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Science Council of Canada

September 1972
Report No. 18

Policy Objectives for Basic Research in Canada

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Information Canada
Ottawa, 1972

August 1972

The Hon. A.W. Gillespie, P.C., M.P.,
Minister of State for Science and Technology,
House of Commons,
Ottawa, Canada.

Dear Mr. Minister:
In accordance with sections eleven and thirteen of the Science Council of Canada Act, I take pleasure in forwarding to you the views and recommendations of the Council as they concern the requirements for Canadian effort in basic research, in the form of a report entitled Science Council Report No. 18, *Policy Objectives for Basic Research in Canada.*

You will note that this report is prefaced by a "Personal Assessment" by Dr. Roger Gaudry, the Chairman of the Science Council Committee on Basic Research. As indicated by its title, this preface presents the personal views of Dr. Gaudry.

This report is coming off the press after the release of Volume 2 of *A Science Policy for Canada*, the report of the Senate Special Committee on Science Policy (the "Lamontagne Report"). There is a considerable degree of overlap in the subject matter of the two documents, but you will find no discussion of the Senate Committee's recommendations in the present report. The Science Council's commentary on the Senators' Report appeared in our Annual Report for 1971-72.

Yours sincerely,

O.M. Solandt,
Formerly Chairman*,
Science Council of Canada.

*Dr. Solandt was Chairman from May 23, 1966 until May 23, 1972, and chaired the Council during the preparation of this report.

A Personal Assessment

From the beginning of the Science Council's existence, its studies have been proceeding along two parallel routes. The first route consists of in-depth studies of the applications of science to specific areas of Canadian economic activities, such as forestry, agriculture, transportation etc. The second involves analytical and synthetic studies of the much more general relationships between national needs and scientific activities. The first example of the latter studies may be found in Report No. 4, *Towards a National Science Policy for Canada.* That report presented science policy as the development of optimized relations between scientific activities and national goals. Continuing development of this philosophy necessitated an inquiry into the extent to which the Canadian effort in basic research should and could be correlated with national goals without stifling the freedom of science.

I was glad to accept the chairmanship of the Science Council Committee on Basic Research, which was set up to study the above problem. The results of the Committee's work, as influenced and approved by the Council, are presented in this report. Because of my deep involvement with basic research, in various capacities, for over thirty years, I wish to add to it some comments which express my personal convictions, without implicating the position of my colleagues on the Council.

How much basic research should be done in Canada is a frequently posed question. The present report does not contain recommendations on the appropriate level of funding, for reasons explained in the Introduction.[1] It is demonstrated in Chapter V (Figures 1 & 2), however, that the statistical arguments purporting to show that Canada is spending too much on basic research are not convincing. An approach based on an analysis of our own needs would be better; steps toward developing such an approach have been taken in this report. Meanwhile, I personally have no doubt that our effort is not too large, but that it could be much more effective with better policy and organization.

During the past year there was much public debate regarding the importance of complete freedom in basic research, versus the need for giving priority to work in areas related to Canadian problems. A discussion of this important subject will be found in the report. I would like to stress here that the protagonists in this controversy tend to create the impression that an extreme position must, or is intended to, be adopted. I feel sure that common sense alone would prevent such a development. There can be no doubt that the leaders of Canadian scientific thought will continue to be expected to lead, which of course will preserve their freedom to decide what they should do in research. Furthermore, no politician or bureaucrat will try to tell any scientist how to do his research. On the other hand, I do not believe that every person qualified as a scientist thereby has the right to use

[1]It may be noted that a recent study on "Purpose and Choice in the Support of University Research in Physics" (the "Lawrence Report", *Physics in Canada*, Vol. 27, No. 5, University of Toronto Press, Toronto, June 1971), which was undertaken by the Canadian Association of Physicists with the support of National Research Council of Canada, also refrained from offering such recommendations.

public funds for doing research on any subject he happens to choose, for any reason, without any accountability to the public.

There is a place in the system for posing challenging and important problems to the scientists, in the hope that they themselves will develop research proposals aimed at generating the needed knowledge. Fortunately, experience shows that most scientists rise to meet a challenge, rather than objecting to it. It appears therefore that the conflict between freedom and orientation in research has been much exaggerated before the public. The real problem is how to involve our scientists in a realistic selection of the areas of emphasis.

A more serious problem stems from a jurisdictional dichotomy. Since the significance of basic research transcends provincial boundaries, the federal government must accept responsibility for supporting it in accordance with national needs. The research work is performed mainly in the universities, which are under provincial jurisdiction. So far there has been no cooperation among the two levels of government and the universities toward drawing up a joint policy for the total financing of university research. The last attempt at arranging a *modus operandi* was at the Federal-Provincial Conference on Financing Post-Secondary Education, in 1966. The federal government was left free to support research in universities directly, as required by its national policies, while provincial governments have, of course, complete freedom to support any research that might be called for by provincial requirements. Unfortunately, the detailed financial arrangements worked out as a result of the 1966 conference, and embodied in the Federal-Provincial Fiscal Arrangements Act of 1967, did not define the cost of research when providing for subtraction of all direct funding of research from the total operating costs of post-secondary education.[2] To exemplify the resulting problems, one can point out that the direct federal funding now tends to be limited to covering a part of the direct cost of research, such as the cost of special equipment, some support personnel, and research scholarships. At the same time the budgets for almost all other research expenditures, including most of the indirect costs such as the cost of floor space, are provincially controlled. These budgets are based on formulae which usually do not take into account the requirements for research not included in provincial planning. As a result there is, for example, no correlation between the development of floor space for research and the demand for it. Successful research scientists who obtain large federal support for their research may be forced to occupy some space intended, according to the provincial standards, for undergraduate teaching.[3] It seems clear to me that the federal funding could cover all the total direct costs of any research it supports. This is a practical proposition, since direct costs can be defined in an accountable manner. In the long term, one should consider a gradual adjustment of the federal financing toward also covering the indirect costs of the research which it supports.

[2]One half of the remaining costs (or $15 per capita on the provincial population) are reimbursed to provincial governments by fiscal transfers and additional adjustments.

[3]The shortage of floor space for research may significantly enhance tendencies toward expanding intramural research within government establishments, where provision of the necessary space is a part of project planning.

This could be done by including the various elements of such costs in the formula for financing, on a one-by-one basis, in line with the progress of the work on defining these elements.

Members of the academic staff who receive large research grants want to devote more and more of their time to graduate and postgraduate training. This poses another problem, since the need for this activity, while being recognized by university administrators, is not uniformly reflected in budgeting for academic staff by the individual provinces. Disparities among the provinces are thus enhanced and undergraduate teaching may suffer, with many serious consequences. One possible way toward reducing this problem would be to provide for more research appointments, financed from research funding. Such academic appointments permit exceptionally successful researchers to devote virtually all their time to the pursuit of new knowledge, without a loss of teaching time to the undergraduates.

The above situation raises the fundamental question of the priorities applied to education and the acquisition of new knowledge. A true university must be involved in the creative activity of research, but research should never become the *raison d'être* for the university. The primary task of universities is teaching. I am referring here, of course, to the broad concept of teaching in the university context, which places stress on promotion of learning and creative thinking among the students themselves. Research has its rightful place there, to improve the quality of teaching, but should never usurp the position of teaching. If it does, there is a rapid deterioration of the primary teaching role. This should be safeguarded by a suitable research policy on the part of the universities, and by more stress in faculty promotions on success as a teacher as opposed to a predominant weight on research publications.

The tendency to over-emphasize research publications in faculty promotion is one of the contributing factors to another difficult problem. It is now usual for a faculty member to do some research, mostly through independent individual projects. However, not all faculty members do good research. Those who do not still consume scarce resources and divert their attention away from teaching, without producing worthwhile contributions to the advancement of knowledge. Industrial experience shows that even gifted research scientists seldom remain creative throughout their careers. Industry usually takes effective steps to redirect its researchers, in due time, to other activities in which they can remain productive. The universities must also find ways to cope with this problem. The necessary step is to re-establish teaching as the most important activity of a faculty member, and to create a situation in which a professor would not regard the lack of financial support for research as a loss of status, but would be happy to seek fulfillment and recognition entirely in the teaching and development of his students.

The federal support of basic research in Canada comes from many sources. This plurality of funding is a good feature of our system, but places great demands on effective cooperation among the funding agencies. Their cooperation has been improving over the years, but is still far from adequate. In particular, gaps exist between fields of science supported by different agencies. If a field is to be left without support, this should happen as a result of an explicit and public decision, not by default. Also, it should be made clear to all scientists where to direct proposals for support of research in any one field. If a proposal is deemed to merit support, it should not be necessary to apply to several agencies before the total funding becomes adequate.

The modern university tends to increase its integration with the social environment and to justify public support by its direct or indirect contribution to the development of that environment. This is reflected in increasing concern with research on local and regional problems, even when such problems might have a secondary priority from purely scientific, or broad national, considerations. Socio-environmental problems are mostly multidisciplinary, and necessitate a well coordinated attack by groups of scientists with diverse backgrounds. This need is not met by the prevailing policy of funding individual research scientists rather than research groups. Group funding not only is necessary, but also should include giving a great deal of freedom of action to dynamic leaders. Conversely, steps must be taken to curtail self-perpetuation of such groups when their objectives have been reached, or when they no longer fulfill their mandate because of either loss of creativity or a shift of interest.

Inadequate communication among scientific groups in government, industry and universities is one of Canada's major problems. This difficulty is one of the reasons why basic research, which is carried out mostly in the universities, is also conducted in government and industrial laboratories. However, the objective of improving communication through this distribution of effort is seldom met to a significant degree. Groups of research scientists who are working toward the solution of certain well defined problems for industry or government must frequently redirect, suspend, or slow down applied research because of a lack of the required basic knowledge. This knowledge can be obtained by creating or expanding basic research groups within government or industrial laboratories. However, the best solution may often be to request more help from the basic research scientists in the universities and work in close cooperation with them. The latter approach is seldom explored and rarely used.

Another matter which deserves to be raised is the "peer judgement" system of evaluation. This system relies on committee evaluation of proposals for research put forward by individual scientists. The committees are usually composed of scientists who have acquired enviable reputations in their field mostly through many years of achievement. Thus they tend to have very definite opinions on the value of research proposals submitted to them. They are, of course, likely to be conservative and to place considerable weight on experience. Their recommendations are generally final. I have doubts about the suitability of these committees to pass judgement on novel and unorthodox proposals. Moreover, while being responsible for judging the scientific merits of the projects submitted to them, they often tend to let political considerations interfere with their decisions, without knowing precise criteria or having sufficient information. Unfortunately, a better system of evaluation has not yet been developed. Until this challenge is met, it would perhaps be advisable to set up mechanisms for reappraising once-rejected projects, especially when these come from very young research scientists and embrace new concepts. I am opposed, however, to the opposite extreme of granting support for independent research to virtually all young scientists, with little regard to the quality of their proposals.

To sum up, we must strive to develop a system in which research would help to fulfill, but would not compete with, the educational objectives. Without forgetting the need for correcting regional disparities, we have to strive for the development of a system which would promote and demand excellence, by giving adequate support to the most creative minds.

Roger Gaudry

Acknowledgements

During the course of this study, much assistance was received from several offices and many individuals who must be thanked without listing them all. The early contributions have already been acknowledged in the Science Council's Special Study No. 21, *Basic Research*, by P. Kruus. In the subsequent work, much use was made of the evidence given before the Senate Special Committee on Science Policy; we are grateful for the helpful attitude of the Committee. Special acknowledgement is also made of help received from the Education Division at Statistics Canada, and from many officers of the National Research Council, the Medical Research Council, the Canada Council and the Association of Universities and Colleges of Canada. The Science Council's project officer on this study, Mr. Jorge Miedzinski, made a very valuable personal contribution.

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I. Introduction

In our first general consideration of science policy for Canada[1], emphasis was placed on applied research and comprehensive major programs oriented toward the solution of some important economic or social problems. However, in the introduction to that report[2] the Council stated:

"This emphasis, however, does not mean that these programs are more important than basic research, but rather highlights the Science Council's opinion that changes are more urgently needed in Canada's applied science than in its efforts in the field of basic research. The Science Council would recommend that basic research continue to be supported at an expanding rate, as it has been in the past, and to have it flourish both as 'curiosity-directed research'[3], and as 'mission-oriented basic research'[3], in fields of general interest and importance to the major programs proposed."

Since that time, the Council has given much attention to the states of both basic and applied research in various scientific disciplines or areas of special interest.[4] Many recommendations relevant to basic research, which need not be repeated here, have been made. However, the need for a general study of the role of basic research in Canada was gradually coming into prominence. Awareness of this need was heightened by indications of some tendency to develop applied research at the expense of basic research, often on the supposition that basic knowledge can more economically be imported.

This report is concerned with the fundamental rationale for doing basic research in Canada and the development of a set of explicit criteria which could be used systematically in the process of selecting either individual proposals or lines of general emphasis. Such criteria should remain relevant for a long time. In contrast, any application of these guidelines to define specific priorities and levels of support will have to be made with reference to the current situation, and will thus not have lasting relevance. The Council has chosen, therefore, to leave such application work to a separate, future study.

The general principles and the criteria for support elucidated in this report can be taken into consideration by scientists and by any agency supporting basic research. However, the magnitude and scope of federal funding has, inevitably, resulted in particular attention being focussed on the patterns of support for research by the federal government.

The groundwork for this report was provided mainly by the material collected for a study on "Basic Research and National Goals", which was concluded in June 1970. A report by the project leader for that study, Dr. P. Kruus, has been published separately.[5] The present report puts forward

[1]Science Council of Canada Report No. 4, *Towards a National Science Policy for Canada*, Queen's Printer, Ottawa, 1968.

