emerging *gradually* from an amalgam of the UN institutes together with existing centres of advanced studies linked in a federation. *In the first instance the emphasis is on post-graduate research and training for research.* Later development of the idea may envisage undergraduate studies and the corresponding institutions.

Let us consider the various stages of the *post-graduate* plan. The important point we wish to make is that every part of the plan has merits of its own, irrespective of whether the later stages follow or not. The first stage is the identification of such existing institutions which already operate substantial international programmes. There is around the world no dearth of institutions of quality which are to a lesser or larger degree international in character, even though their original charters do not specify this. The idea would be to make them even more consciously so. The hope is that a voluntary federation would help in this : at the least in defining norms and making it possible to share experiences ; at the best in raising new funds for the international operation. As a second part of the plan, and if this federation so chooses, a UN charter could be accepted and a formal link established with the UN Institution on Global Problems proposed by the Secretary-General. The centres constituting the federation and covering traditional disciplines together with the Secretary-General's UN University on Global Problems, would make a complementary whole–the beginning of a world university.

Such centres as should belong to the proposed federation must satisfy certain criteria. For example, such centres must possess the highest rating of quality ; they must possess—to a lesser or greater degree—an international faculty of staff and research fellows ; they must agree to spend a minimum proportion (to be fixed, perhaps between 15%–25%) of their resources and their facilities towards furthering the work of high-grade scholars from developing countries.

To illustrate the working of one such centre, one may perhaps cite the example of the International Centre for Theoretical Physics at Trieste, Italy. This case is not typical because the institute is financed and run by two of the United Nations Agencies, but it does provide an example of the type of international academic faculty in actual operation. The centre was set up under the auspices of the International Atomic Energy Agency (IAEA) with the co-operation (and from 1970 equal participation) of the United Nations Educational Scientific and Cultural Organization (UNESCO). The centre is devoted to *imparting training for* and *conducting research in* all disciplines of theoretical physics at the highest level. It draws its scientific faculty (consisting mainly of visitors) and research fellows from (theoretically 100 but in practice) some 50 countries of the East, West and the third world. *Some 50% of its facilities and junior and senior research positions are reserved for scientists from developing countries.* A unique feature is that the Centre offers dual appointments to active senior theoretical physicists from developing countries. Such appointments are held for periods of three to five years ; the scholar spends the bulk of his time—about nine months of the year—in his own country, and the remaining three months of every year in Trieste. In addition, the Centre has built up federation links with some twenty research institutes in various countries—on a cost-sharing basis-which afford mobility of their staffs and research fellows. On the East-West co-operation side, as a UN-sponsored organization, the centre plays an absolutely unique role : it is one of the few places in the world where physicists in subjects as sensitive as plasma research from the East and West meet regularly and for prolonged periods (quarters or years) and with no national pride or sensitivities inhibiting scientific concourse.

The proposed **World Federation of International Institutes of Advanced Study** would include centres with already a large international programme or desirous of starting one. The institutes which would join this federation may operate schemes of *dual appointments* and *federation* with corresponding centres both in developed and developing countries. From informal contacts one knows that a number of institutes in USA, USSR, Great Britain, France and other countries are extremely desirous of widening their faculties internationally to share staffs and visitors with others in the same disciplines and, through the strength given to their international programmes by the fact of belonging to such a federation, be obliged to throw their doors even more widely open to scholars from developing countries.

Why should a federation be created of institutes in diverse subjects ? What advantages could come to the members of the proposed federation ? Should it be *independent* institutes as well as institutes within national universities which should be invited to join ? What about the financing of the international programmes ? And the links to the UN family ?

In answering these questions, one has to ask if the federation could be stronger in any way in carrying out the international aspects of its programmes than any one of its component units ? Would, for example, the Trieste Centre get any benefit by being federated in a sort of loose link with the Institute for Advanced Study at Princeton, or the Salk Institute for Biological Studies ?

In our opinion, the answer to the last question is an affirmative " yes ". The fact that a federation exists is likely to have important repercussions :

1. To get the general idea of international staffs and international use of facilities of scientific institutes accepted in a more " official " manner by the governing bodies of the institutes.
2. To secure a mobility of high grade scientific personnel. Hopefully, there may emerge a UN Laissez Passer for academic personnel to travel freely, at least between the federating institutes, if the UN did get involved with the federation idea.
3. A commitment in respect of scholars from developing countries : a federation to which a fair number of reputable institutions belong would go much further in organizing and getting accepted common standards.

The committing of a certain *percentage* of resources to helping scholars from developing countries, and to scholars from countries with different political systems, is a new idea. Many institutes do set aside certain sums but there is no coherent policy about this. We are hoping that belonging to a federation would provide a visibility to these efforts and a better focus.

4. If we envisage that institutes from developing countries would also belong to such a federation, these institutes will in many cases have to raise their standards in order to qualify to join. This type of pressure would be an excellent tonic for them, and make the tasks of those running these institutes vis-a-vis their own governing councils—and their Governments—somewhat easier.

5. In respect of the question raised, whether it should be independent institutes which should federate or those located within universities, one should keep an open mind. In every case the permission of the governing bodies of the institutes would be needed. I believe this is easier for independent institutions. For the present we may envisage only such institutes being invited, but the matter should be dealt with pragmatically.

6. The question of financing international programmes is a difficult one. It is definitely envisaged that in the first instance the members of the federation would find funds from their own sources for this. Later, collective action may bring extra funding from outside—even from UN sources.

7. A first list of possible independent or semi-independent institutes which may consider forming initial membership of the federation is suggested in the Appendix. It is suggested that a preliminary meeting of Directors of these institutes be held to gain acceptance of the ideas in this note.

Some of the large private and semi-private institutes—and possibly also some of the university departments—may join as Associate Members of the Federation. For example :

1. CERN—Theory Division and Electronics (Technology) Division ;
2. Oak Ridge Laboratory—Life Sciences Division ;
3. Centre d'Etudes Nucleaires de Saclay—Solid State Division ;
4. Philips Research Laboratory, Eindhoven (Director : Dr. G. W. Rathenau).

Note added November 1972

This memorandum was circulated in 1970 in a mimeographed form. The late Professor Arné Tiselius, President of the Nobel Foundation took up the ideas and at two Serbelloni meetings held during 1971 and early 1972, the idea of an International Federation of Institutes of Advanced Study was hammered out.

This Federation, consisting at present of 24 Institutes, was inaugurated at a meeting at Trieste during October 1972. Its offices are located in the Nobel Foundation House, Stockholm. Its Chairman is Nils Stähle and its Secretary is Sam Nisson.

The Federation may become the precursor of a World University.

Appendix of Pure Science

1. International Centre for Theoretical Physics
   Trieste, Italy.

2. International Institute of Mathematics
   Warwick
   (Director : Prof. E. C. Zeeman)

3. Institute of Theoretical Astronomy
   University of Cambridge
   Cambridge, England
   (Director : Prof. F. Hoyle)

4. Medical Research Council Laboratory for Molecular Biology
   Cambridge, England
   (Director : Prof. M. F. Perutz)

5. Weizmann Institute of Science
   Rehovoth, Israel
   (President : Prof. A. Sabin)

6. Niels Bohr Institute
   Copenhagen, Denmark
   (Director : Prof. A. Bohr)

7. Institute for Advanced Scientific Studies
   Paris, France
   (Director : Prof. L. Motchane)

8. Institute for Advanced Study
   Princeton, New Jersey, USA
   (Director : Prof. C. Kaysen)

9. Salk Institute for Biological Studies
   San Diego, Calif., USA
   (President : Dr. J. E. Slater)

10. Instituts Internationaux de Physique et de Chimie, Solvay
    Brussels, Belgium
    (Director : Prof. I. Prigogine)

11. Tata Institute of Fundamental Research
    Bombay, India.
    (Director : Prof. M. G. K. Menon).

12. Institute for Physical Problems
    USSR Academy of Sciences
    Moscow, USSR
    (Director : Academician P. L. Kapitza).

13. Mathematical Institute of the Academy of Sciences of the USSR
    Moscow, USSR
    (Director : Academician I. M. Vinogradow).

14. Institute for Theoretical Physics of the Academy of Sciences of the Ukrainian SSR
Kiev, USSR
(Director : Academician N. N. Bogolubov
(Deputy Director : V. Shelest).

15. Institute for Nuclear Physics
Novosibirsk, USSR
(Director : Academician A. H. Budker).

16. Institut Pasteur
Paris, France
(Director : Prof. Pierre Mercier)

17. Max Planck Society for the Advancement of Science
Munich, Fed. Rep. Germany.

18. The Institute of Immunology
Basle, Switzerland.

19. Cold Spring Harbor Laboratory of Quantitative Biology
New York, USA

20. Institute of Hydrobiology
Lago Maggiore, Italy

21. Foundation J. Oswaldo
Cruz, Brazil.

22. Institute of Mathematics of the Polish Academy of Sciences
Warsaw, Poland

23. Institute for Fundamental Technical Problems
Warsaw, Poland

24. Medical Research Council National Institute for Medical Research
Mill Hill, London N.W. 7
England
(Director Sir Peter Medawar)

25. Division of Radio Physics
Commonwealth Science and Industrial Research Organisation
Sydney, Australia
(Director : Dr. E. G. Bowen)

26. Walter and Eliza Hall
Institute for Medical Research
Royal Melbourne Hospital
Melbourne, Australia
(Director: Prof. G. J. V. Nossal)

27. Institute of Biophysics
Avenida Pasteur 458
Rio de Janeiro
(Director : Prof. Carlos Chagas)

28. Mathematical Centre
49, 2e Boerhaavestraat
Amsterdam, The Netherlands

3—12550

Appendix for Technology and Applied Science

1. Woods Hole Oceanographic Institution
   Woods Hole, Mass., USA
   (Director : Prof. Paul M. Fye)

2. Worcester Foundation for Experimental Biology
   Shrewsbury, Mass., USA

3. Sloan-Kettering Institute for Cancer Research
   New York, N. Y., USA

4. International Rice Research Institute
   Manila, Philippines
   (Director : Dr. R. F. Chandler, Jr.)

5. International Maize and Wheat Improvement Centre
   Mexico
   (Director : Dr. Norman E. Borlaug)

6 Asian Institute of Technology
   Bangkok, Thailand
   (Director : Milton E. Bender, Jr.)

7. Iron and Steel Institute of Japan
   Tokyo, Japan
   (President : Ichiro Fujimoto)

8. Institut Francais du Petrole, des Carburants et Lubrifiants
   Paris, France
   (President and General Manager : R. Navarre)

9. M. Nencki Institute of Experimental Biology
   Warsaw, Poland,
   (Director : Prof. J. Konorski)

10. Carlsberg Foundation Institute for Fermentation
    Copenhagen, Denmark
    (Fermentation Studies)

11. International Centre for Seismology
    Edinburgh, Scotland

12. Institute of Geology
    Delft, Holland

13. Japan Institute of Metals
    Sendai, Japan
    (President : Prof. Yonoshin Imai)

14. (Proposed) Institute on Insect Physiology
    Nairobi, Kenya

15. Rothampsted Experimental Station (Agricultural)
    Harpenden, Herts., England
    (Director : Sir Federick Bawden)

16. International Centre for Advanced Technical and Vocational Training
    Turin, Italy

17. Batelle Memorial Institute
    Geneva, Switzerland
    (Director : M. Thiemann)

18. Cancer Institute
Lyon, France

19. Stanford Research Institute
Menlo Park, Calif., 94025
(President : Charles A. Anderson).

20. German-French Reactor Inst.,
Grenoble, France.

21. Pakistan Institute for Nuclear Science & Technology
Islamabad, Pakistan.

Appendix for Social Science

1. UNITAR (United Nations)

2. Centre for Advanced Study in Behavioral Sciences
Stanford, Calif., USA
(Dr. O. Meredith Wilson)

3. Economic Development Institute of the World Bank
Washington, D.C., USA

4. United Nations Research Institute for Social Development
Geneva, Switzerland.

5. Centre for Cultural and Technical Interchange between East & West
Honolulu, Hawai, USA
(Chancellor : Everett Kleinjans)

6. European Institute for Advanced International Studies
Nice, France
(Director : Alexander Marc)

7. Peace Research Institute
Stockholm, Sweden
(Director : Dr. R. R. Neild)

8. The John F. Kennedy School of Government
Harvard University
Cambridge, Mass., USA
(Dr. Don K. Price)

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The Construction of Biosynthetic Hypotheses

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Relations between structure and biosynthesis have stimulated speculation since early days of natural product work. The subject was, and still is, an art rather than a science, one of its most appealing aspects being that it involves creative speculation, the results of which can ultimately be tested. It involves induction, rather than deduction of the type which marked the early work by classical biochemists. It is characteristic of the organic chemist rather than the biochemist. The great advantage of such speculation is that, if successful, it can suggest appropriate biochemical experiments in connection with complex structures which might be very laborious to tackle by trial and error. The big disadvantages are that with the exception of the acetate polyketide theory discussed below, the units are usually large, and theory often does not indicate how they arise, and that the exact order in which a series of transformations occurs often is not suggested.

