F. B. Watts Memorial Lecture: Science & society

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https://doi.org/10.4224/23000729

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Science & Society by G. HERZBERG

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The F.B. Watts Memorial Lecture was presented at Scarborough College of University of Toronto on March 12, 1974, by Dr. Gerhard Herzberg, winner of the 1971 Nobel Prize in Chemistry and a Distinguished Scientist of the National Research Council of Canada, on the topic of "Science and Society".

The history of science is full of examples of completely unexpected discoveries and inventions that have changed the course not only of the history of science but of history generally. We need only think of the discovery of radio waves, the discovery of x-rays, the discovery of nuclear fission, the invention of the laser, to name only a few of the more recent ones.

A very interesting and striking illustration of the unexpected nature of scientific discovery and invention is quoted by Michael Polanyi in his famous article on 'The Republic of Science'. He describes how in 1945 he and Bertrand Russell were together on the B.B.C. program "Brains Trust". They were asked about the possible technical uses of Einstein's theory of relativity and neither of them could think of any. This was forty years after the publication of relativity theory by Einstein but it was only a few months before the explosion of the first atomic bomb which demonstrated to everyone that the relativistic equation $E = mc^2$ does have an enormous practical significance. If a man like Bertrand Russell could not foresee the use of atomic energy what chance would less able people have of foreseeing similar important developments? It goes without saying that Einstein himself, back in 1905, had not even a vague notion of any practical significance of his discoveries. Indeed, before his discoveries could be applied many other discoveries in physics had to be made that were equally unforeseen.

It is because of the unexpected nature of discovery and invention that it is so difficult to design a science policy, as many of our politicians would like to do. It is my contention that science policy as it is conceived, for example, by Senator Lamontagne and his committee, is not a practical way to proceed if one is interested in the maximum benefit of science to society. Since it is now fashionable to include all of technology and the social sciences in science policy a coherent science policy, if it could be attained, would have to include almost all human activities, except perhaps those dealing with religious and aesthetic experiences. Does anyone really believe there could be a coherent policy with regard to everything? The closest approach to such a system we see in the Soviet Union. Is there any evidence that their system works better than ours?

But let us now return to a more restricted definition of science, excluding technology and excluding the social sciences, and let us ask whether a coherent science policy is a feasible and desirable aim. Professor Warren Weaver, a distinguished American scientist and administrator, former Vice-President of the Rockefeller Foundation, expressed particularly clearly the point I am trying to make when he said:

> "There are those who think that the National Science Foundation in the United States ought to sit like an infinitely wise spider, at the centre of a web which reaches into every governmental activity in science and presumably into every other science activity in our whole nation, planning just how science should advance, tightening up here, slackening off there. I do not think that many scientists hold this view. There is no person, and certainly no committee, which is wise enough to do this."
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> "We should, I think, be glad that this is so. For what keeps the total scientific effort from being chaotic and meaningless is not central planning or any attempt to achieve it, but a kind of grand intellectual homeostasis, under which a multitude of influences interact in a natural way. What science needs is not a lot of planning, but a lot of convenient communication, so that controls may arise naturally from feedback."

Lord Hailsham, the former Minister of Science in the United Kingdom and I believe the first Science Minister in any country, had this to say:

> "There is a sense in which there is no such thing as science, but only sciences. Another way of stating this is to say that science is in fact an all-embracing term, and that scientific researches into particular fields are functions of those fields and not of a comprehensive entity called science. From one point of view, medical research bears a much closer relation to the climate, population, health, diseases and economic activities of a nation than to their nuclear physics. In terms of science policy, as distinct from economic policy, it would be meaningless for a Finance Ministry official to try to block a grant for medical research on the ground that the money was needed for a synchrotron. It is true that both projects must take their stand in the queue for the general investment programme. But they are related to other items in the programme more closely than to one another."

In the past few years there have been many committees that have looked into the organization of Canadian science. Several of them have followed a Canadian habit of belittling our own accomplishments. Many countries in the world envy us the development of the National Research Council and the high standards it has been able to establish, both for in-house work and for the support of research at Canadian universities, and particularly its freedom from bureaucratic rules, a freedom that was especially fostered by the late E.W.R. Steacie. Yet the Senate Committee attacks both C.J. Mackenzie and E.W.R. Steacie, two great Presidents of NRC, because, at a time when the development of industry and the demands of the universities were vastly different from to-day, neither President followed the preconceived notions of the Senators on how science should be organized for the nineteen seventies.

