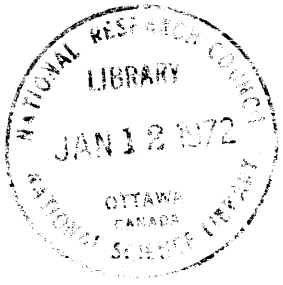
Background Study for the Science Council of Canada



December 1971 Special Study No. 21

Basic Research

by P. Kruus

December 1971

Basic Research

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Peeter Kruus

The author, Peeter Kruus, was born in Estonia in 1939. He began his education in Sweden while there from 1944 to 1949, at which time he emigrated to Canada. He continued his education in Toronto to the graduate level, earning a B.Sc. in Honours Physics and Chemistry in 1961. From 1961 to 1963 he studied in Copenhagen, receiving the Lic. Techn. degree from the Technical University of Denmark. Further studies at the University of Toronto from 1963 to 1965 earned him a Ph.D. degree in Chemistry.

Since his appointment in 1965, Dr. Kruus has been a member of the Chemistry Department at Carleton University. The study on which this report is based was carried out in 1969-70, while he was serving on the Science Council staff on leave from Carleton. His university activities include the teaching of general chemistry at the first year level, and a graduate course on Structure and Dynamics of Liquids, his present area of research activity.

Foreword

In its Report No. 4, the Science Council "elected to concentrate on science and technology as they are harnessed to serve the nation, and consequently little is said of the important position which basic research and little science must continue to play in Canada".*

In the ensuing series of background studies and Science Council reports dealing with specific areas of scientific activity in Canada, comments on the state, strength and evolution of basic research were included, but only in the strict context of the scientific area under consideration. In the latter part of 1969, these studies had progressed sufficiently that the Council authorized a start to be made on a series of overview studies, one of which was to be the report, promised in Report No. 4, dealing with basic research.

At this time the Council was fortunate in having Peeter Kruus as a member of its staff, through his secondment from Carleton University. He became the Project Officer for this overview and directed the study with imagination and determination; through a series of seminars and conferences, he gathered a wealth of information on how various parts of the scientific community perceived basic research in all its baffling simplicity and complexity. When his period of secondment came to an end in June 1970, Dr. Kruus was required to return to Carleton and leave to others the final phases of this work.

This background study, written by Dr. Kruus from his experiences, has provided the excellent basis on which the Science Council is developing its own recommendations; it is hoped that publication of this study will enhance both debate and understanding.

It is also hoped that, with the publication of this study and the Science Council's own report on the subject, those in the field of basic research, who had begun to wonder whether the Science Council of Canada had eyes only for applied research, will receive a clear and welcome reassurance of the recognition, given in Report No. 4, that basic research occupies an important position in Canadian science.

Dr. P.D. McTaggart-Cowan,
Executive Director,
Science Council of Canada.

*Science Council of Canada Report No. 4. Towards a national science policy for Canada. Queen's Printer, 1968.

Acknowledgements

The author is indebted to the participants in the study, as the major part of the ideas in this report came from their discussions and written comments. Special thanks are due to Dr. P.D. McTaggart-Cowan, Mr. J. Mullin and the Science Advisers of the Science Council–in particular Drs. R.W. Jackson, P.L. Bourgault, R. Voyer and W.L. Sauer–for help and ideas during the study. I also wish to thank the other members of the Science Council staff who have helped with the study, in particular Mrs. B. Titus, Mr. D. Hunka and Mr. L. Lafrance. Without their magnificent help, the study could never have been carried along on schedule.

Yet additional thanks are due to Mr. J. Miedzinski of the Science Council for valuable aid in the preparation of the final report, and to a number of people who took the time to comment on drafts of the report: Dr. R.A. Shigeishi of Carleton University; Dr. J. Kruus, Department of Energy, Mines and Resources; Dr. J.S. Minas and Dr. H.E. Petch, University of Waterloo; and Dr. I.B. McDiarmid and Dr. B. Gingras of the National Research Council. Any errors or omissions in the report are of course the responsibility of the author, and not of any of these people; they need not also agree with the opinions presented in the report.

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1
Introduction

In order to avoid the possibility of conflicting or disjointed statements by the Science Council on the individual disciplines covered in its special studies, the Council decided to undertake a series of "Overviews" which would cut across the boundaries of the fields covered by the discipline-based studies. One of the overviews proposed during discussions in the summer of 1969 was "Basic Research and National Goals". At the September 1969 meeting, the Council agreed that work should proceed in this area.

The title was chosen to suggest that the effort in any activity requiring large amounts of public expenditure cannot be considered in a vacuum. Such expenditures must be justified in relation to the long- and short-term contributions the activity makes toward the cultural, social and economic goals of the nation.

A study outline was prepared in November, in the form of a series of questions. The study was designed essentially to gather and to analyze opinions with respect to the following major questions:

What is "basic research"?

What contributions does it make toward fulfillment of our national goals?

What is the future for basic research?

What is the best way of managing it?

What support should be given to basic research, and how should this be distributed among fields?

This report presents the results of the above investigation, as synthesised by the author. Special effort was made to gather representative opinions from many sources during the learning phase of the study, in order to provide a broad base for the subsequent synthesis. Nevertheless, the inevitably subjective nature of that synthesis should be borne in mind by the readers. In particular, it must be stressed that the views expressed in this report do not necessarily represent the position of the Science Council.

Some of the views presented below are supported by factual statistical information, but in many cases this has not been possible–either because the data could be obtained only with a time lag too long to make it useful, or because the arguments are centred on questions of quality, or values. Personal views of the author thus had to be put forward in such cases. It is worth noting, however, that most such views evolved as a result of the exposure to, and the analysis of, the multitude of diverse, subjective opinions gathered during the study.

The statistical data in Table 7 has been brought up to date as of June 1971. However, the essence of the text has not been changed from the original version submitted to the Council in June 1970, except for editorial corrections. Some developments in the situation since that time are not, therefore, reflected in the text.

One of the first things shown by the study was the large amount of fruitless argument due to differences in the assumed definitions of the same words used by various participants in the study. To minimize such misunderstandings it is important to state right at the beginning the most important definitions used in this report. These definitions are discussed more fully in Chapter III, under "Definitions in Detail".

Basic research: original investigation undertaken in order to gain new scientific knowledge and understanding. It is not primarily directed toward any specific practical aim or application.

Free basic research: basic research undertaken without relationship to a practical mission or problem.

Oriented basic research: basic research undertaken because of apparent lack of basic knowledge in some fields, which is holding up, or may hold up, the pursuit of some mission.

Applied research: original investigation undertaken in order to gain new scientific knowledge. It is directed primarily toward a specific practical aim or objective.

These definitions still leave some room for misunderstanding because of differences in the interpretation of words such as "specific", "practical", "mission", etc.

II
Description of the Study on Basic Research and National Goals

II.1 Study Program

The Seminars

The first phase of this study consisted of eight one-day seminars. These seminars were held in order to:

Obtain input and ideas from the Canadian scientific community;

Gauge the attitudes of the scientific community regarding the subject; and

Make the scientific community aware of the complexity of the subject.

The participants were obtained by asking deans of faculties, heads of government agencies, and research directors of companies to nominate participants from their organizations. Three of these seminars had participants from science, medicine and engineering faculties at universities across Canada on a regional basis; one, from just science and medicine at Ontario universities; one, from engineering at Ontario universities and Provincial Research Institutes; one, from government research laboratories; one, from social science at major universities; and one, from industry. A Science Council member was also invited to participate in each seminar.

Participants in the seminars were supplied only with the list of questions in the outline so as not to prejudice their thinking. They were provided with some data only when they arrived. Thus the ideas expressed in the seminars were, to a large extent, the "top of the head" variety.

Summaries were made of the views expressed at the seminars, and the participants were encouraged to send in comments regarding the summaries and the topics covered by the study. Of the 175 participants, 32 sent comments, some of these being substantial essays. The seminar summaries and comments were collected in a book which was sent to all seminar participants.

The Conference

In order to obtain some more thoroughly considered input to the study, a two-and-a-half-day conference was arranged in the Council offices in March 1970. The participants spent much of this time in eight small working groups.

The chairmen of the working groups were chosen by canvassing the Council staff for names of competent, interested, open-minded scientists. From this list, eight people were chosen on the bases of sector of activity, field of interest and geography, to ensure that a wide spectrum of views would be represented. These chairmen were then invited to form their own working groups of six people each. The following agreed to act as the eight chairmen:

Dr. M.J. Keen, Chairman, Department of Geology, Dalhousie University;

Dr. J.R. Moreau, Professor, Faculty of Agriculture, Université Laval;

Dr. R.U. Lemieux, Professor of Chemistry, University of Alberta;

Dr. A.J. Mooradian, Vice-President, Whiteshell Nuclear Research Establishment;

Dr. A. Porter, Office of the Academic Commissioner, University of Western Ontario;

Dr. W.N. English, Division of Applied Physics, B.C. Research Council;

Mr. J. Miedzinski, Special Consultant, Canadian Radio-Television Commission;

Dr. J.C. Beck, Chairman, Department of Medicine, McGill University.

The participants in this conference were supplied with summaries of the seminars and other background material. After the conference, the chairmen submitted written reports on the deliberations of their groups. They then met one month later with members of the Science Council Committee on Basic Research and National Goals to discuss some of the recommendations arising from the deliberations of the working groups.

The Graduate Students' Seminar

Although attempts were made to get a spectrum of ages in the eight seminars, it was felt that there had been insufficient representation of the younger part of the scientific community. These people are intimately involved with topics such as

research and education, and possible future trends.

An additional two-day seminar was therefore held in May for 22 graduate students in science, medicine and engineering, invited through graduate students' societies at the major universities. The participants from the University of British Columbia (Mr. A. Smolensky and Mr. A. Burgess) contributed a substantial brief on "The Role of Education in Canadian Science Policy and the Future of Canada". Four other participants sent in comments after receiving a summary of the seminar proceedings.

Other Inputs

A considerable amount of background material was made available to the conference participants: discussion papers were prepared by four Science Council staff members; three eminent non-scientists, Professor R. Daniells, Mr. A. Edinborough and Dr. E. Sirluck, wrote essays on the subject; contributions were received from Dr. G. Herzberg (as a general commentary), Dr. C.H. Langford (as an essay on "University Education and Undergraduate Education in Science"), and Dr. J.M. Holmes (as a report on "Management of Science in Britain"). Statistical material was also collected on research expenditures, university enrolments, research funding, and scientific publications. Only the essay of Dr. Sirluck is to date available in an open publication.[1]

In addition, nine learned societies were approached for their views of the problems involved, in particular regarding their present and potential role in the communication of basic research in formal and informal ways. The provincial governments, through the Departments of Education or University Affairs, were also asked to comment on those aspects of the study of interest to them.

II.2 Major Points of Consensus and Conflict

The Definition

There was considerable discussion in three of the seminars regarding the definition of basic research. Many participants felt that it was best not to isolate basic research from applied research by defining it as a separate entity. Numerous definitions were offered, based on motivation, degree of generality, working environment, time lag to application, etc.

Seven of the eight conference groups nevertheless found that the OECD* definition of basic research (III.3) was satisfactory. The splitting of basic research into "free" and "oriented" components also seemed satisfactory.

Research and Education

All participants in the study seemed to agree that research at universities is essential to good teaching. There were, however, a number of differences of opinion on some aspects of basic research in universities. Although some participants felt that there should definitely be more research activity at the universities, others felt that teaching too often suffers because of an overemphasis on research. While there seemed to be general agreement that all university teachers should have the opportunity to do some research, there was a difference of opinion as to whether all need actually to do research to remain good, up-to-date teachers.

Some participants felt that an increase in the proportion of applied research at universities would be advantageous; others felt that this would be a threat to the ideals of the university.

The Ph.D.

The relationship between basic research and the Ph.D. program came under considerable criticism, mostly because of the apparent mismatch between the graduating Ph.D. and employment opportunities outside universities. Some participants felt that the Ph.D. is not appreciated and utilized by industry; others felt that the Ph.D. who has been trained mostly through a project in basic research is not flexible enough, and has motivation unsuitable to usefulness in industry.

*Organisation for Economic Cooperation and Development

Most participants, including students, seemed to feel that society has no obligation to supply employment to graduating Ph.D.s, but that it would be a waste of resources if such expensively-trained people were underutilized. A range of schemes for solving the problem was suggested for, from the introduction of quota systems to a complete free-market system with simply an increase in information regarding employment opportunities.

Basic Research in Industry

Some participants in the study felt that the level of basic research in Canadian industry is too low because of short-sighted or conservative management, and that, as a result, the "products" of Canadian basic research are too often not developed in Canada, giving the impression that Canadian basic research is unproductive. Others felt that there is often no valid reason why Canadian industry, in its present structure, should undertake research. Numerous suggestions were made for increasing the level of research; one common suggestion was a greater amount of contracting-out of research by government.

Basic Research in Government

There seemed to be little support for any increase in "free" basic research (III.3) at government laboratories, as universities have developed sufficiently to ensure a high international reputation for Canada. Several participants stressed that the government basic research groups with proven capability should nevertheless be preserved, as such groups are difficult to build up.

There seemed to be consensus that there should be "oriented" basic research (III.3) present in government laboratories to back up government missions. Several felt that such groups could be more effective if they were set up with university involvement.

Communication

There seemed to be fairly general agreement that, although communication inside disciplines is quite satisfactory, there is much room, and need, for improvement in communication between disciplines, sectors, types of research, and scientists and the public. In particular, communication between natural and social scientists seemed nearly non-existent, even though many thought it to be increasingly important. Again there were numerous suggestions for improving matters: less specialized degrees, improved mobility of scientists, contracting-out of research, etc.

Many participants pointed out the importance of basic research in industry and government, for communicating with other research groups and for tapping the world fund of knowledge by ensuring the presence of knowledgeable people.

Culture

A full range of views was found with respect to the importance of basic research as part of a culture. At one end, some participants felt they were willing to have public funds spent on such basic research only if necessary for good teaching; at the other end, some felt that basic research is one of the most important ingredients in world culture.

Several participants noted that free basic research had more than a cultural role, as such "non-relevant" research is necessary to ensure competence in fields which can become very important in the future for unforeseen reasons.

Goals and Missions

Some conflicting views were put forward with regard to the introduction of new missions or major programs which would stimulate oriented basic research. The views ranged from the opinion that any program is better than no program, and that any action should be initiated by someone, to the view, at the other end of the spectrum, that there has been a lack of careful, deliberate planning in selecting major programs in the past, and that such planning is now very necessary. Yet others found the concepts of "nation-

al goals", "missions" or "programs" unacceptable.

Participants in the social sciences seminar felt very strongly that there is a great lack of participation by social scientists in the formulation of major programs, in spite of the fact that many of the problems are of a social rather than a technical nature.

Management

There was consensus that free basic research could not and should not be planned or directed by any centralized agency.

A difference of opinion appeared regarding the question of how oriented basic research should be managed. Some participants felt that considerable planning and centralized guidance are necessary for effective work; otherwise the research would have too great a tendency to become free. Others felt that the "orientation" of the research, at least at universities, should be left to the initiative of the researchers; otherwise the quality of the research would suffer.

Although some felt that information services and public pressure could be effective in directing research toward various fields, there seemed to be consensus that this can most effectively be done through availability of funds.

Funding

There was considerable discussion of the problems of funding basic research. Several participants were critical of the National Research Council (NRC) granting methods for being too egalitarian; they felt that the funds could be used more effectively if concentrated on fewer researchers. The idea of another type of concentration of research activity into "centres of strength" also had many proponents; fear was expressed, however, that this would unduly weaken the quality of education in universities which had not been chosen as "centres".

Another point brought up by several participants was the desirability of a greater diversity of funding sources; others expressed a fear that this would lead to more "grantsmanship", and lessen the quality of work. Some thought that universities could take over a greater part of the funding of basic research from the national committees; others feared that this would lead to "empire building" at universities.

Overall Support

There was rather little discussion regarding criteria for deciding on overall support for basic research, and for distributing this among fields. The "Delphi" experiment (II.3) was thus carried out to get the participants' views of this.

II.3 The Delphi Experiment

The problem of deciding upon the amount of public money to be spent on an activity such as basic research, which defies cost-benefit analysis, is obviously not a simple one. The problem of how such support should be divided among different fields is just as difficult. We therefore decided to see if the "Delphi" technique[2] could provide some answers to these problems.

The Delphi technique has been developed to make effective use of informed, intuitive judgement. The simplest method of achieving a consensus of experts has been the face-to-face discussion, but this approach is open to several criticisms–in particular, the influence of such psychological factors as fallacious persuasion, unwillingness to abandon publicly expressed opinions, and the "bandwagon effect" of majority opinion. The Delphi technique tries to overcome such pitfalls by replacing direct debate with a series of individual interrogations, by questionnaire to assure anonymity, interspersed with opinion feedback derived from previous rounds. It is usually found that after a few rounds, the opinions converge to a reasonable consensus.