Success in such speculative exercises cannot be based on automatic rules : it is dependent on an ability to recognise the biogenetic units in molecular skeletons, despite obscuring subsequent processes. This in turn depends largely on the ability to postulate appropriate chemical reactions, which may well not be known biochemical ones at the time, for the junctions of the units and for subsequent changes. A very good knowledge of organic mechanisms, and a feeling for the compatibility of the postulated processes with what is known of biochemical mechanisms are requisites. The classical biochemist frequently knows too little about the former, and is perhaps unduly inhibited in going far afield in the latter.

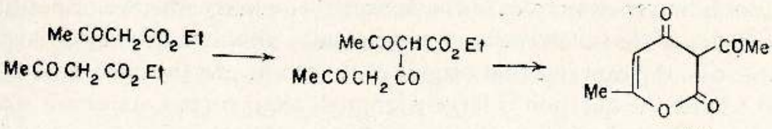
A survey of some previous approaches reveals more of the requirements for success than would an attempt at a systematic treatment of the subject.

The first major attempt at a biosynthetic correlation, the polyketide hypothesis[1], was developed in useful detail only some 45 years after its initial postulation, which was itself completely sterile. Its initial failure is a good example of how the proverbial baby can be thrown out with the bath water. The essence of truth was obscured by unnecessary accretions due to attempts to extend it beyond its scope to an all-embracing philosophy, and to be original at all costs no matter whether or not the point at issue was genuinely relevant to the hypothesis. The fact that it could be used, (although with the exception of orsellinic acid was not explicitly used) to explain a number of natural structures very well was neglected, apparently because of the obvious straining to explain rather fancifully and unconvincingly other structures claimed to be within its scope. Its exposition was vague and in some places contradictory. Two lessons emerge : the hypothesis must be very clearly stated and its limitations circumscribed by the theoretical support available. Later work[2] in the area is an even more explicit example of the approach not to take : to start with a fixed idea that, for example, carbohydrates of any size or chain shape are naturally available, despite lack of evidence that this is so, and then to postulate unlimited enzymic specificity in oxidations, reductions, or ring-closures. It is then possible to make anything on paper, and the results are meaningless in terms of suggesting fruitful biochemical experiments.

The original author of the polyketide hypothesis, J. N. Collie, in fact began[1] with a very good and sound idea, based on his laboratory experiences with "dehydracetic acid" and some of its derivatives, namely that β-polyketones can ring-close or react with each other to give phenolic compounds by aldol or C-acylation procedures (from esters) and that in this way a number of natural products could be structurally explained. If his paper of 1907[1] had been followed up logically, as was the alkaloid biosynthesis paper of Robinson in 1917[3], it could have marked a major step in biosynthetic understanding.

The only subsequent publication was in a textbook[4] by A. W. Stewart, a personal friend of Collie, and the account, from general acknowledgements to Collie and with many paragraphs marked as direct contributions from him, must be considered as authoritative of his views. The clear stream of the original idea is lost in this account in a maze of muddy ancillary hypotheses.

The starting-point of the initial hypothesis was the experimental investigation of dehydracetic acid then wrongly formulated as (1) [actually it is (2) from a branched rather than a straight-chain, and the error is of little consequence to the theory]. The substance is derived from two molecules of acetoacetic ester, and hence from four molecules of acetic ester.



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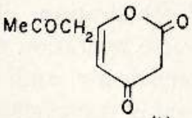
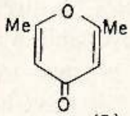


(1)



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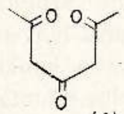

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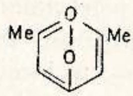
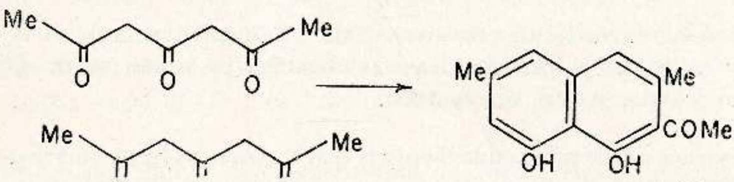


(5)

The action of acid converts it into dimethylpyrone (3) presumably via the " polyketide " (4). The action of alkali on (4) led first to a monocyclic and then to the bicyclic phenol (6) as shown. Under different conditions dehydracetic acid on reaction with alkali gave orcinol (7, R=H) and orsellinic acid (7, R=CO_2H) known from natural sources. The resemblance of (6) to natural phenols, and the identity of (7) with a natural substance led Collie to the idea that such " polyketens " or " polyketides " might be intermediates in phenol biosynthesis. Unfortunately he did not discuss other natural examples.

A further experimental observation was the condensation of orcinol with acetoacetic ester to (8), and decarboxylation of this to (9), which has, as he points out, the skeleton of the monoterpene thymol. He did not in any explicit terms attempt to extend the idea to terpene biosynthesis in general. If it can be assumed that he was thinking in terms of polyketide origins in acetate then this is the first suggestion of such an origin for a terpene.



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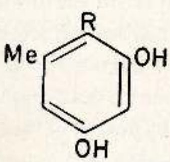

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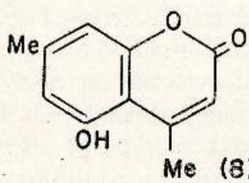

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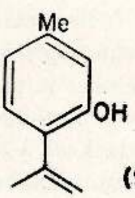


(9)

It is not however clear from his or Stewart's summary whether or not he had any clear idea of the involvement of acetate units, although this might have been assumed from the experimental origins of the work. In the later discussion in Stewart's book the question is largely ignored, apart form a statement[4] (p 276) " keten can polymerise to long chains which then add water to form polyketides " The discussion is taken up almost entirely, however, with the possible origins of polyketides from carbohydrates by directed dehydration. There is also the suggestion that this may be reversible by reverse hydration of polyketide enols, incidentally an unlikely process mechanistically and without laboratory analogy as Collie admits : " it must be frankly confessed that up to the present our laboratory methods have failed to bring about either of these conversions " (i. e. formation of polyketide from carbohydrate or the reverse.)

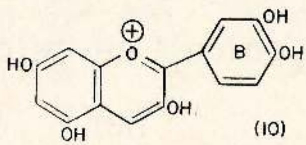
The fact that very unusual carbohydrates would be required, and were not known, was also ignored. Whether the keten units were thought to arise from acetate, and whether the keten idea was tacitly dropped in favour of carbohydrate origins from formaldehyde, of which there is discussion, is not clear. Certainly the confusion of ideas could not have led to much faith by readers in the validity of any of them.

Another confusing aspect is the unnecessary attachment of rather bizarre hypotheses which really have nothing to do with the main one, such as a new formulation of carbohydrates, literally as hydrates of a carbon chain " these water molecules envelop the carbon atoms and that the three atoms of the water molecule rotate round the carbon atom to which they are related ".

$$\begin{array}{ccccccc} C & — & C & — & C & — & C \\ (H_2O) & & (H_2O) & & (H_2O) & & (H_2O) \end{array}$$

This was combined with a rejection of the " antiquated conception of directed valency ". If the polyketide idea was classified by readers with such ideas, it is no wonder it was disregarded.

An essence of the polyketide theory is that it places oxygens and ring-closures in 1, 3-positions to each other. Examples of straining the theory to fit inappropriately substituted examples are given by Stewart[4]., presumably with Collie's acquiescence. The anthocyanins and flavonoids are derived from an unknown C_{15}-sugar " the reaction may be traced directly back to a carbohydrate chain without requiring the intermediate formation of a polyketide derivative at all". This statement is presumably due to the extremely frequent occurrences of 4-hydroxy or 3, 4-dihydroxy B-rings, which do not fit the hypothesis, and the complete lack of 3-hydroxy and 3, 5-dihydroxy B-rings which do (compare cyanidin 10). Robinson later correctly used this oxygenation pattern to relate this part of the molecule to many known C_6-C_3 compounds. Similarly, the alkaloid papaverine (11) was postulated by Collie, in a forced manner, to arise as follows, the α-dicarbonyl chain being formed arbitrarily " by the usual process of dehydration and rehydration ".



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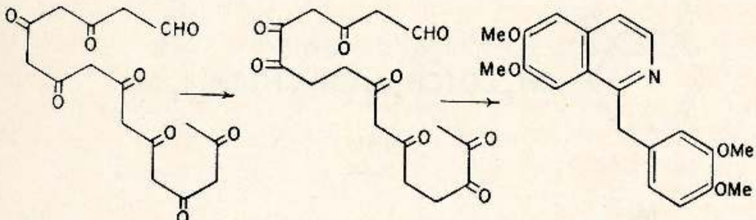
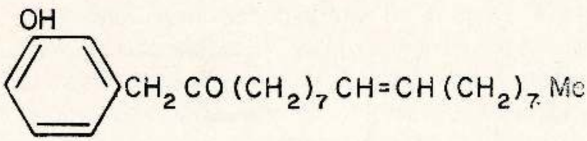


(11)

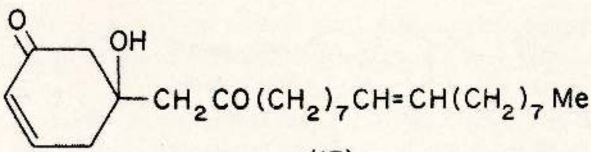
Our own approach to the polyketides taken in 1951-52[5], which owed nothing to Collie, since we were unaware of his work, or to Robinson who first published[6] his ideas after our own (1955), was based firmly on mechanistic considerations. It was fortunate in the availability of two major pieces of information from biochemical sources. One was the origins of some natural benzenoid compounds (particularly 4-oxygenated compounds such as tyrosine) from shikimic acid. The other was a knowledge of the intervention of " acetate units " (acetyl coenzyme-A) in the biosynthesis of fats and steroids.

Our first recognition of polyketide origin was with campnospermonol (12), the side-chain of which, up to the carbonyl, is clearly oleic acid (although the double bond stereochemistry was unknown). Since this acid arises from acetate units, the question was whether the rest of the molecule could come from a continuation of the process, invoking only mechanistically probable processes. Pencil and paper showed that it could arise as shown below, invoking reactions (a) aldol cyclisation (b) reduction of a carbonyl (c) dehydration of β-hydroxyketones and (d) decarboxylation of a β-keto-acid. The intermediate (13) was later isolated as a natural precursor of campnospermonol ;

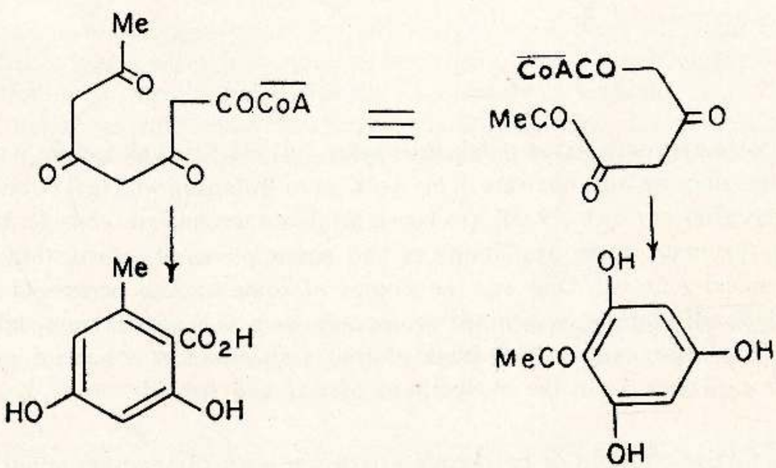
the theory of course did not indicate the order of some of the steps. Proceeding from this to simpler cases, orsellinic acid and acetylphloroglucinol were noted as the prototypes of a series of homologues and analogues, and could be formulated from the same precursor as follows :



(12)