The Bonneau-Corry report "Quest for the Optimum: Research Policy in the Universities of Canada" runs down research in Canada by making statements like 'Canada will never be able to identify many great researchers'. These and other reports were, of course, written by non-scientists who invariably fail to appreciate the way in which science works and scientists work. Without understanding the significance of the underlying procedures of the scientific method they naturally think that they can improve matters by the introduction of new bureaucratic procedures, but in fact they are only following the tactics described almost two thousand years ago by Petronius Arbiter, a Roman official at the time of Emperor Nero, who said: "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."

The reasons why there is nowadays such a strong clamour for a centralized science policy formulated and controlled by an official bureaucracy are not difficult to see. The cost of scientific research has increased tremendously in the last fifty years and the only way to raise the funds required is from the government, that is, from the taxpayer. Naturally the attitude of the taxpayer is 'whoever pays the piper calls the tune' and it is easy to translate this attitude into a demand that all scientific activities should be centrally controlled by an administrative bureaucracy. The rationality of this approach, however, depends on the assumption – which has never been proved – that a centrally organized and planned science is more effective, and in consequence will give the taxpayer more for his money, than science pursued in the traditional ways of the scientific process which in the past three hundred years has shown itself to be the most productive and successful enterprise ever devised by man. One has only to consider the phenomenal developments of this century to realize the tremendous contributions of science to the advance of civilization, to the economic growth of our country, and to the relief of poverty and sickness. All of our communications, our power sources, indeed much of our way of life, has been radically changed – we hope for the better – through scientific developments.

Some thirty years ago, largely through books like Hogben's "Science for the Citizen" and others, the idea was popularized that the only reason for doing scientific research is to improve the lot of man, that is, his material well-being. Let us for a moment assume that this view is justified and let us then enquire what would be the best possible way to ensure that the material benefits of science are maximized. At first sight, it might appear that the best way to accomplish this aim is to concentrate on applied science and to limit the support of basic research to those areas which seem to be ripe for practical exploitation. However, the development of applied science (including medicine) will soon stop if there is not a continuing development of basic science to supply new discoveries which might be applied. It is therefore generally agreed, even among those people who believe that the sole purpose of science is to contribute to the material well-being of man, that basic research has to be done. The problem is only to what extent and how. Some people, like Senator Lamontagne, argue that yes, of course, excellence in basic research must be supported, but on the other hand the Senator proposed that basic research should be completely separated from applied research. Such a separation, in the opinion of most scientists (and I am glad to know also of the Minister of State for Science and Technology), would be about the worst thing that we could do if our objective is to improve the effectiveness of applied science. But Senator Lamontagne, while insisting on a separation which can only make more difficult the interaction between basic and applied research, suggests, nonetheless, that the main effort in basic research should be in fields that are relevant to possible applications. In my opinion it is quite impossible to establish such relevance when one is dealing with a basic scientific research project.

The solution of the more intractable problems is most often found not by research in fields that are obviously relevant but by some basic discovery in a completely unrelated area that throws a new unexpected light on the problem. Thus Fleming's unpredictable observation of the lethal effect of penicillin on his culture led to the antibiotic treatment of infectious diseases and Roentgen's observation of the fogging of a photographic plate led to the discovery of x-rays with all their application to medical practice. (Roentgen was not looking for x-rays.) At the time that Einstein developed relativity theory the believers in relevance would surely have told him he should devote his efforts to something more relevant, since clearly, at that time (and even forty years later), relativity theory was not relevant to human needs.

Let me give you a recent Canadian example, which I found described in a Convocation Address by Professor R.L. Noble of the Cancer Research Institute at U.B.C.

> "Dr. Murray Barr of the Department of Anatomy at the University of Western Ontario was interested in the subject of fatigue in the nervous system and was studying nerve cells under the microscope after electrical stimulation. He noticed a curious dark staining small body near the nucleus in certain nerve cells. In attempting to explain the meaning of this new observation he observed that this body occurred only in the tissues of female animals. After proving this in many species and to be certain of his findings in humans he asked Dr. Linnell, a pathologist in Toronto, to send him histological sections of brain tissue from 100 different post-mortems. Dr. Linnell, who did not know why this request was made, was extremely startled when a few days later Dr. Barr sent him back a list giving the correct sex of each of the 100 patients. This was the discovery of the sex chromatin, a now legally indisputable way to correctly determine the sex of an individual, and an observation which has allowed new areas of research to develop, all over the world, both in patient diagnosis and treatment."