[2]*Loc. cit.*, p. 5.

[3]These will be referred to as "free" and "oriented" basic research in the present report (cf. Chapter II of this report, "Definitions").

[4]See list of publications at the end of this report.

[5]P. Kruus, *Basic Research*, Science Council of Canada Special Study No. 21, Information Canada, Ottawa, 1971. Dr. Kruus undertook the study while on secondment from Carleton University. His report contains an extensive list of references which will not be repeated here, except for a short list of some recent Canadian publications (see Appendix C).

the position of the Science Council, reached on the bases of Dr. Kruus' work and further study by the Committee on Basic Research. The Committee began its work on September 13, 1970, and completed it on September 15, 1971. Some editorial and statistical reference work by the project staff continued until the end of March, 1972.

In addition to the presentation of our position on policy objectives for basic research in the subsequent chapters of this report, Appendix A contains a rephrasing, with occasional amplification, of the same position, in the format of questions and answers. Some readers who have specific questions in mind might find answers there more conveniently than by deducing them from the general line of reasoning in the main body of the report.

We do not mean to imply that the criteria recommended herein are not already being used, at least in part, by some agencies. In many cases the position taken by the Council will in fact support existing practices or trends. If it results in a delineation of the area of consensus and stimulation of enlightened public discussion of controversial points, the report will have served its purpose.

II. Definitions

Basic research is not a unique, distinct, activity. The term represents a broad range of varied activities which occupy one end of that spectrum of creative and interacting endeavours which encompasses basic research, applied research and experimental development. The divisions between adjacent sectors of the spectrum are diffuse. There is little merit in arguing about the classification of border-line activities, unless the results of such classification affect the probability of funding (which should not be the case). However, discussions of policy for basic research, and the accumulation of statistical data, may be meaningless if the same term means different things to different people. Well understood definitions are therefore indispensable to effective communication. A completely satisfactory set of definitions may never be developed, but at least one such set is becoming widely accepted for statistical purposes on the international scene. These are the definitions developed by the Directorate of Scientific Affairs (DAS) of the Organisation for Economic Cooperation and Development (OECD).[1] Generally known as the Frascati definitions[2], they are used by most OECD countries. The detailed wording varies slightly from country to country without affecting the essential compatibility. In Canada, the following text is used by Statistics Canada[3]:

"The definitions below are based largely on the assumed motivation for the work. The motivation to be considered is always that of the program, rather than the personal motivation of the individual scientists and engineers. The criterion of motivation may be supplemented by criteria of probable results and nature of work.

"Basic Research

"Basic Research is original investigation undertaken in order to gain new scientific knowledge with the *primary* purpose of contributing to the conceptual development of science. This is to say, the motivation is to add to the accumulated, objective and systematic knowledge of the inherent properties and interactions of matter, space, energy, natural phenomena and biosystems.

"In *'free' basic research* the original impulse comes mainly from scientific curiosity – a particular problem seems interesting. However, *'oriented' basic research* would be more typical of basic research carried out by the Federal Government.[4] In *'oriented' basic research* the investigation is directed towards the definition and solution of fundamental technical or scientific problems in a general area of interest.

"Basic research yields new hypotheses, theories and general laws. The resulting information is usually non-negotiable and is usually freely pub-

[1] OECD. *The Measurement of Scientific and Technical Activities: Proposed Standard Practice for Surveys of Research and Development (Frascati Manual)*. DAS/PD/62.47. OECD, Paris.

[2] The name comes from the location of an international conference at which these definitions were worked out, in Frascati, Italy.

[3] Statistics Canada. *Federal Government Expenditures on Scientific Activities*. Questionnaire Guide for Fiscal Years 1969-70, 1970-71, 1971-72. Statistics Canada 6602-190: 10-9-70. Information Canada, Ottawa, 1970.

[4] The statement "oriented basic research would be more typical of basic research carried out by the Federal Government" is not intended to imply that free basic research is, or should be, excluded from government laboratories. See also Appendix A, Question 7.

lished in scientific journals or circulated among interested colleagues. Sometimes it may be 'classified' for security reasons. Results often affect a broad field of science and may have several ultimate applications.

"Applied Research

"Applied Research is original investigation undertaken in order to gain new scientific knowledge with the primary purpose of applying such knowledge to the solution of practical or technical problems. It is required either to determine possible uses for the findings of basic research or to select the appropriate method of achieving some pre-determined objective. The results of applied research are intended mainly to be valid for single or limited number of products, operations, methods and systems. It develops ideas into operational forms. The knowledge or information derived from it is often patented but may also be kept secret.

"Examples[5]:

"1. The study of a given class of polymerization reactions under various conditions to the yield of products, and of their chemical and physical properties, is *basic research.* The attempt to optimize one of these reactions with respect to the production of a polymer with given physical or mechanical properties (making it of particular utility) is *applied research.*

"2. The study of the absorption of electro-magnetic radiation by a crystal in order to obtain information on its electron band structure is *basic research.* The study of the absorption of electro-magnetic radiation by this material under varying conditions (for instance temperature, impurities, concentration, etc.) in order to obtain some given properties of radiation detection (sensitivity, rapidity, etc.) is *applied research.*

"3. The determination of the amino-acid sequence of an antibody molecule would be *basic research.* The effort to distinguish between the antibodies of various diseases on the basis of these findings would be *applied research.*"

It is evident from the above quotation that the Frascati definitions recognize two classes of basic research, namely free basic research and basic research oriented toward predetermined areas of interest. We must stress, however, that these classes differ from an administrative point of view only. Oriented basic research is still basic research. That is, there is no difference in the conduct of the work, from the scientific point of view, between free and oriented basic research. The only difference lies in the nature of an agreement. A scientist who accepts funding assigned to oriented research may not use that funding if he wants to redirect his efforts outside the original area of interest to the supporting body. The term "funding" is used here in a general sense. It may represent a grant in a university situation or a position in a government or industrial laboratory. In practice, such problems are seldom likely to arise. The areas of orientation are normally broad, and the scientists working in those areas do so because their interests lie there anyway.

[5]The examples used by Statistics Canada are literally the same as in the Frascati Manual. There is little doubt that they may be regarded as unsatisfactory by many a scientist. This is indicative of the difficulties in classifying research as basic or applied.

It may be noted that the above definitions are completely compatible with the definitions used earlier by the Science Council in its Report No. 4, *Towards a National Science Policy for Canada.* They represent the best tool currently available for policy purposes, but are still open to a number of interpretations or arbitrary managerial decisions. Any statistics based on the use of these definitions have to be treated with caution. A particular problem is posed by low reliability of returns. The difficulties in separating basic research, applied research and development lead to the use of only one general category, R & D, by Statistics Canada in its 1971 special survey of major performers of R & D in industry.[6] However, one should not exaggerate the importance of this problem. Much basic knowledge is produced by applied research. Conversely, much basic research is directly applicable. A single line of investigation may start as basic research and be continued as applied work, or vice versa. Borderline projects present semantic difficulties, but do not pose a policy problem. The real policy problem lies in the appropriate division of effort among those activities which are far on the basic side and those which are far on the applied end of the research spectrum.

[6]Verbal communication from the Science Statistics Section of Statistics Canada.

III. General Principles

General Need for Basic Research

There are only two primary reasons for undertaking basic research: intellectual curiosity and expectation of practical benefits (for the researcher himself or the world at large). These two reasons are not mutually exclusive; rather, they are complementary. Curiosity provides the necessary driving force and short-term satisfaction. Practical benefits permit, among other things, further research. The practical benefits are of two kinds: those arising from the generation of new knowledge and those due to participation in the research activity itself. The educational and stimulating value of research provides virtually immediate benefits which could hardly be obtained by other means. This, of course, is one of the reasons for the extent of research activities in the academic world.

Practical benefits from the application of new knowledge are immense, but they are normally of the long-range type. Each generation benefits from the investments in basic research by earlier generations. Also, the benefits are derived indirectly, following from the intermediate stages of applied research and development.

Many attempts have been made to evaluate the benefits of research in quantitative terms. These efforts are increasingly more successful with respect to applied research, but amount to a futile exercise for basic research. Part of the reason lies in the long time lapse between the costs and the total harvest of benefits. The main reason is that a cost-benefit analysis can be properly carried out only on a closed system, whereas basic research forms part of an open system.[1] Every basic research project draws on the global pool of basic knowledge and contributes to the same pool. The total amount of use, and the value, of any one contribution can never be established. Similarly, the total value of basic research influencing, in one way or another, a particular development project cannot be established, even if the complete network of cause-effect relationships could be traced. Such tracing itself involves costly research projects. Several have been undertaken, with various results.[2]

Nevertheless, it is easy to show that most of modern technology makes extensive use of the results of basic research. Much of it has brought well known, but incalculable, benefits to mankind. The majority of the world population needs more of such benefits, but it is becoming increasingly evident that various negative effects, often unforeseen and also incalculable, are associated with these benefits. In the extreme, the combined pressures of population explosion and accelerating development of technology on a globe of fixed size are bringing the threat of major man-made disasters.

[1]Some artificial constraints are in general needed to make any practical system "closed" for analytical purposes. This is acceptable when the elements excluded from consideration are estimated to have little influence on the outcome of the analysis.

[2]For example, *TRACES: Technology in Retrospect and Critical Events in Science*, NSF-C535, Illinois Institute of Technology, December 15, 1968; also *Project Hindsight*, Final Report, Office of the Director of Defense Research and Engineering, DC20301, Washington, 1969. See also I.C.R. Byett and A.V. Cohen, *An Attempt to Quantify the Economic Benefits of Scientific Research*, HMSO, London, 1969. A more practical cost-benefit analysis based on evaluation of short-term indirect benefits alone (i.e., excluding the value of discovery) was reported by K.M. Hill *et al.* in "How Much Basic Research is Enough?" *Long Range Planning*, Pergamon Press, Elmsford, N.Y., March 1969.

The growing recognition of this threat is causing a reaction against science on the basis of its close association with technology. This unfortunate misconception must be corrected: the dangers of technology arise from knowing too little rather than knowing too much. This dilemma of knowledge has been recognized in age-old adages, but the power and massive application of modern technology amplifies the consequences. Proper understanding of all the effects of technology should have a deterrent effect against inadvertant misuse, or may even prevent a malevolent use, of technology. Humans have been able to harm each other very effectively at all stages of technological development, but have shown a remarkable restraint in the use of poison gas and bacteriological and nuclear weapons during the last quarter of a century, even on occasions when there was no danger of effective retaliation.

The mounting needs of a growing population have to be satisfied as far as possible. To call a halt to development would perpetuate present miseries for the sake of avoiding future mistakes. It is wiser to use research to produce an awareness of future dangers and opportunities, thus permitting assessment and selection of technological developments. Such "technology assessment"[3] should be based on the solid scientific knowledge needed in advance of development decisions. The general social, economic and political effects of a technology, due to both intended and side effects, must be considered; hence, the relevant research requires coordinated efforts in the natural and social sciences, without neglecting the humanistic aspects of life. It is hoped that the resulting close cooperation between researchers in natural and social sciences will help to impart more breadth to the former, and more rigour to the latter, areas of research.

Reasons for Supporting Basic Research in Canada

Every country capable of applying scientific knowledge benefits more from the research contributions of the rest of the world than from its own. Some advocates of a reduction in the effort on basic research in Canada act on the assumption that it would be more economical to concentrate on applying basic knowledge generated by other countries, which is perceived to be available to us anyway. The Science Council has made several recommendations in favour of more applied research, but is well aware, and has stated, that this should not be done at the expense of basic work. The main practical reasons for maintaining a fair share in the global effort on basic research are discussed below.

1. It is generally accepted that research is an indispensable element of a university, for the development of both teachers and students. Thus, Canadian education requires research in Canadian educational institutions. Research at the universities need not and should not be limited to basic

[3]See for example, Harvey Brooks, *Science, Growth and Society*, OECD, Paris, 1971 (The "Brooks Report"), which gives the following definition of technology assessment on p. 82:

"The general aim of technology assessment is to evaluate the social costs of existing civilian and military technologies in the form of pollution, social disruptions, infrastructure costs, etc., to anticipate the probable detrimental effects of new technologies, to devise methods of minimizing these costs and to evaluate the possible benefits of new or alternative technologies in connection with existing or neglected social needs."

research alone. Nevertheless, the search for new and deep understanding of the world which is inherent in basic research makes it particularly important to higher education. A country which chooses not to make its own contributions to this understanding, but merely tries to assimilate the intellectual efforts of others, will become as culturally impoverished as if it were to opt out of making creative contributions to music, literature, painting, philosophy, etc.; it will quickly lose the ability to assimilate the knowledge of others.

2. The Canadian need for knowledge must be satisfied primarily from our own contributions in the fields of science which are of special importance to us but of less interest to other advanced nations. For example, Canada must develop her own basic knowledge of the geology and climatology of her lands. The peculiar properties of the upper atmosphere above these lands (e.g., the auroral belt) is also a problem requiring Canadian study. Having the longest coastline in the world and the largest continental shelf (1.4 million square miles, equal to nearly 40% of her land areas), Canada is very much affected by the interaction of the three adjacent oceans with the coastal waters, the coast line and the air masses moving from oceans over the land. Most of the mechanisms involved in these interactions are not yet fully understood. The wide range of climatic conditions leads to special interest in microclimatology on the one side, and in the biological and physiological adaptations to a cold climate on the other side.