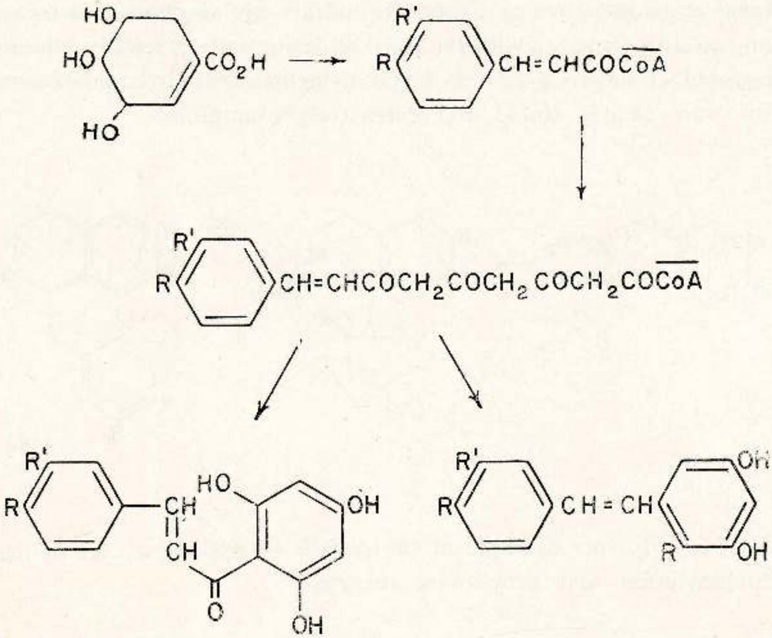


(13)



Loss of expected oxygen through specific reduction of a carbonyl in an intermediate, and the addition of other oxygens by o,p-oxidation readily accomodated other compounds. The hypothesis was rapidly tested, using radioactive tracers in moulds and plants, and shown to be essentially correct. Some 3000 natural structures can now be explained as wholly or partially polyketide. The details are too well known to merit repetition.

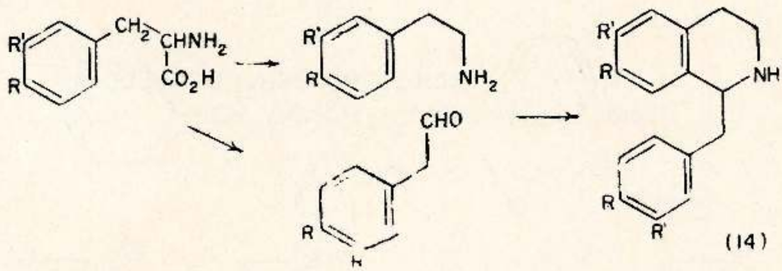
It is worth noting, however, that almost the first use of our hypothesis was to suggest for the first time the correct origin of the flavonoids and anthocyanins from a C_6-C_3 (shikimate) unit and three " acetate " units, the identical intermediate being convertible into the plant stilbenes :



A completely logical approach, using only valid chemical reactivities, thus led directly to what was later shown to be the correct explanation of a long-standing puzzle and as a bonus provided the answer to another.

The still somewhat arbitary assumptions that had in general to be made, and they seem reasonable, is that enzymes determine the number of units involved, the exact directions of cyclisation processes, the situations of any carbonyl groups reduced, and of oxygen introductions. No type of reactivity invoked was novel from the chemical point of view.

Robinson's classical paper of 1917[3] on the biosynthesis of alkaloids is a fine example of a pregnant speculation, despite the fact that many of the details were later shown to be incorrect. It was valid and fruitful because of the main point made with numerous detailed examples : that a logical application of only two reactions, the generalised aldol condensation and the generalised reaction of amines with carbonyl compounds, could lead to a convincing picture of the relations of many alkaloid structures to one another and to those of possible precursors. The predicted and proven origin of benzylisoquinoline alkaloids (e.g. 14) is shown. It probably succeeded in part because of the rather large size and recognisable character of many of the units involved (e. g. C_6C_2N, C_6CCO etc.) and the fact that they are marked out in the relatively permanent main skeletons by C and N, and are not so dependent on oxygen positions on a skeleton, as with the polyketides, a pattern readily obscured by oxidation-reduction processes. It was convincing however largely because its premises were clearly stated and extensively exemplified.

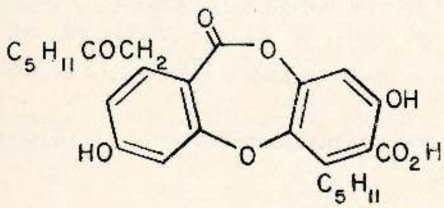


(14)

Finally, as a further example of the pitfalls of hypothesis, let us examine the C-methylation and propionate theories.

Many lichen products, such as (15) indicate from their structures an acetate-polyketide origin, despite some changes of an obvious nature, in this case an oxidative ring-closure. The number of such compounds was so great, that the basic theory seemed obviously correct. There were however some " extra " substituents inexplicable on this basis. By analysing all of the structures in a review the following distribution of substituents on the units (16) was found[7]. The side-chain always has an odd number of carbons, oxygens are always found in the 2, 4-(orcinol) positions and there are several " extra " oxygens at the 4- or 5- positions (and one Cl) which could be introduced by mechanistically acceptable processes. The problem was that 17 of the 57 structures contained a C_1-unit (as CH_3 CHO or CO_2H) at the 3-position. I recall that in a lecture in Liverpool in early 1952, Dr. F. M. Dean raised this as a valid objection to the theory. In conjunction with other evidence, such as the C-methylated acylphloroglucinols (17) (R=H or Me), the structures suggested the introduction to carbon of a C_1-unit, and although it was then not a known biochemical process, the question arose as to whether this could occur through trans-methylation with methionine, known to occur with O-, S- and N-methylation. This expectation arose from the known mechanistic resemblance of such hetero-atom reactions to alkylations of phenol-enol systems, and laboratory analogies of formation of (17, R=Me) from acylphloroglucinols.

The hypothesis was rapidly supported[8] by using [^{14}C]-Me methionine with the mould metabolite mycophenolic acid, which contains both OMe and a similar C-Me, which are found to be equally labelled. Many other examples followed.



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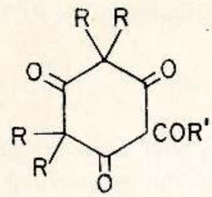
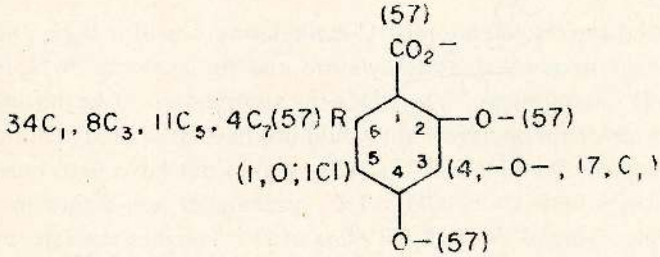


(17)



(16)

Position : 1. 26CO_2H ; 31CO_2- :

2. 55OH ; 20Me :

3. 10H ; 3-OCO- ; 9Me ; 6CHO ; 2CO_2H :

4. 11OH ; 20OMe ; 26-OCO-:

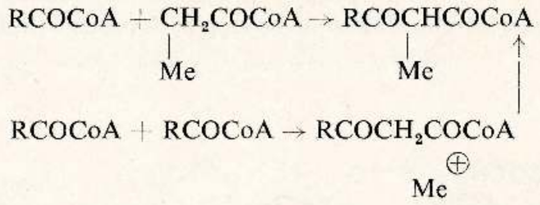
5. 1Cl ; 10H :

6. 33Me ; 1CH_2OH ; 8C_3H_7 ; 10C_5H_{11} ; 1C_7H_{15} ; 3$CH_2COC_5H_{11}$ 1$CH_2COC_3H_7$.

In 1954, at a Gordon Conference, I discussed the acetate-polyketide hypothesis, and R. B. Woodward who was present, and was then working on the structure of magnamycin, suggested the additional intervention of "propionate" to explain the C-methylated units in macrolides, as did Robinson in 1955 for mycolipenic acid. The final structural consequences of the two routes would be the same, as shown by the prototypes :

Propionate

Methionine



The later discovery of the intervention of malonate derivatives is irrelevant so far as the principle is concerned.

In fact both routes were found to occur in different cases, and " Occams razor " is rather dangerous as a tool in biosynthetic speculation ; the existence of one valid explanation does not rule out another possible one under different circumstances.

A second type of methionine C-methylation has also been observed. The classical one involves transmethylation and for example $^{14}CD_3$ is transferred from $^{14}CD_3$-methionine[9]. The observed methylation of an unactivated C=C as in the side-chain of ergosterol, would not have been predicted by the original approach, since the anionoid reactivity would not have been considered sufficient, as it is with C=C-OH. The mechanism was found to be different, transfer occurring of $^{14}CD_2H$, with loss of D. The intermediate is undoubtedly a cyclopropane, and the reaction resembles a carbene addition rather than a carbonium ion alkylation[10]. The initial ideas at least suggested the correct biochemical experiments, leading even to the discovery of an unpredicted process. In this area, any hypothesis is better than none, provided it leads to further experimental investigations.

Investigations of natural product structures are still of great interest in leading to new biosynthetic processes, or variants or refinements of old ones.

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Symbiosis in the Ecosystem

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In 1935 Tansley[1] introduced the term "ecosystem" to comprehend the organisms, plant, animal and microorganism ; and the physical and chemical components of their immediate environment which together form a self-contained ecological entity. Ecosystems, in his view, tend to progress towards equilibrium wherever the factors at work are constant and stable enough for sufficiently long periods of time. He applied to ecosystems the expression " quasi-organism " which registered the fact that wherever conditions are the same and the same components are present, the same system will tend to reestablish itself and progress in the same manner. It is perhaps one of the most important developments of modern ecology that the physiology of ecosystems is being actively studied.

The ideas behind such studies are relatively simple. The organisms of an ecosystem are classified according to their nutrition and physiological activities into groups like synthesizers, consumers, and decomposers. Inorganic substances enter the living part of the system as carbon dioxide, inorganic compounds of nitrogen, phosphorus, and sulphur and other essential elements, and are built by the synthesizers, mostly green plants, into their own body compounds. These form the food substrate for other organisms of the system which require organic carbon compounds, including amino acids, proteins or other preformed foods. In all stages in the consumption or decomposition of material initially produced by synthesis, carbon dioxide is released by the animals and heterotrophic plants such as fungi and bacteria. In the final stages, in addition to carbon dioxide, inorganic nutrients are released and may be reused by the synthesizers.

Although these processes are simple in outline they are very complex in detail and their study in quantitative terms is very difficult. It presents complex problems of definition, sampling and estimation. It has two essential aspects, quantity and rate of change. The first concerns the estimation of total of carbon, nitrogen and other substances present in various forms in both the living material and in the non-living phases of the system. The second requires the estimation of rates of absorption of substances into the living parts of the system, their rate of passage through complex consumption and decomposition cycles and their rates of release again in inorganic form.

Such investigations are so very difficult that detailed measurements have not yet been obtained for any land ecosystems, although good general estimates have been made in some cases. In this essay it is not proposed to review the subject of nutrient cycling itself but rather to consider some interesting close symbiotic relationships between heterotrophic and autotropoic organisms and their place in the physiology of ecosystems.

Forest Ecosystems

In forests, organic matter containing most essential elements for living organisms reaches the soil in large measure by the fall of leaves, stems, branches, bud scales, flower parts and by the abscission of roots. These are generally already attacked by microorganisms during their senescent phases, before they become detached.[2,3] These microorganisms, especially fungi and bacteria, bring about the early stages of degradation which are continued later by interaction in the soil by more diverse organisms, plant and animal. At first mineralization of the inorganic nutrients is slow especially in temperate climates. In the plant material carbon compounds are relatively plentiful but other nutrients are in relatively short supply because in the plant the skeletal matrix consists in large measure of carbohydrates such as cellulose. As a result the litter and highly organic horizons of the soil are regions where growth may be much limited by the availability of nitrogenous and other inorganic nutrients. In later stages of decomposition when much carbon has been utilized and carbon dioxide eliminated, the system including the bodies of the organisms becomes relatively richer in nutrients, and progressive mineralization accompanies decomposition. It is to be expected in climax communities which are approximately stable over long periods of time, that within one season the equivalent of the carbon in the materials returned to the soil in that period is eliminated as CO_2. The amount of organic matter which is present in the soil especially in the surface layers is not therefore an indication necessarily of the rate of breakdown. It is more an indication of the turnover rate of half-life of soil organic matter.