Remember, Dr. Barr was not looking for sex chromatin (it was not known when he started his research), and of course no committee had asked him to do so.

The only real criterion whether or not a certain basic research proposal should be carried out is whether it is scientifically significant and, even more important, whether the proposer is competent.

One of the catchwords in recent years has been "rationalization", rationalization of research at universities and elsewhere. This is, of course, only another way of saying that there must be a "coherent science policy" with regard to university research. In my opinion, and I believe that of many other scientists, such a rationalization of research can only be detrimental to the output of first-class research results.

One of the questions that always comes up in this connection is the problem of neglected areas in research. When I made my first speech on the subject of science policy I dismissed this preoccupation with weak areas as an unnecessary concern and was most strongly attacked for this particular opinion. I still believe that a country like Canada cannot be strong in all scientific areas and that this is nothing to worry about. I have found support for this attitude among others in a public statement by one of the foremost Soviet scientists, Peter Kapitza, who said:

> "When we in the Academy arrive at the conclusion that some field of science is lagging in our country, at once the question is raised about material support for some laboratory or even about the construction of institutes in this field. But it should be understood that it is impossible for us to maintain all fields on the same high level, so it is rather more correct to concentrate our efforts wherever we are powerful and where there are already good scientific traditions. Science needs to be developed in those directions where we are lucky to have a great, bold and talented scientist. It is well known that no matter how much you support an ungifted person, all the same he will do nothing great and purposeful in science. In the development of any particular field our first duty is therefore to proceed from a consideration of the creative forces of the person who is working in this field. You see, our science is a creative vocation, like art and music. It cannot be thought that by setting up a department for writing hymns and cantatas we shall get them: unless there is in this department of the conservatory a great composer equal in power, for instance to Handel, nothing will be produced. The lame cannot be taught to run, no matter how much money you spend on this. It is the same in science as well. The governing body of the Academy should seek out, attract and support the most talented people, and it should be engaged on this even more than on thematics."

The main point is, as Kapitza says, to find and support creative scientists. They are in a far better position to select their research topics than anyone else and, in particular, to select topics which are at the time ripe for successful investigation.

Another aspect of science about which people proposing rationalization are worried is duplication of research. No scientist in his right mind would want to duplicate the results of other scientists unless intuitively he felt that some critical factor had been overlooked. It is inevitable, of course, with the fantastic increase in scientific literature, that once in a while such duplication happens unintentionally, but it occurs rarely since every scientist is aware of the problem. The spontaneous machinery of the scientific process is infinitely more effective in eliminating duplication than any "rationalization" could ever be. On the other hand there are many instances where duplication of certain experiments is necessary. I need only refer to the experiments on gravitational waves by Weber which, if verified, would represent a major advance in our understanding of gravitation and relativity theory. A number of groups throughout the world, including one in Regina, are now trying to duplicate Weber's experiments and it is not yet sure whether they will. Only if the experiments of Weber can be duplicated will his results be accepted as an important advance in physics.

I can do no better than close this particular section of my talk by a quote from Michael Polanyi's paper in which he said:

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

A more mundane way of expressing this opinion would simply be to say that scientific research is the art of the possible and the people who know what may or may not be possible are the research workers who are familiar with the whole background of the subject.

In applied science and technology it appears, at first sight, that a "coherent science policy" is a desirable way of proceeding, and certainly far more planning has to go into technological ventures than into studies in basic science. But, even here, planning does not always lead to the best results from the point of view of contributing to the economy and welfare of the country. Let me give you a recent example from one of the applied Divisions of the National Research Council. Two scientists in our Radio and Electrical Engineering Division conceived of a new principle of electrical measurement and adapted it to the development of a new potentiometer, an order of magnitude more sensitive than previous instruments. This has now been put into production by a Canadian company. Orders which they have received indicate clearly that this instrument will soon be an indispensable tool in every standards laboratory in the world. It is interesting to note that this potentiometer was not developed by scientists in our electrical standards laboratory (to whose mission it would have belonged) but in another laboratory of NRC, and it was developed not because the Council was asked to find a more sensitive potentiometer but because the two scientists were interested in an idea they had and were given the freedom to pursue it just to see what would come of it. In this way the important – and the profitable – scientific discoveries are made. Think what might have happened if the scientists had been so circumscribed that they could do research only on immediately practical problems. Since no request for a more sensitive potentiometer had been formulated they would never have been allowed to "waste" their time following up their scientific interest and would have been assigned to more practical problems.