The seminar participants and the conference participants were two obvious groups for participation in the experiment, as both groups had had consider-

able exposure to the problems but were chosen from the scientific community by different methods. In addition, the Council of SCITEC*, graduate students from the seminar (second round only), the staff of the Science Council, and the Science Council itself participated as separate groups. The results of the last two of these six groups were held internal to the Council during the study. The results might otherwise have been misinterpreted as being the final judgment of the Council instead of the results of a phase where it was only beginning consideration of the problems involved.

The first questionnaire was divided into two parts:

In *part A*, the participants were asked to respond to the following question, giving their reasons: "In 1968-69 federal support for basic research through grants was about $50 million. In terms of constant (1968-69) dollars, what should be the support in 1974-75?"

In *part B*, the participants were given the distribution of support among fields as shown in the second column of Table 2 (except for "multidisciplinary"). They were then asked to show what this distribution should be like in 1974-75, to give their reasons, to underline their field of competence, and to add any new fields not included in the list.

The response rate to the first round is shown in column 3 of Table 1. Such a response rate is excellent relative to those of similar, previously held Delphis. The results were collated group by group, so that members in one group were not generally aware of the results in the other groups.

In the second round, the participants received the numerical results of the first round, together with the reasons given for choices below the lowest quartile and above the highest quartile. Some clarifying notes were included to indicate that the figure is primarily for "free" basic research in the natural sciences (including medicine and engineering), and that the actual expenditures for this were roughly double the figure, as the grants do not cover overhead, salaries of principal investigators, etc.

Two changes were made on the second questionnaire:

A *part A2* was included, as several participants thought this to be an important question. It read: "The corresponding 1968-69 figure for the social sciences and humanities was roughly $\frac{1}{18}$ that for the natural sciences. In 1974-75 it should be....."

A new "field" was also included, as many participants thought studies covered by a title such as "Multidisciplinary Studies of Complex Systems" would become increasingly important in the future.

The numerical results for part A are given in Table 1. It shows some convergence (lowering of the interquartile, I.Q. range) in only three of the groups. Table 2 shows the results for part B. It is interesting to note that in part B the agreement between the groups is quite noticeable in the second round, considering that there had been little intergroup communication. A summary of the main reasons given for the choices by the participants in the second round is given in Tables 3, 4 and 5.

Some people were sceptical about the meaningfulness of the results, expecting that the net result would reveal nothing more than the distribution of the vested interests in the participants. The answers seemed nevertheless for the most part to be given in good faith, and not in an attempt to boost funds in one's own field at the expense of others. A "bias index" was calculated as the sum of the changes in the respondents' fields of speciality. It was slightly negative for three of the five groups taking part in the first round.

The proportions of participants from medicine, engineering, physics, chemistry and biology were far higher than those from psychology, earth science and mathematics. Yet it was the latter fields that were thought to be worthwhile supporting. This can be interpreted as another sign

*Association of the Scientific, Engineering and Technical Community of Canada

Table 1–Comparison of the Results of the Delphi Experiment, Part A

Group	No. in group	Responses		Part A1 (Mean)		Part A1 (Median)		Part A1 (I.Q. Range)		Part A2	
		Round 1	Round 2*	Round 1	Round 2	Round 1	Round 2	Round 1	Round 2	Mean	Median
Group 1 (Seminar participants)	177	102 58%	68 39%	79.2	67.2	72.5	70	30	20	1/11.8	1/10
Group 1A (Graduate students)	22	NA	10 45%	NA	72.9	NA	75	NA	32	1/6	1/5
Group 2 (Conference participants)	51	34 67%	24 47%	86.0	88.6	75	75	35.5	19	1/14	1/13.5
Group 3 (SCITEC Council)	31	17 55%	16 52%	82.8	82.6	80	80	29.5	35	1/11.2	1/12
Group 4 (Science Council)	28	8 29%	6 21%	84	81.4	81	80	28	30	1/11.4	1/15
Group 5 (Science Council staff)	19	16 84%	11 58%	63.2	62.7	65	60	24	13	1/12.3	1/12

*Some of these responses arrived too late to be included in the analysis.

Table 2–Comparison of the Results of the Delphi Experiment, Part B

	68-69 Dist.	74-75 Distribution First round (mean)					No. of Gps.		74-75 Distribution Second round (mean)						No. of Gps.	
		Gp. 1	Gp. 2	Gp. 3	Gp. 4	Gp. 5	Rel. inc. (+)	Rel. dec. (−)	Gp. 1	Gp. 1A	Gp. 2	Gp. 3	Gp. 4	Gp. 5	Rel. inc. (+)	Rel. dec. (−)
Medicine	26	24.6	25.5	24.8	24.6	24.1	0	5	23.1	20.2	24.6	21.4	24.1	23.8	0	6
Psychology	3	3.6	3.9	3.8	4.1	4.5	5	0	3.8	6.8	3.4	3.7	3.8	4.0	6	0
Biology	17	16.3	16.8	15.8	15.0	17.7	1	4	16.0	17.9	14.3	18.2	16.5	17.5	3	3
Chemistry	12	11.3	11.3	11.9	11.2	9.7	0	5	10.5	8.2	10.4	11.6	11.0	9.6	0	6
Physics	16	13.7	13.8	13.6	14.7	12.8	0	5	12.5	9.8	11.6	12.6	13.7	12.2	0	6
Earth Science	6	7.6	7.2	7.3	8.1	8.3	5	0	7.7	9.8	6.7	7.6	7.2	8.1	6	0
Engineering	16	17.1	16.6	16.4	16.1	15.2	4	1	15.7	10.0	15.8	12.3	15.7	13.3	0	6
Mathematics	4	5.8	4.9	6.4	6.2	7.7	5	0	5.4	7.2	4.7	6.0	5.6	6.8	6	0
Multidisciplinary	0	0	0	0	0	0	—	—	5.3	10.1	8.5	6.6	2.4	4.7	—	—

Table 3–Part A1. Federal Support for Basic Research in the Natural Sciences

Reasons for Lowest Choices	Reasons for Highest Choices
The arguments for higher funding are "me too" and subjective; they ignore the priority issue.	The spending cutback will end, and there will be a return to the previous level of increases.
Increase should go entirely to applied.	The identification and solution of Canadian problems should be given added support.
Amount going into basic research is disproportionately large compared with industrial research.	Basic research is the nourishment for all future progress and is neglected in Canada; we cannot compete if we are not the initiators.
In 1974-75 Canada will still not have enough truly outstanding people to merit more than my previous figure.	As one of the most developed countries, Canada should increase in such research.
More money on more important priorities (e.g. poverty) in order to avoid the American disaster where the world's largest R & D budget still doesn't make it safe to walk the streets in the daytime.	Increase as training for professional science participation in industry and government.
It is fallacy to think that science can cure society's ills without a major re-emphasis in priorities.	Increasing numbers of researchers and demand for more sophisticated equipment.
Necessary new methods of achieving higher education will not *per se* involve increase in performance of basic research.	To facilitate the development of an educational and research infrastructure more in line with other Western countries.
Relate to the growth of GNP.	To meet needs in areas that are still underdeveloped.
University research has not been sufficiently relevant and productive, both in terms of eventual practical results and training of the type of scientists needed by our country.	Figure represents 15% per year as an efficiently usable increase; must approach 3% of GNP during decade for Canada to have viable independent economy.
Need to discourage oversupply of Ph.D.s.	Need for growth and maintenance of independent, intellectually unprejudiced group of scientists as hedge against uncertain future.
Basic research and big dollar outlays do not necessarily go together.	Enrolments will continue to survive; mission kick will abate.

that most people participated in good faith. There may however be a psychological factor present in having the smallest fields increased most. The choice of "fields", and perhaps even their titles may thus have some bearing on the results.

II.4 Concluding Remarks

The activities described above have achieved a variety of things. First of all, they have produced a better understanding of the influence (whether direct or indirect) of basic research on the groups affected by it.

A large number of suggestions have also been made as to what the possible answers are to the questions asked at the start of the study. All this information was, hopefully, sufficiently well documented to be passed on effectively to the Science Council. The documentation was considerably more extensive and detailed than that published in this study report. Copies of such background documentation can be obtained from the Science Council.

Many of the views and recommendations expressed were contradictory, as indicated in II.2. This is to be expected in the consideration of an activity such as basic research, which has a cultural component, and which leads essentially into the unknown, thus defying even crude cost-benefit analysis on a long- or short-term basis. The full range of views present in the scientific community seems to have been obtained.

What is perhaps more important in the case of a study of an individual-centered activity such as basic research

Table 4–Part A2. Relative Support for Social Sciences and Humanities

Reasons for Low Support	Reasons for High Support
Difficult to delineate real projects from garbage.	Restrictions in past due to lack of trained people; too long dominated by opinionated experts with few facts to back up their opinions.
The social sciences should be helped to advance, but this cannot best be done by grants for pure basic research.	Unable to make use of present technology because of lack of understanding of social implications; must ensure that technical advances are applied without damaging society.
Not impressed by the general calibre of the individuals involved.	Studies of human and social problems of Canada have never been seriously attempted.
There are too many postgraduate students.	Offer hope for a civilization not resting on a purely materialistic basis.
Cost of teaching not likely to rise as fast as cost of science teaching.	Increase at maximum possible rate consistent with healthy growth.
Level of development and importance of subject seems adequate.	Need for knowledge about human family as great as for the physical world.
See little evidence of sound results applicable to social problems emerging from current research; money is squandered more here than in natural sciences.	Critical to foster Canadian studies and solutions by Canadians in these fields, though recognizing at the same time that contributions can be made by professionals of other countries.
Receive far more support on the applied side than do natural sciences: books, theatres, art galleries, government contracts, consultancies, etc.	Due to an increase in multidisciplinary programs involving more than "hard" science and technological hardware.
It is sometimes contended that social sciences are starved; in that case the per dollar return ought to be high; no indication that this is indeed the case.	Rapidly rising enrolments and interest at present lay the base for more pressure and capability for research.
It is questionable whether "more research" is really needed to solve basic social problems.	Predictive theories needed in social sciences.

is that interest in the topics outlined in the study seems to have been stirred up within a considerable part of the scientific community. Any eventually effective changes must involve the individuals actually doing the work, and must in many cases be initiated by them. Much of the Canadian scientific community, as judged from the seminar discussions, has worked in relative isolation, and has not devoted much thought to the reasons why they pursue their scientific work, i.e., to science policy.

The Science Council is a relatively new body. Through this study it became better known in the scientific community, as about 250 people active or interested in science came to the Council offices and spent at least one day in discussing the topics outlined in the study. The backgrounds of these people ranged from Victoria to St. John's, Fort Saskatchewan to Windsor, undergraduates to university presidents, eminent basic researchers to directors of engineering. An additional 80 people participated in the Delphi experiment. Only the eight chairmen of the conference received honoraria; all other participants gave freely of their time.

Table 5–Part B. Distribution of Support Among Fields

	Reasons given for choice in the lowest quartile	Reasons given for choice in the highest quartile
Medicine	Support available elsewhere. Transfer to basic sciences. Funds inefficiently utilized. More effort on practice, health care.	Could add greatly to quality of life. New medical schools, social medicine. New problems appearing need new knowledge. Spin-off to small-scale industry.
Psychology	Non-productive in past. Real answers do not lie here. No indication it produces useful results.	Component of increased social science. Problems real and costly. To enable enjoyment of technology, clues for behavioral problems.
Biology	Activity in other countries sufficient. Must be made relevant. Transfer to multidisciplinary. Grossly over-financed.	Ecosystem studies needed. Breakthroughs possible. Canadian specialization. Opportunities for real fundamental understanding.
Chemistry	Overproduction. Past relative growth peak. Transfer to multidisciplinary. Join with physics, biology. Responsibility with industry.	Great contributions in past. Extra to man-related projects (neurochem, pollution, metallurgy). Manpower available.
Physics	Leave "big physics" to U.S.A. More discrimination. Cannot afford space race. Available elsewhere. Overproducing.	Space, astronomy up. Fundamental source area. Weather control, biophysics up. High degree of generality.
Earth Science	Mining firms to supply funds. Too much spent and left unexploited. Greater applied interest, not basic.	Northern development, water resources, pollution, others particular to Canadian conditions. High land:people ratio.
Engineering	Unimpressed with productivity, graduates. More support from industry. More oriented. Support from contracts, not grants.	Improve secondary industry, innovative capacity. Grossly underfinanced. Increasing technological role. Needed for future economy.
Mathematics and Computing	Support from industry. Results from elsewhere readily available. A fad that will end.	Advances make all other more effective. Problems more complex. High generality. More for computing.
Multidisciplinary Studies	This is applied research. Do not believe in it. No real advance in basic science through such an approach. All oriented. Immature, cannot absorb much money.	Would coordinate scientific research to relate to actual problems. More need for interdisciplinary approach. Lowering of harmful disciplinary boundaries. Team work more productive.

III
Basic Research in Canada: An Outline of Issues Involved

III.1 Preamble

This part of the report is the result of the study described in Part II. The main purpose of the discussion is to try to identify and organize the issues which appear in the consideration of a policy for basic research in Canada. Suggestions for changes are made throughout the discussion, and in Part IV.

Sections of the report will no doubt leave the reader frustrated. Yet it is not realistic to make precise assessments of problems, let alone precise formulations of solutions in many sections of this report. Some of the difficulties of attempting to write a fair, useful report on "Basic Research in Canada" are discussed in III.9.

To the "hard scientist" reader, as well as the author, there seems to be a shocking lack in the report of "facts" on which the arguments can be based. Arguments centred on questions of quality, value-judgements and goals can, however, seldom be based on quantitative data. Even when data could be used to support arguments, they are in most cases available only with a time lag so great that they are of questionable value when used in the context of looking into the future.

In Part III, therefore, there occur numerous statements of opinion with no supporting evidence given. These are personal views of the author but are nevertheless included. Such views were developed mostly in the course of the study, as described in Part II, during which the author was exposed to the views of a considerable, broadly based segment of the scientific community. They thus reflect in some degree a weighted consensus of the Canadian scientific community.

What is "Basic Research"?

III.2 A Look Back

Long-term History

The expression " 'basic' or 'fundamental' research.... the search for new knowledge without specific application in mind"[3] seems to have originated at the beginning of this century.

Before the 20th century, more often than not, technology preceded science. Technological advances were made; then only after the deed was it explained why and how it had been possible. Thinking of "technology" as being primarily "applied science" seems to be a relatively modern concept.[4]

Patterns of spending for scientific research and development (R & D) in Canada (Table 6) may still reflect the vestiges of this "non-utilitarian", "cultural" concept of science, combined with a desire to keep pace with new technological developments.[5]

International Comparisons

A comparison of spending on R & D and basic research by various countries is shown in Table 6. The figures should not be taken as being very reliable, owing to the difficulties of collecting data, and differences in the interpretations of words in different countries.

As can be seen, Canada's total research and development effort as a percentage of gross national product (GNP) is not very different from that of other similar-sized western countries. The higher R & D as per cent of GNP and the lower basic research as a percent of total R & D in the large powers is due often to large military-related activities involving much development work. The levelling off of R & D as a per cent of GNP is not unique to Canada and the U.S.A. Preliminary reports indicate that this is also the case in at least Germany and Japan.[10]

The Growth in Canada

Data showing the growth of basic research expenditures in Canada for the past few years are given in Table 7.

The growth was very rapid in the early 1960s, but seems now to have slowed considerably. Today there are of the order of 5 000 scientists engaged in basic research in Canada, with about 1 000 Ph.D. degrees granted per year,

Table 6–Gross Expenditures on R & D in Various Countries

Country	Year	Total R & D as % of GNP	Basic Research as % of Total	Basic Research as % of GNP
U.S.A.	1953[6]	1.4	9.4	0.13
	1965[6]	3.0[a]	13.9	0.42
	1970[6]	2.7	14.6	0.39
U.S.S.R.	1967[6]	3.1	—	—
U.K.	1964–65[5]	2.3	12.5	0.29[b]
	1966–67[7]	2.7	—	—
France	1963[5]	1.6	17.3	0.28[b]
	1966[6]	2.4	—	—
Germany	1964[5]	1.4	—	—
	1967[6]	2.7	—	—
Italy	1963[5]	0.6	18.6	0.11[b]
	1968[6]	0.9	—	—
Japan	1966[6]	1.3	—	—
Netherlands	1964[5]	1.9	27.1	0.52[b]
Sweden	1964[5]	1.5	—	—
Norway	1963[5]	0.7	22.2	0.16[b]
	1967[6]	1.2	32.0[c]	0.38
Canada	1965	1.2[d]	22.4	0.29[b]
	1966	1.2[d]	—	—
	1969	1.3[e]	28.0	0.36[b]
	1970	1.3[f]	28.2	0.36[b]

Note: numbered superscripts refer to documents listed in the References at the end of the report; letter superscripts refer to these footnotes.
[a]53.6 per cent of these outlays were for defence (32.0) and space (21.6) related activities (Reference 6).
[b]Estimates of gross Basic Research (including capital expenditures on basic research).
[c]Scandinavian Research Information Notes, September (1968). This figure is for 1966.
[d]Based on figures in Proceedings of the Senate Special Committee on Science Policy, No. 26.
[e]Based on unpublished estimate of GERD from DBS.
[f]Assuming gross expenditures = 1.2 × current expenditures.

mainly for work in basic research.[11] The increase in the last half century is truly phenomenal, if it is remembered that by 1917 fewer than a dozen Ph.D. degrees in "pure science" had ever been granted in Canada.[12]

The reason for this phenomenal expansion of basic research is to be found in the attitudes illustrated by the following quotations. They would seem to express the views not only of most scientists and science administrators in the 1950s and 1960s, but also of students, university administrators, and politicians.