It has commonly been assumed that the soil is a region of intense activity of microorganisms especially in the horizons of high organic content. This arises in particular from the facts that very large numbers of many diverse species of fungi, bacteria, and actinomycetes may be isolated from it employing cultural procedures and that there appears to be a considerable reservoir of organic material within it. This last point is dubious because we have seen that the organic material is mainly in the process of breakdown rather than a reserve, even in temperate soils. The idea of large populations of microorganisms arises from the fact that most methods for their isolation do not distinguish between active organisms and dormant spores.[4,5] Hence most of the methods used measure more the potential biological activity rather than the active biomass. The soil population of these microorganisms should be viewed in reality as consisting mainly of dormant spores and resting bodies 'lying in wait' for suitable substrates but given these, capable of great diverse activity.

The addition to a soil of a suitable substrate may call into activity a wide variety of dormant microorganisms which together in parallel or in series bring about decomposition.

This concept of the soil as a reservoir of potentially great biological activity but with a lower mean level of activity has been aptly emphasized by Gray and Williams.[6] They make two important points which have not previously received adequate stress.

First the amount of carbon falling as litter upon the soil, for instance in a forest ecosystem, is inadequate to maintain the growth of the apparently active heterotrophic organisms in that soil except on the average at a very slow rate. Secondly, the measured rate of CO_2 output from the soil may far exceed the apparent losses of carbon compounds from the soil estimated by analysis of litter disappearance. For example it would appear that Witkamp[7] and Rainers (1968) working in temperate forests were only able to account for about 60% and 30% respectively of the CO_2 emission from the soil by the carbon added to it as plant remains. A large part of the soil respiration must arise from some other source. Similarly Wiant[8] showed that even if CO_2 emission from a forest soil was assumed to continue at the lowest rate estimated during the year (0.20 g per M^2 per h) the litter fall would only maintain the rate for 136 days, not a full year. Such figures are so discrepant that if we were to suggest that they were due solely to errors of estimation of biomass or of CO_2 emission, it would be tantamount to calling into question the kind of evidence on which most of the existing ideas about carbon cycling are based. We must therefore look for other rational explanations whilst accepting that some degree of error is certain. Some have ascribed the excess of CO_2 production to the respiration of roots, but this does not seem sufficient to explain the large discrepancies.

One possible explanation is that there are particular sites where easily available supplies of organic compounds are present which are not derived from the fallen litter and abcised roots and on these active populations of microorganisms occur. The environs of plant roots, that is the rhizospheres and root surfaces of plants, seem to be possible examples of such sites.

The Rhizosphere

Rhizospheres contain numerically larger active populations of bacteria, actinomycetes, and fungi which differ from those of the rest of the soil both in number and in physiological type of microorganism. Measurements of various chemical activities and concentrations of substances have led to the view that cellulose decomposition, nitrification, and solubilization of phosphate, release of simple nitrogen compounds and production of CO_2 may proceed faster in the rhizosphere due to the greater activities of the microorganisms.[9,10,11,12]

The explanation of this activity in the rhizosphere has been sought in the exudation of substances by roots, some of which may be of a vitamin or accessory nutrient nature and others may provide carbonaceous substrates. Indeed the study of exudations of roots, and the release of substances from them and from senescent root hairs, root cap cells etc., has shown them to contain a multiplicity of organic substances, carbohydrates, soluble nitrogen compounds, organic acids, vitamins and so on.[13]

Although the identification of the compounds has been possible, good estimates of their quantity have been difficult to obtain. Harmsen and Jager[14] give a table of values which are a little help. For instance vetch seedlings released the equivalent of 1.6 to 2.9 mg of carbon per 100 gms dry weight of root in six weeks when grown on soil. Gray and Williams[6] point out that this corresponds to the equivalent of 3.6% of the root mass in six weeks and must be a minimum because it is only that which has not been used by the rhizosphere microorganisms. In more recent work Dr D A Barber informs me that he has been able approximately to estimate the rate of exudation from young cereal roots in sterile conditions. In their first three weeks of growth, the rate of exudation may approach a quantity equal to 10% of the weight of the roots produced during the same period. He believes that Dr A Rovira in Adelaide has obtained rather similar rates. A very graphic impression of the amount of material released from the root cap is obtained from the work of Clowes.[15] He described the root cap of maize, which consists of about 10,000 cells, as being completely replaced by new cells every twenty-four hours. In addition the external cells of the cap secrete mucilage in the last stages of maturation which forms considerable interstitial layers between the cells[16] and constitutes an additional release of carbon to the substrate. The organic content of such exudates and sloughed cells serve to bridge the gap between measured CO_2 production from the soil and the estimated supply of carbon in the litter. But there is an additional more important bridge namely those mycorrhizal symbioses where the carbon currently fixed in photosynthesis by one of the partners supplies the needs of both.

Mycorrhizal Infection

The two commonest mycorrhizal infections are ectomycorrhiza (ectotrophic mycorrhiza) and vesicular-arbuscular mycorrhiza. In both kinds of infection fungal hyphae inhabit the root system and are connected to the mycelium in the soil and in due season form fruit bodies within or upon the soil. The mycorrhizal fungi are known to intervene in the nutrition of their hosts and to derive carbon compounds from them. The extent of these interactions and their influence on carbon balance in the ecosystem as well as their effects on the growth of the photosynthetic partner are both considerable.

There has always been some doubt about the frequency and extent of mycorrhizal infections in natural conditions. They are indeed much more common than is often believed. For instance Dominik and Boullard[17] to take one example only, showed that 58% of all angiosperms in a single Fagetum were mycorrhizal and this agrees with the observation that most of the angiosperm families, all the gymnosperms and many of the pteridophyte group contain mycorrhizal species. To be more specific large numbers, perhaps most of the species of great forest trees of the world are mycorrhizal. Some, notably Pinaceae, Fagales, Eucalypts and some Dipterocarpaceae are ectotrophs. The remaining conifers and angiospermous trees have with few exceptions (amongst those examined) vesicular-arbuscular mycorrhiza. In addition vesicular-arbuscular mycorrhiza occurs in countless herbs and shrubs and it seems more usual for angiosperms and gymnosperms to be mycorrhizal than uninfected.

In the case of ectomycorrhiza there is strong experimental evidence for the direct supply to the mycorrhizal fungi of carbon from photosynthesis rather than from the soil. This has been summarized by Harley[11] and by Harley and Lewis[18]. Melin and his colleagues in an important series of papers showed that the fungi depended on simple carbohydrates and were not capable of using the lignin and cellulose of leaf litter and humus. Melin and Nilsson[19] demonstrated that during experiments of a few hours duration $^{14}CO_2$ provided to pine seedlings was photosynthetically fixed and ^{14}C labelled products translocated to the root system and out into the mycelium of the mycorrhizal fungus. Shiroya *et. al.*, Nelson[20] and Lister *et. al.*[21] and Reid[22] have performed similar experiments in some of which more ^{14}C labelled photosynthetic products were translocated per unit time to mycorrhizal root systems of *Pinus* than to non mycorrhizal ones. By a different method Lewis and Harley[23] showed that ^{14}C fed as sucrose to excised mycorrhizal roots of Fagus was translocated to the tip of mycorrhizas. After twenty-four hours some 60-75 percent of the sugar reaching the mycorrhizal apices had been incorporated into the fungal layer. Although the species of *Endogone* of endomycorrhizas have been shown to be selective in their absorptive physiology and not to be capable of growing in culture alone even when provided with soluble nutrients, simple carbohydrates and accessory factors, direct evidence for their being supplied from host photosynthesis has not yet been published. However in at least one laboratory experiment is in progress and seem to indicate a direct flow of photosynthate to the fungus.

It would seem therefore that respiration of mycorrhizal fungi must play an important part in the release of carbon dioxide from soil especially in natural habitats using current photosynthesis as a source of carbon.

Since the fungi involved in all these symbiotic unions are believed to be directly supplied with carbon from photosynthesis it is essential to make even a rough estimate of the quantities involved. Estimates based on ectotrophic mycorrhiza can be made in a number of ways and two examples are given in Table 1. Of the two separate estimates given, the first of these is based on a record of Romell[24] of the total dry weight of fruit bodies of the mycorrhizal fungus *Boletus bovinus* found in a spruce forest by I. Larsen. The second is based on the experimentally determined weight of fungal sheath and its respiratory CO_2 production. These two kinds of estimates are additive and must be taken together to give the order of magnitude of the drain on photosynthesis if the fungus were supplied with carbon by its host. There is in addition the mycelium, its maintenance growth and respiratory evolution of CO_2. A minimum conservative estimate might be of 500 Kg carbohydrate per hectare per year expended by the trees above of a beech forest, upon the mycorrhizal fungi and finding its way eventually to CO_2.

Tranquillini[25] made estimates of the carbon balance sheet in *Pinus cembra* woods in the Alps. Gasometric measurements showed that as much as 40% of the photosynthetically fixed carbon dioxide was unaccounted for and was believed to be lost as root exudates or used by mycorrhizal fungi (Table 2).

In addition, since so many of shrubs and herbs, as Boullard and Dominik[17] showed, also have vesicular-arbuscular mycorrhiza the total carbon dioxide evolution by all the mycorrhizal fungi in a woodland must be very great. Rough as the estimates must be with the present data, it is clear that it is likely to go some way to explain the discrepancy between carbon disappearing from leaf litter and humus and CO_2 release from the soil and merits further investigation.

Saprophytic Angiosperms

If the emphasis that has been put by Gray and Williams[6] on the relative lack of carbon to support the microorganisms of the soil and which has been elaborated above is credible, it would be expected that saprophytic angiosperms would be rare. In spite of this so called saprophytes are not uncommon. All Orchidaceae are non-photosynthetic during their early development and many lack photosynthetic equipment throughout life. In addition Gentianaceae, Monotropaceae, and Burmanniaceae and Triuridaceae and other families contain examples of non-photosynthetic angiosperms. The bulky prothalli of *Lycopodium*, *Ophioglossum*, and Psilotaceae and the colourless bryophyte *Cryptothallus* are other examples of the same habit.

Nor are such so called saprophytes always small in size. Indeed some may be, like *Galeola hydra* described by Burgeff,[26] very large plants. All of them however are mycorrhizic and it has been assumed on much indirect experimental evidence that they, in contrast to ectomycorrhiza and vesicular-arbuscular derived their supplies of carbon via their mycorrhizal fungi from their substrates. The ability of *Dactylorchis purpurea* to absorb carbon in this way has been directly demonstrated by S. E. Smith.[27] This however does not solve the problem of the actual source of carbon under natural conditions. It is becoming progressively clearer that for such plants there are two possible sources commonly exploited.

The first was adumbrated by Kusano as long ago as 1911.[28] He showed that the orchid *Gastrodia elata* was associated mycorrhizally with the destructive fungal parasite *Armillaria mellea* through which it obtained nutrients including carbon from coniferous trees and other plants which *Armillaria* attacked and and parasitized. Burgeff[26] emphasized that the fungal symbionts of many tropical saprophytic orchids had the power of lignin and cellulose destruction and since then several destructive plant parasites have been implicated in orchid mycorrhiza. Hence they may well exploit resistant carbon sources of dead or living plants before incorporation in the soil. Ruinen[29] further showed that orchids epiphytic on living trees were connected with their hosts by fungal hyphae and their activities diminished the vigour of the host. In these examples therefore the organisms concerned were not dependent on the carbon compounds of the humus layer of the soil but upon carbon of living or dead hosts obtained by them by parasitic on destructive fungi.

The second mode of nutrition of saprophytes was demonstrated by Björkman[30] and later work has suggested the probability that it may be quite widespread.[31,32] Björkman showed that *Monotropa hypopithys*, a saprophyte shared a mycorrhizal fungus with *Picea* or *Pinus* under which it grew. ^{14}C-labelled glucose injected into the tree was translocated via the fungus to *Monotropa* plants 1-2 metres away. The experimental results indicated that *Monotropa* is essentially parasitic on the tree for carbon via the hyphae of the common mycorrhizal fungus.