Another example I found in a recent paper by Dr. David V. Bates, Dean of Medicine at U.B.C. He reports "about a major advance in the technique of radiology of the brain which was occasioned not by an agency identifying the problem as one of high national priority, not by a committee of radiologists forming a task force and identifying goals, not by a society specifically voting money for this purpose, nor by any consumer-contractor relationship dear to the heart of Lord Rothschild, but an intelligent and stimulating remark to a highly creative scientist who had never before considered the problem, plus the means and potential to work toward its solution. And so it will always be."

Even at the development stage of a technological innovation it is extremely difficult to forecast the usefulness or economic advantage of a device. We need only think of the Arrow aircraft, the STOL aircraft, the CNR turbo train and other projects. At this development stage the funds involved run into hundreds of millions of dollars, and yet some projects had to be discontinued and for others it is still doubtful whether they are economically justifiable. If there is such uncertainty at the development stage, how can one, at the much earlier stage of basic research, expect to be able to predict which research project will and which will not be useful? Here the expenses are far smaller and it appears much wiser to choose as the only criterion for the support of such basic research (even basic *applied* research) the quality of the scientists who want to do it. They are in a far better position to judge which particular facet is likely to yield significant results, significant in the framework of the particular science and its applications.

Again the question of neglected areas, now in applied science, comes up. For example, in medical research, attempts have been made to concentrate government support on a few important areas, that is, areas considered important by a group of "wise men". There can be no question that such restrictions are detrimental to the future development of medical research, simply because, just as in other research, the inspiration of a creative scientist cannot be controlled in this way. If a scientist of proven excellence can come up with a proposal in a different field are we really wise to discourage him simply because the proposal does not fit neatly into one of the chosen fields?

Two years ago I had the privilege, at a meeting of the Pontifical Academy of Sciences, of listening to a paper by Professor Szent-Gyorgyi (who in 1937 was awarded the Nobel Prize for his discovery of vitamin C). His paper, on the subject 'Cell Division', presented a new approach to the nature of cancer. He said in his introduction:

> "If cancer research did not make the progress it could have made, this may be due to two factors. The one was that we were too anxious to relieve suffering and cure before understanding. To try to cure, that is repair such a complex mechanism as a cell, without understanding it, is a shortcut to failure. The other reason may have been that we asked the wrong question: why do cancer cells divide? As I will show presently this is the opposite of what we should have asked."
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> "There is a simple experiment which can put these problems into the proper light. The experiment is this. We take a rat, open its abdominal cavity and cut out two-thirds of its liver, then we sew up the wound and open the animal again eight days later. To one's amazement one finds a complete liver, as if nothing had happened; the cut has elicited an explosive growth which seemed to stop when the liver reached its original size. This is amazing, because a cut cannot create a new mechanism. It can create but one thing and this is disorder. The ability to proliferate must have been there and our cut could only release a suppressed ability. The problem of cancer is then, not why a cell grows. The problem is what has kept a cell at rest before? If a car, parked on a slope, begins to run, you do not ask what makes it run. You ask what has gone wrong with the brake? We are thus faced with the failure of a complex regulatory mechanism."

Szent-Gyorgyi considered that his new approach to the problem would provide a hope 'that one will be able not only to cure but also to prevent cancer'. However, he was unable to obtain the modest funds required from the National Science Foundation.

On the other hand, at about the same time the United States Congress, against the advice of many responsible scientists, set up, at a cost of one billion dollars, a new organization entirely devoted to the fight against cancer.

The contrast between Szent-Gyorgyi's inability to find support for his basic research proposal and the setting up of a billion dollar organization illustrates nicely the attitude of the taxpayer and the politician. Governments are willing to spend huge amounts of money for a new project devoted to a clearly marked aim with thousands of employees, most of them bureaucrats keeping scientists in check, but they are reluctant (if not unwilling) to support an individual, even one of proven excellence. The reason is presumably that the support of an individual is a gamble: the individual may turn up with a result that may be important but may have nothing to do with the original proposal, while the big project will at least come up with a thick annual report that can be presented to the taxpayer.