The significance of national contributions to science grows as one moves from physical sciences through life sciences to social sciences: the laws of physics are the same in all countries; but living organisms are deeply affected by climatic conditions; furthermore, certain significant behavioural characteristics and social mechanisms differ considerably from one society to another, even in similar physical environments. As the progress – or regression – of mankind becomes more and more determined by the interaction between society and technology, the importance of scientific investigations oriented by social and geographical influences will also grow. As pointed out above, this applies especially to Canada, which contains such a diversity of climate and geography. It must be remembered that by the time the basic research of today affects the daily life of the next generation, the pressure of a growing world population and the shortage of some useful resources will place an entirely different value on what now appear to many to be unattractive territories.

3. In fields of global interest (such as physics, chemistry, a large part of biology, etc.), our main need is to have access to the vast flow of knowledge generated abroad but needed in Canada. This requires human expertise that will recognize the most important elements of progress and will promptly inject that knowledge into the flow of Canadian scientific work. It also requires creative contributions as an "entry fee" into the "invisible college" of peers, which disseminates the most advanced knowledge through direct communication well ahead of open publications. The only method for satisfying both of the above requirements is to participate in the creative effort of the global scientific community on a level that commands respect and recognition.

4. In order to make worthwhile contributions in the areas both of special interest to Canada and of global interest, Canada must have its

share of highly creative scientists. As a mature nation we should be able to provide for the development of a rich variety of talent. Gifted scientists should have opportunities commensurate with those existing in the world at large. Where a disparity develops, the best people emigrate in search of self-fulfillment.

5. Canadian scientists are in an exceptional situation because of Canada's proximity to, and close cooperation and partially common language with, the nation harbouring the largest scientific effort in the world. This has an inevitable impact on our selection of options in research. It is therefore necessary that we take special care to avoid duplication of work which is so readily accessible, while at the same time recognizing that our capability for benefiting from it must be maintained.

A particular problem, arising from the greatly different magnitudes of effort in the two countries, poses the danger that Canadian research projects which are novel at their onset might be surpassed, before completion, by U.S. projects begun at a later date. This situation calls for especially careful selections and adequate funding of major projects.

6. The future of mankind clearly depends on international cooperation. Scientists are increasingly being recognized for their ability to spearhead international cooperation despite existing political or other impediments. The bridges thus built can later be crossed and enlarged by politicians and others to solve economic and political problems among their nations. Canadian interest in peace-keeping demands the maintenance of a capability for international cooperation through basic research, at least as much as it calls for a capability for international police activities.

It should be clear from the above that Canada, by making a fair contribution to international science, acts in her own best interest. There remain the problems of defining the "fair share", and of determining the criteria for selecting the level of effort in major fields of activity and for the granting of support to specific projects.

Environment for Creative Research

Basic research has an important characteristic in common with other creative activities. It is an activity in which effort alone does not necessarily produce worthwhile results. Bad research is less than worthless. It has a negative value. In addition to wasting its own time and costs, it wastes the time of other scientists assessing it. Meanwhile, it can be misleading. Therefore, when basic research is supported, care must be taken at the same time to provide an environment in which good research can flourish.

In order to be successful, a research scientist must either have considerable talent himself or else work in cooperation with, or at least within the influence of, highly talented people. The need for influence, through creative interaction with and among the intellectual leaders, is particularly acute. Thus, adequate funding of travel and other means of communication is of special importance in research budgets.

The time spent on the administrative problems of obtaining and using grants should be kept to a minimum. This means that:

1. Grants or contracts should be large enough to cover in principle a complete project by a single decision, with a minimum of intermediate

paperwork.

2. The application and reporting forms and the procedures should be simple, and uniform for the various sources of support.

3. The grantees should have reasonable freedom to change the direction of approach to their problems, since they can often clearly see the need for such change long before it can be documented for those not involved in the project.

4. When a grantee follows a wrong track for a while and has to back out, this should not be taken as a sign of failure before the complete project can be evaluated for its overall success. Not all supporting bodies recognize that the essentially systematic nature of basic research is not identifiable with planned discovery. Quite often, even the course of work cannot be planned for more than the initial phase of a research project. Excessive accent on planning may lead only to exercises in futility, or selection of the least challenging areas for work. This does not, however, detract from the need for a clear statement of objectives for any project.

The significance of the results is partially determined by what the scientists set out to do. The potential scope of their proposals should not be restricted by the relative difficulties of obtaining support for certain types of activities[4], but their ingenuity should also not lack the stimulus of stiff competition. This implies:

a) the need for competition at the national level (as far as practical);

b) confidence that the value of basic research as the foundation of scientific expertise will continue to be recognized, without drastic oscillations in the division of support between basic and applied research;

c) the need for granting policies to be *clearly seen* to be responsive to interdisciplinary proposals, with coordination among the granting agencies going right down to their committees.

Although they necessarily strive for recognition among their peers, scientists are becoming increasingly more influenced by public opinion. Public criticism is necessary, but destructive criticism, especially when based on wrong premises, can discourage the all-out effort which is indispensable to success in creative scientific work. For example, the press has been reflecting much concern among politicians over the relatively poor economic benefits from R & D expenditures in Canada, with the implication that the fault lies with the scientists. While a number of improvements can be made in the conduct of basic research in Canada, it is well known that any such improvements will have little economic effect[5] compared to such factors as: the degree to which applied research is integrated with industrial innovation; the government's industrial, fiscal and technical policies; the technical and commercial acumen of governmental negotiators of international agreements; etc., etc. Many other factors, such as entrepreneurial initiative and ability, or availability of risk capital, are also of prime importance. None of these has anything to do with the performance of basic research as such. In other words, healthy science provides a necessary, but not sufficient, condition for an economically healthy technology.

[4]E.g., high risk/high gain research; that is, projects in which the probability of success is small but the significance of a successful accomplishment would be very high.

[5]The problem of impediments to successful innovation in Canadian industry is discussed in the Science Council of Canada's Report No. 15, *Innovation in a Cold Climate*, Information Canada, Ottawa, 1971.

IV. Criteria for Selection of Projects

The number of research projects that are proposed and can be undertaken by scientists usually exceeds available resources. The process of deciding where to give support and where to withhold it is particularly difficult in basic research. Since basic research is not directly aimed at the solution of practical problems, a comparative cost-benefit analysis is not applicable. Nevertheless, a logical set of evaluation criteria can be developed by examining the merits of any research proposal from four points of view: scientific, technological, social and operational. (The term "social" is used here in its broadest sense, including considerations of a cultural, economic or general science policy nature.) There is much overlap among the last three elements of the set, which also share a common feature, consideration of the non-scientific merits of proposals for research in science. It is convenient, therefore, to divide all the criteria of merit into two principal groups[1]:

- criteria internal to science, representing evaluation by scientists (the peers) within the context of science;
- criteria external to science, representing evaluation outside the context of science.

The internal and external points of view can be represented by an arbitrarily selected number of equally arbitrarily named and defined criteria, but the objective is to select a set which will cover all the most significant scientific and non-scientific aspects of proposals, as explicitly as possible, and with minimum overlap. The following list represents a particular approach toward meeting the above objective:

Internal Criteria	**External Criteria**
Significance	*Educational value*
Connectivity	*Communication value*
Promise	*Relevance*
Validity	*Applicability*
Strength	*Technology assessment*
	Default
	Side benefits
	Means
	Cost

Each item on this list is described in the following text.

[1]Several references to the literature on this subject will be found in Science Council Special Study No. 21, *Basic Research*, by P. Kruus (Information Canada, Ottawa, 1971). The present approach is closely related to A.M. Weinberg's analysis in *Reflections on Big Science* (MIT Press, 1967). The total list of criteria developed here encompasses the same elements as are used in a more or less formalized way by various agencies in several countries.

Internal Criteria

All but one of these criteria involve evaluation of proposed research on the basis of the state of science in the global context. Only the last (*strength*) takes into account the specific local situation.

Significance
(scientific significance of research objectives)
This can be evaluated on the basis of the anticipated impact which a project will have on science if its objectives are reached. There are clear differences between initially mapping out a fresh field, filling in some missing details, testing the range of applicability of a proven relation, and designing a crucial experiment intended to disprove a generally accepted theory which has wide ramifications in science.

Connectivity
(breadth of potential influence of results on adjacent fields of science)
Such influence may occur either through transferability of theoretical developments from one field to another, or by the opening up of new experimental possibilities. For example, research which leads to establishment of a new method of observation (spectroscopy, electron microscopy, etc.) always has a high degree of connectivity.

Promise
(the likelihood of significant advance in a given field)
This can be estimated either on the basis of maturity, i.e., the firmness of the present knowledge in that field as a foundation for further expansion, or, conversely, on the basis of novelty, i.e., recognition of an important gap in knowledge which can now be filled in.

Validity
(scientific validity of the problem posed, and the approach toward studying it, as put forward by the proposer)
This may be very difficult to establish for proposals which embrace concepts or approaches so novel that there is no experience available upon which to base a sound judgement. The perennial problem of how to tell a genius from a crank has to be faced in connection with this criterion.

Strength
(degree of ability and of knowledge in the field of proposed investigation, or the appropriate related field, possessed by the proposer and available among his collaborators and associates[2])
Here one evaluates capabilities of the proposer independently from the merits of the proposal, but in relation to its subject.

This last internal criterion can be used effectively only in conjunction with a reasonably objective method for assessing the capabilities of in-

[2]The adequacy of the available (or proposed) experimental facilities should also be taken into account in this connection, from a scientific point of view. This is different than the economic considerations under the external criterion, *means*.

dividual scientists and comparing them on a national or international scale. A traditional approach, relying heavily on the number of publications, has led to the well known "publish or perish" syndrome. The resulting flood of redundant publications poses one of the major problems of modern science. More sophisticated methods of objective assessment, now under development[3], are most urgently required.

External Criteria

In contrast to the former group, the external criteria are primarily concerned with the special Canadian or local requirements. Most of the evaluation still has to be done by the scientists, but mission managers, institutional administrators, social or economic planners, and those responsible for science policy in general, also need to be involved in various ways in defining the technological, social and operational concerns which must be considered with respect to research proposals.

Educational value
(its value as an educational activity as distinct from the value of discoveries that may result from it)
The educational value is immediate and more assessable than the long-term benefits from discovery, as pointed out on page (22). The educational merits of research accrue to the leading investigator and to his assistants, particularly when they are students. The indirect effects may benefit all those who are in intellectual contact with the researcher, whether they be his colleagues or his students. The benefits to students not involved in research are diminished by the reduced amount of time available for teaching activities. The relevance of the subject of research to the curriculum taught by the researcher is of great significance to its indirect educational value.

Communication value
(its potential for improving communication across disciplinary, social or international boundaries)
Such potential is explicit in proposals which necessitate setting up an interfaculty research group, a cooperative university-government or university-industry project, or an international research undertaking. Projects involving participation by non-scientists, or field work (as opposed to purely laboratory experiments), may have special social merits.

Relevance
(relevance to requirements for more basic knowledge arising from a specific developmental mission)
The economic and social importance of problems on which the new knowledge might have bearing needs to be taken into account in assessing the importance of such relevance. The relevance of the subject to the concerns

[3]E. Garfield. "Citation Indexing for Studying Science". *Nature*. Vol. 227, August, 1970. pp. 669-70.

of the region in which the research is to take place should also be considered.

Applicability
(anticipated technological applications for the results of proposed research in the foreseeable future, outside the areas of established missions)
This includes the need for more basic knowledge to support a current technology that may be rapidly evolving or expanding ahead of scientific understanding.

Technology assessment
(possible contribution of the new knowledge in assessing potential side effects of current and proposed technologies, new products, etc.)
Such assessment usually requires much better fundamental understanding of all the physical, biological, and social phenomena associated with the above activities than that required to achieve their direct objectives (cf. the definition of technology assessment on pages 22-23).

Default
(potential adverse effects which may result from not doing research in a given area)
For example: excessive dependence on foreign expertise in an important area; inability to carry out adequate technology assessment; etc.

Side benefits
(miscellaneous benefits of the research activity itself)
For instance: the potential economic value to Canadian industry of the instrumentation required for a research project; the economic benefits of a field station to a neighbouring community; the "insurance" value of building resident expertise that may be needed for consultation on possible future problems.

Means
(relation of the required means to the availability of existing facilities and personnel resources)
A proposal that will make fuller use of partially idle equipment scores better than one calling for new investment[4], particularly if such investment would duplicate facilities available elsewhere. The long-term usefulness of new equipment should also be considered here.

Cost
(commensurability of cost with the importance of scientific objectives and the benefits assessed by the above external criteria)
In the consideration of a large number of proposals, it is desirable to strive for a balance, between projects involving low risk with low potential gain, and those of high risk with high potential gain. Also, no single project should take an excessively large share of the total available resources.

[4]This type of merit criterion should be used with special care. In particular, it should not override the consideration of scientific merit, or we would end up doing poor research for the sake of keeping obsolete equipment in use.

Relative Importance of Various Criteria

The relative importance of the criteria listed above depends on the type of research, the kind of researchers and the social environment of a project (i.e., the sector of performance). However, the criterion of *validity* must be met in all cases.

Type of Research

The internal criteria are always of importance, but they are particularly so in establishing support for free basic research, i.e., research which is free from the need to show merit with reference to any external criteria (within a practical range of *means* and *cost*). Such research will not necessarily have any direct relevance to Canadian needs. When this is the case, it can be justified only when it has such a high assessment against the internal criteria of merit that it is expected to make a significant contribution to the world pool of knowledge. This logical requirement will set a practical limit to the amount of free basic research that should be undertaken in Canada outside the areas of priority. However, one should not equate freedom with irrelevance. Free basic research might happen to have a high merit rating on the basis of external criteria, and it may be hoped that it will increasingly do so. On the other hand, the definition of oriented basic research implies that all projects in this category must show a high merit on the basis of some external criteria, as well as showing at least a satisfactory standing with respect to the internal criteria.