Since Björkman's observations, a series of papers by Campbell has emphasized the complexity of these problems and the existence of the two forms of nutrition in saprophytes. In the genus *Monotropa* in America Campbell[33] found *Monotropa uniflora* to be associated with the parasite *Armillaria mellea* yet

Monotropa hypopithys was associated with a mycorrhizal fungus of neighbouring trees. However the latter association was not so harmonious as was implied by Björkman's work in Sweden because the cortical cells of the roots of the trees were short lived. Campbell's other work further illustrates the variability in metabolic behaviour both between and within genera of 'saprophytes'. In the genus *Gastrodia* in New Zealand,[34,35,36] *G. Cunninghamii* and *G. sesamoides* have mycorrhizal fungi which are parasitic on the hosts *Nothofagus* and *Acacia* respectively, but in *G. minor* the fungus is associated mycorrhizally, as in *Monotropa hypopithys*, with the host *Leptospermum*. The situation is similar in some ways in species of *Corallorhiza*[33] in Michigan where the mycorrhizal fungi were either weak parasites of tree roots or described as mycorrhizal with them. In *Yoania*[37] the fungus *Lycoperdon perlatum* once again seems to be both mycorrhizic with the host, *Beilschmiedia tarari* and also with the rhizome of the orchid, as in *Monotropa hypopithys*.

In any event these nutritive relationships serve to emphasize the dependence of many 'saprophytes' on living hosts via fungi mycorrhizal or parasitic on them, not dependent on humus or litter. These facts then fall into place when viewd alongside the observations of Gray and Williams.[6]

The Movement of Carbohydrates between Symbiotic Partners

In the saprophytes which we have been considering there is in most cases a visually obvious means by which substances could be transferred from the mycorrhizal fungus to its host. This is the destruction or digestion of the hyphae within the cells. This is an analogous process to that in which a necrotrophic fungal parasite destroys the cells of its host and absorbs their contents. The process of digestion in orchids and other saprophytes differs in the fact that the host cells may again be colonized by hyphae and destroy them a second or a third time. It has been described as parasitism of the fungus by the higher plant.[38] It is unlikely that this process is the only mode of interchange of material in such cases. Unlikely because in other examples of mycorrhiza, especially ectomycorrhiza, no digestion regularly takes place yet organic material moves from host into the fungus and inorganic materials from fungus into the host. In addition, in vesicular arbuscular mycorrhiza where digestion of the fungus occurs, carbon is believed to move from host to fungus rather than in the reverse direction.

Recent thoughts and experimental work on this problem have brought into juxtaposition a number of different kinds of symbiotic association in respect of the movement of carbon compounds between photo-synthesizing host and biotrophic partner[39]. Smith *et. al*[40], have emphasized the mechanistic similarity of the carbohydrate movement between the partners in ectomycorrhiza, obligate fungal parasites (e.g. Erysiphales and Uredinales) and Lichens. In each case a particular photosynthetic product seems to move to the heterotrophic partner and to be built into a peculiarly fungal product such as an acyclic sugar alcohol (e.g. mannitol) or trehalose or glycogen. In biotrophic associations of other kinds, parasitic angiosperms and host, invertebrates and algae (e.g. corals) similar factors and indeed sugar alcohols play a part.

Indeed mutualistically symbiotic or biotrophic association of autotrophic and heterotrophic partners are common and widespread and it is becoming clear that their physiology is mechanistically similar. The mutualistic symbioses are especially developed in conditions of nutrient deficiency-mycorrhizas in deficient soil, coelenterate and algal symbioses in tropical waters, nitrogen fixing symbioses in pioneer and nitrogen deficient situations and lichens in barren habitats. All these are expected adaptations either to the direct use by heterotrophic biotrophs of carbon compounds currently fixed in photosynthesis, as in ectomycorrhiza, vesicular-arbuscular mycorrhiza, obligate parasites, corals and other coelenterates or the direct use of the carbon of resistant plant structures bipassing the processes of gradual breakdown by association with appropriate organisms as in higher plant saprophytes, ruminants, and many arthropods.

The Effect of Rhizosphere Organisms on their Hosts.

The populations of rhizosphere organisms and the mycorrhizal associates of roots are not only nourished by their host but also occupy a position in regard to them which may affect the availability and the rate of absorption of nutrients from the soil.

The rhizosphere populations have been extensively investigated over the last seventy years and their general features are well described. By and large they contain many organisms with special nutritional requirements for substances which are available in the products of living roots but not in the soil in general. They affect growth of their hosts sometimes in a positive sometimes in a negative manner. This arises from the balance of two sorts of circumstance. Many of the kinds of reaction detailed above which occur at increased rates in the rhizosphere, may increase the availability of nitrogenous, phosphatic, and other essential plant nutrients. On the other hand since the requirement of the microorganisms for trace elements as well as macronutrients is in broad lines similar to that of the host, they may in deficient circumstances successfully compete for these. The well known work of Gerretson[41] illustrates the possibility that rhizosphere organisms can increase phosphate availability. Yet in some of his experiments this very increase of soluble phosphate seemed to diminish host growth by causing a deficit of available iron. By contrast Gerretson[42] and Timonin[43] showed that rhizosphere organisms might exacerbate manganese deficiency. Recent work using controlled experimental methods in solution culture[44,45,46,47] have served to illustrate the complexity of the effects of rhizosphere organisms and it is clear that broad generalizations of their effect on nutrition cannot be made.

In addition the rhizosphere and root surface populations have been shown to have a significant effect on disease incidence. As a result of much work since the 1920's, biological antagonism within the zone of the rhizosphere has been shown to exert some natural control over the spread of disease organisms. For instance it was early shown that the susceptibility of hosts to disease was highest in artificial sterile conditions and might be greatly diminished by the presence of normal soil populations[48]. The whole subject has been reviewed more recently in The Ecology of Soil-borne Plant Pathogens[49].

Mycorrhiza and nutrient absorption

The effects of mycorrhizal fungi upon nutrient absorption from the soil has been much more clearly characterized and defined. From the latter half of the nineteenth century for almost a hundred years there have been many experimental demonstrations that mycorrhizal infection, both ectotrophic and vesicular-arbuscular, may increase the growth of the host[11]. When this happens it is not always clearly realized that the activity of the fungal symbiont must affect some factor which is limiting the growth of the host. The majority of the host plants are quite capable of growing well uninfected provided that they are given appropriate cultural conditions. For instance food plants like maize, onion and tomato are grown for crop production in highly fertilized soils and develop well without mycorrhiza but there are good experiments to show that in nutrient deficient situations mycorrhizal plants of these very species grow much faster. The same is true for forest trees. Young trees grow perfectly well uninfected in artificially fertilized situations but in natural woodland soils infected plants develop well and uninfected plants poorly[11, 50].

The conclusion is therefore drawn that the fungi of ectomycorrhiza and vesicular-arbuscular mycorrhiza intervene in nutrient absorption. If one is seeking to explain the relative advantage of the mycorrhizal habit that has led to its persistence since Devonian times to the present day and its presence in so many plants one must seek it in the nutrition of the two partners. The fungi limited in their growth rate by carbon supplies escapes from the stagnation so common to other soil organisms by extracting photo-synthetic products from their hosts. The hosts limited by inorganic nutrient supply for which they compete with soil organisms and with rhizosphere organisms in particular, obtain them from the mycorrhizal fungi.

To explain how the mycorrhizal infection improves nutrient supply to the host, the mechanism of absorption has been investigated using the same kinds of experimental method that have been used with roots[11]. By and large the same factors affect the process of absorption in much the same ways as they affect the absorption by other plant organs. Absorption of nutrients by mycorrhiza is dependent on the maintenance of normal metabolic processes within them. The rate is associated with respiratory turnover so that low oxygen supply, low carbohydrate concentration, low temperatures, metabolic inhibitors and the like diminish it. Rates of uptake are concentration dependent as are those of roots. Two points of difference from non-mycorrhizal roots have been demonstrated. The first is that the primary destination of absorbed material is into the fungus, in which considerable accumulation of absorbed inorganic nutrients may take place. The second is that the rates of uptake of nutrients by mycorrhizas is very often much greater than by uninfected roots.

These findings raise further problems which have been investigated to some degree.[18] The mechanism by which ions, accumulated in the mycorrhizal fungus, are released and shared with the host tissues has only been investigated in the case of phosphate in ectotrophic mycorrhizas. Phosphate is, perhaps the world over, the most conspicuous major nutrient which is deficient in natural habitats. It is the one whose uptake has so far been found to be most affected by mycorrhizal infection. Baylis in New Zealand[51] has pointed out that many native forest plants of that country are so conspicuously phosphate deficient in natural forest soils that they are unable to grow unless mycorrhizal. In the case of the European *Fagus sylvatica* not only are mycorrhizas several times more active in phosphate uptake per unit area than uninfected roots but also the fungus may pass accumulated phosphate to the host by a mechanism which is metabolically dependent. Similar mechanisms have not yet been successfully sought in the case of vesicular-arbuscular mycorrhizas although accumulation in the fungus has been demonstrated, nor has evidence yet accumulated on the mechanism of absorption of other nutrients than phosphate.

Three features go far to explain the high uptake rates of mycorrhizal systems. These are (1) that the fungus may modify the root system so that the extent of the absorbing surface is relatively increased, (2) that the hyphae which emanate from the mycorrhizas into the soil are themselves an additional absorbing area in close physical contact with the soil, and (3) that the fungi themselves have a high avidity for nutrients and perhaps a potential for bringing them into solution.[38]

It is not proposed here to elaborate further on the nutritive activities of mycorrhizas which have been satisfactorily reviewed in the references already given and elsewhere. An important recent development is the demonstration that mycorrhizal fungi by virtue of their dominance in the root region and of their antibiotic activity play a part in the resistance of their hosts to disease. The possibility was emphasized by Zak,[52] and Marx and Davy.[53] Marx[54], has recently reviewed the later developments of this subject and has demonstrated experimentally the potential of ectomycorrhizas in this regard. The full importance of this process of antibiosis especially in the increase of efficiency of absorption by mycorrhizas by antagonism of competitors as well as disease organisms has not yet been demonstrated. Lewis[38] has attempted to put the problem in its context within the ecosystem.

Nitrogen Supply and Symbiosis.

Let us now turn to the question of the supply of nitrogen in the soil system. We have mentioned that inorganic nitrogen and other nitrogenous nutrients are released gradually from plant material in the later stages of decomposition when the ratio of carbon to nitrogen in the system has fallen. In the earlier stages in the decay of leaf litter, nitrogen deficit may limit the growth of micro-organisms and animals so that an exogenous supply will increase this activity and so accelerate decomposition. Moreover the contention of Gray and Williams[6] that rates of growth of soil microorganisms are restricted by carbon availability is likely to apply with equal force to heterotrophic nitrogen fixation. Aerobic nitrogen fixing organisms, such as *Azotobacter* require much soluble organic matter because their rates of respiration are very high and hence the process of nitrogen fixation may be very inefficient with respect to carbon consumption.

As is well known such heterotrophic nitrogen fixers are not the only source of nitrogen compounds. Nitrogen fixation is more directly linked with photosynthesis in photosynthetic bacteria, in blue green algae and especially in nodulated angiosperms such as Leguminosae, *Alnus*, *Casuarina*, *Coriaria*, *Hippophae*, *Dryas*, and many others. These latter symbiotic systems are analogous to the mycorrhizal systems which we have been considering. The maintenance of the nodular tissue, the maintenance of the microorganism (bacterium or actinomycete) and the act of nitrogen fixation are all more or less directly powered by photosynthesis. For instance in the experiments reported by van Schreven[55] additional light supply increased growth, nodulation, and nitrogen fixation per gram of nodular tissue, although excessive carbohydrate supply by sucrose addition and extra light diminished them.

The process of nitrogen fixation as well as the growth of the host plant are of course dependent on suitable inorganic nutrition as well as carbohydrate supply. It is therefore not unexpected perhaps that nodulated plants are often, perhaps nearly always, mycorrhizal in addition. This is certainly true of Leguminoseae where vesicular-arbuscular mycorrhiza has been reported in many of the herbaceous as well as in woody species. By contrast species of *Alnus* have ectotrophic mycorrhiza. There is no reason to doubt that the mycorrhizal infections of nodulated nitrogen fixing plants have a similar function to those of others. Asai[56] experimented with mycorrhizas of Leguminosae and showed reasonably convincingly that mycorrhizal infection stimulated nodulation and growth. Mejstrik and Benecke[57] described the ectotrophic mycorrhizas of *Alnus viridis* which forms nitrogen fixing nodules and showed that they had higher rates of phosphate uptake than uninfected roots.