"The development of industrial research involves first the strengthening of university postgraduate schools, and, secondly, ensuring that the best of the graduates remain in the universities to train further research students. It is possible to have first-rate university research, with little or no industrial research, and in fact this has been our history. It is absolutely impossible to have first-rate industrial research without first-rate university research."[17]

"Science is thus in a dual position as part of a humanistic education (after all it is a branch of philosophy), and as the basis of technological development. One danger of the importance of science to technology is that science in its own right as a branch of knowledge is apt to be overlooked or minimized."[17]

"The pursuit of knowledge needs no defense. A better understanding of the universe and our own terrestial environment must lead inevitably to an improved society, both materially and intellectually."[18]

The Achievements of Canadian Basic Research

The preceding sections have talked in terms of expenditures. What actually has been the result of these expenditures?

Table 7–Current Expenditures on R & D in Canada (By Sector of Performance)

Year	Industry			Federal Government			University			Totals					GNP
	Total R & D $ Mil	Basic $ Mil	Research % of Total	Total R & D $ Mil	Basic $ Mil	Research % of Total	Total R & D $ Mil	Basic $ Mil	Research % of Total	Total R & D $ Mil	Basic $ Mil	Research % of Total	Total R & D % of GNP	Basic Research % of GNP	$ Bil.
1962	124.5	5.	*4*	148.9	25.2	*17*	60.5[13]	42.3	70	313.9	72.5	23.1	.77	.18	40.6
1963	160.2	6.4	*4*	159.8	27.2	*17*	72.6[13]	51.0	*70*	392.6	84.6	21.5	.86	.16	45.5
1964	188.3	7.6	*4*	164.9	29.0	17.7	86.6[13]	60.8	*70*	439.8	97.4	22.3	.88	.20	49.8
1965	235.0	9.1	3.9	181.5	36.4	20.2	100.0[8]	70.0[8]	70	516.5	115.4	22.4	.94	.21	54.9
1966	247.9	11.1	*4.5*	200.5	44.0	22.0	115.0[13]	80.0	*70*	563.0	135.	24.0	.92	.22	61.4
1967	292.9	13.9	4.6	235.0	58.0	23.6	177.0[a]	124.0	*70*	705.0	196.	27.8	1.07	.30	65.7
1968	302.5	15.1	*5*	259.8	49.5	19.1	222.0[a]	156.0	*70*	784.0	221.0	28.1	1.10	.31	71.4
1969	341.	15.0	4.4	285.5	50.8	17.8	260.0[a]	182.	70[b]	886.0	248.0	28.0	1.13	.32	78.6
1970	338.0	17.0	*5*	300.0	49.4	16.5	268.0[a]	189	70[b]	906.0	255.0	28.2	1.07	.30	84.5

Note: Number superscripts refer to documents listed at the end of the report, letter superscripts to the footnotes. All italic figures are estimated on the basis of other data. The remaining data are from DBS publications.

[a] Estimated as 2.2 x the federal support for scientific activities at universities in order to be consistent with data for previous years. There is a large degree of uncertainty in these figures because of the difficulty in estimating indirect costs, salary contributions, etc. A factor of 2.2 does not seem to be too high in the light of data in reference 46. However, DBS has estimated a figure of $142 M. for 1967 (as compared to $177 M. estimated here), and $190 M. for 1969 (as compared to $260 M.). Data for federal support of university scientific activities are from Reference 14 and Government Estimates.

[b] Consultations to determine the validity of this estimate showed that DBS officials estimated it to be 60% in 1969, while O. Levine of NRC estimated it to be 70 to 80%. The figure 70% is used here to be consistent with previous years' data. It does not seem unreasonable in comparison to the corresponding U.S. figure. The NSF estimated (Reference 6) that basic research accounted for about 77% of R & D expenditures in universities. There is difficulty in drawing a line between oriented basic research and applied research.

The OECD, in its review of Canadian Science Policy[9], concluded that the quality of basic research in Canada compares favourably with international standards. However, *A Selection of Canadian Achievements in Science and Technology, 1800-1964*[19] lists only a very few contributions by basic research in Canada: Rutherford (1902), Saunders (1908), Banting (1921), Penfield (1934), Lemieux (1953), Bartlett (1962).

There have certainly been many more internationally acclaimed contributions to basic research coming from Canada. There seems, however, to be very little interest in, and essentially no literature on, the history of basic research in Canada, pointing out such achievements. Presently available books reviewing Canadian science,[9,11,20] concentrate on the administrative or the development aspects.

If we are to have a genuine, lasting civilization in Canada, then more interest in the past should be stimulated, as well as in the future. A sense of perspective, of continuity, is obtained if there is some knowledge of and interest in past achievements and failures.

It is of importance to document and encourage such discussion for practical as well as cultural reasons. An evaluation of past successes and failures is necessary when changes are being considered. If we are aware of past and present attitudes and the context in which these developed, then it should also be easier to discuss, to plan, and to implement changes that are thought desirable.

III.3 Definitions in Detail

The Whys of Defining "Basic Research"
When deciding on a definition of "basic research"–and thus really outlining the activity to be discussed in this study–it is essential to ask why we are defining it. It may then be easier to decide on what criterion the definition should be based.

The primary reason for defining an activity in this case would seem to be to aid in the allocation of effort so as to achieve the most effective management of a country's, a company's, or some other institution's resources. Thus the expected outcome, i.e. the reason for undertaking the activity, is the best criterion for the definition. It must be realized, of course, that the expected outcome of basic research activity is often not the actual outcome.

Other criteria which could be used might be the content of the activity, the time or effort before application, or the environment in which the research is undertaken. A definition based on the reason for undertaking the activity would in many cases agree substantially with definitions based on these other three criteria.

Since there seems to be no compelling reason for formulating a definition radically different from the generally accepted one, the latest Organisation for Economic Cooperation and Development (OECD) definition[21] will be used in this report: "Basic research is original investigation undertaken in order to gain new scientific knowledge and understanding. It is not primarily directed towards any specific practical aim or application."

This is to be contrasted to "applied research", where the investigation is primarily directed toward a specific practical aim or objective. "Experimental development" is the use of scientific knowledge in order to produce new or substantially improved materials, devices, products, processes or systems.

The original reason for calling research "basic" (at that time "pure"), rather than "applied", stemmed from an effort to keep technology and science separated. This separation now seems to be increasingly artificial in an increasingly sophisticated technological society. The definition also contains words open to subjective interpretation; it is naive to expect a consistent, clear interpretation of the definition.

Thus in forming an "overview" by

looking at only basic research in the various disciplines, we may well perpetuate and even sharpen a distinction which is becoming less meaningful. It would nevertheless seem to be worthwhile to discuss "basic research" in a report, if only to point out the continuity in the "research spectrum" and to encourage the disappearance of any artificial boundaries.

Still More on "Basic Research"

The OECD definition is essentially the same as the definitions used by the Dominion Bureau of Statistics (DBS) in Canada, the National Science Foundation (NSF) in the U.S.A[6]., and the Ministry of Technology in the U.K[7]. The definitions in the U.S.A. and the U.K. both use the term "specific commercial objectives" instead of "practical aim or application".

The OECD (and the DBS) further subdivide basic research into "free" (or "pure", or "curiosity-oriented", or "random", or "undirected") and "oriented" basic research. The "free" basic research is that undertaken without relationship to a practical mission or problem; it is generally the scientific interest of the investigator which determines the subject studied. The "oriented" basic research is that undertaken because of apparent lack of basic knowledge in some fields which is holding up, or may hold up, the pursuit of some mission; in this case, the organization employing the investigator will normally give some broad direction to the work. Basic reesarch should perhaps only be called "oriented" if it is done in integration with the subsequent applied work which may lead to the practical aim or application.

In a definition based on aims or motivation, it is important to ask "whose motivation?". It is quite possible for a scientist to feel that, as far as he himself is concerned, he is doing free basic research. However, the research manager, sponsor, etc. can justifiably regard the scientist's work as oriented basic research, having deliberately selected the scientist because his special field is relevant to the mission. If the reason for defining the term is to increase effectiveness in resource allocation, then it would seem that the sponsor's or the research manager's reason for having the work undertaken is relevant in classifying the work, rather than the motivation of the working scientist. It is also the manager who fills out DBS forms.

"Science"

When considering basic research in science, we should have a clear idea of what we mean by the term "science".

In French, "science" implies more than the natural sciences; in German, "Wissenschaft" implies knowledge as a whole; in Russian "nauka" includes social sciences and humanities.[22]

In English, the term "science" has tended to become associated to a large degree with the natural sciences; technology is often included, social sciences at times, and humanities never. The current interpretation of the word "science" can be seen through the membership of the "Science Council". This Council would more exactly be called "The Council for Natural Sciences and Technology".

There are many advantages to breaking down the natural/social science barrier and considering basic research in "science" as a whole. Many current problems require an intimate dialogue between the two. Yet the Science Council as constituted now seems to lack the mandate to speak out for the social sciences; thus this study restricts itself to the natural sciences–from parts of psychology to parts of mathematics.

Although engineering is at times referred to as "applied science", there seems nevertheless to be "basic research" involved in it. "Medical" research has also a considerable "basic" component, as indicated by the Medical Research Council (MRC) figures, and thus has aspects in it which could be considered in this study. A "research continuum" is however often achieved within a group or even in an individual. Some of the research done in medical faculties could be done in science

faculties, and vice versa; the same is true regarding the engineering/science interface. Basic research in medicine and engineering are thus included in the study.

"Research"

Activities such as "scholarship" and "scientific data collection" are, in nearly all cases, part of "research". They can also be carried on outside the course of research.

The retrieval and organization of knowledge is usually thought of as "scholarship". It is an important part of any research activity. Scholarship pursued for its own sake often leads to new understanding and knowledge, thus becoming "research". However, such scholarship, not carried out in the course of research, is not included in the figures for research.

Scientific data collection, or "the collection and arrangement of scientific data on natural phenomena", is also an important part of most research. Geological and geophysical surveys are not included under research figures unless they result in "scientific or technological advance"; astronomic, entomologic, etc. data collection is included if "done in the course of research".

The word, and the concept of, "research" is also used increasingly throughout the educational system, but it will be used here in a relatively strict sense. Otherwise the discussion will broaden to include everything from gold prospecting, to cramming for a math exam, to John Q. Student's essay on the causes of the American Civil War.

A plea is made to the reader to keep these definitions in mind while reading the report. The meaning of terms is built up through usage. Unfortunately, the term "basic research" has developed a number of meanings. Numerous fruitless arguments and misunderstandings can be avoided if there is agreement on the meaning of a term; misunderstandings are often caused more because of differences in the assumed meaning of words than because of differences in actual points of view.

III.4 What Really Goes On

Criteria of Merit

The criteria for judging the merit of a research contribution are often divided into "internal" and "external" criteria. Although the basic activity, the "scientific method", is the same for basic and applied research, there would be differences in the weights given to the different criteria, according to which the merit of a piece of research is judged.

As internal criteria of scientific merit, M. Polanyi[23] suggests the following: (i) plausibility (no obvious absurdities, no unsound conclusions), (ii) scientific value (accuracy, systematic importance, intrinsic interest of its subject-matter), and (iii) originality (the degree of surprise).

Truly great advances in basic research have often been rejected because they have had too much originality, and peer judgements have considered them to be implausible. In Kuhn's terminology[24], they have gone out of the paradigms of normal science to cause scientific revolutions. A quotation[24] originally by Max Planck illustrates such problems:

"a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."

It is thus possible to overemphasize the plausibility and scientific value criteria in judging scientific merit, with a resulting enforcement of conformity. Many Newtons, Plancks, Penfields or Bantings could be snuffed out because of an atmosphere which does not encourage rebellion against conformity.

In the case of free basic research, only the internal criteria would seem to be applicable. Free basic research need not be irrelevant to missions, but its direction must not be influenced by requirements of missions. In the case of oriented basic research, the texernal criterion (degree of relevance to a mission) would

be important as well. In applied research, the external criterion would be the predominantly important one for continuing a project.

No matter how "relevant" a research project is to some mission, it would still have to have merit according to the internal criteria. Things that are "relevant" today may be "irrelevant" in the future, and vice versa, but basic research which meets the internal criteria has lasting merit.

Judging the Merit of Research

The merit of basic research comes under evaluation on submission of work for publication in reputable scientific journals. The papers are scrutinized by qualified referees prior to publication. The published results can then also be checked by other researchers, and any errors pointed out.

Judging the merit of an individual scientific paper at a point in time is a difficult, abstract process, open to subjective judgement. The merit may also change with time. One way of judging it is through a "citation index" to show how valuable the work has been to other researchers, but this method also has its pitfalls.

The merit of basic research is also evaluated when funds for work are applied for. In the case of MRC grants[25], the scientific merit of the proposal, as judged by external referees and a committee, is given considerable weight; the competence of the applicant is, of course, also given consideration. In the case of NRC grants, the "scientific excellence" of the applicant seems to be given primary consideration and the merit of a particular proposal is secondary. In the case of NRC, the criteria on which research grants are given are not necessarily the same in all committees.[26]

It seems to be important that granting bodies inform prospective applicants as fully as possible regarding all facets of the granting, especially if changes are made. Lack of knowledge regarding both the criteria and the mechanisms used in allocating research grants can lead to some injustices in grant allocations, or at least to suspicions of injustices.

The Peers and the Judgements

The evaluation of free basic research is made by "peers". Judging of oriented research should be done by both peers (for internal criteria) and research managers (for external criteria). Such peers must be chosen in a sufficiently democratic manner, so that applicants feel that they are judged by genuine peers and not some self-perpetuating "scientific establishment".

In judging the applications, it seems increasingly important to consider the quality as well as the quantity of previous work. The applicant must also be made aware that judgements are not essentially made on the basis of the number of publications alone. The impression–whether right or wrong–that the number of publications is all that matters is obtained if only a very brief description of the suggested project is allowed, if it seems that little time has been taken in considering it, and if there is no indication as to why the request was incompletely funded or turned down.

Judgement on the basis of the number of publications would seem to be quite acceptable if a publication is an indication of a quantum of good research. This is not thought to be the case by many people. The feeling that judgements are made by quantity alone, fortified by subtle–and sometimes not so subtle–pressures by the universities to publish, can cause frenzies of paper-writing. The papers too often become trivial, redundant or unscholarly, and many researchers are so busy "doing their own thing" that when they act as referees they do not always give close scrutiny to the papers of their peers. Faith is thus lost in the concept of publication as an indication that the research is of high merit.

The application for, and judging of, research grants could be looked upon as being far more than just a simple matter of distributing money. In preparing the

application, the researcher should derive benefit from hard-headed thought about what he has done, is doing, and will try to do, although he should not be held strictly to the planned project. In the process of judging the application, the judges should find their visions broadened, and the applicant should receive judgement on the quality of his past work, the promise of the proposed project, and, perhaps, even advice.

These things will happen only if there is more time and effort spent in preparing and judging the application, and if there is a feed-back from the judges to the applicant. To achieve this will require time and money, but the additional information transfer that would occur may well be worth it.

The Norms of Science

A considerable literature has been building up on the sociology of science.[27, 28] Some of the latest contributions, however, have been classed as efforts in "muck-raking" rather than social science. [29, 30] Some autobiographical material [31] has also appeared.

A popular topic for discussion is "the norms of science", i.e., what are the normal characteristics of scientists. The following are suggested as the basic norms: universalism (science is an international community), organized scepticism (the scientist is responsible for the validity of previous research on which his work is based, and obligated to make his criticism public), communality (findings are to be shared freely and without favour), disinterestedness (the search for professional recognition as an explicit goal is prohibited), rationality (faith in reason and empirical test), emotional neutrality (emotional involvement in work must be controlled to prevent unintentional distortion).

The "muck-raking" books supposedly portray the real scientific community in the U.S.A. This will most probably be extrapolated by readers to describe the scientific community in Canada. Some of the norms of science set up by the sociologists seem so idealized in the light of these latter efforts, that a scientist following the norms might more likely be regarded as a deviant rather than a normal member of the scientific community.

The reason for bringing this up is that it is wise for basic researchers in Canada to consider the public image that their colleagues in the U.S.A. are developing. Unless the Canadian public has respect for scientists, and confidence that its funds are used effectively and wisely by them, it will be unwilling to give much public support for basic research. Scientists could take more of a lead in developing a more "civilized", less "jungle-like" society, as much of the public expect them to do.

Changes in the Nature of Basic Research

In reflecting on the changes in the goals of research in biology, Weiss[32] describes trends in modern science as follows:

"A mass of single-tracked workers tends by its sheer momentum to amplify any trend once that trend has started rolling. A fashionable course thus becomes grooved ever more deeply, draining interest, attention, encouragement, and talent away from solitary prospecting ventures . . . . Breadth is given up in favour of depth, and universality and versatility are traded for the thrust of concentrated effort."