The experience of the past fifty years, both in Canada and in other countries, has shown unmistakably that the most effective – and the most profitable – way of distributing research funds is to make grants to individual scientists who have either proven their excellence by past performance or (in the case of young scientists) who have shown great promise in their graduate work. It is individual scientists (not a team) who make discoveries. This is true even of big research projects; they are successful only to the extent that they are able to obtain first-rate individual scientists. But even if they are successful in hiring able scientists, the sheer size of such programs places an emphasis on organization that tends to encourage bureaucratic procedures and to inhibit the spontaneous creativity of the individual scientist. We have prided ourselves in Canada that, through the institutional pattern of NRC and through its enlightened administrative policies, we had developed a government research activity that was free of the worst aspects of bureaucracy. But the recent move to centralize certain personnel and administrative functions of the government, and the proposal that the Ministry of State for Science and Technology should control and supervise the budgets of the scientific agencies of the government, have greatly altered the atmosphere of research in Canada. The great danger facing Canadian science is *not* a lack of coordination or even too great an emphasis on basic research; what is apt to kill Canadian science is the development of bureaucratic controls and the denial of the intellectual freedom that allows the individual scientist to exert his creative talents to their limit.

The very slight increases of funds for individual research grants in the past ten years have been quite insufficient to keep up with inflation and the increasing sophistication of scientific instruments. The distribution of research grants seems to be more and more concerned with the correction of regional disparities rather than with the support of excellence wherever it is to be found. The latter somehow seems undemocratic to many politicians. And of course more and more of the available funds go into administration and bureaucratic control. The separation of the grants program from the National Research Council is bound to lead to a big increase in the administrative expenses of the program and therefore to a reduction in the amounts available for grants.

When people talk about pure and applied research they do not always realize that there is a continuous spectrum from the purest of the pure to the applied. In many instances it is impossible to say whether a given piece of research should be classified as applied or as pure. What is, however, important is that there should be close contact between pure and applied scientists and the possiblity that one and the same scientist at one time might carry out in the same laboratory a piece of pure research and the following year one on a semi-applied topic. The suggestion by the Senate Committee of separating completely pure and applied research would most certainly be detrimental to the development of applied research.

Just as there is a continuous spectrum between pure and applied science there is also a continuous progression in the motivation of scientists, from the purely philosophical motivation to the desire to improve the lot of man. Isaac Newton and Einstein were clearly motivated by philosophical questions. Their objective was to expand the conceptual basis of science so that it took in a wider range of natural phenomena and interpreted natural events more precisely. Indeed, they considered physical science as natural philosophy. On the other hand much of the work in medical research is motivated by the desire to help suffering humanity. Of course, there are other less altruistic motivations, such as the ambition to find something new or to invent something useful, or simply to make a living. It is natural, of course, that in basic research often the philosophical motivation is preponderant, and since philosophical questions try to get to the root of things this motivation is the one most likely to lead to entirely new results. It is, however, fair to say that in whatever part of the spectrum the motivation of the scientist falls, it is usually a very strong motivation.

It is often asked by non-scientists: Why should the taxpayer support a person just for doing what he likes doing? There are two answers to this question. One is: If we do not support creative scientists in the work that they find interesting we will not reap the harvest of basic discoveries that are necessary for the applied sciences. The other is: Society supports a lot of activities that are far less desirable than the activities of scientists. Is it not better to pay a scientist to do what he wants to do than to pay others to produce and sell goods which society neither wants nor needs? In supporting the scientist there is at least a good chance that something significant and perhaps even something useful will result, useful from the point of view of the taxpayer.

It is, of course, true that not every scientist will produce important discoveries, but in order to produce a few outstanding scientists we must have a broad base from which the exceptional men can develop; even the less gifted scientists can produce something important and useful by filling in some of the many minor gaps in the scientific edifice.

Even though a conclusive argument can be made for the support of pure research on the basis of the usefulness of the results for practical purposes it would, in my opinion, be tragic if society felt compelled to support science solely for this reason. The prime motivation for scientific research is the desire to understand nature. It is an urge that, just as art and literature, lifts man above animal, it is an enterprise of the human spirit. Even to the layman the great changes that science has brought about in man's spiritual relation to the universe must be clear and obvious: the removal of the planet earth (and therefore of man) from the center of the universe by Copernicus, Galileo and Kepler, the discovery of universal gravitation and the laws of mechanics by Newton, the discovery of the circulation of blood by Harvey, the formulation of the evolution of the species by Darwin, and even today the advent of interplanetary travel and the unravelling of the genetic code.

It was good to learn a few months ago that the Canadian government has given final approval to the construction, jointly with the French government, of a new telescope at the top of Mauna Kea, a mountain on Hawaii. It is an important indication that our government, and therefore the people of Canada, do appreciate the striving of scientists for knowledge of our universe irrespective of any possible applications. One would hope that this action of the Canadian government will be followed by a relaxation of the austerity regime in science that has been in effect now for about ten years and has dulled the spirit of discovery among Canadian scientists.