We see therefore that the nodulated mycorrhizal plants may be adapted extremely well to habitats of low availability both of nitrogen and of other nutrients especially phosphate. An instructive example is found in glacial outwash gravels where symbiotic nitrogen fixers are of course conspicuous and species of Leguminosae and *Alnus* often play a pioneer role[12].

Conclusion.

In any ecosystem there is a gross cycling of carbon, from CO_2 fixed by autotrophic organisms through consumer and decomposer cycles which result in the release once again of carbon dioxide. In addition to this, there are tighter cycles of carbon utilization which involve, in the extreme cases, direct use of photosynthetic products by biotrophic heterotrophs. It is the burden of this essay to emphasize the potential importance in quantitative as well as in qualitative terms these tighter cycles. It has been shown, taking one example, that in terrestrial especially in forest ecosystems the tighter carbon cycle through mycorrhizal infections may afford an explanation of some of the discrepancies found in the study of their carbon turnover.

Many aspects of the subject have pointed to the need for knowledge of the mechanisms of interchange of material between symbiotic partners. This is not by any means an academic exercise for a number of reasons. Already there are clear indications that similar kinds of mechanism operate between partners of various mutualistic symbioses. Ectomycorrhizas, biotrophic fungal parasites of green plants, coelenterates symbiotic with algae, and lichens have been shown in the papers of Smith *et, al.*[40], Harley[39], and in the works quoted by them to have carbon metabolisms which show many points of similarity. Comparative work will therefore have an important bearing on many ecologically and economically important subjects.

In the mutualistic symbioses the absorption of nutrients other than carbon is commonly the function of the heterotrophic partner. Hence the symbiosis is an adaptation to the absorption of nutrients by processes in the heterotroph powered by carbon compounds derived from the photosynthesis of the autotrophic partner. Diverse types of mutualistic symbiosis are therefore particularly adapted to nutrient deficient situations as are for instance the corals to trophical waters, mycorrhizal plants to deficient soils, and lichens to barren habitats.[50,11,12] It is common for angiosperms symbiotic with nitrogen fixing organisms, also to be mycorrhizic. In such cases the adaptation to habitats both deficient in nitrogen and other nutrients is quite clear and evident from ecological observations and involves tripartite symbiosis.

Finally it may be stressed that since mycorrhizal fungi are in general not closely specific it is possible that plants of different as well as the same species may be joined in a consortium by the mycelium of a common mycorrhizal species. This has been seen to occur in the case of the so-called angiosperm saprophytes which are parasitic in an indirect manner through a fungus which may be mycorrhizic or parasitic on a green host. The point was made by Lewis[50] that the cross nourishing of individuals by the same or different species having ectomycorrhiza or vesicular-arbuscular mycorrhiza through their common mycelium may also be possible and he quotes Reid and Woods[58] who demonstrated a carbon movement between individuals of *Pinus taeda* that were linked by a common mycelium.

All these considerations emphasize the possibility and importance of placing the results of experimental study of mutualistic symbioses in their ecological context.

PARTIAL ESTIMATES OF CARBOHYDRATE DRAIN ON HOST BY ECTOTROPHIC MYCORRHIZA

Based on sporophore formation. after Romell[24]

Spruce Forest

Dry weight *Boletus bovinus*	—	180 Kg/hectare/year
Carbohydrate required for formation	—	400 Kg/hectare/year
Equivalent volume of timber	—	1 m^3
Equivalent in medium quality spruce stand	—	10% of potential timber production

Beech Forest

Dry weight Fungal sheath of mycorrhiza	—	40% of dry wt. of feeding roots
CO_2 emission of this fungus	—	50% of CO_2 production of feeding roots
Feeding roots are	≏	10% of root mass
∴ Fungal Sheath	≏	4% of root mass
CO_2 from Fungal Sheath	≏	20-25% of total root CO_2
CO_2 emission per year (8-80 years) by Beech Root Systems	≏	500-900 Kg CO_2/hectare/year
CO_2 emission per year by Fungus	≏	100-250 Kg/hectare/year

(Based on Harley[11] and Möller *et. al.*[59])

PHOTOSYNTHESIS AND RESPIRATION IN PINUS CEMBRA

After Tranquillini[25]

All figures $MgCO_2$ per g dry weight of leaves.

Gross photosynthesis		7830 mg
Respiration in growing season :		
leaves	Day	1551 mg
	Night	556 mg
roots		447 mg
when snow covered		439 mg
Total loss per gram of leaf		2993 mg
Net synthesis		4837 mg
Equivalent dry weight of net synthesis		2.2 g/g leaves
Actual dry weight increment		0.65 g/g leaves
dry weight equivalent missing		1.65 g/g leaves

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Loss of Gear Teeth

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Gears are a very widely-used element of machinery and many millions of them continue in service without giving any trouble. On the few occasions when difficulty arises, the cause can usually be found quite quickly. Now and again, however, something happens that at first seems to defy explanation and may indeed continue to do so, but it is usually possible to identify something that might be a contributory cause. An extraordinary one is described below.

The installation concerned was one in which a large synchronous induction motor drives two centrifugal blowers through speed-increasing gears of the single-helical type. One wheel about 54 inches in diameter ran at 1,500 rpm and drove two pinions, one at 6,650 rpm and the other pinion at 10,250 rpm. It was in the last-named gear that the trouble seems to have originated. The main dimensions were

		Gear Wheel	Larger pinion	Smaller pinion
Diameter (in)	d	54	12.2	7.9
Speed (rpm)	n	1500	6650	10250
Number of teeth	t	321	76	47
tdn		26×10^6	6.2×10^6	3.8×10^6
Centrifugal acceleration at the teeth		1700 g	7500 g	11500 g

The speed of the teeth relative to the common plane of the axes of the gears was about 21,000 feet per minute and this is very much higher than the average for gears of all types but not exceptional in what may be called 'high speed gears' such as are widely employed in driving centrifugal compressors or being driven by steam or gas turbines.

The gears ran continuously for 24 hours every day and (ideally) 365 days per year. Because of the nature of the driving motor, speed departed from constancy only between the close limits maintained by the frequency of the electrical supply.

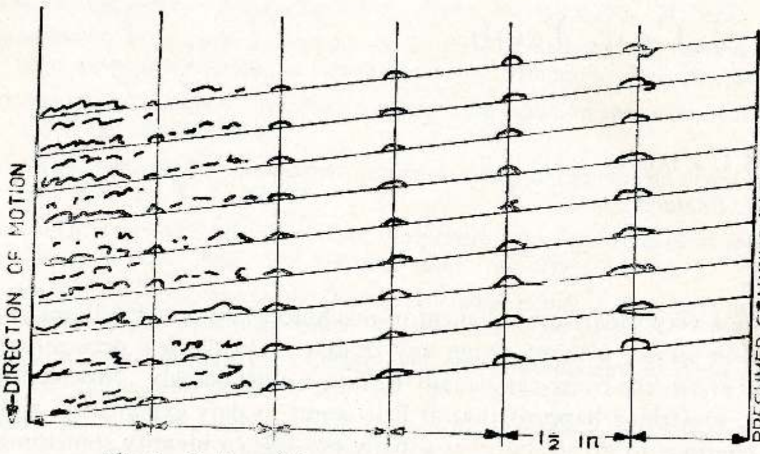

Fig. 1 — Pattern of damage to the teeth of the large wheel.

Any gears of this size and speed make noise while they are running and the men in charge of the plant become accustomed to the noise and are sensitive to any quick change in it. This is specially the case where the running speed does not vary and where there are only small changes in the load. The gears concerned in this episode were noticed to have become distinctly noisier and over some 10 hours the noise increased progressively after a succession of periods of a few minutes during which loud noise was accompanied by noticeable vibration. When the vibration during one such period had developed into a hammering, it was decided that the plant should be shut down.

So the load was taken off the compressors and the motor disconnected from the electrical supply. The speed of rotation diminished gradually to zero in about 6 minutes which was normal for the installation, but a decidedly abnormal feature was that the small pinion ceased to rotate while the other gears were still running slowly. When the gear-box had been opened it was found that all the teeth had been stripped from the small pinion and that it could be rotated by hand because it was not touching the teeth of the wheel at all. It had to be concluded that the pinion had effective teeth immediately before shut-down, but lost them during run-down.

Every tooth of the wheel was found to be damaged over a width of about half an inch in each of five annuli with an axial spacing of about 1½ inches. The damage was mostly to the crests of the teeth and did not extend very far into the depth of the teeth ; it was remarkably consistent on all the teeth in any one annulus of damage. Such consistency is not uncommon in certain types of gear-failure as if there is no common factor in the numbers of teeth of the mating gears, any piece of broken tooth that becomes jammed between adjacent teeth of either gear acts equally to damage all the teeth of the mating gear.

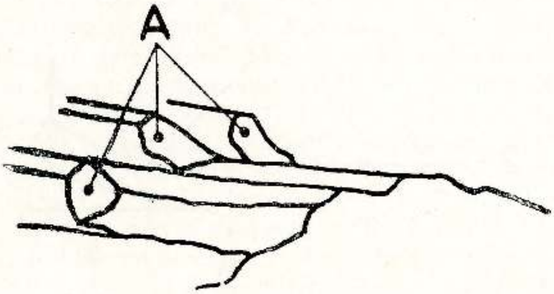

Fig. 2—Nature of fractures of pinion at intermediate stage.

The two annuli adjacent to one side of the wheel showed considerably more damage than the others, and moreover there was some damage to parts of the wheel teeth between them. Damaged areas were, in general, smooth as if from prolonged running in repeated contact with some malformation of the mating teeth. There were, however, some areas in which the teeth appeared to have flaked off in a brittle manner.

All that the body of the small pinion retained of its original 47 teeth were an equal number of stumps so short that they cleared the tips of the wheel teeth. The fractured surfaces were in general concave, were matt in appearance rather than smooth, and showed divisions corresponding to the annuli of damage on the teeth of the wheel.

From the bottom of the gear box some 250 pieces of broken pinion teeth were retrieved. They were not accurately counted as in view of their number it was perhaps thought that it did not matter if a few were lost or kept as curios. It seems reasonable to assume, however, that the original number was $47 \times 6 = 282$, corresponding to the distinguishable divisions of fracture-areas on the body of the pinion.

The root-surfaces of most of the detached pieces of pinion-teeth were convex and had the smooth appearance characteristic of a creeping fracture whereas most of the corresponding surfaces on the body of the pinion were matt. About 30 of the pieces of pinion teeth, however, had fractures of concave form and of granulated appearance consistent with fracture after a few applications of load that produced high bending stress.

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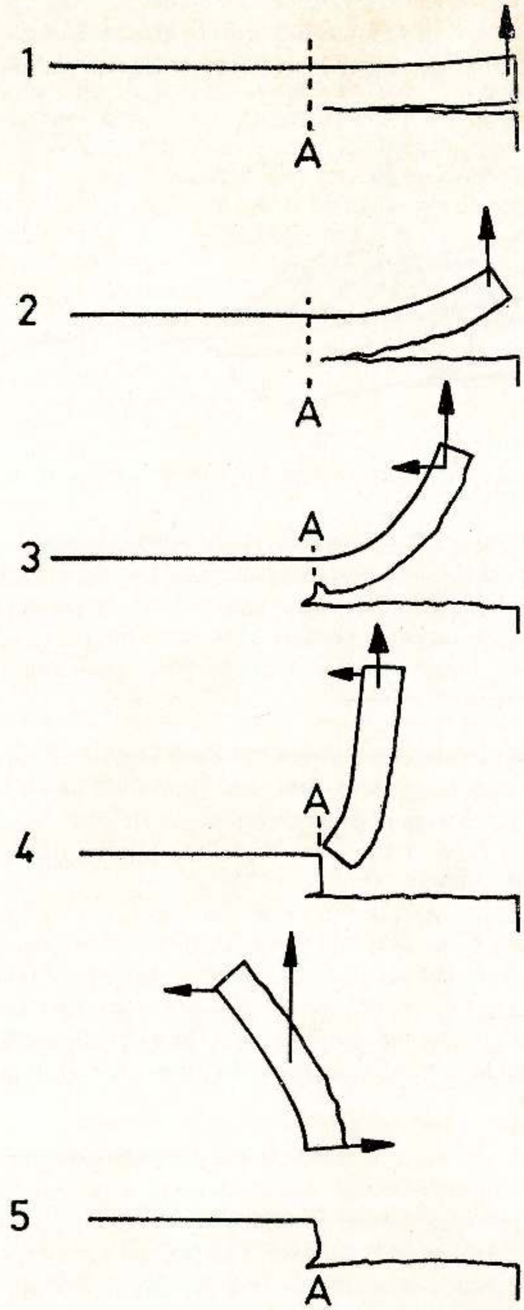

Fig. 3—Probable progress of fracture of a tooth.