When breadth and versatility are present in science, then it is quite possible to achieve uniformity of scientific standards throughout science, through the homeostasis described by Polanyi[23] in *The Republic of Science*. This does not seem possible in science in the 1970s, when it is common to push ahead rapidly with increasingly narrow specialization.

The type of research which develops breadth and versatility does not seem to be in vogue. Yet many important advances are likely to arise from the study of the concepts and techniques of different specialties, and their original reorganization to form new concepts. Such activity may not really be accepted by

many people as being "research", as it is likely to involve a great deal of "scholarship" with long periods between publications; and, as pointed out above, there are good reasons for the individual scientist to stress quick, publication-producing research at the expense of scholarship.

Dissemination of Basic Research Results

The dissemination of scientific and technical information has been discussed in detail in Science Council Special Study No. 8, *Scientific and Technical Information in Canada*[33], and Science Council Report No. 6, *A Policy for Scientific and Technical Information Dissemination.*[34] Some of the recommendations have already been implemented by the government. Some of the sociological aspects of communicating basic research results have been discussed by Price.[35]

Basic research results are essentially an international commodity. It is more or less inherent in the definition that there are no proprietary rights to published results. Thus Canada's contribution is a very small fraction put into the total world pool, available for all. Judging from the number of scientific authors, Canada is contributing about 3.2 per cent of the world total.[36]

In justifying basic research expenditures in Canada, it is often claimed that basic research is necessary to give Canada a competitive, independent, technology-based economy. If Canadian contributions in basic research are to be more useful to Canada than to other countries, then the authors must do more than just publish the results. More direct contacts are necessary, with people in applied research and development, as well as with the general public–through visits, conferences, and other forms of scientific "entrepreneurship". The results are then fed more effectively into higher stages of the innovative process in industry or government, or else made available to the general public as a part of the culture which people can enjoy. Otherwise, the results of Canadian basic research are as likely–actually far more likely–to contribute to the economy or culture of the U.S.A., Japan, etc.; however, in that case, Canada would still have derived the intrinsic benefits which the activity itself provides, as described in sections III.5 to III.8.

What contributions does Basic Research make?

III.5 Relations with Education in General

Pre-University Education

It is well-nigh impossible to generalize when talking about education in Canada, whether pre-university or university. According to the British North America Act [37], the provinces may make laws in relation to education, so that the pre-university school systems differ from province to province, and each university is to a considerable extent an autonomous body.

At the pre-university level, however, there seems to be a general increase in the emphasis given to a setting where students can "investigate freely, discuss, evaluate, think and decide"[38], rather than to "indoctrination". The "problem-formulating, problem-solving" attitude that research should foster seems thus to be of importance at all levels of education. Teachers who have had, and continue to have, exposure to research should be better able to stimulate this type of attitude in students.

This process can go overboard, however, to give rise to a proliferation of badly supervised, uncritical "research" in schools at the expense of learning basic principles. The proper balance must be determined by every school system, or school, or even individual teacher, on the basis of their convictions as to what is "high quality" education, and how much effort can be devoted to achieving it.

In a world increasingly dependent upon technology, it would seem desirable for the general public to have some literacy in science, from a cultural as well as a practical point of view. It thus

seems very important that students have some exposure to science in their education, even if they do not enter a sience-based profession.

In a "democratic" education system where students choose their courses, it is thus necessary to have stimulating, attractive science courses. However, indications are that interest in science courses is increasing only slightly, if at all. A stimulating science curriculum could be achieved if scientists active in basic research took a greater interest in the pre-university education. Hopefully, the educators would not be hostile to such interest, but would instead use it positively.

University Education

Before discussing the place of basic research in undergraduate education, it would be helpful to see if any generalizations can be made about the purpose of a university education. This subject has been widely discussed, but there is not, and should not be, a consensus on it in Canada.

Under the present system, there is a diversity of universities in Canada, each to some extent free to determine its own function. Within each university, there is offered a variety of programs. This diversity of institutions and programs would seem to be in tune with the present heterogeneous, individual-centered society that we have within Canada.

Some generalization could be ventured about education in a specific subject or discipline. Education in a subject would seem to consist of the acquiring by the student of three ingredients: factual information (what), a way of thought or synthesis (how), and motivation (why). When the subject is problem-rather than discipline-oriented, then there is already another answer for "why", besides pure curiosity. It is difficult to see how education can be successful without any one of these three ingredients. The extent to which each ingredient is present and the method of presentation will vary greatly with the level of education, the subject, the institution, the teacher, finances, etc.

The Undergraduate Student and Research

"Research", defined loosely, would seem to be an excellent way of introducing the "how" ingredient into education. This "research" would include laboratory courses if these were not of the strictly-planned, manipulation-teaching variety. Oriented research would also help to introduce the "why" ingredient.

An increased integration of research into the undergraduate curriculum could be very valuable; much of the material now taught can be put into perspective for the student through research activity. If the graduate were then to go into teaching as a profession, he or she would also be better able to handle a more "research-oriented" curriculum.

The above arguments hold also for those obtaining a general education. In the future, students may more often have to change their field in the process of their education. Such flexibility should be encouraged in a rapidly changing world. Although a mass of factual information can be useless when transferred from one discipline to another, a way of thought and of approaching problems is often common to a number of disciplines.

Research and the University Teacher

Science is dynamic; there are always advances and shifting concepts in the disciplines. A professor needs to be aware of the developments in his discipline, and to have intimate knowledge of it. Otherwise he teaches without real authority and becomes simply a regurgitator of books who could be replaced by a book. Intimate knowledge is also needed in a discipline in order to define what is legitimate in the discipline, as courses must be designed and changed as the discipline develops, so that fundamentals rather than trivialities are covered.

The above is true in particular in the case of advanced courses. In the lower years, there is a large amount of "learn-

ing of the alphabet" to be done. In teaching such courses it may well be more important for the teacher to be a good communicator rather than an authority in his subject.

From the point of view of teaching, authority in a subject is nevertheless useless unless it is combined with the ability and the desire to communicate with students. Conversely, the ability to communicate with students is useless unless the professor has qualities which are worth communicating.

It is difficult to see how a professor can maintain authority in his subject for more than a few years unless he is active in research, or active as a scholar in a research atmosphere. It is more likely the research atmosphere itself, rather than any research activity of an individual professor, which is of real importance in providing a good scene for education.

Stereotypes of the "perfect professor" should no doubt be avoided, as there is room for a variety of talents and interests at any university. There should be flexibility present so that each faculty member's talents can be used most effectively.

Conflicts Between Teaching and Research

Even though research and teaching are complementary, they are often in conflict. It is not so unusual for a professor with teaching commitments to spend so much of his time doing and publicizing his personal research that he neglects his teaching. The rush for publication seems to stem to a large degree from the feeling that promotion at universities is judged on the basis of publications only. It is no doubt difficult to evaluate the real contribution of a professor to a university. To try to avoid making such qualitative judgments, quantitative judgments are at times substituted. The most obvious one is the quantity of publications; another the amount of research support.

The above situation is, of course, not general to all universities. In many cases genuine qualitative evaluation is attempted, including teaching evaluation with student involvement. There seems to be a feeling, however, that in some cases only lip service is paid to rewarding teaching–more properly education of students–and that the only effective criterion for advancement is the quantity of publications.

An overemphasis of research can, in this way, detract from the quality of education. Below are two views given by Americans regarding the situation in the U.S.A. It is obvious that there is not widespread agreement there on what is the correct balance and interaction between research and teaching.

"To fail to see the connection between campus revolt, talk about publish or perish, the student search for relevance, and the problems of financing scientific research and development is simply to have one's eyes shut. . . Has the time come to flatly and frankly admit that the great emphasis on research in recent years has harmed the quality of teaching?"[39]

"...research is alleged to result in the victimization of students by a faculty that doesn't care about the undergraduates. Actually, the situation is quite the opposite, at least for the sciences. Teaching is better today than it has ever been, and is best where research thrives. Although student unrest has upset many campuses, the cause of the trouble is not too much research. The difficulty may lie, in part, in having too little research and too few students personally engaged in discovery."[40]

It must be recognized that any change in the level of basic research in universities will have an effect on undergraduate education. If research activity is too low, then the quality of education will suffer; if research activity, or the prestige of research, is too high, then the quality of education will again suffer.

It is in the end the responsibility of

the universities themselves to guard against excesses. Universities could, and perhaps should, monitor the quality of research in their institutions. They also have the right to refuse to give faculty members the necessary indirect or direct support for research if such research seems detrimental to the interests of the university.

Free, Oriented or Applied

When thinking of research as a component of education, it is the activity itself, rather than the results of the activity, which is of importance. The contributions of the results of the activity will be discussed later.

Applied research, which often has problems of proprietary rights associated with it, would not seem to be a suitable type of activity in which undergraduates should take part. The performance of such research with involvement of undergraduates will also not be very effective. Thus, if the applied research leading to a commercial innovation is really worth pursuing, then it should not be performed at universities for the sake of education in general, but in industry, close to the point of innovation. This does not mean that no applied research should be present in universities. Applied research may be quite suitable in the case of graduate studies, especially in engineering.

Basic research would however seem to be a more proper type of activity from the point of view of education *per se*. Some of the "why's" of a discipline could be answered more effectively if such basic research were oriented. If only free basic research is present in a discipline, then the discipline may seem to some students too much like a closed, self-perpetuating system with no relevance to present problems.

Another advantage of the inclusion of oriented basic research is the contact it necessitates, between professors and students, and institutions outside the universities. This contact seems to be a very necessary, but presently poorly developed, part of university research and education. Working toward the solution of present-day problems also often requires group effort, involving not only many disciplines but sometimes several institutions. Oriented basic research could thus be helpful in developing communication between people in different disciplines.

A base of free basic research should nevertheless be present, as what is relevant today may be irrelevant tomorrow, and vice versa. Too much dependence on applied and oriented basic research funds may also make it difficult for a university to maintain its role as an institution dedicated to having complete freedom of enquiry and acting as a critic of society. Under the present system, each individual university can to an extent decide whether it puts more emphasis on its role as producer of trained manpower, or on its role as detached critic of society. With a large number of universities, there should be room in Canada for a spectrum of emphases.

III.6 Relations with Graduate Studies

Degrees and Motivations

A Ph.D., i.e., a "Doctor of Philosophy", seems to conjure up two very different caricatures. On one hand it is a widely-read, thoughtful person, who has a critical, questioning mind searching for the deepest generalities, and who is able to formulate and solve problems. On the other hand, it can be someone who is a narrowly-trained, inflexible specialist, with a "myopic view of the Universe".[44]

Although it is difficult to pin down the motivation for entering Ph.D. studies, most students would seem to enter for economic reasons. Some, no doubt, enter because of an initial curiosity or a human concern; some just because the program exists; others because they feel it intellectually disreputable to terminate their studies at the bachelor's or master's level.

Many of Canada's best undergraduates go on to graduate work in other countries, and about half the graduate students in science and engineering in Canadian universities have been attracted from outside Canada.[11]

The other widespread graduate degree, the master's degree, is considered by some as a "flunked Ph.D."; by others as the first level at which "a complete scientist" can be produced. Anyone with competence should, in this second view, be encouraged to proceed to the master's degree, instead of stopping with the bachelor's. No alternative graduate degrees seem as yet to have been adopted by science and engineering departments.

The Program

Whether the Ph.D. is to be a deep-thinking, probing generalist or a highly trained professional specialist, participation in research would be an excellent method of training such a person. In this case, the activity in itself is important, but the net result of the activity should also be considered. Any research that a Ph.D. candidate does should be of a high quality, as such work carried out during graduate studies forms, to a large degree, the standards he will apply to his work for the remainder of his career.

Overemphasis of publishable research in the Ph.D. program can nevertheless be detrimental. It can turn Ph.D. "studies" into a race to produce something publishable, and deflect the student's interest from scholarship and a genuine understanding of the fundamentals in his own and related disciplines.

The thesis research need not necessarily be free basic research. It is certainly easier to design a Ph.D. problem if the research is free; it also allows the student to pursue his project with no concern as to whether the project is drifting away from the relevance of a mission. This freedom, on the other hand, can result in a frivolity which can be a liability in later work. Even though the professor has free research funds, this freedom is not passed on to the student if the thesis problem is designed to fit into the overall program of the supervising professor, or narrowed excessively in order to produce a publication.

There are some good reasons for projects being in oriented basic research or even applied research, especially in engineering. For one thing it is, overall, more likely–at least in the short run–that the results will be of some help in solving the problems of a mission. New knowledge generated by research is more likely to be useful if there seems to be some demand for the results. It may also make the transition of the Ph.D. to future industrial or government employment easier, by avoiding an attitude mismatch. The origin of this mismatch is described by Schiff[42] in the following terms:

"We science professors are a bunch of inbred snobs! We make sure that nothing we teach is contaminated by the outside world. This is because few of us have ever been inside an industrial laboratory. We therefore cover our ignorance by assuming the self-righteous attitude that only 'pure' science is respectable."

The research for a Ph.D. degree could even be performed in industrial or government laboratories if the equipment were available there. Such research could still be under supervision from the university, and contact with the university atmosphere could be kept through other means.

The Product

The impression is that the potential industrial employers of Ph.D.s generally consider the product to be a hardworking, well-trained specialist, who is, however, generally unable to work in groups, unwilling to change specialty, lacking in entrepreneurial spirit, and naive, "like boy scouts going into a crap game".[43] Some industries thus prefer a bachelor graduate with some years of on-the-job experience to a Ph.D. On the other hand,

the newly emergent Ph.D.s often claim that industry looks only for specialists, does not give them an opportunity to show their flexibility, and does not appreciate the importance of their potential contributions to industry's effort.

From the point of view of the universities, the Ph.D. is probably still the best kind of training for an academic career, although other types of backgrounds should not be excluded. The Ph.D. has even, to a large extent, become simply a "union card" demanded by universities and government as a proof of specialized training, perserverance, and perhaps even native intelligence.

Utilization of Highly Trained Manpower

In Canada at the moment there seems to be an "oversupply" of Ph.D.s in some fields, and an undersupply in others. The actual oversupply situation[44] seems to be worse now than even the most pessimistic forecasts of the NRC suggested.[11] The argument can also be approached from the view that there is an "underutilization" of highly educated people. In one sense, there can by definition be no oversupply of highly "educated" manpower, even though there can be an oversupply of highly "trained" manpower.

The public pays a considerable sum for the training of Ph.D.s. Thus, even though governments would not seem to be obliged to guarantee employment to highly trained people, it would be absurd to waste such manpower. Underutilized manpower is, of course, not wasted if it can only be utilized as a part of a "consuming" activity. Besides being a possible economic waste, such underutilization also causes much distress to the individuals involved, whether construction workers or Ph.D.s. Some supply-demand mismatch may nevertheless be desirable for cross-fertilization and communication between fields. The impediments to changing fields at the Ph.D. level seem, however, to be too great for individuals to overcome without some aid.

There are possibly too many people proceeding to the Ph.D. degree in some fields of the natural sciences. A great emphasis on the quantity of research publications as a criterion for excellence and for career advancement at many universities motivates professors to increase their research groups and accept students for a Ph.D. degree even though the students are not always of high quality. Such mediocre Ph.D. students tend to serve the purpose of the research supervisor and the university as research and teaching assistants. They undergo only what is more properly described as an apprenticeship as technicians and do not develop the originality and flexibility that well-educated Ph.D.s should have.

Such a system may be self-regulating in the case of Canadian students, who are better aware of employment opportunities here. It may not be self-regulatory in the case of students from outside Canada, who have been actively recruited by departments. The following quotation[45] illustrates the problem:

"...the problem of finding employment for graduates with senior degrees outside of universities now confronts us squarely.

"This brings up a point. In the January issue of *Chemistry in Britain*, a copy picked at random, there are twenty-nine ads for academic posts. Ten of these are from chemistry departments of Canadian universities, inviting students to apply for fellowships which would enable them to pursue graduate studies. I don't know in what other journals across the world these ads are repeated, but the question arises–why are we doing it? If our aim is to train students from developing nations so that they can return to their homelands to make use of their newly acquired skills, this is fine. We do not do nearly enough to enable the nations less fortunate than ours to learn to help themselves. However, no such argument surely applied to Britain. The real reason

may be that we have built a capacity in the way of graduate schools well beyond our needs which now, like the proverbial monster, must be fed."

Future supply-demand mismatches could be lessened if it would be possible to make available even crude estimates of future manpower demands. Students and universities could then be informed of what to expect and make field choices on the basis of this information if they wish, although there would still be a lag of three or four years present in any feedback. The information could be obtained by a part of a "clearing house" for scientific manpower. Such a clearing house could also have a register of available manpower; the casual (often "old-boy") system of recruitment of professionals seems inappropriate in the 1970s.

A type of quota system could be introduced if, as suggested by the Macdonald report[46], graduate student support through research grants be terminated unless the services of the student are essential to the performance of the research. Student numbers could thus be more directly controlled by scholarships. Such regulations may, however, be difficult to apply in practice.