The relative importance of internal and external criteria is also dependent on the field of science. For example, it is evident that, in selecting basic research projects in pure mathematics, much more consideration is likely to be given to internal criteria than will be the case with projects in such fields as animal and plant biology, which are particularly related to the needs of fisheries and agriculture.

Major research programs involving a large investment in hardware ("big science") must obviously be required to show a very high degree of merit on the bases of both internal and external criteria. One of the most important criteria in such cases is the one of *side benefits*, e.g., the degree to which high-cost equipment will be provided by Canadian industry.

Kinds of Researchers

The researchers may be divided into four groups:

a) full-time researchers in government, industry and research institutes, or holding research appointments at universities;

b) academic staff heavily involved in graduate research programs, with a relatively light lecturing load;

c) academic staff whose primary role is teaching[5], who need some engagement in research, mainly for maintaining their teaching potential;

d) graduate (and occasionally undergraduate) students engaged in research primarily as an educational experience, in preparation for a professional career.

[5]The above distinction between (b) and (c) represents the two extremes of a continuous distribution in the relationship between teaching and research duties.

Among the projects undertaken by the first group, a high rating should be expected under virtually all internal criteria, with the exception of cases when one of the external criteria dictates a need to build up strength in a location or specialization in which the present strength is not commensurate with Canadian or regional needs. Weak performance can be acceptable in such a case, but only as an interim measure.

For the second group, the internal criterion of personal *strength* should predominate. Among the most important additional requirements is the need to select projects containing elements suitable to the time span of graduate education. This would be considered under the external criterion of *educational value.*

Members of the third group can be engaged with great advantage in the less competitive (and usually less costly) gap-filling research.[6] If they want the additional stimulus of working in a topical breakthrough area, they are more likely to be successful as members of a team; the relatively small amount of time they can devote to research would otherwise render them unsuitable for independent work in an area receiving much attention from full-time researchers. They would thus be exposed to a greater risk of losing support in times of tight resources.

As regards graduate students, scientific merit in their work is necessary, but often not sufficient alone. Projects which also have a significant score under the external criteria relating to technological and social value are likely to provide experience that is both interesting and very useful. An accent on social and technological values related to the local environment would be in keeping with the increasing attention now being paid to the social role of the university.

Sector of Performance

The external criteria of *relevance*, *applicability* and *default* are of particular significance to basic research projects involving industry or government. In government, additional emphasis should be placed on *technology assessment* and *side benefits.* The latter benefits can be very significant. The instrumentation requirements of all the research work in Canada is of considerable importance to our instrumentation industry. Industry may also be involved in some of the basic research needed for *technology assessment*, but there is little doubt that the competence required for leadership in such work must reside in the public sector. It also offers a field for very fruitful cooperation between government and universities.

The criterion of *educational value* should of course be given the highest weight whenever the use of the external criteria is appropriate to university projects. It must not be disregarded, however, in selecting the few basic research projects which are undertaken in establishments otherwise devoted to applied research and development. In these establishments the example given by the presence of some basic research assists in keeping up the standards of the applied work. In addition to such indirect educational influence, the presence of basic researchers serves as a communication link to the latest discoveries arising from basic research.

[6]See also remarks regarding the scholarship of synthesis (p. 39).

The concept of external criteria of merit for basic research carries with it the dangers of unwarranted expectations or demands. The knowledge gained from basic research can help in guiding the development and use of technology for economic and social benefits. However, we must warn again that the realization of such benefits is not likely to be a direct outcome of current basic research. These benefits will be determined to a much greater extent by the basic research of previous generations, by industrial and economic policies, and by international influences, than by Canadian research policy alone. An important exception occurs when some action is urgently needed to ameliorate a drastic problem. Some form of technological "quick fix" must then be resorted to, usually at a very high cost. In most cases the cost-effectiveness of such investments would be much higher if the relevant basic knowledge were readily available. A typical example is provided by the extensive construction of sewage treatment lagoons in the colder parts of Canada, which has been based on experience in the south where such lagoons do not freeze over. Such technology transfer without the relevant basic biological knowledge can be effective and economical merely by chance.

Research into the Future

The use of external criteria for research policy purposes should be backed by the ability to foresee future practical problems some ten to twenty years ahead, and to deduce the nature of basic knowledge that will be needed as a foundation for solving those future problems. This itself requires another kind of research – research into the future. One can generalize that any attempts to orient basic research toward areas of specific concern are bound to be wasteful unless we can predict reasonably well the nature of the most important future problems. The random process of free research is likely to be more effective than misdirection of research effort caused by bad forecasting. Thus, research into the future, to improve forecasting of major problems, is as necessary as it is challenging. Some aspects of it will certainly be in the area of basic research. Adequate provisions for such research are urgently needed. "A Canadian institute to conduct studies of long-range policies" was recommended by Dr. O.M. Solandt in the fifth annual report of the Science Council.[7] Since then, the government has released a report by Dr. R. Ritchie, proposing "An Institute for Research on Public Policy"[8], and announced[9] that its author has been asked to establish such an institute. The institute proposed by Dr. Ritchie is not as broad in scope and horizon as Dr. Solandt's concept. The effectiveness of the institute (for its planned purpose) will depend largely on the willingness of provincial and private sectors to contribute one half of its expenses.[9]

[7]O.M. Solandt. "Annual Report of the Chairman: Population and Policies for the Future". Science Council of Canada, *Annual Report 1970-71*. Information Canada, Ottawa, 1971.

[8]R.S. Ritchie. *An Institute for Research on Public Policy. A Study and Recommendations.* Information Canada, Ottawa, 1971.

[9]The Prime Minister of Canada's press conference in Ottawa on August 12, 1971.

V. Major Problem Areas

In earlier chapters, we discussed the value of basic research, both as a cultural activity and as an investment leading to future practical benefits. We stressed the fact that there are two types of benefits. The first type consists of the educational benefits; these are predictable and manageable. The benefits of the second type spring from practical applications of new discoveries; the exact nature of these applications is not predictable, but the potential is known to be there.

Educational objectives are twofold:

a) to train the minds of the next generation and bring them to the cutting edge of new knowledge through association with their teachers engaged in research.

b) to develop in the research community the expertise to identify from the world's supply of new knowledge that which is of importance to Canada.

The importance of the second objective is frequently overlooked. However, as about 97 per cent of the new contributions to the global pool of basic knowledge originates outside Canada, the need for this expertise is great. Meeting this need requires the selection of areas in which our ability to tap into the global pool must be particularly good, as well as areas in which we have to rely primarily on our own discoveries. In both cases excellence of creative Canadian contributions to science is called for.

The apparent simplicity of the situation, thus stated, is misleading. There are still major problem areas, and among the subjects requiring special attention are the following:

- Promotion of excellence
- Protection of freedom in science
- Selection of areas of special importance to Canada, and the role of scientists in that selection
- Relation between disciplinary and transdisciplinary research[1]
- Distribution of effort among universities, government and industry
- Concentration, versus dispersion, of research in universities
- Regional and cultural considerations
- Priorities between generation and systematization of knowledge
- Attitudes of students
- Rate of support.

A brief review of these matters is given below.

A major problem is how to identify the areas of science requiring special concentration of Canadian effort to develop needed excellence. Some areas can be identified with ease, through their direct relation to established missions or the generality of their scientific importance. In most cases, however, special effort is needed to predict the problems of the future in view of the lead-time required to derive practical benefits from newly discovered basic knowledge. A "Futures Institute" to focus on national needs for these predictions is one of the main requirements here, as stated before. The studies by such an institute could do much to facilitate an extension of the Council's approach to the problems of selecting areas of

[1]We use the term "transdisciplinary" to denote any or all of the activities qualified as "inter-", "multi-", or "pluri-disciplinary".

emphasis. As explained in Report No. 4[2], the concept of major programs can be used to select and link together large numbers of scientific, technological and socio-economic activities in a limited number of broad programs. Such programs would have a definite relation to recognized national goals and should be large enough to permit positive achievements through excellence.

The need to selectively emphasize Canadian basic research work in certain areas of science poses the question of freedom in basic research. The freedom of individual investigators to define their research projects in detail (selection of specific topics, objectives, methods of approach etc.) must be protected. It is the selection of general areas of investigation that is in question.

It would be very convenient if a majority of research scientists and engineers, guided by their curiosity and insight, freely selected for investigation the areas deemed important on the basis of criteria external to science – namely the social, technical and operational criteria of merit discussed in Chapter IV. We are far from such a situation at the present time. It should be possible to increase the degree of synergism in Canadian research by involving scientists to a greater extent in the interactive processes which are necessary to select realistic objectives for the various steps toward national goals. Many scientists could do this through their professional societies and public discussion. The priorities so derived would represent an integration of social needs with scientific possibilities – i.e., a combination of what is desirable and what is achievable. An increasing number of individual scientists would then respond to these challenges, both by proposing specific research projects in the areas of importance to Canada and by participation in the assessment of such proposals for the granting councils or mission-oriented agencies. If good communications of the above nature are developed, the policy for the orientation of scientific effort will be credible to research scientists, and their positive response will be assured.

Naturally, we do not expect the majority of scientists to become so involved. Most laboratory researchers strive to achieve excellence by concentrating their efforts on the scientific work alone. However, there are a good many "Young Turks" in the laboratories, and their talents and interests should be used. Also, few scientists can remain on the frontier of individual research throughout their professional lives; as they move into various positions of broader and more managerial responsibilities, the needs and opportunities for becoming actively concerned with problems in science policy increase rapidly. Such scientists can provide good channels of communication between their colleagues at the bench and the non-scientific world.

A number of scientists will still elect to work in areas outside those of recognized significance to Canada. If their proposals have outstanding scientific merit, they should be supported. In addition to making contributions to the general pool of scientific knowledge, some of them might make

[2]Science Council of Canada Report No. 4. *Towards a National Science Policy for Canada.* Queen's Printer, Ottawa, 1968. pp. 29-34.

discoveries which will change our perception of what is important to Canada. The question of "outstanding scientific merit" raises the problem of evaluating the merit of individual scientists and their proposals. The Granting Councils[3] should continue to address themselves to the problem of developing more objective methods of assessment.

A greater involvement of the scientific community in the dialogue on social needs for progress in science will assist in improving understanding between scientists and laymen. It will also help in establishing a better balance between discipline-oriented research and the transdisciplinary effort required in connection with research relevant to complex social problems. Because of inherent difficulties in setting up transdisciplinary research, the granting councils should ensure that the necessary encouragement be given to such initiatives.

Since basic research is a source of knowledge and expertise, provisions are needed to ensure that this knowledge and expertise is made available to the required extent to industry, government and universities. Industry can make use of basic knowledge in connection with its involvement in development and applied research. Where major innovation activities exist, some basic research within that industry should be encouraged and assisted. Elsewhere, cooperative ventures and closer links between industry, university and government laboratories need to be developed, to provide industry with more effective means for tapping the pool of basic knowledge.

The greater the role an industry plays in the national economy, the more important it is that it does not become obsolete. The need for modernization is not purely economic. Elimination of pollution, facilitation of recycling, etc., are some aspects of modernization which grow in importance with the size of an industry. Hence, the forecasting of future requirements for basic knowledge in support of our major industries is most important. This includes assessment of the roles of present and future technologies. Both their economic roles and their impacts on the quality of life must be assessed, through advanced research, early enough to prevent costly or tragic mistakes. Much of this technology assessment can best be done by close cooperation between university and government research, as a public service. Nevertheless, industry must be involved in that work, since it is there that most of the knowledge of technology resides and new technology is being developed.

There are several areas of basic research in which government must be directly involved, in order to: provide foundations for its in-house applied research; permit assessment of basic research which it supports outside; provide expertise for international liaison, etc. In all cases, however, the following questions should be asked:

1. Can the necessary expertise be obtained by supporting research outside the government and recruiting (or exchanging) trained personnel?

2. Can external advisers be used to a greater extent?

3. Are there advantages in having this research done in industry or the universities?

[3]The three Councils which have the major mission of supporting research through disbursement of grants – i.e., the National Research Council (NRC), the Medical Research Council (MRC), and the Canada Council.

The work should be done in-house only if the answers to all the above questions are negative, or if there are special reasons for undertaking this research in-house (e.g.: a need for very long-term continuity; contributions to cooperative undertaking with industry or universities; research in support of public responsibilities for regulation and control etc.).

Universities provide a particularly suitable location for basic research. Unfortunately, the educational need for involving most teaching staff in research conflicts with the need for concentrating research work in centres of strength for the sake of high quality and economy. Quality is fostered by interaction within a significantly large group, with good leadership and good facilities. These elements cannot be provided everywhere for each field. Painful decisions need to be made to bring about a sufficient degree of concentration. The needs of smaller universities will require special consideration.[4]

The above conflict between the optimum requirements for scientific productivity and those for educational effectiveness is only one example of conflicts between the criteria of scientific (internal) merit and various socio-cultural (external) requirements. In the complex modern society, there is a need to develop a high level of scientific competence in every major region of a country as large and differentiated as Canada. Only a part of that competence has to be derived from basic research. The requirement for concentration of effort (the internal criterion of *strength*) should not be sacrificed for the sake of a uniform spread of research opportunities. Instead, local initiative to propose research that can be particularly effective in its own environment should be encouraged. This approach has been successful in several places.

Some centres of excellence are set up as research institutes on a long-term basis; this is particularly the case when large capital investment of a highly specialized nature is necessary. Their continuing existence should depend on productivity in the area of orientation. This is largely determined by the ability of leadership and management to change the emphasis of the studies with time, in tune with the changing natures of the most important problems. A major re-evaluation of productivity may be needed from time to time; for example, when one of the principal leaders departs.