Explanation of the origin of this failure is to some extent speculative, but it is reasonable to believe that each annulus of damage corresponded to one of the short periods of specially noisy running. It is quite difficult to see why all the teeth should have failed while the tooth load corresponding to the torques applied to the gears produced a tensile stress not exceeding about 2 tons per square inch. Glossing over this, and accepting that, somehow or other, a crack was produced in the material at the root fillet at the end of a tooth, it can be recognised that such a crack would tend to extend along the root fillet and also across the thickness of the tooth to the opposite root fillet. Further extension of the crack means that a part of the tooth is held to the main body of the gear only by the other part of the same tooth and that centrifugal force on the semi-detached part of the tooth is exerting a bending moment on the cross-section of the tooth at the end of the crack. The further the crack extends, the greater the bending moment becomes ; it is in fact proportional to the square of the length of the crack. This centrifugal effect is considerable as the centrifugal acceleration of the teeth of the small pinion is 11,500 times the gravitational acceleration.

Pursuing this idea numerically, it was easily found that the bending moment exerts a stress equal to the ultimate tensile strength of the material when the crack has extended to a length of about $1\frac{1}{2}$ inches and in fact the broken pieces of pinion teeth had an average length of this amount.

Detachment of a part of a tooth in this way leaves a bent stump at the fractured end of the part of the tooth that remains attached to the body of the gear, and until this stump has been hammered down by repeated contact with the teeth of the wheel, the running is likely to be noticeably noisy. The gears still continue to run, but with a short crack extending across the width of the damaged tooth near its fractured end, and this crack gradually extends along the root of the tooth exactly as before, to produce another fracture about $1\frac{1}{2}$ inches from its predecessor. In this way, the progressive breakage of a tooth over its whole length is readily explained.

The evidence was however, that *all* the teeth broke in substantially the same way and the implication is that they all had similar initial root-cracks although the bending stress corresponding to the applied load was far too small to start a crack in the material of the pinion.

The implication is that, right from the start, the bending stress at the root fillets was very much higher than that corresponding to the applied torque. In any circumstance of this kind in fast running machinery, the possibility of load-magnification by resonant vibration naturally occurs to any investigator. A disconcerting feature of this occurrence is that any such resonance seemed to persist even after progressive damage to the pinion had markedly altered its stiffness so that natural frequencies associated with it had also been altered, whereas the frequencies of possible origins of vibration-excitation had remained unchanged. Reflection on this encourages one to seek a type of vibration of such a nature that its associated natural frequency is not affected by shortening of the pinion teeth. Perhaps it is a mode of vibration whose natural frequency is determined only by the diameter of the pinion ?

Confining attention, for initial simplicity, to a single transverse section of the pinion it is easy to realise that application of load to any tooth in a very short time produces at its root a momentary shear stress in the circumferential direction. A result is that a wave of shear stress travels diametrically across the circular transverse section of the root cylinder, is reflected from the vicinity of the root of the tooth diametrically opposite to the first-considered tooth and returns to it.

In steel a wave of direct stress travels at about a million feet per minute ; a wave of shear stress travels at that speed multiplied by $\sqrt{}$ (Modulus of elasticity for shear stress/Modulus of elasticity for direct stress) and this is equal to about 0.63.

If the reflected wave reaches the vicinity of its origin at the instant at which load is applied to a tooth adjacent to the first one, it is likely to add to the effect of the load in producing stress. Repetition of this amplification at the instants of load application to succesive teeth produces the cumulative effect of resonant vibration restrained only by the small damping by hysteresis in the steel. In these circumstances, a magnification of the order of 200 is possible and this is far more than sufficient to start a crack at every tooth.

A little arithmetic applied to quantify this concept leads to the conclusion that resonance of this kind is possible if *tdn*, the product of pinion speed, pinion diameter and number of teeth in the pinion, is about 3.8×10^6. The figures for the pinion that failed have almost exactly this product.

If this type of resonance was in fact the origin of the dangerously high stress in this pinion one may conclude that sheer bad luck, in conjunction with the constancy of speed of a synchronous induction motor, was the origin of this very spectacular failure.

In a gear in the form of a thin ring, transmission of a wave of shear stress across a diameter is clearly impossible but transmission of a wave of direct stress round the ring can occur. It is extremely interesting to note that the ratio of the speeds of propagation of the two types of wave is almost exactly $2/\pi$ the ratio of the lengths of the paths to be followed by the waves in returning to the starting point, and so the critical value of *tdn* is the same for both types of stress. Hence it is reasonable to conclude that whether a steel gear is a solid cylinder or a thin ring or is a solid of revolution of any intermediate form, it is desirable to avoid running it at the speed that gives *tdn* any value between about 3.6×10^6 and 4×10^6, lest there should be a recurrence of the type of resonant vibration that may have been a contributory cause of the extraordinary gear failure described here.

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Protein - Calorie Malnutrition – The Need for a Proper Perspective

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Protein calorie malnutrition is one of the biggest nutritional problems facing the developing countries of the world. The diseases known as kwashiorkor and marasmus represent the extreme forms of protein calorie malnutrition. Protein calorie malnutrition is not a disease of acute or explosive onset. In a child subject to the stress of undernutrition, there is a continuous and insidious transition from the stage of normality to the stage of kwashiorkor or marasmus. In poor communities we can see cases in all stages of this transition.

Our assessment of the magnitude of the problem of protein calorie malnutrition will vary depending upon the diagnostic criteria we adopt. Thus, in India, on the basis of the incidence of growth failure, we may assess the prevalence as 80% among pre-school children of poor communities, while on the basis of occurrence of kwashiorkor, the prevalence at any point of time, will be as low as 1.2%. Kwashiorkor and marasmus represent the extreme and result in a chain of events extending over several months. It will be poor strategy both from the public health and economic points of view to concentrate a major part of our meagre resources on the treatment and rehabilitation of this group. And yet, when we survey the vast plethora of literature on the subject of protein calorie malnutrition, we will be struck by the fact that till recently, we were mostly preoccupied with the study of the kwashiorkor state. Our approach to the problem of protein calorie malnutrition has been largely clinical rather than epidemiological. Some of the current distortions in our approach to the problem of protein calorie malnutrition have stemmed from this. Fortunately, in recent years there have been a number of epidemiological studies which have helped us to see the problem in proper perspective.

The impression has been propagated that the major factor underlying widespread protein calorie malnutrition in early childhood is primarily protein deficiency. This concept had unfortunately greatly influenced the general thinking and planning with regard to this problem, with the result that there has been overwhelming emphasis on " protein foods ", " protein concentrates " and the so-called " protein gap ". Probably, a major reason for this was that precise data on the actual dietaries of pre-school children in poor communities were scanty. Fortunately, to-day, thanks to the extensive studies undertaken by the Indian Council of Medical Research in several centres in India using standardised methods, the problem is being seen in proper perspective.

The Indian Council of Medical Research organised a country-wide survey of the dietaries of poor pre-school children. This survey was carried out in six different centres of the country–Hyderabad, Vellore, Poona, Bombay, Delhi and Calcutta, by carefully trained personnel using standardised methodology. As a result of this exercise, to-day we have reliable information regarding the dietaries of poor pre-school children in India. The survey showed that the daily protein intake of these children ranged from 2.8 g/kg body weight to 1.7 g/kg-levels which on the basis of national and international recommendations, could be considered adequate. On the other hand, the daily calorie intake was very low being of the order of 70 to 75 Kcals/kg as against the figure of 100 Kcals/kg which is generally considered adequate for children of this age group The protein quality of the diet was found to be satisfactory.

It may be argued that the above figures of average intake may be misleading in view of the variations in the intakes of individuals. In order to overcome this objection, the relative deficiencies of proteins and calories in the dietaries of pre-school children were studied through the device of cumulative frequency distribution of children according to the intake. According to this analysis, on the basis of accepted desirable levels of intake, 92% of the children were found to be deficient in calories ; of these 35% were deficient in proteins as well. Even with regard to these latter 35% of children, if the food intake had been raised to meet their caloric requirements, the protein needs would have been automatically met and there would have been no protein deficiency. There was no situation where the child was adequate with regard to calories but deficient with regard to proteins alone.

The position that emerged clearly from this analysis was that in the current dietaries of our poor pre-school children, the major bottleneck is calories and not proteins. It seems from this analysis that if the children were to get greater quantities of their traditional cereal-pulse based diets in amounts that would meet their calorie requirement, their protein needs would be met. It is essential to emphasise this point, because in the last few years, there has been considerable emphasis on the so-called " protein gap " and several protein concentrates have been recommended to combat the situation. The provision of protein concentrates in the face of calorie deficiency is a wasteful approach. Where there is calorie deficiency, a high proportion of protein tends to be utilised for energy purposes.

Similar surveys of dietaries of pre-school children in Guatemala, the Carribean, Thailand and other countries have also clearly brought out that the real bottleneck is calories and not proteins. Unlike African dietaries, which are predominatly based on starchy foods like tapioca and plantain, and which are extremely poor sources of protein, Asian dietaries are based on cereals and pulses which are fair sources of protein. If poor Asian children can be fed the usual cereal-pulse based diets in amounts sufficient to bridge their calorie gap, the problem of protein calorie malnutrition will largely disappear. What we are, therefore, really dealing with is a " food gap " rather than a " protein gap ". In practical terms these observations imply the need for a radical reorientation in our thinking and approach to this problem and the removal of the undue emphasis on protein.

It must, however, be emphasized that the existing diets of our pre-school children in Asia are deficient in several minerals and vitamins, such as riboflavin, vitamin C, iron and calcium and extremely low in vitamin A. Even by increasing the level of consumption of current diets to levels necessary to satisfy caloric needs, there would still exist deficiency of vitamin A, iron, riboflavin, vitamin C and calcium. Therefore, the real need with regard to the dietaries of our pre-school children is to bridge the calorie gap and bring about qualitative improvement with regard to several minerals and vitamins. The latter can be achieved to a considerable extent through the inclusion of green leafy vegetables in the dietaries. At present, the dietaries of our pre-school children do not include significant amounts of green leafy vegetables.

In malnourished communities, there is a high incidence of infections and worm infestations. While malnutrition increases the susceptibility to infections, infection tends to aggravate malnutrition, and our poor communities are caught in this vicious cycle. It has been suggested that the protein intake of children in poor communities should be raised to provide blanket coverage for possible increased requirements caused by infections. A careful examination will show that this suggestion is impracticable, uneconomical and unscientific. Infections cause increased requirements of not only proteins but of calories and a number of other nutrients. The logical answer is to control infections and infestations through improvement of environmental sanitation of the community. Obviously, where the pot is leaky, the answer is to plug the leak and not pour more water into the pot to provide for the leakage. Where food is scarce, our strategy must be to control factoirs which prevent the utilization of food.

Moreover, it is difficult to quantify the effect of infection on protein requirement and to decide on the quantum of blanket coverage. The body tends to lose protein at the height of infection irrespective of the level of protein in the diet. On the other hand, there is an attempt to conserve protein in the post-infection phase. At any given time, in any poor community, there may be some children in the stage of active infection, some in the post-infective phase and some in the incubation stage. Under the circumstances, provision of increased protein as a blanket coverage will be wasteful in a large proportion of children. In any case, the provision of more proteins alone will not eliminate the several other effects of infection. Even after providing more proteins in order to ensure the health of the community, infections have in any case got to be eradicated. Thus, the argument for increased protein levels on the ground that our poor communities suffer from infections will not bear scientific scrutiny.

On the basis of carefully gathered data, it is now evident that the average calorie deficit in the dietaries of poor pre-school children in many Asian countries is of the order of 300 calories per day. For supplementary feeding programmes for pre-school children, therefore, the minimum quantity of supplement needed to meet this calorie gap would be about 75 g. At this level of intake, a protein content of 12% in the supplement would be more than sufficient to meet the protein requirement. On the other hand, the present protein concentration of preparations now widely used for feeding programmes, is about 22%. Such a high level of protein concentration is totally unnecessary. We can achieve a wider coverage at less cost using supplements with lower concentrations which will correct the calorie deficit. The conclusion that supplements providing as low as 10 to 12% of protein will be quite effective and that supplements providing as high as 20% protein are unnecessary is of tremendous practical importance. A 12% concentration of protein can easily be achieved using locally available foods, and such supplements do not call for elaborate processing ; on the other hand, recipes containing as high as 22% of protein will necessarily involve considerable processing and cannot be obtained under rural conditions.