III.7 Basic Research in the Functions of Industry and Government

Why Basic Research in Industrial and Government Laboratories?

The reasons for performing basic research in an industrial or government laboratory consist of the activity itself as well as the particular results of the activity, just as in the cases of the previous discussions. The results are only a small fraction going into a world pool which is available to everyone; the benefits from the activity itself are localized.

One reason for having basic research present is to attract good but industrially inexperienced personnel from the universities, and to aid in the transition from an academic to an industrial environment.

Activity in basic research is also important for a "coupling effect" with the outside.[47] In order to take advantage of what is learned elsewhere, it is necessary to have people who can evaluate the literature critically. Basic research activity thus serves as a communications link with the outside; applied research cannot perform this function as well because of proprietary considerations. It must be remembered that about 97 per cent of the world's knowledge is produced outside Canada. Much of this is as likely to be of interest to a Canadian industry or a Canadian government agency as the three per cent or so of locally-produced knowledge.

Yet another reason for the presence of basic research in some industrial and government laboratories has been referred to as the "golf pro" effect, whereby basic research helps set the "tone and standard" of other research activity.[48] This should not be interpreted as meaning that there is any inherent superiority about basic research or basic researchers. Nevertheless, the basic researcher must maintain a conceptual self-discipline because of submission of his work to outside criticism during publication; applied researchers can at times get by with technological empiricism, and do not always have the salutary experience of publishing results openly.

The results of basic research can, of course, result in a breakthrough, but this "high risk" investment aspect is often of small importance.[48] Many industries nevertheless do support basic research, as a high-risk investment, in the hope that they will benefit from some major breakthrough. However, the results are more likely to be of benefit through pointing out what can or cannot be done, and thus aiding in the choosing of new projects.

Those scientists active in basic research also provide an important internal source of consultation for applied work, a competence for evaluation of contracts and grants, and a reserve pool which can be

marshalled in times of crisis. Only seldom are such scientists active exclusively in basic research.

Most, if not all, of such basic research in industry is oriented basic research. Beuche[49] points out that most scientists in industrial laboratories are very anxious to see something practical and useful result from their work. Thus it is quite possible to rely upon the scientist in the laboratory to "orient" himself and thus, essentially, to work with little or no direction. In a skillfully managed industrial laboratory, the amount of basic research done may appear to vary by a considerable factor, depending on whether one canvasses the scientists or their director.

Basic Research in Canadian Industry

In Canadian industry, basic research expenditures have been about 5 per cent of total R & D for the last decade. Nearly all of this basic research is in the manufacturing sector, with drugs and paper accounting for nearly half the total.

A quantitative analysis in the U.K.[50] gave basic research an optimal percentage of 10 to 15. The same figure has been suggested by several people in the U.S.A. Such "optimums" vary, of course, with the type, size and philosophy of the company, as well as with the effective definition.

There can be various reasons why the level of basic research in Canadian industry is lower than the above "optimal" figures. To profit from basic research, the industry must be large enough to support a critical size of scientific community. In Canada, there are few giant technology-based companies which can reach this critical size. In the case of subsidiaries, the critical mass which can effectively use basic research is often achieved only in the laboratories of the parent companies.

Then again, the 10 to 15 per cent may be optimal only in some special industries. In 1967 in Canada, basic research in paper and drugs was well above 10 per cent of total R & D. In food, rubber, primary ferrous metals, non-metallic mineral products, and scientific instruments, the figure was about 10 per cent. However, in the aircraft and electrical products industries, which accounted for 42.4 per cent of all R & D in industry, only 0.8 per cent of this R & D was basic research.

In the U.S.A., contract funding of oriented basic research by the government would seem to inflate the percentage of basic research performed in industry. Nevertheless, only 3.9 per cent of industrial R & D in the U.S.A. in 1970 was expected to be basic research.[6] In the U.K., the corresponding figure was 4.2 per cent in 1968[7], and in Sweden, in 1964, less than one per cent.[51] In comparison, the basic research effort by Canadian industry seems quite normal, or even high.

What about research as a whole in Canadian industry? In Canada over the last few years, R & D in industry has been effectively constant (Table 7), despite federal incentive programs for research. The reasons for the incentives probably reside in the faith that an increase in the extent of research undertaken in a country will automatically produce economic growth. This faith may however be misplaced. The expenditure on R & D in the U.K. has been greater than in all countries except the U.S.A. and the U.S.S.R. (Table 6), but the economic growth has been very small. Langrish[52] gives two possible explanations for this:

"It has therefore been assumed that Britain is not very good at using science, and that it has allowed other countries to make off with British scientific discoveries and exploit them elsewhere. The alternative possibility–that scientific discoveries do not really contribute to economic advance except in exceptional circumstances–has not been seriously considered."

There would at the moment seem to be no valid reason for an additional stimulation of basic research in industry above the level considered by industrial managers to be most favourable–nor of

R & D itself for that matter. Progressive, well-managed, technology-based companies will use R & D, and basic research, to an optimal degree.

It seems altogether unwise to make any standard formulations. Each company or government agency (or university) should know its missions and purposes, and then itself decide on the best way of furthering them. Thus the managers (as well as the universities) should not be restricted by guidelines drawn by "centralized bureaucrats".

Good management may not exist in all industries. In that case, a more logical approach would be to increase the effectiveness of management through education and research into management, rather than trying to manage R & D *for* industry by means of more incentive programs.

Basic Research in Government

Some people question the necessity for any intramural government research and development. There are nevertheless several reasons why it has become a substantial component of the total Canadian effort (Table 7). The scientific projects which the government has decided to undertake in the national interest may have been too large for universities or industry to undertake. In other cases, universities and industry have not been, or may not be, interested in pursuing such missions. There may also have been a reluctance to have anything except essentially free basic research in the universities. The government has thus set about undertaking such missions "in-house", by setting up its own laboratories. Basic research would constitute part of this research effort for the reasons discussed previously.

In 1968, 19 per cent of the R & D effort in government was devoted to basic research (Table 7). Some people claim that in the light of the 10 to 15 per cent rule of thumb discussed previously this is excessive. As in the case of basic research in industry, it may be dangerous to quote general rules of thumb which might eventually become hard and fast rules, especially when ill-defined activities like basic research are involved. The amount of basic research obviously varies with the project and with the stage of the project.

Weinberg's quotation[53] may nevertheless apply to the Canadian as well as the U.S. scene:

"Yet, though this trend from missions (or projects) to basic research can be discerned in many federally-supported laboratories as their original missions lose their focus, I believe the national interest is not served by allowing the mission-oriented laboratories to lose their mission orientation as they grow older."

According to the Treasury Board of Canada[54], any basic research which no longer contributes to government missions will not be supported for its own sake.

Numerous suggestions have been made that basic research of interest to government missions should be done in universities or in industry. The argument is that in universities basic research serves the function of education, and that in industry it indirectly raises the level of scientific capability and hence productivity in the industry, with the possibility of foreign sales of expertise gained. In government, it often remains more isolated and produces fewer indirect benefits. Contracting work out may also make it easier to initiate and, in any case, to terminate projects.

Claims are also made that research in industry is more effective overall because of easier evaluation of accountability, which prevents the buildup of self-perpetuating research empires. In universities the research is under the stimulation of students, and of frequent criticisms by peers when grants or contracts are up for renewal. Review of intramural government basic research programs by outside peers may be one way to ensure that the quality of intramural work is high.

These "two birds with one stone" arguments sound convincing, but there must be a limit to the extramural work. Some competent basic researchers should be doing intramural work so as to have the competence to advise on research contracts or grants. The government would also seem to have responsibilities, other than "missions", which may require intramural basic research activity: they must have the knowledge to advise politicians regarding scientific matters, to carry out regulatory or standardization functions, to advise on international programs involving basic research, and to have background knowledge available for emergencies.

The Usefulness of University Research to Government and Industry

Free basic research performed in a Canadian university and published in the open literature is of no more value *per se* to Canadian industry or government than it is to industry in other countries. It is actually far more likely to benefit U.S., Japanese, German, etc. industry, because Canada has a relatively small capability for exploiting new knowledge to the point of innovation. The same arguments apply to the usefulness to Canadian industry of basic research publications from the government laboratories. Such basic research could even be detrimental to Canadian technological progress, by depriving applied science of the prestige necessary to attract able and ambitious minds.[55]

The expertise gained through basic research can be of considerable value if personal contact can be achieved between basic researchers and the potential users of the expertise. A seeming lack of understanding and respect among the three sectors in the Canadian scientific community hinders this. OECD describes the situation as follows:

"Wherever we went, all over the country, we heard complaints from the academics about the naivete of industrialists and their failure to appreciate research, and from the industrialists about the excessive academicism of the universities."[9]

Many steps can be taken to stimulate the flow of ideas and people among the sectors in the scientific community: exchanges of personnel, cooperative research institutes, the use of government and industry personnel as lecturers, allowing students to do thesis work outside universities, contracting-out of research, more contact through learned societies, publications giving brief descriptions of current problems and advances, etc. Considerable efforts have been made by various groups to stimulate such interaction, but much more could be done yet.

The learned societies can have a crucial part in such communication. Although Canadian societies have not been as successful in effecting this "homogenization" as, for example, the American Chemical Society, the Canadian societies seem to becoming increasingly active, as exemplified by the formation of SCITEC. It is doubtless more difficult to have strong scientific societies in Canada, when there are great advantages for Canadian scientists in joining and participating in the activities of American societies.

Universities must not go overboard in making research programs of more potential use to government and industry. There must be enough free basic research activity present so that the university remains a reservoir of knowledge and of independent intelligence. It is naive to think that the foresight exists today to determine what will be of importance decades from now. Thus, enough support must be given to free basic research to maintain up-to-date knowledge in the whole spectrum of fields. The whole spectrum of knowledge need not, of course, be present in every university with uniform emphasis.

However, if the level of support for free basic research is too high, it will satiate the desire or the ability of the universities to do research. The consequences would be little oriented and applied research, or a neglect of teaching,

or a pressure on the provinces for university growth.

III.8 Culture and Prestige

Basic Research as Culture

Public support of free basic research could be interpreted as support of science for its own sake, i.e. for its intellectual value. The public has more probably given much of this support because of the great material benefits (and in spite of some grave dangers) that the pursuit of science for its own sake has brought–and no doubt will continue to bring–to mankind. Nevertheless, the furthering of science for its own sake, i.e. the increase in knowledge of man and the universe, is definitely considered by many to be "one of man's crowning cultural achievements".[3]

It would seem that scientists have advanced science for its own sake, either because they did not need public support to carry out their work, or else by "hinting" of the potential usefulness of their work to material or military goals.[56]

These days, arguments based on unknown future benefits accruing to mankind through the free, undirected advance of science are, at times, cast into disrepute. Segments of the public seem to fear new knowledge in some fields. Even though science itself is morally impartial, there is a fear of humans coming into control of the power that new scientific knowledge gives. Such new power has often been used unwisely in the past. Is there any guarantee it will be used more wisely in the future?

In spite of apathy toward, or even resentment of, the natural sciences among some students, there does nevertheless seem to be a considerable and growing interest in knowledge, genuinely for its own sake, on the part of some of the public–as evidenced by attendances at science museums, planetaria, etc. Such interest would be far greater if scientists could communicate better with the public and transmit to the people the exhilaration that the discovery of new, timeless knowledge can bring. Perhaps basic researchers have been too hesitant in attempting to communicate with the public and share with it the excitement of their activity. Weisskopf[57] gives the following rebuttal to the argument that the layman cannot comprehend science:

"If you cannot explain science in simple terms to the layman, you have not understood it yourself".

It is obviously important today to consider the attitudes of the public and the political representatives, if a "cultural" argument for supporting basic research is to be valid in a democracy. Government is totally dependent upon public funds, and universities to a very great extent. Although in the past the public and politicians have not really questioned the desirability of increases in science, times have changed, especially in the recent government austerity years.

One of the major functions of universities is to find new knowledge *per se*. Instead of considering such basic research to be a means for achieving a better life, the public could become disenchanted with it and force it to become a very secondary function of the universities. Any creation of new knowledge would then be supported only as a by-product of the teaching function. Unless the public feels that basic research contributes to its more immediate culture, as well as to the culture of the international scientific community, it may well prefer public funds for culture to be spent on parks, arts centres, Hockey Canada or the CBC, instead of on basic research.

Private funds, whether of some patron or of the researcher himself, could be used more extensively for pursuing free basic reeasrch if the pursuit is primarily in the cultural interests of a limited group. Many of the great contributions of basic research were never supported by any public agency.

International Commitments and Prestige

Canada is one of the richest nations in the world. We thus would seem to have an obligation to contribute in considerable proportion to the world's scientific knowledge through support of basic research. Some of this contribution can be made through oriented basic research, some through free basic research, and some through international cooperative missions.

Studies involving the ecology of natural and human systems will no doubt become increasingly important. Many such studies should be undertaken on a global or international scale. As a relatively rich country, Canada should lead in such international developments.

The prestige of a nation is to some degree predicated on her contributions of basic knowledge, although such prestige is, to a large extent, restricted to the scientific community. National prestige can perhaps more effectively be obtained through high quality exports, pavilions at Osaka, or an international hockey championship.

Prestigious basic research groups are nevertheless an aid in opening international doors and in indicating a high back-up capability for technology. Such groups, whether free or oriented, in whatever sector, should be preserved. It is a slow, difficult process to build up a research group with international prestige; it is all too easy to destroy such a group. Some such groups seem to be afraid of just such destruction, however, under present conditions.

Basic research is also of use as a type of international glue. Because of the lack of proprietary rights, and the universality of the results, it is often easier for basic researchers to communicate openly across national boundaries than it is for politicians or businessmen. Such communication can lead to better general understanding and tolerance between countries.

Big Science

Basic research in the form of "Big Science" has many attractions that at times can outweigh the costs. It requires cooperation among the whole spectrum of the scientific and technological community; it stretches the capability of technology to higher levels of performance; it invites a sense of public participation and confidence as a nation; it raises national prestige and demonstrates a high level of technological and scientific competence; it develops technical management.

Feasibility studies are an important phase of "Big Science" projects. They require highly organized, expensive investments, and involve already the interaction of a spectrum of people. Only a few organizations in Canada are capable of carrying a "Big Science" project through the feasibility stage. Universities lack the structure and, in most cases, the size for the preparation of such proposals. There is, moreover, a lack of opportunity for organizations to get funds for feasibility studies of large projects.

There must also be commitment to carry through a project. This is a political question. The only agency in Canada large enough to fund any "Big Science" project is the federal government. The probability of commitment increases with the unanimity of endorsement by the scientific community. This represents agreement on the internal criteria of merit for the project.

Evaluations of the external criteria of "Big Science" proposals containing relatively large amounts of basic research have been made in recent years in Canada–e.g., the Intense Neutron Generator (ING)[58], the CARSO telescope[59], and involvement in the Batavia accelerator.[59] The criteria for these evaluations were not explicitly stated even though such criteria were present. A set of criteria for such evaluations has been suggested by Weinberg[53], and has been systematically applied to the CERN 300 Gev. accelerator. In anticipation of a growing number of "Big Science" proposals, it may be helpful to develop criteria for the Canadian context so that the proposals would get fair evaluation.

What is the future for Basic Research?

III.9 Designing Change

Defining Problems

This report suggests "problems" that exist in the area of basic research activity in Canada. "Problems" come into existence only when defined–and what is a "problem" to some is not necessarily a "problem" to others. Such "problems", i.e. aspects of basic research which are detrimental or inefficient, are in the end based on the value judgments which define "detrimental", "inefficient", and other such words.

What right then has anyone to say: "this is an area where a problem exists and where change is desirable"? In this study, the mandate for defining problems is an intensive study, over an eight-month period, as described in Part II. Reliance on the direct personal experiences of a single individual in defining problems is often dangerous rather than desirable. An isolated personal experience does not at all justify generalizing that experience to make judgements in a broad area. It may be just as probable that the experience is of an exceptional, rather than of a normal, nature.

The credibility of the views obtained through seminars, conferences, etc. must nevertheless be gauged, and the "problems" then defined accordingly. If there were consensus in all cases, then it would not be as necessary to base the definition of problems on value-judgements. But in this study, there was often a lack of consensus, especially on points which could not be resolved by reference to statistical data or experimentation.

Thus in nearly every case there is room for argument as to whether a problem even exists. In some cases, the definition of a problem is so obviously a matter for each individual or institution to decide, that at best one can only suggest that the problem could be present in some cases. It is then up to the individual or institution to initiate self-examination to decide whether there is a problem, and whether something should be done about it.

Solutions to the Problems

"Americans and Canadians are inclined to think that whenever there is a problem it can be solved".[61]

Once the "problems" have been defined, the next step is to look for "solutions", or at least ameliorations. Such "solutions" involve changes. The question then becomes "changes for the benefit of whom?" With every change, some will benefit more, and others less; some may even lose. Bringing in a time scale complicates the matter still further.

As this is a report for the Science Council of Canada, the answer would be: "the nation should benefit." Benefit to the nation can be equated to the furthering of the national goals of Canada (III.10). These goals would then be the foundation for defining "benefit".