Many smaller centres of effort can be set up on a project basis, as interfaculty, inter-university, university-government, or university-industry study centres. This is in keeping with developing concepts for new universities and can bring about many advantages. Flexibility, in changing the orientation of work or in discontinuing a group that is no longer productive, is among these advantages.

There is at the present time a greater pressure toward the production of new knowledge than toward the systematic organization and preparation for dissemination of existing knowledge. This latter type of work (the scholarship of synthesis) requires equal creativity and is just as important; however, it is now largely scorned by the researcher, neglected by management, and ignored in the granting practice. It is urgent that its importance be recognized and that it be made eligible for federal grants immediately.

[4]Some suggestions for amelioration of this problem may be found in Appendix A, Question 13.

The magnitude of the necessary grants could be very small, perhaps just enough to provide for some assistance, access to resources and travel to meetings. Other strategies to encourage work in this area should also be developed.

The attitudes of students toward research also needs attention. Too often, they perceive their training as preparation for continuing research in the same field. It is necessary to develop recognition that their involvement in research is a part of a general education in the methodology of scientific work, which can be applied in a broad range of professional activities.

The last major question to be considered here is the rate at which basic research should be supported. The optimum expenditure on basic research might be defined as the minimum that will:

a) permit full development of all exceptionally gifted Canadian scientists; and

b) provide the required number of personnel with research expertise to generate, or permit importation of, all the knowledge that should be applied to meet national goals, including the requirements of education.

There is no known way of deriving the dollar value of this minimum from first principles. Some analysts place much reliance on international comparisons. Figures 1A and 2A show two examples of such comparisons: basic research (BR) as percentage of gross national product (GNP); and BR as percentage of gross expenditures on research and development (GERD).[5] Both figures show Canada among the countries providing moderately high funding for basic research. Many people take these statistics as a proof that our funding is too high. Any attempt to draw conclusions from such comparative statistics is based on several assumptions, usually implicit, such as:

1. An optimum relation between basic research and GNP or GERD exists and is the same for all countries.

2. The data for various countries is truly comparable; i.e. the same rules have been used for calculating expenditures under the corresponding headings for each country.

The second assumption is known to be not quite correct (cf. Appendix B, pp. 58-65), even though much effort is being exerted by the OECD to improve the compatibility of data. Inconsistencies arise for two main reasons. First, the degree to which indirect cost of research (particularly in universities; e.g. academic salaries) is taken into account depends on arbitrary decisions based on opinions, but such decisions may have a major effect on the total cost figures. This problem is exemplified by the difference between the expenditures on basic research in Canada as shown in this document (Appendix B, Table 1), and as shown in Special Study Report No. 21 (Table 7, p. 29).[6]

[5]The latest data available from the Organisation for Economic Cooperation and Development (OECD) is used for the purpose of this comparison, in preference to data from direct national sources, because OECD made a special effort to maximize the compatibility of international statistics.

[6]P. Kruus. *Basic Research.* Science Council of Canada Special Study No. 21. Information Canada, Ottawa, 1971. The nature of the various problems in compatibility of data from different sources is discussed in the source references quoted in Appendix B. There seldom are any objective reasons for selecting one approach in a report in preference to another.

travaux. Nous avons expliqué dans le Rapport n° 4[2] que l'on peut s'inspirer du concept des programmes majeurs pour choisir et articuler de nombreuses activités scientifiques, techniques et socio-économiques au sein d'un faible nombre de grandes opérations visant à atteindre des objectifs nationaux, grâce à leur ampleur et à leur excellence.

La recherche fondamentale a plus d'importance pour notre pays dans certains domaines que dans d'autres, posant ainsi la question de la latitude dont jouit le chercheur pour choisir son domaine d'investigation. Il nous semble normal qu'il puisse librement poursuivre ses recherches en traçant les limites précises du problème à étudier et en choisissant ses méthodes de travail, etc. C'est le choix des grands secteurs d'investigation qui est le problème crucial.

Il serait bien commode que les chercheurs, guidés par leur curiosité et leur perspicacité, choisissent d'explorer des domaines jugés importants d'après des critères externes, tels les critères socio-économiques, techniques ou de réalisation des projets dont nous avons examiné la valeur au chapitre IV. Mais, actuellement, nous sommes loin de compte. On pourrait mieux articuler les efforts de la collectivité scientifique canadienne si l'on associait davantage les scientifiques au processus du choix des étapes pratiques menant vers des objectifs nationaux. On pourrait y parvenir par l'intermédiaire des associations scientifiques et par des débats publics. Les priorités seraient ainsi choisies par une comparaison des besoins de la société et des possibilités scientifiques, c'est-à-dire entre ce qu'il faudrait faire et ce qui est faisable. Un nombre grandissant de scientifiques soumettraient alors des projets dans des domaines importants pour notre pays. D'autres évalueraient ces projets pour le compte de conseils subventionnaires ou d'organismes spécialisés. Si cette bonne communication existait, les chercheurs auraient foi dans les principes d'orientation de l'effort scientifique et ils réagiraient favorablement.

Bien entendu, ce ne serait pas la majorité des chercheurs qui participeraient à cette entreprise. La plupart d'entre eux concentrent leurs efforts sur le seul aspect scientifique des problèmes pour atteindre à l'excellence. Cependant, les laboratoires comptent bon nombre de jeunes novateurs dont il faudrait reconnaître les talents. Peu de scientifiques peuvent se maintenir à la pointe de la recherche indépendante pendant toute leur carrière; en accédant à des situations comportant des responsabilités de plus en plus importantes, ils prennent une part active à l'étude des problèmes de la politique scientifique et ils assurent une excellente liaison entre leurs collègues de la recherche et le monde non scientifique.

Certains scientifiques désirent travailler dans des domaines sans grande importance pour notre pays. Il faudra cependant financer leurs projets s'ils ont une valeur scientifique hors ligne: en enrichissant la masse des connaissances, ils peuvent susciter des découvertes modifiant notre perspective sur les domaines importants pour notre pays. Cette évaluation du «mérite scientifique hors ligne» du chercheur ou de son projet est difficile. Nous avons besoin de méthodes d'évaluation plus objectives, et les

[2]«Vers une politique des sciences au Canada», p. 31-35. Cf. la liste des publications à la fin du présent rapport.

Figure 2 – Basic Research (BR) Expenditures, and Gross Expenditures on Research and Development (GERD) in Various Countries

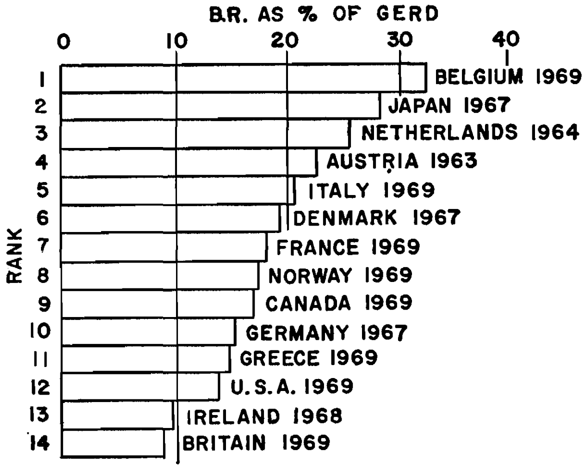

FIGURE 2A

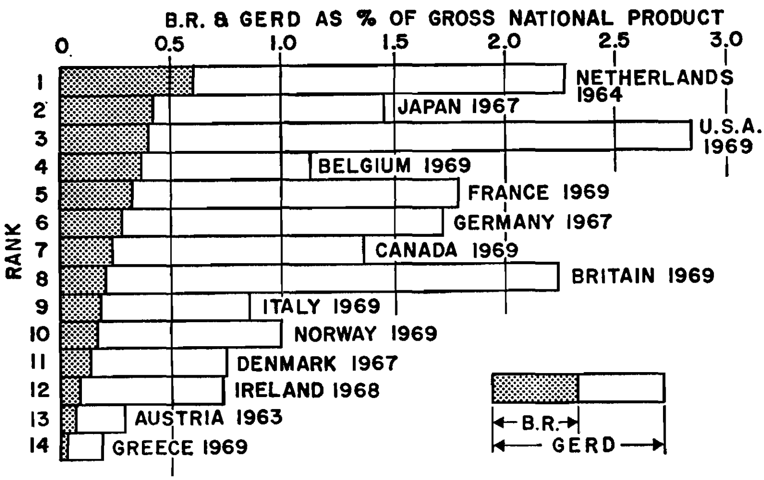

FIGURE 2B

The second main reason for inconsistencies in the comparative data lies in the difficulty of making an objective distinction between basic and applied research. The respective definitions (cf. Chapter II, pp. 17-20) differ only in motivation for the work (at an unspecific level of decision-making), which may be difficult to establish. A significant percentage of the total work may therefore be classified as either basic or applied, depending on matters of opinion or convenience.

Returning now to the more fundamental problem of optimum relations between basic research and either GNP or GERD, we see no reason to assume that such a simple direct relation should exist or should be generally applicable. We note first of all that the ranking of fourteen countries by basic research as percentage of GNP (Figures 1A and 2B) is very different than their ranking by basic research as percentage of GERD (Figure 2A. All data for Figures 1 and 2 may be found in Tables 1 to 3 of Appendix B). Although the rank of Canada changes only from 7 to 9, we find that the U.S.A. has rank 3 in Figure 1A, but rank 12 in Figure 2A; in the same Figures Denmark moves up from rank 11 to rank 6, while Britain drops from rank 8 to rank 14. The reason for these differences between the results of the two methods of ranking is indicated in Figure 2B, which shows both GERD and BR as percentage of GNP. The ranking of basic research with respect to GERD is seen to depend on national activities in applied research and development, which are likely to be governed by policies quite independent from those governing the amount of basic research. Which example should we take? If we followed the U.S.A., we should approximately double both GERD and basic research effort relative to GNP. If we followed Britain, we could slightly reduce basic research while still needing a major increase in GERD. A proportionately lower effort in both GERD and basic research can be found only among much smaller countries (Belgium, Norway, Denmark, Italy, Austria, Ireland, Greece). However, the above statements depend on taking GNP as the basis for comparison. It would be at least equally sensible to take GNP per head of population as the basis. The latter parameter is a better measure of wealth than GNP alone, and it could be expected that the percentage of GNP which can be invested in basic research should be a function of wealth. This possibility is illustrated in Figure 1B. A pattern appears there which indicates that, for most countries in the group under comparison, basic research in terms of GNP is clustered about the line of 0.1 per cent per $1 000 of GNP per capita. If one ranks the basic research effort of various countries in terms of the vertical distance from the above line in 1969 (or the latest year for which statistics are available), the following ranking is obtained:

1. Netherlands	(1964) : 0.349%		8. Ireland	(1968) : -0.032%	
2. Japan	(1967) : 0.295%		9. Austria	(1963) : -0.052%	
3. Belgium	(1969) : 0.132%		10. Greece	(1969) : -0.068%	
4. Germany	(1967) : 0.64%		11. U.S.A.	(1969) : -0.070%	
5. France	(1969) : 0.48%		12. Norway	(1969) : -0.079%	
6. Italy	(1969) : 0.024%		13. Denmark	(1961) : -0.110%	
7. Britain	(1969) : 0.008%		14. Canada	(1969) : -0.117%	

It would be just as wrong to attach some fundamental importance to the above relation as to the data in Figure 1A. All we can say is that the total expenditure on basic research represents, in most countries, the cumulative results of a large number of decisions taken on the basis of many independent considerations. The pattern in Figure 1B indicates that the communal wisdom of 14 countries, represented by the aggregate of their decisions for the latest year for which basic research funding could be separated in the information available to us from the OECD, places Canada as spending on basic research less than all the other countries, in relation to national wealth! It should be clear, of course, that the relative ranking of several countries, obtained by this method, could be changed by making a different arbitrary selection for the slope of the reference line.

In addition to the latest OECD information used for the purpose of international comparison, Figure 1A shows OECD data for earlier years and, for Canada and the United States, 8-year sequences based on national data (cf. Appendix B, Tables 1 and 3). The compatibility of data for one country over several years is generally better than the international compatibility. Thus the time plot for each country provides some insight into the development of its national policies for funding of basic research. It is apparent that very few countries maintain their funding at fairly constant levels as percentages of GNP. Drastic policy changes are clearly seen, with some countries expanding their basic research effort while others are reducing it. For Canada, a significant policy change is shown in 1967. Up to that year, basic research funding followed the growth of GNP per capita. From 1967 to 1970 the trend was reversed.

In the foregoing paragraphs it has been shown that the results of international comparisons are very susceptible to subjective selection of the bases of comparison. Thus, the apparent amount of basic research effort in one country with respect to other countries can be varied over quite a range by the arbitrary decision of a statistician. Such comparisons are not likely, therefore, to produce reliable guidelines for policy purposes. Work will have to continue on development of other methods for determining optimum expenditures on basic research. It may be hoped that this report will make a contribution to the development of a method based on assessment of Canadian needs.

The annual expenditures on free research and some part of oriented research can be postulated as a general research budget (on the basis of what the nation can afford) which is then divided among the various fields of research activities. The expenditures on oriented research in direct support of developmental missions should result from independent assessment of the needs of each mission, and be incorporated in the mission budgets, rather than in the general research budget.

VI. Conclusion

The objectives of a policy for basic research in Canada should be to ensure continuing development of vigorous basic research activities for the purpose of:

a) developing Canadian experts who are members of the international community of scientists. Through them Canada can benefit from, by contributing to, the world pool of knowledge.

b) making special contributions to the generation of basic knowledge in the fields in which our particular interests cannot be met to a sufficient degree by the rate of progress elsewhere.

c) maintaining the quality of higher education and exerting positive influence on R & D activities in general.

d) preparing a base for meeting future problems.