Several nutritious recipes based on inexpensive locally available foods involving minimal processing have been developed by the National Institute of Nutrition, Central Food Technological Research Institute and other centres in India and other countries. Details of these recipes developed at these centres have been published by the National Institute of Nutrition, Hyderabad. The development and propagation of such a wide range of recipes based upon locally available foods suitable for different regions and different countries should form the basis of a massive decentralised programme of nutrition uplift of our poor rural communities. There is no major problem of malnutrition in any Asian country which cannot be solved by locally available foods. The processes used must be such as are capable of application at the home or village level. This approach may lack the glamour associated with expensive, elaborate and sophisticated processing, but in the long run may prove more rewarding and yield more tangible results in our country-side.

A challenge to our food technologists is to device procedures which will help reduce the bulk of cereal-based diets. An ounce of rice, after cooking, swells to about three ounces. In view of their bulk, cereal-based diets have to be fed to very young children several times in the day so as to ensure adequate intake. The inclusion of fats or oils in the diets will reduce the bulk and increase calorie density, but fats and oils are expensive, and our poor communities can hardly afford them. Most of the recipes based on cereals become bulky when they are cooked and ready to serve. Many feeding programmes have failed for the simple reason that young children are unable to consume this amount at one sitting.

Under these circumstances, methods have to be divised by which the bulk of the cereal-based diets can be reduced. Simple traditional methods for reducing bulk of cereals have been in vogue in Asian countries for centuries. Technologists must rediscover and improve upon these methods. " Beaten-rice ", " exploded " and " puffed " cereals and pulses are well-known in India. Beaten rice is pre-cooked and on reconstitution do not swell to the same extent as untreated grains.

In the ultimate analysis, the nutritional uplift of our poor communities can be achieved not through any state-sponsored supplementary feeding programmes or through sophisticated processed foods, but through the improvement of economic and living standards of our people and through educating village communities to effectively utilize locally available foods for better nutrition.

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Geological Survey in a Modern Society

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THE object of a geological survey is to produce maps showing the distribution and relationships of the consolidated rocks of the earth's crust exposed at the land surface or on the sea bottom, and of the drift formations which may cover them. The purpose of this review is to consider the scientific justification for such activity, the methods by which it may be carried on, and its relevance to the practical needs of a modern society. Geological surveys may be made by individuals for pure scientific objects, or by industry in connexion with the search for fuels and minerals, or with the assessment of foundation conditions for civil engineering works ; but in nearly all countries, be they developed or developing, they are also an activity of government. Special emphasis is placed here on this latter aspect.

In Britain, the oldest scientific institution wholly supported by the state is the Royal Greenwich Observatory, founded 1675, through which passes, by international agreement, the zero meridian of longitude. The second oldest is the Geological Survey of Great Britain, founded in 1835. Though now merged with the Institute of Geological Sciences, it continues its work and I write as thirteenth director in line from Sir Henry De la Beche FRS, its founder. The remarks of one of its most influential sponsors at the time of foundation, Sir Charles Lyell, are wotrh quoting :

" not only as calculated to promote geological science, which would alone be a sufficient object, but also as a work of great practical utility, bearing on agriculture, mining, road making, the formations of canals and railroads, and other branches of national industry.

This review draws, therefore, upon experience of the oldest continuously existing scientific institution of its kind in the world.

William Smith had already published his geological map of England and Wales in 1815 ; mineral maps had appeared before the end of the 18th century in France, and De la Beche himself had coloured a series of the newly-published 1 mile to 1 inch topographic sheets for the Ministry of Agriculture. Some experience was, therefore, already available. Though evidently very heterogeneous in its lithological make up, the earth's crust was known to be capable of rational interpretation in terms of widely extensive rock formations in three-dimensional geometrical relationships with one another. Cuvier in France and Smith in England had already demonstrated that layers of sedimentary rocks often contained fossils of a highly characteristic order by which they could be recognised in widely separated outcrops. A division of geological time into eras, to which corresponded broad systems of rocks, was already being constructed in an international context.

The survey was recorded on the 1 inch (1 : 63 360) maps and published, hand coloured, at the same scale. The formations represented were described lithologically but classified as to geological system, the recognition of the age depending, as far as practicable, in the case of sedimentary rocks, upon the identification of characteristic fossils. The attitude of the layered formations (dip and strike) was recorded as well as the positions of faults shifting the run of the formation lines. For igneous rocks intrusive contacts were recorded where they could be shown to exist. Unconsolidated superficial formations, particularly river and marine alluvium and terraces, sand and gravel and boulder clay were shown by either ornament or washes.

In Britain the 6 inches to 1 mile topographic maps (1 : 10 560) began to appear in 1852 and this quickly became the main surveying scale, the advantage of its use being that in most areas, all individual rock outcrops could be recorded, and boundaries established with greater accuracy. The publication scale for areas other than those of special mining interest, remained the 1 inch. For each 1 inch sheet the mass of data collected proved to be too great to include on the map, and the practice of issuing descriptive memoirs, summarising this data, one for each sheet, was introduced.

It must be recognised at this stage that geological maps differ from topographic maps in an important respect—they necessarily embody an element, often a considerable element, of interpretation. Even in the best-exposed parts of the crust, for example, along the sides of ice-cut fjords and in arid mountain ranges, the solid rocks are never one hundred per cent visible. In most parts of the world, due to the cover of unconsolidated materials, soil and products of decay of vegetation, the percentage exposure is far less ; indeed in some places, the solid formations are scarcely revealed at all. Thus the geologist must use his skill to place the interpretation of the distribution of solid formations upon a basis of highest possible confidence. He was often aided, however, even in the earlier days of survey work, by subsurface information derived from wells, borings, mine workings and, occasionally, natural caverns. However it is well to recognise the limitations of such penetrations. The deepest mine, even today, in no more than 4 Km, and the deepest borehole 9 Km deep. Most are far shallower, so that of a continental crust considered on geophysical evidence to average perhaps 30 Km thick, we have direct access to a very shallow zone only.

Nevertheless, the surface and subsurface information available enables three-dimensional models of the upper part of the earth's crust to be constructed. The practice is to represent these on sections, often accompanying the maps or memoirs.

The process here described in outline, has been going on in the UK for 135 years. By this time we have a cover of mapping at the 1 : 10 560 scale that includes more than 75 per cent of the country and 1 inch maps have been published for all Great Britain and Ireland, save for some small small parts of the Scottish Highlands and Islands. It is reasonable to ask, has this long-continued work yielded results worthy of the effort entailed ?

Considering first the scientific results of this and similar work in many other countries, it may be said briefly that they have provided the raw material and framework for the whole picture of invertebrate and vertebrate evolution, and they have revealed much of the complex history of the continental crust, including the establishment of the geosynclinal and orogenic tectonic belts that have affected it. They have revealed its response to loading with sediment and with thick ice on a continental scale. The details of volcanic activity and of igneous intrusion have emerged, at many different levels of erosion.

On the practical side, four areas of results of significance to society may be claimed. The nature, spatial positions and (to some extent) the origin of the concentrated deposits of fuels and useful minerals have emerged in perspective. The distribution and movement of groundwater without which life could not be maintained in many parts of the globe has been traced. The nature of foundation conditions for all kinds of engineering works, dams, buildings, and whole cities with all their complicated requirements is beginning to be understood in terms of Quaternary geology. Finally, geological survey has, or ought to have, provided an archive, preserving the details of expensive penetrations below the earth's surface for posterity.

The question may also be asked, why has not finality been reached before this time in the advanced countries ? The answer here is that improvements in techniques of geology and methods have been very considerable during the past century, and each advance has tended to render obsolete, in part at least, maps made without it. Of equal importance has been the fact that the accelerating pace of industrialisation and the demands of two world wars have given rise to large amounts of new subsurface information, all of which needs to be correlated and taken into account, calling for revision of existing maps, or production of new ones.

As examples of new techniques, the fast-developing field of geophysics and geochemistry must be cited. Sophisticated physical measurements have proved a remarkably effective means of obtaining subsurface information covering not only details of the crust but giving the position of its base (the Mohorovicic discontinuity) and information about the marvels beneath. Interpretation of the passage of seismic waves, natural and artificial, does all this and is a major aid to three-dimensional modelling. The variations in the magnetic field and in the force of gravity (the latter when suitably corrected for topography and for the geoid) bring out the positions of concealed sedimentary basins and intrusives. Electrical measurements, for example of resistivity, self-potential and induced potential are most valuable aids in the search respectively for water and metallic minerals.

In geochemistry, the development of physical methods of chemical analysis, notably atomic absorption, automated spectrometry, X-ray fluorescence and the electron microprobe have made it possible to deal effectively with vast numbers of samples. It is, for example, within the scope of a fairly modest laboratory to determine 1 million elements per year. Hence, technique is now available for the study of variation in major and trace elements on an areal and regional scale.

Geophysical and geochemical mapping thus now forms a normal part of geological survey.

The relevance of all this to modern society must finally be considered. Two features of modern society stand out immediately ; first the emergence of the independent developing nations, for the most part eager for industrialisation Secondly, the environmental pressures arising from the exponential increase in world population. In both areas I suggest that geological survey has a part to play that is vital to society.

The need for surveys in the developing countries, especially directed towards the development of water supplies and the discovery of useful mineral deposits is universally recognised, and receives the support of the governments of the countries concerned, of the United Nations Special Fund, as of much bilateral aid. Nevertheless, doubt may be expressed whether as much is being done as the situation demands. Progress can be far more rapid, at the reconnaissance stage, than was possible by traditional methods, by employing airborne photogeological methods, accompanied by ground traversing, and by using airborne geophysics. This does not yield detail at the degree of confidence already available in those countries mapped on scales such as the 1 : 10,000, but it is a valuable start from which target areas for more detailed attention may be selected. Perhaps there is no case for taking districts which are never likely to carry large conurbations or extensive industries up to the 1 : 10,000 scale of mapping ; but the gap between this and what generally exists at present is so large that effort beyond the present scope is certainly called for.

The environmental problem contains two highly important areas for geology. One is pollution and it is only necessary to note that a far wider application of the techniques of geochemistry is going to be called for if a proper appreciation of true background, against which chemical pollution can be assessed, is to be obtained.

The other area is that of non-renewable resources, dwindling in many cases at an excessively rapid rate. Search for new (and perhaps lower grade) areas where fuel deposits and concentrations of metals can be obtained will depend increasingly on geological surveying by all available techniques, especially after 'short cut' possibilities have been exhausted.

Perhaps for a time the sea will redress the balance of the land. Occupying 70 per cent of the earth's surface, it is the new target for geological survey. Here, geophysics, far more effective than on land, forms the spearhead of the attack ; sea-bed sampling, shallow boring, deep seismic shooting and deep boring follow. This is the sequence for the continental shelf and slope, both with large hydrocarbon potential. The deep ocean, which demands different survey methods, may yet yield up its nickel and copper. But here, the dividend so far has been in pure science, where the breakthrough represented by continental drift, sea-floor spreading and plate-tectonics concepts has revolutionised thinking about our planet. It was the result of geophysical surveying at sea.

The pursuance of geological survey is, I feel sure, as much a matter for attention in Sri Lanka as it is elsewhere, both on shore and offshore. These reflections on our experience are offered with all good wishes for the success of the new Journal.

Hannover and London,
June, 1973.

Policy

The National Science Council of Sri Lanka has decided to publish a journal, in order to disseminate the results of scientific research.

This journal, which will be titled, "The Journal of the National Science Council of Sri Lanka", will accept research papers, short communications and reviews in all fields of science and technology.

Papers presented at Symposia may also be accepted. Papers could be either reports of original work done or reviews.

Reprints of papers published by Ceylonese scientists in foreign journals will be reproduced, provided the author has obtained written permission.

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Manuscripts in triplicate should be sent to :

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Submission of a paper to the Editorial Board implies that it reports unpublished work; which is not under consideration elsewhere; and if accepted, will not be published elsewhere in the same form in any other language's without the consent of the Editorial Board.

Detailed Instructions on the preparation of Manuscripts in different subject fields are available on request from the Secretary to the Editorial Board.

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