The Senate Committee and the Science Council have maintained that basic science is over-supported in Canada. It is easy to establish that this is *not* so. According to the OECD Report Canada is spending *per capita* about one-half of what the U.S. is spending on "fundamental research". We are spending about the same amount as the Netherlands or the U.K. or France. Since Canada, because of the history of its industrial development, is low, very low, in the spending on development, the *ratio* of fundamental research and development comes out high, and that is what the Senate Committee was comparing. It appears to me that Canada should be able to afford the same per capita expenditure on basic research as the U.S.; in other words, Canada should gradually double the expenditure for basic research over the next few years. There is no question that such an action would help Canadian scientists in increasing substantially the yield of basic discoveries and therefore the pool of new information from which applied science can draw.

The question is often asked: How can we justify spending time and money on problems of pure science when untold millions of people in India and other countries go hungry? This question, just as the statement that basic science should be done only insofar as it contributes to economic betterment, shows a complete misunderstanding of human goals. Of course we must do all in our power to help the poor to increase their standard of living, but should it be done at the expense of those activities that are connected with our culture? Are there not vast non-cultural expenditures that could be re-deployed in order to eliminate poverty? Would it be worth saving the human race from extinction if it could be done only by giving up all those creative efforts in the arts and sciences that are *not* directly related to survival but represent the strongest justification for the attempt to survive? Surely preservation and advancement of our culture should have the highest place in our system of priorities. Human culture, in the words of a very distinguished Dutch physicist, Professor H.B.G. Casimir, "did not begin when man started to make and to use tools, it began when he found time to decorate and to embellish his tools. The essence of culture is always in those things that from a purely utilitarian point of view are unnecessary, superfluous, or even wasteful." In other words, if we support universities, the sciences, the arts only to the extent that they are economically useful we shall soon destroy human culture. In my opinion the fraction of government expenses devoted at present to these activities is too small and should be substantially increased.

It is fortunate that the most efficient way of supporting science for utilitarian purposes is also the best way of supporting it for cultural purposes. What we need is support of scientists of proven excellence and younger scientists of promise, without circumscribing their work and slowing it down by bureaucratic rules. Just as in this country we do not tell the artist or the writer in which way to write or to produce his art, we should not, as taxpayers, attempt to tell the scientists what they should do, but we should ensure that the highest standards are applied in all granting procedures and demand of the scientists only that they do their best. In that way we shall produce not only good science but we shall also produce sicence that is good for practical applications.

It is clear that in a talk of this kind it is impossible to touch upon all facets of the connection between science and society. Other speakers with different experiences would have emphasized different aspects of the problem. I do feel, however, that the cultural aspect of science is so often and so easily forgotten that I have emphasized it more than perhaps other people would have done.

Let me summarize in conclusion some of the points I have been trying to make.

The promoters of the idea of a coherent science policy fail to realize how science works, how scientists work. I have given examples (and a whole book could be filled with them) how a scientist either has a bright idea or by careful observation finds something that has escaped earlier workers and how this idea, this observation, can lead to important practical developments. But at the initial stage it is impossible to foresee these developments. Relevance is not a sensible criterion at this early stage because it if often impossible to establish. Unless we support basic scientific research without worrying about relevance we shall not have a harvest of discoveries of importance for practical applications. Basic research is not over-supported in Canada. We are spending on it per capita only half of what the U.S. is spending. The way to support science, basic or applied, is to support good scientists and let them decide which work appears to them as most significant.

Science is a creative activity which cannot be controlled from outside. Any attempt to do so in order to have a coherent science policy if successful is bound to stop the really novel developments in science.

Finally, I suggest that it is time for the taxpayer to realize that the mere survival of the human race and the improvement in the standard of living is not an ultimate good unless it is coupled with increasing support for creative individuals who will advance our cultural heritage. If, as taxpayers, we do not reserve a reasonable fraction of our taxes to the activities in art, literature and science we are bound to end up in a society not worth preserving.

The support of creative scientists without bureaucratic procedures will, at the same time that it advances our culture, also lead to discoveries that may be of great practical use.

Michael Faraday, 150 years ago, was supported in his work at the Royal Institution solely because he was a creative scientist who by his discoveries contributed immeasurably to the understanding of our universe. Yet the whole production of electric power to-day is based on his discovery of electromagnetic induction.

Let us learn from the lessons of history.

published through the generosity of
the scarborough college alumni association

design:
graphics & photography department
scarborough college university of toronto