To date, our policy for basic research has succeeded in establishing a firm base for Canadian science and an increasingly adequate supply of highly educated personnel with research training. This supply of personnel was developed to meet the main demand, which was from the universities, with the more limited demand in government and industry. The changing situation requires policy development to meet new conditions; for example, there is currently a perceived oversupply in some disciplines. In the long run, the developments should include placing relatively greater importance on requirements for research knowledge and highly trained personnel outside the academic world. In this connection we wish to recall one of the recommendations made by OECD investigators[1]:

"It seems to us to be of the utmost importance for Canada that part of its highly sophisticated fundamental research effort should be directed towards the development needs of the country as a whole, rather than towards the increase of knowledge in general, very often to be exploited elsewhere. Such research, if wisely stimulated, need not conflict with day-to-day academic freedom, and its intellectual and educational value is unchallenged".

The Council endorses the above recommendation, on the understanding that the expression "...directed towards..." is interpreted as meaning "...oriented toward...".

The above policy objectives require that two distinct types of criteria be employed in assessing the relative merits of various research proposals. One type is concerned with evaluation within the context of science itself (the internal criteria). The other introduces consideration from the technological, social and operational points of view (the external criteria). A double set of criteria, covering both the internal and external types of consideration, has been derived. Consistent application of these criteria could ensure that the same set of considerations would be applied to all proposals being evaluated for selection purposes. The main value of such criteria thus lies in a systematization of the subjective process of assessment now in use. To be applied successfully in the selection of emphasis for research work, these criteria must be used with reference to sets of relative

[1] OECD. *Reviews of National Science Policy: Canada.* OECD, Paris, 1969, p. 392.

priorities for the need for more basic knowledge in various fields of science. Identification of these priority areas, in a way involving the scientists and credible to them, emerges as one of the essential elements for research policy.

In conclusion, we wish to reiterate that our support of the principle of greater orientation of basic research toward areas of special interest to Canada includes the clear understanding that a significant proportion of that research must continue to be conducted free from any influences external to the inner logic of the science itself. At the same time, we feel that there is a tendency to overemphasize the difference between free and oriented research. *A priori* postures toward the merits of researchers and their work, on the basis of such work's being qualified as free or oriented, are not justified. Attention should first be concentrated on promoting excellence in basic research of either kind.

Appendices

Appendix A

Answers to Familiar Questions

Several questions have been identified as representative of public concerns regarding the role of basic research in Canada. The opinions of the Science Council on these familiar questions are presented as answers[1] in the following pages. Some of the questions may appear simplistic to scientists, who will realize anyway that they imply incorrect alternatives. Nevertheless, these questions are asked. The answers given to such questions include some indications of the incorrect natures of the questions.

1. Is there an intrinsic value in basic research even if no application of its results can be foreseen at the time the research is undertaken?
Basic research is the prime source of our knowledge and understanding of the physical and biological universe. There is an inherent cultural value in reducing our ignorance of ourselves and our environment. Thus, educational benefits are always there. No one can predict other values for what has not yet been discovered, but historical evidence gives countless examples of the immense benefits originating from improved basic knowledge.

2. To what extent can basic knowledge needed in Canada be imported?
This question has been discussed in Chapter II. In short, there is no limit to the amount of knowledge that is needed, but it is known that Canada generates only about 3 per cent of the world's knowledge. Great care must be taken to ensure that this small potential is deployed so as to optimize our coupling to the world pool of knowledge, particularly in areas of special importance to Canada (cf. pp. 23-24). In addition, it must generate that part of the specially important knowledge which is not available from outside to a sufficient extent. The necessary part of the remaining 97 per cent must be imported.

3. Should the selection of topics for basic research be left solely to the insight and curiosity of research scientists, or should it be influenced by considerations external to science?
This is the problem of orientation in research versus free research, already discussed in Chapters IV and V. All research must have its roots in the insight and curiosity of scientists if it is to be good. It is proper to expect, however, that many, if not most, basic researchers will be interested in the social, economic and technological needs for the knowledge which they seek, in addition to being concerned with the high merit of their proposals based on the internal criteria of science. It is particularly important to stimulate an atmosphere in which the best of Canadian scientists will wish to lead basic research projects in areas recognized to be of specific importance to Canada. Nevertheless, free research outside those areas must also be supported when justified by scientific merit alone, provided that

[1]These answers necessarily repeat many of the opinions already presented in the main text of this report. They are assembled in a different order here, for the convenience of some readers. An expanded treatment of a few problems is also given.

such merit is exceptionally high. This is the only way to overcome the limitation of human foresight regarding the areas of knowledge which will have practical significance.

4. What are the major impediments to improved effectiveness of basic research in Canada?

Basic research in Canada has produced many achievements, but its effectiveness could be improved in several ways. The main opportunities lie in the directions of:

a) greater concentration of effort in fewer areas of investigation to develop centres of strength with a greater degree of world leadership.

b) more emphasis on quality everywhere.

c) better coordination of effort and more cooperation among individuals and small groups.

d) improved communication among researchers in different disciplines and among those in different social sectors (universities, government, industry).

e) increased emphasis on problem areas of national or regional priority.

f) closer two-way interaction between basic research and applied research or development.

The last-mentioned interaction is particularly important, in order to permit a faster transfer of basic knowledge to applied work, and to challenge basic scientists with the problems impeding the progress of applied projects (or created by them). Basic research to date has been setting foundations for the progress of modern technology. In the future it should increasingly be concerned with developing knowledge that might help us to assess whether or not some technology is desirable ("technology assessment").

5. What approaches could be used to increase the amount of oriented basic research?

The first step toward well-based orientation lies outside the domain of research policy. It involves translation of national goals into problem-oriented sets of specific aims and objectives and definition of the role of each sector of society in reaching these objectives. It should be a continuation of the process of defining national problems and major programs which is already underway within the Science Council. The federal and provincial governments should take leadership in accelerating this process, but this must be done in full consultation with the respective industries and professions. The results will be the definition of realistic research objectives in various fields, including transdisciplinary endeavours. At the level of specific research projects, this definition should be made by the active research scientists interested in the relevant areas. Such a process of self-orientation can result in oriented basic research's being virtually identical with free research from the working scientist's point of view. As an additional advantage, the act of relating research objectives to broad national problems will help to ensure that the spectrum of oriented basic research will not be too narrow. The most difficult part of this process is a credible selection of areas of emphasis, looking sufficiently far ahead to permit meaningful long-term research. The need for concentrated effort in this direction, in the form of an institute for research into the future, must be reiterated (cf. Chapter IV, p. 34).

6. What approach should be taken to improve communication between scientists and taxpayers?

This question is partially equivalent to Question 4. After accepting a greater role in explaining their work to the public, the scientists should become active participants in the dialogue leading to the selection of the areas of priority for research. The professional societies could list their priority areas, with justifications. The granting agencies – federal, provincial and private – and the policy bodies – the Science Council, the Economic Council, etc. – should respond with their own definitions of critical areas and requests for proposals. All these should be widely disseminated through the professional press, agency newsletters, etc. They should also be discussed at annual meetings and major symposia. A lively dialogue such as this is indispensable if research policy is to be well formulated and found credible by the scientific community. Finally, the respective governments have to make their decisions well known to the public.

7. What criteria should be used for the distribution of basic research effort among the three social sectors, universities, government and industry?

The difficulty of effecting the transfer of information from one sector to another and the need for improved interaction among all levels of research suggests that basic research should be undertaken within all three social sectors. However, the proportions need to be changed.

The principal locations for basic research now are the universities and university-based research institutes, as will be further indicated under subsequent Questions. Basic research in industry should be encouraged to the extent to which industry can make use of it to improve its capacity for coupling into the general pool of knowledge. There is no point in supporting basic research in small companies which do not engage in major development projects. Close links with the universities, industrial research institutes and government laboratories should suffice and ought to be supported.

On an industry-wide scale, it is necessary to examine very closely the need for basic research in those areas which provide the foundations for industrial processes in industries which are the mainstay of our economy, because any obsolescence in those quarters has a national significance.[2] Industries undergoing rapid changes are also vulnerable. Technology assessment is very necessary for such industries, but it has to be made well ahead of these changes, not when they are already underway.

Many of the arguments which apply to industry apply also to government. As a large performer of applied research and supporter of basic research, the government must have a certain in-house expertise. However,

[2]The degree of foreign control in such industries needs to be taken into account (cf. Science Council Report No. 15. *Innovation in a Cold Climate*. Information Canada, Ottawa, 1971. pp. 32-35).

the government may be better served in many cases by recruiting or borrowing scientists from industry and universities, who would bring not only the scientific expertise but also a much needed understanding of the other sectors of society. For these and for several other reasons, the government should show fortitude in curtailing in-house research efforts in favour of contracting out to the maximum possible extent.

There are, of course, some areas in which basic research must necessarily be done within non-industrial laboratories, for example:

- research necessary for protection against industrial errors or negligence and vested interests (safety of industrial products);
- research in areas of strong public interest and contention;
- research in support of standardization of measurement, calibration and certification services, and in support of government regulations and control functions;
- provision of major research facilities which should be available to competing firms (operation of such facilities could, however, be contracted out).

In addition, government laboratories or public research institutes provide a more suitable location than universities for long-term research projects which require continuity of personnel over many years, and/or group effort, thus being unsuitable for the present type of Ph.D. projects.[3] A very important overall criterion for the distribution of any new support of basic research is that it should help to promote cooperation among the three social sectors. For example, it might be possible to reserve more funding (grants or contracts) for joint research by universities and industry.

8. Which institutional mechanisms for the public funding of basic research are most effective?

Several mechanisms for the funding of research operate in parallel at present. These are[4]:

- various types of support from the Granting Councils;
- industrial support programs;
- grants and contracts from mission-oriented government departments.

These mechanisms operate at both levels of government, but at a very much lower intensity at the provincial level. Only very rough estimates can be made of the proportion of basic research in these fundings, but it is highest in the Granting Councils, which are the only bodies that provide major support for free extramural research. While this plurality of sources of support is a very positive feature of the Canadian funding system, better coordination is imperative.[5] There is no explicit mechanism for supporting transdisciplinary research, particularly when it should involve natural, health and social sciences together. It is now necessary to approach separately several agencies, which then have to reach an agreement on simultane-

[3] All of the above arguments are equally valid with respect to applied research, but we refer here only to the smaller, but necessary, basic component of this work.

[4] There is also funding by private foundations, but the amount of money involved is a small fraction of the total.

[5] A great step forward has been taken by creation of the Tri-Council Coordinating Committee. An "Information Exchange Centre for Federally Supported University Research" is also being set up as a unit within the National Science Library.

ous funding. This does not preclude such funding (which in fact takes place), but it tends to discourage much-needed evolution in Canadian research.

9. Is it better to support individuals, teams or institutions?
The principles of flexibility and plurality of the possible modes of support apply here as much as to the sources of funding. A discussion of the mechanisms for providing such funding is outside the scope of this report. A general comment can be made, however, that team work is of increasing importance in research; funding procedures should therefore facilitate, and thus encourage, group proposals. Negotiated development grants are an important instrument of policy. The Science Council is pleased to see their growing use. Also, ways have to be found for enhancing the development of coordinated, long-range research policy at each of the universities, without losing the advantages of competition, coordination, and long-range planning at the national level. Only national coordination can reduce the amount of scattered effort and build up centres of strength.

10. Should research proposals be evaluated on their own merits, on the merit of the individual (or the competence of the team) applying for support, or on some combination of these elements? How should the evaluation be organized?
As a general principle, all research proposals should be evaluated on the basis of both their own merit and the competence of the individual or the team. However, such evaluation might be very cursory in the cases of outstanding scientists at the peak of their productivity. It should also be accepted that a gifted individual who would merit support for free research if he were at a university should not have to move to a university in order to obtain such support. In other words, applicants for individual grants at an outstanding level of merit should be eligible while working in government or industry.

The problem of how to evaluate capabilities of individuals and the merits of their proposals is perhaps the most crucial problem for a policy on basic research. It received much attention in a recent study by the Canadian Association of Physicists (CAP).[6]

Much effort is being devoted in several countries to developing objective methods of evaluating the quality of the previous work of an individual (or a team). For the time being, we shall restrict ourselves to recommending that the Granting Councils support research in that area, possibly through a joint international effort.

The importance of quality in research has been emphasized in Chapter IV. Relatively few scientists can be really productive in independent research. Even fewer can be inspired leaders. Those who have these qualities should have the means to develop them, while those who are not productive on their own should be encouraged to participate in a supporting capacity as members of a team, or to leave the field of research. This could be achieved by placing a limit on the length of time an individual can be supported in independent research, through "starting grants". The allowed

[6] "Purpose and Choice in the Support of University Research in Physics" (The Lawrence Report). *Physics in Canada*, 27, No. 5, June 1971 (special issue).

time should be long enough (e.g. six years[7] of sustained effort) to provide an opportunity for a gifted man to gain reputation and to win an award in stiff competition by international standards. Different requirements for quality should, however, be applied in various areas of research, under the influence of the external criteria of merit (see Chapter IV).

11. How should one integrate the use of research to produce new knowledge with its use to develop human expertise?

The primary objective of research carried out at a university is the value of research as a learning experience, although the objective of generating knowledge is only slightly less important. University research succeeds when research staff become better scientists and teachers, and when students learn the methodology of creative scientific work. Research activity should also improve communication between teachers and students, as well as communication between either of these and the outside world. The above sequence of priorities does not displace the requirement for high quality of research. The minimum standards of quality will necessarily be different for different types of personnel – e.g., the teaching staff in undergraduate schools, as opposed to virtually full-time researchers in graduate schools.