The national goals suggested by the Science Council[3] were: health, education, freedom, security and unity, leisure and personal development, world peace, and environmental improvement.[6] These "God and Motherhood"-type goals seem beyond reproach and acceptable to most people. There is, however, no weighting indicating the relative importance of the goals. All the goals are most certainly not advanced to an equal degree by any one decision in the allocation of resources, and often one goal is advanced at the expense of another. Such a set of quite general goals with no indication of priorities is thus of little aid in the selection of problems and the formulation of many of the solutions.

In many cases recommendations of solutions are obviously political, in the sense that they are based on value-judgements. In the case of such recommendations, where politics are involved, the politicians should be supplied with alternative recommendations, with the most probable consequences of each outlined. Alternatively, the politicians could provide a reasonably specific set of weighted goals to be used as the basis

of formulating recommendations. In cases where the problem is one of management, with little political input, then specific recommendations can be made.

Limitations of Change

Some people feel that "change for the sake of change itself" is desirable. It keeps organizations flexible, used to change, and thus better able to follow changes in society as a whole. This seems especially true for science-based organizations, as science is advancing and changing especially rapidly. An excess of change can however have the results described in a quotation attributed to one Petronium Arbiter, living in 66 A.D.:

"We trained hard–but it seemed that every time we were beginning to form up into teams we would be reorganized. I was to learn that later in life we tend to meet any new situation by reorganizing and a wonderful method it can be for creating the illusion of progress while producing confusion, inefficiency, and demoralization."

Drastic, step-function changes lead in a great many cases to inefficiencies. Also, it is often not possible to be certain that a change will produce a solution. Thus "pilot plant" changes are in many cases desirable. These changes must then be monitored continuously to see if the change is an effective solution.

Realistic recommendations for change must also be based on the present overall organizational structure in Canada, even though it is not necessarily an ideal structure with regard to management of basic research. Some such structures could be changed to better accommodate effective basic research management. Although a new Canadian constitution, for example, will not be designed primarily to make management of basic research most effective, this consideration should be taken into account.

Recommendations could be made for changes in organizational structures, but some things would seem to have to be "sacred". Democracy, constitutional rights, free enterprise, and university autonomy would largely fall into such a "sacred" category from the point of view of this report.

The structure into which basic research must fit is particularly complex and difficult to define at this time in Canada. We have a heterogeneous, individualistic society with an organizational structure which seems to be based more on tradition than on rationality. Moreover, this structure may change quite suddenly. The federal government funds research at universities; individual professors have "academic freedom" in universities; universities are "autonomous"; the provinces have exclusive jurisdiction over "education". The federal government also gives incentives for research in industry; it operates laboratories in support of industry; it is involved directly in industry; industry operates in a "free enterprise" system; industries are often controlled by provincial regulations; much of industry is controlled from outside Canada.

This section will probably leave the reader with a feeling of frustration–and well it might. It is certainly not easy to design a reasonable policy for a multifaceted activity such as basic research in an organizational structure which is vague, and which may change in unknown directions.

III.10 Futures for Basic Research

National Goals

The Science Council of Canada started its development of science policy by seeking to further the "national goals" of Canada.[3] Even though there are also individual, family, community, regional and global goals, as well as goals of agencies, businesses, groups, parties, etc., the Science Council should nevertheless be mostly concerned about national goals, because it is a "national" body. Many problems in science and technology, especially those concerning basic research, can nevertheless be discussed

effectively only in a global context.

Introducing the concept of national goals does not mean that individual, regional, etc., goals are not valid and should be suppressed. On the contrary, a national goal of Canada would certainly seem to be the preservation, and maybe even the increase, of the amount of freedom for individuals and groups. At the same time, national goals may have global characteristics, such as a national goal of world peace and aid to less fortunate nations. Another national goal may be to increase our knowledge about the world per se, i.e., to support basic research for its own sake.

Quite obviously, such goals and their priorities should, in a democracy, be expressed by the citizens of the nation, in the form both of individual decisions and of the decisions of the elected representatives. In the absence of clearly expressed goals on which to base science policy, the Science Council suggested a set of goals.[3, 62] There are, no doubt, other sets of national goals possible. In fact, there is no consensus that it is even desirable to think normatively about the future and introduce such notions as "goals".

Such goals are of course dynamic; the goals change and priorities change. Thus, although short-term goals could and perhaps should be specific, care must be taken that the long-term goals are such that they do not hinder changes in them with time. New goals can be set up by advances in science and technology; others can be realized by such advances. Yet others, however, cannot be realized because of the past influence of science and technology on society.

Basic Research in the Achievement of National Goals

Let us get back to the question of what basic research has to contribute to the future. Science is neither national nor predictable. It is thus not possible at this point to link basic research with specific Canadian programs or goals; one can only indicate the general contributions that it might make towards Canada's future.

Some more obvious contributions that wisely-managed basic research could make towards national goals would read as summarized below. Some of these contributions are, of course, not exclusive to basic research. Sections III.5 to III.8 give a more detailed discussion.

1. Basic research activity can help to stimulate a probing, truth-seeking atmosphere in the educational institutions.
2. Basic research activity can be an excellent way of "training students in the art of training themselves" and producing people with the most up-to-date knowledge in a field, with the ability to attack problems, and with reflective, searching minds.
3. Basic research activity can be an aid to achieving meaningful international communication, as it is based on objective, open information.
4. The results of basic research may lead to new innovations of great benefit to society, including some which clean up problems caused by unwise use of technology in the past; they may also prevent the introduction of innovations which may eventually be harmful to society.
5. The results of basic research can open up new horizons in the minds of people and extend their experiences to beyond the present environment, thus often creating a richer life, and even new goals for society.

Canada must, of course, be active in basic research if it is to benefit through the first three contributions listed above. It must also be active in basic research to benefit from the last two, however. Even though the results of basic research are openly communicated internationally, the meaningful use in Canada of the results of basic research performed in other countries requires that some of the related activity be present here.

The above contributions cannot be achieved with basic researchers isolated from the rest of society. Genuine communication between natural scientists, technologists, social scientists, business-

men, politicians, students, etc., as well as the general public, is necessary. Such communication, which results in mutual respect and cooperation, seems to be the most important "node of influence" in making it possible for basic research to contribute to the future of Canada.

Forecasts on Future Trends

As we enter the 1970s, the U.S.A. is in "an R & D depression", and examining its basic research policy very critically. As can be seen from the titles of articles gathered in Part V, many other countries are doing the same. Dedijer[63] finds two alternative explanations for this depression:

"The optimists see in the current U.S. R & D depression a stimulus to larger and more diversified international cooperation in joint R & D projects, and even the beginning of a more rapid development of a world R & D policy.

"The pessimists are inclined to see in the current R & D depression a first sign of the end of the U.S. age of science. The landing of men on the moon, they argue, may be the highest achievement of U.S. civilization, just as the Pyramids, the Parthenon, and the Taj Mahal are symbols of the highest achievements of other civilizations."

The majority of the politicians, planners and managers of today will not be around in a generation's time when the fruits of their labours mature. The long time span between the action and the eventual consequences is especially extended in the case of basic research–a generation is often quoted as being the average delay. It is the youth of today who are most intimately concerned with the future.

Many young people today sense that the world is spurred on by technological innovations at a rate of change faster than what people can adjust to. More people are shaking off the passive mentality where "successive changes are written off in favor of a sort of fatalistic advance towards a technocratic socety".[64] Even the U.S. National Academy of Science [65] is deeply questioning whether all technological innovation is "progress:"

"Even among those who readily concede that technological advance has, on the whole, been a great boon to mankind, there has emerged a deep strain of skepticism towards proposals and projects that, in an earlier day, might have been hailed as the very symbols of human progress. Whereas a few years ago, for example, the idea of a supersonic transport seemed to many the obvious fulfilment of man's airborne destiny, today some who might once have greeted the SST with unbounded enthusiasm are asking whether it is truly a sign of progress to fly from Watts to Harlem in two hours, vibrating millions of ears and windows in between."

Science invariably gets mixed up with technology, to a large extent because basic researchers have used technological advance as a justification for public support of their work (III.8). For centuries, science has had an enviable reputation, being at times on the level of a religion which was thought to be the only hope of mankind. Because of the tragic consequences of some technological advances, however, science has come to seem to many, especially among the young, to be necessarily inhuman, regimenting, and even diabolical.[66]

The above ideas apply more to the U.S.A., and probably not very generally to Canada today. But the probability that they will apply in a few years is fairly high. The overflow of U.S. news and ideas will influence people, and in striving to catch up to the technological level of the U.S.A., Canada may blunder into the same problems.

No doubt the only solution to some of those ills of today which are caused by misused technology is new, well-utilized technology, much of which can be achieved through advances in basic research. The credibility gap, between scientists and

technologists on the one hand, and the scientific community and the public–especially youth–on the other hand, must be kept to a minimum in Canada. If the scientific community cries "wolf!" (more accurately, "more funds for science and technology!") too often when not really justified, then it may not be listened to when really justified. The public may then insist that "no" technology (and no basic research), instead of "new" technology, be the cure to bad technology–even though "no" technology is not the answer.

What is the best way of managing Basic Research?

III.11 Where to Manage and Where not to

Cases For and Against "Management"

Just the words "management", "planning" or "direction", used in conjunction with basic research, seem to strike fear in the hearts of many people involved in this activity. The fear of planning, organizing or coordinating basic research is epitomized in the views held by Steacie[17]:

"To him science was a scholarly pursuit and the scientist a creative individual. He could accept no image of science that did not leave to the scientist his independent initiative. From these beliefs can be traced those strong oppositions which run through the speeches: his aversion to all attempts to plan, organize, or coordinate science, his objection to professionalism, and his abhorrence of secrecy. To him these concepts struck at the very roots of a vital science and were to be tolerated only as recognized evils."

It is obvious that no "centralized bureaucrat" or science administrator can direct a scientist to discover some new law of nature. The very definition of basic research makes this impossible. Wise management of basic research will carefully guard the independent initiative of the scientist. But a "hands-off" policy is even in itself a form of overall management. Individual scientists also seem increasingly to be working themselves into narrow specialties (III.6); in a sense, "managing" themselves increasingly restrictively.

For the purpose of getting clearer views of some of the problems of management, it is probably best to think of the whole spectrum of decision-making as being on three levels: "political", "managerial" and "scientific". At the "political" level, decisions are based on value-judgements, and concern the social, cultural and economic goals that the nation or some organization wishes to pursue. At the "managerial" level, decisions are made as to how the goals set at the "political" level can most effectively be achieved. At the "scientific" level, the actual work is done. These levels most often do not fall into sharp categories, but blend into each other. The type of management that should be present at the different levels varies considerably.

Management at the "Scientific" Level

At the "scientific" level, it is almost axiomatic that the basic researcher should have the freedom to decide on the details of pursuing his research problem. This has been expressed by Herzberg as follows[67]:

"It is only the working scientist, immersed in all the aspects of a given subject, who can pick the right and feasible problems that are relevant and may be worth doing. The bright ideas, the hunches, arise in a person's mind when he has struggled, sometimes for years, with a given subject. They don't occur to an administrator or a committee trying to direct the scientific activities of a country."

How far down the line should such freedom of individual initiative go, though? Should it go right down to the graduate student doing his research thesis? Surely it should, near the end of his studies, if the project is one in "little

science". But in cases where group effort is involved, such as "Big Science", not only graduate students but also working scientists must temper some of their individual freedom if the effort is to be effective.

At this "scientific" level there would seem to be no essential difference in basic research activity between free and oriented research. Oriented basic research can, for example, be effected at this level simply by choosing researchers with an appropriate field of interest, and by keeping them informed as to what could be important for furthering the mission.

Free basic research should not be affected even by management practises such as the above. There should be no "management" of the individual scientist up to the top of the "scientific" level, where peer judgement is made of the excellence of the researcher and his work. Above this there is a scientific-political level, where the amount of funds available for free basic research in a field is decided upon, and at the very top is the political decision of how much of the resources should be available for free research overall.

Management at the "Managerial" level

In the case of oriented basic research in government and industry, there could be several managerial levels between the scientific and the political levels. Decisions regarding the research must here be made using both the internal (scientific) criteria and the external (relevance) criteria. The upper levels would be of a "managerial-political" nature, where decisions must be made regarding the problems to be solved, culminating in the political level, where broad policy decisions are made.

The effectiveness of particular managerial systems for directing basic research in industrial and government laboratories is a topic which has received considerable study[68], and will not be discussed here. The methods used are, of course, decided on by the individual research managers involved, and vary with the sector of performance, the type of problem that is to be solved, traditions, the individuals involved, etc. In an increasing number of cases, an interdisciplinary approach is necessary. This often produces new and complex management problems, especially when social science and political inputs are included.

In the case of universities, the availability of funds can certainly be an effective method of managing oriented basic research (III.12). It should also be possible to make basic research more effective in solving existing technological problems if researchers are made more aware of the problems which exist.

Such awareness could be brought about through a concise newsletter circulated to the scientific community, pointing out problems about which new knowledge is required by government departments and perhaps even industry. Mission-oriented agencies could also hold meetings to discuss their programs and problems with past and potential research contractors and other interested parties. Feedback mechanisms in the giving of grants may be yet another method (III.4).

In conclusion of this section, it should be emphasized that in a creative, individual-centred activity such as basic research, no good work can be done by poorly motivated, poorly trained scientists, no matter how good the management. Nor, of course, will any form of management be effective if the people involved in management are not competent. Some things just cannot be improved through changes in management practice; in such cases, only an improvement in the quality of the people involved is effective.

Centres of Strength

The effectiveness of basic research activity can be increased in some areas by a concentration of effort. In order to reach the "critical mass", the further development of "centres of strength" in Canada has been suggested by numerous people.

In this modern day of fast communication and travel, such a centre of strength

need not necessarily be a geographic centre. Also, it should embrace a whole complex of activities–from basic research all the way through to development–in order to make useful interaction more likely. It could also involve all of the sectors–universities, government and industry.

There are numerous examples of centres of strength involving basic research which have been, or are being, built up in Canada: the University of Toronto Institute for Aerospace Studies; the Pulp and Paper Research Institute; the Bedford-Dalhousie Complex for Oceanography; the Canada Centre for Inland Waters; Atomic Energy of Canada Ltd.; the Tri-University Meson Facility; and others. These centres of strength have all arisen in different manners. Some have grown because of an individual or a group who has had ability and conviction, others through accident, and yet others through deliberate planning by government. Each individual case should probably be considered separately, as there seems to be no ideal model.

One suggestion is that government-funded research units be set up initially around people of excellence at universities; government researchers could be full or adjunct members of departments at universities. If such a centre were to achieve excellence, then industry might naturally be attracted to it. There should be periodic reviews of any such centre, to prevent it from perpetuating itself when it is no longer of high quality. An important aspect of such centres would be the communication which they would stimulate between scientists in different sectors.

The geographical and political structure of Canada puts up some impediments to the idea of centres of strength. Increased efficiency in transportation and communications will lessen the geographical impediments. But it is difficult to predict whether the political impediments in the form of inter-provincial, inter-regional, and inter-university jealousy will become greater or smaller.

The concept of "centres of strength" can also be used on a microscopic level. A common complaint against the present evaluation system for NRC grants is that it is too egalitarian; funds are spread over a large number of people, whereas it is probably more effective to produce individual "centres of strength" by complete funding of people of high quality.

III.12 Management by Money

Federal-Provincial Relations Relevant to Basic Research

A provincial government would have an interest in research from the point of view of the effects which it might have on the provincial economy. It would also have an interest in basic research insofar as it might contribute to the maintenance of a high quality of education in the province.

The quality of education in the primary and secondary school systems would seem to be closely dependent upon the quality of education at the universities (III.5). The universities themselves have the freedom and responsibility to determine what constitutes a high quality of education, even though they are supported directly by the provinces to a very large degree.

The federal government gives indirect support to the general operation of the universities[69], but also supplies direct support through grants-in-aid of research, scholarships, etc. In a statement by the Prime Minister[70], the position of the federal government was expressed as follows:

"Nor does the federal government agree that it is precluded from concerning itself with research by reason of the provincial responsibility for 'education' or, alternatively, that it must limit its support according to the subject matter in relation to areas of federal and provincial jurisdiction."

This was not the position of all provinces, however. The position of Quebec

was expressed as follows[71]:

"Education and research are indivisible at the university level. Under the terms of provincial jurisdiction in the education field it must be recognized that university research is also under provincial jurisdiction."

In Report No. 5, the Science Council[14] recommended that federal granting agencies take a more active role in providing guidance to the direction taken by university research. This may result in a threefold planning of university research, with some potential for chaos unless federal, provincial and university planners cooperate.

The present system of federal funding of university research affects provincial priorities in several ways. Too high a level of federal research support may saturate the research capability of the universities and lessen their ability to do research of more immediate benefit to local society. A province then has only weak indirect control in guiding the research in directions of relevance to the province if it should wish to do so.

Federal grants-in-aid do lighten the burden of the universities (and, indirectly, the provinces) when the research in question would have been undertaken in any case–at the complete expense of the universities if there were no federal funding. However, incomplete funding can, if extended to high levels, distort the priorities of provinces and universities.[1] The indirect costs of federally-funded research must be carried by a university, which can either pass this financial burden on to the provincial government or try to bear it by rearranging its priorities. When the provincial government agrees to carry the costs, then its priorities are affected.

Even should the present method of federal funding be interpreted by some as being unconstitutional, there are nevertheless very good arguments for federal involvement in basic research at universities. The product of much of this research is highly trained manpower, an expensive and very mobile commodity.[46] Thus the planning and financing of basic research in universities does not seem out of place in a national context with federal leadership.

Another reason for federal involvement is the high cost of "Big Science", or centres of strength. Most of the provinces would not have the finances to fund the critical mass necessary for an effective centre of strength, and such centres are too expensive to be set up with unnecessary duplications. Also, as pointed out in III.4 and III.8, basic research results are an "international commodity", and many programs require international cooperation.

In the 1970s there will most likely be some changes in the organizational structure into which federal funding of basic research must fit.[69,1] If the scientific community makes its views known to both provincial and federal politicians, then it could well be that future constitutional changes would take them into account. A clarification of "who is responsible for what" would certainly be an aid in the formulation of science policy.

Effects of Federal Financing of Universities

The present method of funding research in universities can have some effect not only on the type of research but also on the teaching programs of the university. The OECD[9] concluded that the present method of funding basic research by the federal councils in Canada has three effects: it establishes a national hierarchy of scientists in each discipline, the ultimate social and economic purpose of the research is lost, and scientific activities at Canadian universities tend to become an imitation of those in the United States.

This present system certainly does influence the attitudes of university faculty and, through this, their research programs, their teaching, and the courses and degree programs offered at universities. Whether such influences are "good" or "bad" depends on the interests and philosophy of the people concerned.

Such influences cannot be overlooked, however, in searching for solutions to some of the problems suggested in III.5 and III.6. The system also makes it more difficult for individual universities to be adventurous and to try to form new programs and university structures.

The system of incomplete funding may be useful in preventing the formation of a "grantsmanship jungle" in universities. A very high level of funding in the natural sciences can, however, distort university planning even to the point of affecting the quality of education in the humanities.[1] The indirect costs of such research can strain a university's budget. If provincial aid is not forthcoming to meet this strain, then the university must deprive other activities of support.

In theory, any such distortion in university priorities is not the fault of the federal government's making research funds available. Such funds can be accepted by individuals only with the consent of the university concerned. In practice, however, it seems unlikely that many universities will pre-referee research applications closely and refuse to forward some applications for research funds. The lure of instant money, bringing with it the possibility of instant prestige through publications, seems likely to overcome any fears of distortions in the programs of the universities.

There seem to be good reasons for giving universities a greater share of the responsibilities for funding research. A university could be given a larger, general-purpose research grant with the understanding that it be responsible for funding of the young, the radical, the mediocre and the more teaching-oriented faculty. There would also be the advantage that this would cut down the number of applicants for grants from federal councils and make it easier to deal more thoroughly with the remaining applications (III.4). If these grants come from the federal government, however, constitutional problems may arise.

The academic colleagues of a young faculty member are probably best able to judge whether he has promise or not. Universities are in any case responsible for starting the new scientist off, by giving him a faculty position. A radical young researcher with what seem to be harebrained (but are actually sound, though very original) ideas is also very likely to be turned down by his peers on granting bodies, who do not know him as well as his immediate colleagues. When research is done more for the contribution that the activity gives to teaching than for the research results *per se*, it seems more appropriate for the support to come directly from the universities.

There seems to be some fear that this would produce "empire-building" by administrators in universities, and a decrease in the quality of research. Surely, if empire-building or other unwise use of funds occurs at a university, that university will have difficulty in maintaining or attracting good faculty. This seems to be a case in which the competition between universities has constructive effects and should be utilized. It would be sad if university administrations and faculty were unable or feared to act in a responsible, fair way, and instead abdicated some of their responsibilities. Also, with signs of increased democracy in universities, the administration and the faculty should perhaps not be considered as different "classes". The motto "people get the government they deserve" holds if there is reasonable democracy, and responsibility may in the long run improve the ethics of the decisions.

The power of university administrations will in any case be tempered. There would then be recourse to other funds for university research, and universities could set up their own peer-judgement mechanisms to judge grants, including outside referees if necessary.

Funding of Oriented Basic Research

One could generalize and say that the federal government has, in the past, guided basic research by setting up its own laboratories, and chosen to leave university research "pure". But, as poin-

ted out in III.5, III.6 and III.7, there are some good arguments for increasing the amount of oriented research in universities. There are also some dangers to guard against.

The advantages of oriented research could be summarized as follows: an increased sense of relevance for students; a smaller mismatch between highly trained manpower and potential employers; greater relevance of research to Canadian problems; better communication among sectors and disciplines.

Some of the dangers are: decrease in the quality of research due to excessive stress on the external (relevance) criterion at the expense of the internal; increase in "grantsmanship"; interference with academic freedom; decrease in the ability to handle new problems which cannot be foreseen.

Basic research can be "oriented" by various means: for example, by giving funds solely for the excellence of research, but providing individual researchers with information about current and anticipated problems in the hope that they will themselves orient their work; or by using relevance to a current or anticipated problem as a criterion for funding. The latter method seems desirable for a number of reasons, even though some of the dangers mentioned above are inherent in it. Giving funds for free basic research, and hoping for the proper orientation of the faculty through knowledge regarding current or potential problems, would seem to guard against interference in university autonomy; but this does not seem to have been very effective in the past, either in maintaining "autonomy" in the broad sense or in orienting research. Funding by mission-oriented departments or industry has the additional advantage of increasing the contact and transfer of information between sectors.

Steps can be taken to guard against some of the dangers of oriented basic research mentioned above. All aspects of "grantsmanship" are not necessarily bad, and some forms of grantsmanship will be present, no matter whether free or oriented work is involved. In trying to point out the relevance of the suggested project to the evaluating committee, it is possible that good ideas are produced, both for the applicant in trying to design projects, and for the granting department in obtaining new ideas. Unless the "norms" of science (III.4) are held to, however, there is danger or fear of misallocation of credit for ideas, and subsequent suspicion.

A system of committees, parallel to the free basic research committee system, could be set up. This system would be problem-oriented, as compared to discipline-oriented in the "free" case. Committees of peers, research managers, etc. could be set up, one for each major government mission and one for each major segment of industry. The peers on these committees should not all be the same as those on free basic research committees. Otherwise, some of the advantage of diversity of funding sources would be lost.

In order to prevent a "class distinction" from arising between free and oriented research grants, and to guard against "grantsmanship", there should perhaps be a common application for both free and oriented grants. Only a single application would then be necessary, but it could be considered by more than one committee. Some type of "clearing-house" would be necessary to process grant applications.

This system would be dependent on the ideas coming from the applicants. In addition to this, each mission-oriented agency or industry would still have funds available to contract-out for work which it had identified as being useful or necessary to its missions. If there were mutual trust between researchers in various sectors, there should be a useful interchange of ideas appearing from such oriented basic research grant and contract work.

Science itself will probably not be advanced as rapidly by increasing the proportion of oriented funding. Polanyi[23] is really correct by definition, in saying:

"Any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science."

Yet, taking a broad view, it would seem to be to the benefit of the nation to deflect a part of research to solve more imminent problems, at the cost of advancing science in the most "rapid" manner. It could even result in a more rational, effective advance of science.

If the ratio of oriented to free basic research is increased, then it will be necessary to guard against a "pendulum effect". A base of free basic research must remain, however, to give broad coverage to all fields and to provide for unforeseeable problems.

Toward a Just Scientific Society

The selection of peers for granting committees has been discussed previously (III.4). A sense of participation by the scientific community in choosing peers seems of some importance. However, there are other aspects of "democratization" which may make basic research activity more effective. Equalization of opportunity as a goal for the scientific community seems to be in keeping with the current general mood.

Judging from the criteria which seem to be used for giving out grants, federal grants are given primarily according to the excellence of the researcher, in the case of the NRC in any case. This, by definition, would imply that the grants are for the support of free basic research for cultural reasons, i.e., for obtaining new knowledge *per se*.

Why should grants for free, or for that matter for oriented, basic research then be available only to university faculty? Why could not scientists in industry and government compete to obtain such grants? This could also be helpful in breaking down some of the inter-sector barriers. One way in which industry in Canada may be disadvantaged is that there is no source of funds from government agencies for an unsolicited idea from a scientist in industry.[72]

Such "democratized" funding would most probably have to be complete rather than incomplete. Otherwise, industry might not support any applications, at least initially, when it is not accustomed to the idea that it may benefit from such work. The grants must also be organized and judged fairly so that universities do not subsidize the research and thus make it impossible for industry or government to compete for grants, whether for free or oriented research. In the same spirit, free basic research carried on in government departments should be opened to external, peer judgement, and should compete for funds with free basic research in universities.

Maybe, even with all the present and the above-suggested additional funding sources, there should be a "Council for Lost Causes", or something like a Canadian (public or private) version of the Ford Foundation. Such a Council could be active in encouraging cooperation and the bridging of gaps, whether these gaps be between disciplines, sectors, institutions, or political levels. At the same time it could be another place where the unusually novel, but still sound, young researcher would turn for funding and encouragement. Funds for feasibility studies could also come from such an agency.

To top everything off, it might be a good idea to have some real "plums" in the way of basic research awards. These would be prizes rather than grants, and should be available to researchers in all sectors. They would be, in effect, a Canadian version of the Nobel prizes, and would inspire real excellence in basic research.

What support should be given to Basic Research?

III.13 Overall Support

Activity in Oriented Basic Research

The overall level of activity in oriented basic research, as well as the distribution by field, should be governed by the de-

mand for the results. This would simply be the oriented basic research effort necessary, as described in III.7, to solve the problems or pursue the missions of government and individual enterprise in the most effective manner.

Thus the overall level would be dependent essentially upon the type and number of missions or problems pursued, the amount of scientific involvement necessary to pursue the missions most effectively, and the amount of basic research activity necessary to best encompass these scientific aspects.

The missions to be undertaken, and their relative priorities, would be decided at the highest level on political grounds, whether in governments or in industry. Then they would be outlined in more detailed form at managerial levels. The Science Council has a major role in advising what the missions and their priorities should be; it has approached this role through the concept of "Major Programs".[3] The problem of choosing and developing missions is not central to this study, and will therefore not be discussed.

Criteria for National Expenditure for Free Basic Research

It is difficult to find meaningful criteria for deciding upon the amount of free basic research that should be undertaken. There should be at least enough to keep all fields of science "healthy", whether or not a particular field is of importance to some mission at a particular time. There should also be enough to ensure that students can obtain comprehensive, high-quality education. (This may be saying the same thing twice over, however.) All fields need not have comprehensive coverage in each individual university, however.

An argument that is often used for determining the level of free basic research activity is the "overhead" argument.[73] According to this argument, a percentage of the applied research and university teaching expenditures is charged as an "overhead", which is then used for free basic research. Maybe this notion is worth considering, but the word "overhead" seems to do disservice to the importance of basic research. Although "overhead" refers to "non-specific distributed costs", which are very essential, there is a tendency to interpret it as a non-essential which naturally gets lowered to the absolute minimum, or to oblivion.

An absolute minimum for free basic research could be suggested in terms of a percentage of GNP, in the absence of other criteria. Whatever percentage is decided on can be revised up or down after some period of time. The growth in GNP has been about the same as the "sophistication-inflation" factor.[13] If this continues, then this "formula" would mean that the amount of free basic research activity in the country would remain essentially the same. Other criteria are suggested or implied in Table 3.

Cost-benefit analyses of basic research do not seem to be feasible for the past, let alone for the future. Some attempts have been made in the United Kingdom[74] and in the United States.[75, 76] Such studies are all very vague because of the difficulty of evaluating cultural benefits, as well as the difficulties in differentiating between national and international benefits. The results can be interpreted different ways, depending on the values of the observer, and they can even be contradictory.[75, 76]

In cases where means and ends are mixed together, and where value judgements abound, it seems possible only to "muddle through". Schultze[77] gives a brief justification for such "muddling through" in cases where "... specification of objectives is not only intellectually difficult but pragmatically objectionable". Essentially, the Delphi technique, as described in II.3, gave groups from the scientific community an opportunity to "muddle through".

The overall level of public support for free basic research is, in the end, to a very large degree a political decision. However, even though specific promises of "delivery of goods" are unrealistic,

advice can and should be given to the politicians, explaining what such activity contributes to the nation and pointing out the most probable consequences of some alternative courses of action.

In considering possible recommendations, it may be of use to have some comparative figures in mind:

a) current basic research expenditures in Canada in 1967, $196 million; of this, perhaps $100 million was for free basic research (approximately 0.15 per cent of GNP);

b) net expenditures on price supports for dairy products in fiscal 1967-68, $135 million;

c) excise taxes on cigarettes, tobacco and cigars, collected in Canada in 1967, $251 million;

d) Canada Council grants in 1967-68, $16.9 million;

e) International Development Assistance through the Department of External Affairs in 1967-68, $50 million;

f) operation expenses of the Indian Affairs Branch in 1967-68, $121.5 million;

g) payments for Fitness and Amateur Sports, $5 million;

h) Department of National Defence expenditures in fiscal 1967-68, $1 753 million.

Expenditures for free basic research do not seem extravagant as compared to some of the above figures. On the other hand, $100 million would buy a lot of beer, housing, or technical assistance to underdeveloped countries.

It could well be that the problem of the level of overall support of free basic research should not even be approached in this manner. Instead of sizing up the pie and then slicing it, it might be better to consider the separate pieces first and then make up the pie. The latter approach has more often than not resulted in a politically indigestible product, however.

Free Basic Research in Industry and Government

In industry, there has been a negligible amount of free basic research. Some such activity may appear in industry if funds for this activity are "democratized" (III.12).

There would seem to be some "free" basic research in government laboratories. Those groups that have achieved international prestige for their excellence should no doubt continue to be supported. Such groups are difficult to build up, but they do contribute to the nation, not only culturally but in other ways (III.7).

With many capable universities present in Canada now, it would seem best to build up any new free research groups, not in the government laboratories, but instead at universities, or at least in conjunction with universities. Exceptions could be made if a man of exceptional quality, who wishes to devote himself to free basic research, appears elsewhere. Government researchers could also be given the opportunity to obtain free basic research grants if such grants were made available to all scientists.

This does not mean that the total amount of basic research in government laboratories will necessarily decrease. Those responsible for managing the missions of their laboratories are best able to decide what amount of basic research is desirable for an optimal approach to their mission. They may find that an increased amount of oriented basic research is necessary in those government laboratories providing a backup for industry.[78]

"Many of the technologies of today are approaching a plateau of development where the cost of further advances in refinements and modifications is out of proportion to probable benefits. New technology, derived from applied science that is based upon new knowledge, is required for progress."

The Dangers, Difficulties and Desirability of Being Specific

All the uncertainties and frustrations mentioned in III.9 appear when actual figures are discussed. In addition, another question arises, namely: "How

honest can one afford to be?" Is it ridiculously naive to attempt to be completely honest? The danger here is that any decision-maker, in looking at the figures, will interpret them as having some "extra fat" on them in the expectation that the figures will be trimmed, and will thus feel obliged to trim them.

There is also the danger that a person familiar with a specific activity tends to know its many inevitable inefficiencies better than others, and in consequence tends to be excessively hard on that activity. Such "over-objectiveness" is impossible, or at best difficult, to quantify rationally. Decisions on such allocations should really be made on a comparative basis. It is difficult enough for a mere mortal to reach some degree of understanding of the benefits of an activity such as basic research; to get the same degree of understanding of all the activities which are in competition for funds is impossible.

Another difficulty in quoting figures specifically is that the structure in which these figures are applicable is as yet undefined. It must thus be assumed that the present structure will remain, together with the revisions that seem to be most probable.

There is virtue in being specific, in spite of these dangers and difficulties. A specific number gives a basis for criticism and discussion. With no specific final statements, a report can be so vague as to be of little practical use.

Free Basic Research at Universities–Too Much, Too Little, or Just Right?

The contributions by university research to the nation are discussed in III.5 to III.8. Some possible conflicts are also mentioned.

Too little or too much research overall can detract from the functions of the universities. Too little research can cause a decrease in the quality of undergraduate and graduate education, and detract from the other contributions that university research can make to society. Too much research can again decrease the quality of education. In addition, too high a level of funds for research in general can lower the quality of research, by shifting the emphasis from people and thinking to fairly standard expensive equipment. It may also cause distortions in university planning.

Too high or too low a proportion of free basic research also can detract from the functions of the universities. Too high a proportion may lead to an ivory-tower atmosphere, with neither the research itself nor the graduates being of optimal benefit to Canadian society. Excessively easy availability of funds for free basic research will reduce the will of the faculty to undertake other types of research. It may also dampen their entrepreneurial drive and willingness to develop contacts outside the university. Too low a proportion of free basic research may jeopardize the university's position as an impartial critic of society and a comprehensive repository of knowledge, and its ability to provide high-quality education in all fields.