There is a type of scientific activity which, at present, is not receiving sufficient recognition. This is the scholarship of synthesis, in which a scientist not only acquires a broad range of knowledge, but also applies his creative intellectual powers to reorganizing and systematizing it, to perceiving new correlations, and to preparing the results for dissemination. It includes organization of access to the work relevant to one discipline but performed within another discipline. The amount of knowledge that can be used is, in fact, increasingly limited by the quality of management of existing knowledge. This important form of advanced scholarship should therefore receive immediate recognition as basic research work, qualifying for grants to support it. It can be expected that the influence of such scholarship on improvement of teaching will be particularly strong.

As repositories of the nation's knowledge, universities should give priority to advancing knowledge in directions of particular concern to the nation, with special regard to the concerns of their region or province. Thus, universities should be in sympathy with the concept of oriented basic research. Furthermore, the direction of orientation must be compatible with Canadian needs. For example, it makes little sense to develop, through research, the type of expertise which will inevitably lead to emigration because there is insufficient opportunity to use that expertise in Canada.

Special attention will have to be paid to developing a more flexible attitude toward future work among those higher-level graduates with intensive training in research. As more and more of them will have to look for a career outside the university, provisions are needed for developing their ability to generalize their experience in research, as a preparation for work other than basic research. This has to take place while they proceed through their Ph.D. studies, or a different type of a higher degree might be

[7]It is taken that most grants can be given for a three-year period.

necessary. The above problem could more easily be resolved if broader provisions existed for bringing about a closer contact between the universities and the world of professional employment outside the university. The NRC initiative in granting deferred fellowships and industrial fellowships represents an approach which should be extended as broadly as possible. Part-time appointments of scientists to academic and industrial positions offer another way of bridging the gap in communication.

12. Should the concentration of university research be increased, and how could centres of strength be developed?

There is a very strong requirement for concentrating research work in a given field in "centres of strength", to permit the interaction of personnel and the provision of better facilities which are required for productivity in research. This is contrary to the educational need for some research in most departments of all universities. Pragmatic solutions to this inherent problem must be sought, without discriminating against smaller educational establishments. The remedies may include:

a) transfer of more research from government to universities;

b) concentration through multidisciplinary cooperation;

c) the setting up of centres of leadership and coordination, with members of a team located in various institutions[8];

d) use of research opportunities outside universities, with formal academic recognition of such research.

e) giving more recognition to research aimed at systematization of existing knowledge rather than discovery (see previous Question, and Chapter V, p. 39).

The last possibility is of particular significance. Both industrial and government laboratories could be involved. Major research facilities like those of NRC could in fact act as research institutes affiliated with a number of universities, and be extensively staffed with graduate students and university staff on sabbatical leave.

Where research is closely integrated with education, it may offer interesting opportunities for research on the learning process itself. The effectiveness of research activities in the learning cycle, their effect on teacher-student relationships, etc. could be studied, possibly in conjunction with the departments of education or psychology. Basic research on the process of learning, particularly with reference to educational technology, should also qualify for national support.

[8]Broad-band electronic communications can supplement travel for frequent interaction within a dispersed team. An extensive discussion of such possibilities may be found in a report on "International Electronic Highway", prepared by the WGBH Educational Foundation, Boston, under sponsorship of the Ford Foundation (April 6, 1970). A similar network has recently been proposed for Ontario: *Ring of Iron: A Study of Engineering Education in Ontario*, A report to the Committee of Presidents of Universities in Ontario, by P.A. Lapp *et al.*, Toronto, December 1970. The recommendations in Science Council of Canada Report No. 13, *A Trans-Canada Computer Communications Network* (Information Canada, Ottawa, 1971), are also relevant here.

13. What are the roles of interfaculty groups and autonomous centres of strength in basic research?
The need for excellence in research, for the full utilization of some costly equipment, or for providing research services to industry or government may require more effort than can be provided by the teaching staff alone, without conflict with their normal academic obligations. In such cases it is preferable to set up special research groups, perhaps supported by research professorships of limited duration. This may be done within an existing university structure, but sometimes on an interfaculty basis, for interdisciplinary studies. Major and longer-term requirements warrant setting up autonomous research institutes, even through much can often be gained by locating such institutes on a campus and retaining university affiliation. Such affiliation is especially necessary to permit graduate students to carry out their research work at the institutes.

Mission-oriented institutes should be completely financed by the mission's budgets, including floor space[9], overhead and salaries of staff (except for the academic staff). Their work would necessarily include a major proportion of applied research, but with a strong basic underpinning – otherwise, it might just as well be contracted out to industry.

Some centres of strength should be set up on the basis of a limited lease on life for any particular orientation of work. It would be known in advance that a dispersal or re-orientation would occur automatically unless an exceptional vitality justified extension of the original mandate. This applies particularly to those centres of strength which can be organized as leadership structures, without major investment in buildings, etc.

[9]The floor space may have to be rented, unless there are solid prospects for long-term use of a specially-constructed building.

Appendix B

Statistical Data

This appendix contains the numerical data on research activities in Canada and some other countries, in support of the graphs and bar charts in Figures 1 and 2 of Chapter V. The qualifications applicable to the footnotes, and the references for them, are given below.

For purposes of comparison, all figures are presented in terms of U.S. dollars; the exchange rates used in the OECD sources have been used here. For Canadian figures not taken from OECD publications, the exchange rate of $1 Canadian = $.925 U.S. has been used up to and including 1969; for 1970, an exchange rate of $1 Canadian = $.97 U.S. has been used.

The data on basic research expenditures are for current expenditures only, except where otherwise stated. Thus, capital expenditures are excluded, in contrast to the figures quoted for Gross Expenditure on R & D (GERD). This is due to the fact that the data available for most countries exclude capital expenditures. The use of current expenditures wherever possible thus improves the compatibility of the figures in these comparative tables. However, the international comparableness of data is subject to many reservations. The nature of possible inconsistencies is explained by the OECD in: *A study of resources devoted to R & D in OECD member countries in 1963-64. Statistical Tables and Notes* (Vol. 2: pp. 17-27; 73-96; 187-221; 285-302; 333-356. Organisation for Economic Cooperation and Development (OECD), Paris, 1968).

Many of these explanations apply also to later surveys. The specific notes for the later years will become available as the final results of the successive biannual surveys are published by OECD. Meanwhile, some preliminary data from the 1967 and 1969 surveys are included in our tables; however, due to the provisional nature of these data, fewer detailed qualifications of them can be included in our explanatory notes, particularly for 1969.

The dollar figures quoted in these tables represent totals obtained by combining expenditures for all sectors (business enterprises, government, higher education, non-profit institutions), unless otherwise indicated. Expenditures are attributed to the country in which research is performed, not to the country providing the funding. This is significant in the cases of some smaller countries housing major international research laboratories.

Table 1 – GNP and Expenditures on R & D in Canada

The GERD and basic research data shown in Table 1 were extracted from revised data supplied by the Science Statistics Section of Statistics Canada, the official source of Canadian statistics on the subject. The GERD data represent the sum of intramural performance of R & D within each social sector.

The data for basic research, based on the revised GERD series, differ considerably from those shown in Special Study No. 21.[1] The variance is greatest in the university sector statistics, and results mainly from: a) the different primary sources used in each case; b) the fact that Statistics Canada took 60 per cent of the total estimated current expenditures on university R & D, while Study No. 21 took 70 per cent; c) the intention in Study No. 21 to include the cost of academic time.[2] Statistics Canada has recently suggested to us that, while the university sector data are fairly accurately estimated within the context of the arbitrary assumptions involved, the dependence of the results on these assumptions is so great that little *absolute* value should be given to these data.

The government sector data were taken from a Statistics Canada survey, and they include data on the provinces, the provincial research councils and the crown corporations (almost all of the latter are in the federal sector). The indirect costs of government intramural expenditures on basic research, which were excluded in some earlier compilations, are included in the present data. These indirect costs are for accommodation services provided by other departments, and for administrative program costs attributable to scientific activities.

The data for the industrial sector come from a Statistics Canada survey of industrial R & D. The survey directly covered only the odd calendar years, but provided a basis for knowledgeable estimates for the even years.

The Statistics Canada data in Table 1 are not very different from the OECD data on basic research for Canada in 1967 and 1969 (see Table 2). In view of this measure of agreement, it seemed justifiable to show the Statistics Canada figures for all the available years in the international comparison chart, Figure 1B of Chapter V. However, only the OECD figures were used for ranking purposes.

[1] P. Kruus. *Basic Research*. Science Council of Canada Special Study No. 21. Information Canada, Ottawa, 1971. Table 7, page 24.

[2] In this respect, the approach taken in Study No. 21 followed the reasoning of the "Macdonald Report". See: John B. Macdonald *et al.*, *The Role of the Federal Government in Support of Research in Canadian Universities*. Science Council of Canada Special Study No. 7. Queen's Printer, Ottawa, 1969.

Table 1 – GNP and Expenditures on R & D in Canada (See explanatory notes in the preceding text)

Year	GNP		GERD		Expenditure on Basic Research		
1	2	3	4	5	6	7	8
	$ Billions U.S.	Dollars per capita U.S.	$ Millions U.S.	As a % of GNP	$ Millions U.S.	As a % of GNP	As a % of GERD
1963	40.1	2 121	413	1.03	68	.169	16.5
1964	43.8	2 270	506	1.15	78	.178	15.4
1965	48.2	2 455	622	1.29	95	.197	15.3
1966	53.7	2 680	720	1.34	113	.210	15.7
1967	57.4	2 810	821	1.43	145	.252	17.7
1968	66.1	3 188	871	1.31	145	.219	16.6
1969	72.7	3 459	959	1.31	156	.214	16.3
1970	81.9	3 777	1 025	1.25	168	.205	16.4

Specific Sources:
Column 2 – GNP:
a) 1963-1967 inclusive: Canada Dominion Bureau of Statistics (DBS). *National Accounts Income and Expenditures, 1967.* DBS 13-201, Queen's Printer, Ottawa, 1968. (Table 1, page 18).
b) 1968 and 1969: Canada DBS. *National Income and Expenditure Accounts, Preliminary Fourth Quarter and Annual, 1969.* DBS 13-001, Queen's Printer, Ottawa, 1970. (Table A, page 22).
c) 1970: Canada, Statistics Canada. *National Income and Expenditure Accounts, Third Quarter, 1971.* DBS 13-001, Information Canada, Ottawa, December 1971. (Table 1, page 16).
Column 3 – GNP, dollars per capita:
a) 1963-1969 inclusive: Canada DBS, Census Division. *The Population Projections for Canada, 1968-1984, Analytical and Technical Memorandum No. 4.* Queen's Printer, Ottawa, April 1970. (Table 1, page 10).
b) 1970: Canada, Statistics Canada. *Canadian Statistical Review.* DBS 11-003, Volume 46, Information Canada, Ottawa, September 1971. (Table 1, page 18).
Column 4 – GERD:
1963-1970 inclusive: Revised estimates, Science Statistics Section, Statistics Canada.
Column 6 – Basic Research:
1963-1970 inclusive: Science Statistics Section, Statistics Canada.

Table 2 – GNP and Expenditures on R & D in Various OECD Countries
The annual periods of accounting covered in this tabulation may vary from country to country, by sectors, or even within sectors. In most cases calendar years are covered, in others fiscal years, and in a few cases academic years.

For example, Reference F (below) states that the 1967 data on Japan, for all sectors, refer to fiscal years. In the same document the data for Britain's government and private non-profit sectors cover fiscal years. The United States data for the government and higher education sectors use fiscal years. In all other cases, the data are for calendar years.

References

A. Organisation for Economic Cooperation and Development (OECD). *Main Economic Indicators.* Paris, February 1970. Page 136.

B. *OECD Observer,* 3rd year, No. 26. "The OECD Member Countries". Paris, February 1967.

C. *OECD Observer,* 5th year, No. 38. "The OECD Member Countries". Paris, February 1969.

D. *OECD Observer,* 6th year, No. 44. "The OECD Member Countries". Paris, February 1970.

E. *OECD Observer,* 7th year, No. 50. "The OECD Member Countries". Paris, February 1971.

F. OECD. *A Study of Resources Devoted to R & D in OECD Member Countries in 1963-64. Statistical Tables and Notes.* Vol. 2. Paris, 1968.

G. Provisional information from OECD.

Table 2 – GNP and Expenditure on R & D in Various OECD Countries (See explanatory notes in the preceding text)

Country	Year	GNP		GERD		Expenditure on Basic Research		
1	2	3	4	5	6	7	8	9
		$ Billions U.S.	Dollars per capita U.S.	$ Millions U.S.	As a % of GNP	$ Millions U.S.	As a % of GNP	As a % of GERD
Austria	1963	7.85	1 090	23.2	0.29	5.24	.066	22.4
Belgium	1963	13.9	1 510	137.0	0.98	21.9	.157	15.9
	1967	19.6	2 050	226.3	1.15	62.5	.319	27.6
	1969	22.8	2 360	261.1	1.14	84.0	.368	32.2
Britain	1964	86.1	1 810	1 850	2.15	181.3	.211	9.8
	1966	105.1	1 910	2 466	2.35	266.3	.253	10.8
	1967	109.2	1 980	2 480	2.22	255.4	.234	10.3
	1969	109.4	1 970	2 438	2.22	224.4	.205	9.2
Canada	1967	55.2	2 810	828.3	1.50	147.1	.266	17.7
	1969	72.9	3 460	979.3	1.34	167.3	.229	17.1
Denmark	1967	12.2	2 320	90.4	0.74	17.4	.142	19.2
France	1965	94.1	1 920	1 921	2.04	345.7	.367	18.0
	1967	109.2	2 190	2 507	2.39	438.0	.401	17.5
	1968	126.2	2 530	2 627	2.08	499.1	.395	19.0
	1969	139.6	2 770	2 495	1.78	454.8	.325	18.2
Germany	1967	121.4	2 030	2 084	1.71	325.4	.267	15.6
Greece	1963	4.70	550	7.9	0.17	1.5	.031	18.9
	1966	6.60	760	11.3	0.20	2.0	.031	17.7
	1969	8.40	950	15.1	0.18	2.3	.027	15.2
Ireland	1963	2.30	810	10.5	0.46	0.34	.013	2.8
	1967	3.2	1 080	17.2	0.54	1.85	.057	10.5
	1968	3.04	1 040	22.4	0.73	2.18	.072	9.8

Italy	1963	47.6	954	290.8	0.61	43.1	.090	14.8
	1967	67.1	1 280	447.1	0.67	56.6	.084	12.6
	1969	82.0	1 520	694.3	0.85	144.9	.176	20.8
Japan	1967	115.5	1 150	1 684	1.46	472.6	.411	28.1
Netherlands	1964	14.7	1 130	330.4	2.25	85.0	.578	25.7
Norway	1963	5.74	1 570	42.4	0.74	7.5	.131	17.6
	1967	8.32	2 200	80.7	0.97	13.9	.166	17.2
	1969	9.73	2 530	96.9	0.99	17.0	.174	17.5
U.S.A.	1963	599.7	3 170	21 035	3.51	2 144	.357	10.2
	1966	756.0	3 840	23 613	3.12	3 121	.412	13.2
	1969	947.8	4 660	26 595	2.81	3 761	.396	14.1

Notes:

Column 1: For U.S.A. government, university and other non-profit sectors, include social sciences and psychology.

Column 3: Britain (1964): GNP is calculated from GERD as a percentage of GNP and GERD data (as in Reference F).

Denmark (1967): GNP is calculated by multiplying the average of the population figures from 1967 (Reference C) by the 1967 GNP per capita figure (Column 4).

Greece (1966): GNP is calculated by multiplying the average of the population figures for 1965 and 1967 (References B and C) by the 1966 GNP per capita figure (Column 4).

Italy (1963): GNP is calculated using 1963 GERD and the 1963 percentage of GERD in GNP (Columns 5 and 6).

Column 7: – The OECD basic research figure for the U.S.A. is for 1964; the GNP and GERD data from the same source is for 1963.

– Except for Austria, Ireland and the Netherlands, for which total expenditures are quoted, all entries in Column 7 are for current expenditures within each country.

– Business enterprise sector expenditures are excluded for Denmark (they are probably very small).

Specific Sources:

Column 3 – GNP: 1963 and 1964 – Reference A, page 136 and Reference B, pages 19-26; 1966 and 1967 – Reference C, pages 19-26; 1967 and 1968 – Reference D, pages 19-26; 1968 and 1969 – Reference E, pages 19-26.

Column 4 – GNP per capita: 1963 and 1964 – Reference A, page 136; 1966 and 1967 – Reference C, pages 19-26; 1967 and 1968 – Reference D, pages 19-26; 1968 and 1969 – Reference E, pages 19-26.

Column 5 – GERD: 1963 and 1964 – Reference E, Table T, pages 36 and 37; 1966 to 1969 – Reference G (Conversion rates for GNP and GNP per capita for 1969 are taken from OECD, *Main Economic Indicators*, February 1971, page 137).

Column 7 – Expenditure on Basic Research: 1963 and 1964 – Reference E, Table T-3, pages 58 and 59; 1965 to 1969 – Reference G (Conversion rates for GNP and GNP per capita for 1969 are taken from OECD, *Main Economic Indicators*, February 1971, page 137).

Table 3 – GNP and Expenditures on R & D in the U.S.A.
The data for U.S.A. government, university and other non-profit sectors include social science and psychology.

These National Science Foundation (NSF) data on "Total Expenditure on R & D" is not quite equivalent to the GERD of OECD data (Table 2) or Statistics Canada data (Table 1), because total funds for R & D here refer to current operating costs. However, these comprise both direct and indirect costs, including depreciation and, in some cases, obligations for capital items (as explained by NSF in *National Patterns of R & D Resources, 1953-1971*, *Technical Notes*, NSF 70-46, page 25). Comparison of figures for corresponding years in Tables 2 and 3 show a rapidly diminishing discrepancy between Total Expenditure on R & D and the OECD estimate of GERD for the more recent years.

Table 3 – GNP and Expenditures on R & D in the U.S.A. (See explanatory notes in the preceding text)

Year	GNP		Total Expenditure on R & D		Expenditure on Basic Research		
1	2	3	4	5	6	7	8
	$ Billions U.S.	Dollars per capita U.S.	$ Millions U.S.	As a % of GNP	$ Millions U.S.	As a % of GNP	As a % of Total R & D Expenditure
1963	595	3 150	17 371	2.91	2 146	.369	12.64
1964	632	3 300	19 219	3.04	2 559	.404	13.31
1965	685	3 520	20 439	2.98	2 853	.416	13.95
1966	748	3 800	22 266	2.97	3 127	.418	14.04
1967	794	3 980	23 642	2.97	3 363	.423	14.22
1968	865	4 300	25 083	2.99	3 638	.420	14.50
1969	931	4 590	26 175	2.81	3 735	.401	14.26
1970	976	4 760	26 850	2.75	3 800	.389	14.15

Specific Sources:

Columns 3 and 4 – GNP and GNP per capita:

a) 1963: U.S. Bureau of the Census. *Statistical Abstract of the U.S.A., 1968.* 89th ed. Washington D.C., 1968. (pages 5 and 312).

b) 1964-1968 inclusive: U.S. Bureau of the Census. *Statistical Abstract of the U.S.A., 1969.* 90th ed. Washington D.C., 1969. (pages 5 and 310).

d) 1969: U.S. Bureau of the Census. *Statistical Abstract of the U.S.A., 1970.* 91st ed. Washington D.C., 1970. (pages 5 and 311).

e) 1970: U.S. Department of Commerce, *Survey of Current Business.* Vol. 51, No. 2. Washington D.C., February 1971. (Table 1, page 9).

Column 5 – Total Expenditure on R & D: (GERD is not calculated in the U.S.A.; see explanatory notes in preceding text).

1963-1970 inclusive: U.S. National Science Foundation (NSF). *National Patterns of R & D Resources, 1953-1971. (Funds and Manpower in the United States).* NSF 70-46. Washington D.C., December 1970. (Table B-1, pages 28 and 29).

Column 7 – Expenditure on Basic Research:

1963-1970 inclusive: U.S. NSF. *National Patterns of R & D Resources, 1953-1971. (Funds and Manpower in the United States).* NSF 70-46. Washington D.C., December 1970. (Table B-2, pages 30-31).

Appendix C

Selected References

This report is based on an extensive study which included a major amount of direct, interactive information-gathering, as well as a study of the world literature. The principal reference is therefore Science Council Special Study No. 21, *Basic Research*, by Dr. P. Kruus (Information Canada, Ottawa, 1972). Several background documents which resulted from that study have not been published, but are available for reference in the Library of the Science Council. They are:

1. Summary of seminars on Basic Research and National Goals.
2. Background material for the conference on Basic Research and National Goals.
3. Reports from the March conference on Basic Research and National Goals.
4. Summary of the chairman's conference on Basic Research and National Goals.
5. Summary of the graduate students' seminar on Basic Research and National Goals.

Outside our own study, the most comprehensive source of information on the Canadian situation and activities is to be found in the evidence given before the Senate Special Committee on Science Policy (Chairman: Senator M. Lamontagne) as reported in Hansard:

Canada, Parliament. The Senate. Special Committee on Science Policy. *Proceedings of...*:

a) Phase 1, Second Session of the Twenty-Seventh Parliament, 1967-1968. Queen's Printer, Ottawa, 1968. 328 pages.

b) First Session of the Twenty-Eighth Parliament, 1968-69. Nos. 1 to 80. Queen's Printer, Ottawa, 1969.

c) Second Session of the Twenty-Eighth Parliament, 1969-70, Nos. 1 to 3. Queen's Printer, Ottawa, 1970.

The Senate Committee's own reports are published as:

Canada, Parliament. The Senate. Special Committee on Science Policy. *A Science Policy for Canada*:

Volume 1, *A Critical Review: Past and Present.* Queen's Printer, Ottawa, 1970.

Volume 2, *Targets and Strategies for the Seventies.* Information Canada, Ottawa, 1972.

The first volume of these reports contains an index to the evidence (Annexes C and D. pages 298-327). An additional index, with subject classification, is also available in:

Mardon, J. *et al. Analysis of Briefs Submitted to the Senate Committee on Science Policy 1968-1969.* National Business Publications Ltd., Gardenvale, Quebec, August 1970.

The Science Council's Background Study No. 21 contains 84 references to pertinent literature. Many more can be found in the Senate Committee reports. There is no need, therefore, to repeat a comprehensive list in this report.

For the convenience of readers, the Canadian publications referred to in this report are listed again below, except for purely statistical publications. One non-Canadian publication is included (Reference 4, below) because it is concerned exclusively with Canadian science policy and pays considerable attention to basic research. The publications of the Science Council are excluded because they are listed elsewhere in this report. In addition, a few references are given to other Canadian publications as examples of recent developments in the thinking on policy issues relevant to the subject matter of this report.

To our knowledge, no single report on the rationale and principles for the selection of basic research projects in Canada covers the same ground as the present report. The closest parallel may be found in the Canadian Association of Physicists (CAP) study (Reference 2, below), but its concern was confined to university research in physics. The report of the Association of Universities and Colleges of Canada (AUCC) Commission to Study the Rationalization of University Research is expected to treat many of the same problems as our study, but will not become available until the summer of 1972.

References Quoted in the Report

1. Canada. Federal-Provincial Fiscal Arrangements Act, 1967.

2. Canadian Association of Physicists. "Purpose and Choice in the Support of University Research in Physics". A report prepared by a Study Group of the Canadian Association of Physicists (Chairman: Dr. G.C. Laurence), *Physics in Canada*, 27(5): 1-37. Toronto, June 1971. Special Issue.

3. Ritchie, R.S. *An Institute for Research on Public Policy*. Information Canada, Ottawa, 1971.

4. OECD. *Review of National Science Policy: Canada*. OECD, Paris, 1969.

5. Lapp, P.A. *et al. Ring of Iron. A Study of Engineering Education in Ontario*. A report to the Committee of Presidents of Universities in Ontario. Toronto, December 1970.

Additional References (in order of dates of publication)

1. MRC of Canada, Report No. 2. *Canadian Medical Research: Survey and Outlook.* Queen's Printer, Ottawa, September 1968.

2. NRC of Canada Forecasting Committee. (Chairman: Dr. P.L. Bonneau) *Projections of Manpower Resources and Research Funds, 1968-1972. Science and Engineering Research in Canadian Universities.* Queen's Printer, Ottawa, 1969.

3. NRC of Canada. *A Commentary on Science Council Special Study No. 7.* Ottawa, June 1969.

4. Report of the Commission on the Government of University of Toronto. *Toward Community in University Government.* University of Toronto Press, Toronto, 1970. pages 67-83.

5. Association of Universities and Colleges of Canada (AUCC). Waines, W.J. *Federal Support of Universities and Colleges in Canada.* AUCC, Ottawa, 1970.

6. Smolensky, A.M. and Burgess, A.E. *The Role of Education in Canadian Science Policy and the Future of Canada.* Vancouver, University of British Columbia, 1970. 41 pages.

7. Symposia on Agricultural Research, University of Manitoba, January 20, February 24 and March 9, 1971. Hogg, B.G. "Graduate Students' Training and Research" in *Proceedings for Symposia on Agricultural Research.* Canada Department of Agriculture, in cooperation with the Department of Agricultural Economics, Faculty of Agriculture, University of Manitoba. C. Davidson, Editor. (In Press).

8. Symposia on Agricultural Research, University of Manitoba, January 20, February 24 and March 9, 1971. Migicovsky, B.B., "The Role of Basic Research in Mission-Oriented Research Agencies" in *Proceedings for Symposia on Agricultural Research.* Canada Department of Agriculture in cooperation with the Department of Agricultural Economics, Faculty of Agriculture, University of Manitoba. C. Davidson, Editor. (In Press).

9. Université Laval. *Rapport du Comité de planification. 11 Mai, 1971.* Laval, Québec, 1971. pages 103-154.

10. Ontario. *Commission on Post Secondary Education in Ontario.* (Draft Report). Queen's Printer, Toronto, 1971.

11. Peitchinis. S.G. *Financing Post Secondary Education in Canada.* Report on a study for the Council of Ministers of Education, Canada. Toronto, 1971.

12. Porter, A. *Towards a Community University.* Report of Academic Commissioner to the Senate of the University of Western Ontario. University of Western Ontario, London, 1971. pages 149-164.

13. Porter, J., Blishen, B., *et al. Towards 2000. The Future of Post Secondary Education in Ontario* (based on report prepared for the Committee of Presidents of Universities of Ontario by its Sub-committee on Research and Planning, presented as a brief to the Commission on Post Secondary Education in Ontario). McClelland and Stewart Ltd., Toronto, 1971. 176 pages.

14. Québec. Comité des Politiques Scientifiques du Québec. *Les Principes de la politique scientifique de Québec.* Ministère de l'Education du Québec, 1971.

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Fourth Annual Report, *1969-70* (SS1-1970)
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