On the basis of the above considerations, the following are offered as "ridiculously naive" specific suggestions on which discussions can be based. As indicated in III.13, and in the previous discussion in III.9, they cannot be justified on the basis of quantitative, scientific analysis. They provide, nevertheless, one of a set of alternative recommendations. The Delphi experiment (II.3) provided others, and no doubt the Science Council itself will provide yet another, or even several, in its report on this study.

In Canada at present, a great increase in the total amount of research at universities would not seem to be necessary to an increase in the quality of education. Such an increase is more likely to lower the standards of teaching unless student/faculty ratios are decreased. The benefits of the increase in research results and highly trained manpower are not likely to outweigh the costs. An increase in research, if any, would seem likely to be of most benefit in the industrial sector, or possibly in the more specifically orien-

ted government departments.

The proportion of free basic research seems too high to be an optimum, if research is not funded primarily from a cultural point of view. Thus, a reasonable five-year future for federal funding of university research would consist of holding the level of support of free basic research effectively constant, while that of oriented basic research increases, so that the total "amount" of basic research per faculty member remains about the same. It is assumed that more oriented basic research can actually be of benefit, which should be the case. The proportion of free to oriented basic research–as well as that of basic research to applied research and development–will of course vary with the university (depending on its outlook) and the field (depending on its nature).

The increase should make up for a "sophistication factor" (of about 4 per cent)[13], and a growth factor. The rate of increase in the number of faculty members is, however, difficult to estimate. A "guesstimate" would be that it is not likely to increase very rapidly in the next five years; here too, 4 per cent per annum may be reasonable. Some disciplines will no doubt grow much more quickly. Thus the total "amount" of free basic research per faculty member would decline by approximately 8 per cent per annum. The "amount" of total basic research per faculty member would remain constant, with total expenditures for research rising by 8 per cent in constant dollars. There may, of course, be large increases in some disciplines, and even actual decline in others.

After four or five years, the effect of the changes in funding policies should be reviewed in detail, although there should also be continuous monitoring of the effects throughout that period. It is necessary to have assurance of continuity in research funding, as many projects take years to complete; thus it would also be desirable to have a long-term minimum commitment by government for overall support. A "political quantum" of four years is perhaps a reasonable period of time for committed planning, although for some projects the commitment must be for far longer periods.

Comparisons with Other Suggestions

The effective freeze of free basic research funds (i.e. increase only to take account of inflation) is lower than most of the answers received during Part 1A of the Delphi experiment (II.3). The approximate per annum increases for the median in the Delphi were, for the six groups, 6, 7, 7, 8, 8 and 3 per cent. If the part allotted to "multidisciplinary studies" is considered as a part of oriented basic research, then the corresponding per annum increases would instead by 5, 5, 5, 7, 8 and 2, respectively. It is not unlikely that there would be considerable agreement within the scientific community for a freeze, if this were conditional upon an increase in funds for oriented basic research, and if a "lobbyist" view were not taken.

The Bonneau report[11] estimates that the increase in NRC funding of research for the period of 1971-73 should be about 18 per cent per annum. The Medical Research Council (MRC) estimates a 20 per cent per annum increase, 13 per cent of this being due to an increase in medical faculty.

III.14 Cutting the Pie

Oriented Basic Research

The distribution of effort in oriented basic research should be determined essentially by the distribution of programs or missions undertaken. It should, in other words, be based on the "demand" for the knowledge likely to be produced.

Such oriented basic research is carried out by government, by industry, and by universities. In universities it is carried out both through research contracts or agreements and through grants. In the case of contracts, the organization giving the contract has, to a large extent, determined the problem that needs to be solved. In the case of grants, the applicant

for the grant proposes the project.

If the funds for oriented basic research grants in a field are very plentiful, then there is a possibility that some of the projects will be funded just to use up the funds, and will have negligible relevance to the mission. The granting agency need not, of course, use up all its funds; such funds could be transferred to other missions. It may thus be best to reverse the order of decision.

Committees looking after oriented basic research could first see which applications show promise of research that will meet both the internal scientific criteria of excellence and the external criteria of relevance. The decision of how much should go into each mission can then be made after the applications have been appraised. This is more like the MRC system than the NRC system of distributing funds among fields.

Criteria for Distribution by Field for Free Basic Research

It is not obvious what criteria should be used in allocating free basic research funds in order to maintain a broad, healthy scientific base (i.e. to play a "balance-wheel role"), and at the same time to encourage excellence in a few fields. How does one determine the amounts needed to keep different fields "healthy"? How does one recognize a field in which Canada could or should become a world leader?

Weinberg[53] has suggested the following criteria: internal or feasibility criteria (Is the field ready for exploitation? Are the scientists in the field really competent?); and external or desirability criteria (technological merit, scientific merit and social merit). Scientific merit is a necessary criterion, as a discipline can develop branches which are increasingly more complex, while making insignificant contributions to anything other than their own perpetuation.

A cost criterion should be considered, as some fields are "more costly" than others. This cost is probably worth paying if the field is of some importance to society, and not just science for the sake of science. The cost may be high, however, because the field is exploited to a very advanced degree. Feynmann[82] describes it as follows:

"It seems to me that what can happen in the future is either that all the laws become known...or it may happen that the experiments get harder and harder to make, more and more expensive, so you get 99.9 per cent of the phenomena, but there is always another phenomenon... and it gets slower and slower and more and more uninteresting. That is another way it may end. But I think it has to end one way or another."

Maybe many disciplines in the natural sciences are well advanced toward "the end", relative, say, to some social sciences.

In the past, the number of competent researchers was limited. In such a case, the funds should no doubt have been distributed according to the distribution of such researchers. However, in Canada we now seem to be in an era when the funds available, rather than the number of competent researchers, determine the research effort in the fields.

Still other criteria worth some discussion would be: the number of proposals, regional or linguistic considerations, international reputation of a field, manpower supply, popularity with students, demand for financial support, etc. This problem of relative allocations is too complex to reduce to any one single applicable criterion, or even to a formula of some kind.

It may not be wise to have a direct relationship between the level of research activity and manpower requirements. If all research activity at a university is expected to result in highly trained manpower, then an excessively large number of highly trained people may be produced in those disciplines which are taught to a greater number of undergraduates, and which thus require a relatively greater number of faculty. Some disciplines may

be taught to a relatively large number of undergraduates either because they are popular for general education, or because they are the foundations of other disciplines, and not because there is a demand for highly trained manpower in these disciplines.

How to Make the Decisions

Any final advice to the political level should ideally be a set of alternative recommendations, pointing out what would be the most probable consequences of the alternative courses of action. The politicians would then be able to make their final decisions on the basis of the value-judgements for which they are responsible. The politicians may well prefer the scientific community to make the decisions in the case of relative levels of support.

One way of making such allocation decisions would be to have each discipline submit an application for funds, putting forth the best arguments as to what the discipline can accomplish, what funds are needed, etc. A "jury" made up of scientists, politicians and laymen could then make the decisions as to allocation of funds, possibly making use of the "Delphi" technique. The Science Council could also act as such a "jury".

Some form of "muddling through" would in any case seem to be necessary at the present time, even though it is still worthwhile to continue searching for more systematic, "scientific" ways of making such allocation decisions. But it seems inescapable that decisions such as these must, in the end, rest on the foresight of a group of "wise" men in the country.

Table 8 shows properly weighted and averaged second-round results of the Delphi experiment (II.3) from a total group of 114 people. Comparative figures from other countries are also included.[81] As the input data are not very accurate, only the trends, rather than the absolute ratios, should be considered seriously.

It is interesting to wonder at what level science should be left to scientists, so that the "principle of spontaneous coordination of independent initiatives" might operate as described in *The Republic of Science*.[23] At the present time funds are limited, and an increasingly large number of scientific specialties are coming into being. In a situation like this, maybe this principle is applicable only at a lower level, say within a single discipline. The situation described below[11] does not really seem to make for the most efficient possible organization of scientific progress:

"The tendencies for departmental needs to keep roughly in line, over the period studied, merit further examination...The apparent egalitarianism of this process is not so much a product of research priorities as that of 'free collective bargaining' under which each department tries for at least as big an increase in support as its neighbours."

If scientists are unwilling or unable to decide on the distribution of effort among

Table 8–Relative Distribution of Support of Basic Research in Universities

Field	Canada 68-69	74-75	% Change
Medicine	26	23.0	−11
Psychology	3	4.0	+33
Biology	17	16.2	−0.5
Chemistry	12	10.4	−13
Physics	16	12.2	−24
Earth Sciences	6	7.7	+28
Engineering	16	14.7	− 8
Mathematics	4	5.6	+40
Multidisciplinary	0	6.2	N.A.

Field	Canada	U.S.A.	Netherlands	Norway
Medicine	27.0	28.3	29.8	33.0
Agricultural Science	8.1	14.6	5.0	10.4
Natural Science	47.3	42.9	42.2	47.4
Engineering	17.6	14.2	23.0	9.2

fields, it is nevertheless possible to turn more of the problem over to the politicians. Instead of separate votes for NRC, MRC, Atomic Energy Control Board (AECB), etc., the number of independent votes could be increased, thus extending the base for political decision-making. Separate funds could be voted for, say, a Mathematics Research Council, a Physical Sciences Research Council, a Life Sciences Research Council, etc.

Problems of Boxes

The "fields" chosen, and even the titles of the fields, are of some importance. If "Earth Sciences" were replaced by "Geology", people might be less willing to support it. Also, if "Computing Science" were not included with Mathematics, the enthusiasm for "Mathematics" might be less.

It is also worth questioning whether some of the traditional disciplines are worth preserving. These disciplines are shifting in the problems with which they primarily concern themselves, so that they are not stagnant. Yet often the label of a discipline straitjackets a researcher to the detriment of the advancement of science. Working in interdisciplinary or multidisciplinary fields seems to be increasingly profitable.

Such inter- and multi-disciplinary work, which can lead to the formation of new disciplines, has impediments in its way. Scientists engaging in such work often have a low place in the "pecking order", as they must necessarily be more generalists than specialists. More encouragement could be given to forming multidisciplinary people at universities, instead of sticking to the traditional disciplines in all cases.

An increasing amount of research should no doubt be carried out in "new" disciplines, such as water resources, urbanology, bioengineering, etc. It may be best to leave such new disciplines, or problem-oriented areas, for funding through the oriented, rather than the free basic research, mechanism.

Proliferation of Councils

Some fields could be left out of this "overview" study. There have been suggestions regarding the formation of a Health Sciences Council, which would make decisions regarding basic research in medicine. It is not inconceivable that there will be suggestions regarding a Mathematics Council, or a Council of Technology to look after the interests of engineers, for example.

Councils and committees with rotating membership can be of aid in achieving communication between people through exposing them to problems outside their specialties, as long as these Councils or committees are not formed along the lines of present disciplines. In Canada, there seem to be more than enough difficulties in communication among sectors, disciplines, types of research, the scientists, the public, etc. Also, the solutions to our major problems seem to be increasingly dependent on cooperation among people from many fields and all sectors. The formation of more discipline-ordered Councils would only seem to put up more barriers to communication, and to force politicians to make decisions regarding scientific matters in the areas where scientists will inevitably have conflicting recommendations.

It would seem far better to have scientists, including social scientists, iron out their differences before sending recommendations to the politicians. This is necessary if scientists do not wish to abdicate some of their decision-making potential at the political end of the spectrum. Also, series of conflicting recommendations from various parts of the scientific community have seldom been effective in raising the prestige and credibility of scientists in the eyes of the politicians or the public.

IV Summary of Possible Changes

The summary is divided into eight parts for easier digestion. Some of these parts would seem to correspond to "nodes of influence" for producing the changes which are likely to solve or ameliorate the "problems" identified in the report. The parts are not really mutually exclusive, however.

The suggestions made at this stage of the study are necessarily somewhat vague. Some of them may become unrealistic with changes in the organizational structure for basic research in Canada. Others may even now be outdated because of ongoing developments. The suggestions are not all for changes; some indicate areas in which the present situation should not be changed.

More Emphasis on Quality Rather than Quantity

More thorough evaluation of research proposals (longer periods of tenure for grants, greater monitoring of university research by universities themselves).

Review of intramural government basic research; retention of high-quality, free basic research groups within government laboratories.

Building up of "centres of strength"; full funding of high-quality grant applications.

Inclusion of peers on committees evaluating oriented basic research applications.

More Breadth and Flexibility in the Interests and Abilities of Scientists

Inclusion of topics on the history, philosophy and sociology of science and technology in university curricula.

Increased interaction between natural scientists, social scientists and humanists, at least in Science Council committees.

Financial aid for reorienting of highly trained manpower.

Improved Communication Within the Scientific Community

Full, open information on the procedures and policies of publicly funded granting bodies.

Exchanges of personnel between sectors, thesis work outside universities, contracting out by governments, etc.

A clearinghouse to handle information about free and oriented basic research grants, and about employment prospects for highly trained manpower.

Discussions between mission-oriented agencies and potential research grantees and contractors, regarding their problems and programs.

Improved Communication with the Public

Objective, realistic evaluation of the future contributions of science and technology to society.

Aid to the learned societies and SCITEC for carrying out their communications functions.

Greater Democracy Within the Scientific Community

A democratic system of obtaining peers for committees evaluating research applications.

A set of criteria for comparative evaluation of future proposals for large scientific projects.

A Council to consider applications which seem to belong in no one particular place.

The possibility of applying for basic research grants by researchers in government and industry.

Coupling Power With Responsibility

Clarification of which government or organization is responsible for what.

Freedom for basic researchers to determine the details of their research programs.

Freedom for industrial and government managers to decide themselves on the appropriate amount of oriented basic research to be undertaken.

More responsibility for universities to fund research by faculty; assurance that direct incomplete support of research does not distort the priorities of the universities.

Greater Attempts to Orient Basic Research

A system of problem-oriented granting committees for oriented basic research,

in parallel with discipline-oriented committees.

Specific Suggestions for Overall Support
In the end, a set of alternative recommendations to the politicians regarding overall support.

Enough federal funds to allow free basic research to keep up with inflation.

Increase in federal funds for oriented basic research to keep average research activity per faculty member the same.

A continuous monitoring of changes, with a thorough review in four or five years.

V
An Anthology of Statements Concerning Basic Research

During the course of the study a number of interesting, as well as irritating, quotations were obtained. A selection is enclosed here to illustrate the range of views on this controversial topic.

Selected Definitions from Seminar Participants

"Basic research–that to which one cannot apply a cost-benefit ratio."

"Basic research–research I want to do; applied research–research you want me to do."

Selected Comments from the Delphi Experiment

"Looking at this list, my own reaction is that it has no basis at all in modern science, being a list of the classical departments of a university which in themselves have no basis for existence today beyond undergraduate work."

"These questionnaires are kind of childish. Really, what do you want to achieve? More spending of ink and saliva while our politicians delay and have not the courage to do their duty and behave as responsible, enlightened representatives."

"This is likely to reveal little more than the distribution of the vested interests of the population being sampled."

Selected Titles and Quotes from 1969-70 Illustrating Attitudes in Other Countries

"Adieu à la recherche pure". *Atomes*, 1969

"Basic research is dead". *Industrial Research*, 1970

"Romania: academy links basic science to current needs". *Science*, 1969

"Research that pays: Israel's science will be geared to applications, says government policy". *Science News*, 1969

"Mao Tse-tung, however, denies the validity and utility of basic research, and has castigated Chinese scientists during the cultural revolution for wasting resources on impractical and esoteric experiments." *New Scientist*, 1970.

Some More Views on Basic Research and Basic Researchers

". . . that science had oversold itself in terms of being useful . . . . I think it is too bad to let that kind of an attitude develop. I mean it is very difficult, isn't it, for science to oversell itself because, in fact, it is the basis of our whole technological civilization." A.M. Clogston.[73]

". . . is the lying in their teeth by scientists to justify their existence to society on the grounds that they are useful to society. They have been doing this since the beginning of history. Archimedes did it you know. Archimedes claimed that he would be useful to the military strength and the economy of the Kingdom, yet when he was killed by the soldier he was doing pure geometry. This was his real contribution and what he wanted to do, but he lied in his teeth when he said that he could burn fleets with mirrors and all the other stuff." D.J. de Solla Price.[56]

"If a student is to acquire knowledge with sufficient thoroughness to be able to use it in professional practice, he now has to specialize. But the price of specialization is a myopic and distorted view of the Universe. An effective specialist makes, all too often, a defective citizen and an inadequate human being." A. Toynbee.[41]

"When I list all the people I know with practical or natural science background and . . . backgrounds in the social sciences, and compare them . . . those with the liberal education come off rather badly. They are generally not more flexible, broadminded, discerning, humane or exciting. If anything, they are more often snarled up in their personal relationships, more arrogant, more self-righteous and frequently condescending towards practical people." W.B.S. Trimble.[82]

"If we were to add up all the money which mankind has ever spent on basic research, the sum would probably be less than the amount the Pentagon spends in one year." A. Szent-Gyorgyi.[83]

"When members of the scientific community discuss basic research vs. non-scientific projects, they find it easy to

argue for more research. However, when the scientific community begins to argue with itself–space vs. high-energy physics vs. chemistry–the pure intellectual excitement of another scientists's field doesn't seem to have any more appeal than it does for the nonscientific community." D.F. Hornig.[84]

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