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THE BASTERFIELD LECTURE SERIES

SCIENCE and SOCIETY

by

Gerhard Herzberg

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LECTURE NO. 17 - OCTOBER 12, 1973 UNIVERSITY OF SASKATCHEWAN, REGINA CAMPUS REGINA, SASKATCHEWAN In his talk Dr. Herzberg deals with one of the more vexing problems of our time: the role science should play in our society. It is a problem which has attracted the attention of governments throughout the world. To this problem Dr. Herzberg brings the clarity of vision and maturity of thought gained from a lifetime spent in the service of science. The lesson he imparts is the one he has learned: it is that the health and vigor of science, its deepest values, and ultimately even its practical usefulness, are all inescapably dependent on the preservation of the spirit of free enquiry.

SCIENCE AND SOCIETY

When I came to the University of Saskatchewan in 1935 one of the people who was particularly helpful in the process of my adjustment to the new surroundings was Stewart Basterfield. The first contacts soon developed into a friendship that lasted until Basterfield's untimely death. I consider it, therefore, a special honour and privilege to be asked to give one of the lectures in the series that has been established by this University in honour of Dr. Basterfield.

One of Basterfield's strong interests was in the history of science. He published a number of papers on the subject and I recall discussions with him at various occasions about problems in this field.

The study of the history of science is important because it gives us valuable guidance when we consider the relation of science and society, the topic that I was asked to discuss in this lecture.

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. I think it is fair to say that Bertrand Russell was second only to Einstein as one of the greatest intellects of this century. If he 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 many of his predecessors and successors, is not a useful way of proceeding if one is interested in the maximum benefit of science to society. It is as if one were to expect a five-year-old boy or girl to make a plan for the rest of his or her lifetime. It is just not possible, and for very similar reasons

to the ones that apply to science policy - simply because there are so many unknown and unexpected factors entering the picture and because a five-year-old just hasn't the necessary grasp of the subject, even if one could foresee the unknowns. With regard to science policy, we are clearly at the stage of a five-year-old, or worse.

In the past few years there have been many committees that have looked into the affairs 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 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-Cory 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 and scientists work. They think they can improve matters by introducing new bureaucratic procedures following the old adage stated so beautifully 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".

The development of the last hundred years has shown the importance of scientific discoveries for the development of our civilization, for the economy of our country, and for 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. Therefore, the argument goes, we must not continue doing science in such a haphazard fashion as has been done in the past, we must more systematically apply the best possible organization to ensure that the benefits of science are maximized, and, so the thinking goes, this can best be done by a centrally developed and controlled plan.

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 develop applied science to the maximum 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 improve the lot 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 he proposed that basic research should be completely separated from applied research. Such a separation, in the opinion of many, would be about the worst thing that we could do if we wanted to improve the work in applied science. Again, Senator Lamontagne and many others before and after him have suggested that even in basic science the main effort 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 are 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. 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.

The only real criterion whether or not a certain basic research proposal should be carried out is whether it is scientifically significant and 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 it 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 and so on. 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 good scientists. They are in a far better position to select their research topics than anyone else and, in particular, 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 unnecessarily results of other scientists. 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 and knows that to claim, as original, results that duplicate those of another scientist is almost as great a sin as to publish incorrect results. 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 here 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 can 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."

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 not developed 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.

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 devices. At this development stage the funds involved run into hundreds of millions of dollars, and yet some projects had to be discontinued and others are still of doubtful usefulness in terms of the pay-off. If there is such uncertainty at the development stage, how can one expect at the much earlier stage of basic research to predict its usefulness? Here the expenses are far smaller and it

appears much wiser to choose as the only criterion for the support of such basic 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 interdisciplinary connections.

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, just as in other research, simply because 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 should we really discourage him from carrying out the proposal simply because it is not in one of the chosen fields?

A year ago I met an American Nobel Laureate working in the field of medical science close to the problem of cancer. He had a new approach to the nature of cancer which he thought would be an important step in the solution of the problem. 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.

These two incidents illustrate 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 thousand 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 an (important) result that has 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 demands of the Science Council and the Senate Committee for a "coherent centralized science policy", 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.

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 continuous contact between pure and applied scientists and the possibility 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 spectrum 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 extraneous 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. As an illustration of that I would like to quote from an editorial in "Science" written by Lewis Thomas, Dean of Medicine at Yale University:

"Scientists at work have the look of creatures following genetic instructions: they seem to be under the influence of instinct. They are, despite their efforts at dignity, rather like young animals engaged in savage play. When they are near an answer, their hair stands on end, they sweat, they are awash in their own adrenalin. To grab the answer, and grab it first, is for them a more powerful drive than feeding or breeding or protecting themselves against the elements.

It sometimes looks like a solitary activity, but it is as much the opposite of solitary as human behavior can be. There is nothing so social, so communal, so interdependent. An active field of science is like an immense intellectual anthill: the individual almost vanishes into the mass of minds tumbling over each other, carrying information from place to place, passing it around at great speed.

In the midst of what seems to be a collective derangement of minds, with bits of information being scattered about, torn to shreds, disintegrated, reconstituted, engulfed in an activity that seems as random and agitated as that of bees in a disturbed part of the hive, there suddenly emerges, with the purity of a slow phrase of music, a single new piece of truth about nature.

In short, it works. It is the most powerful and productive thing human beings have learned to do together in many centuries - more effective than farming, or hunting and fishing, or building cathedrals, or making money."

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 produces important discoveries, but on the one hand in order to produce a few outstanding scientists we must start from a sufficiently large number, and on the other hand even the less gifted scientists can produce something important and useful by filling in some of the many minor gaps in the scientific edifice.

The case for support of pure and applied science by society is clear-cut even if it were to be based entirely on the usefulness of scientific results for practical purposes. This support is needed not only to improve further the standard of living, particularly of the less-favoured nations, but nowadays, perhaps even more important, to overcome and control the undesirable consequences of modern technology. Without strong continuing support of science, including basic science, these aims could not be reached. It would, however, be tragic if society felt compelled to support science solely for the reason of its usefulness. 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 species by Darwin, and even to-day 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 other committees maintain 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". This is about the same amount spent by the Netherlands or the U.K. or France. Since Canada, as everybody knows, (and for obvious reasons) 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. Should Beethoven's contemporaries have asked him how he could justify spending all his time on compositions when millions of people in Europe at that time went hungry? It is obviously a meaningless question. 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.

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, demand of the scientists specific things but only demand that they do their best work and that in all granting procedures the highest possible standards are applied. In that way we shall produce not only good science but we shall also produce science 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 have done.

I can do no better than end my discussion with the quotation of the concluding statement in the paper by Stewart Basterfield on "The Place of Science in a World View" (published in 1952):

"In the good society the aim of education should be the acquisition of a philosophic outlook, concrete, general, and critical. There should be understanding of the world of men and things, there should be moral and aesthetic insight, there should be idealism and a sense of the high value of life itself. We proclaim the importance of raising the standard of living throughout the world and we invoke the resources of practical science to this end. It is a worthy ideal, but is it not more important to look a little higher and, as one writer has put it, 'to raise the standard of sensibility, or artistic perception and capacity, of cultural inspiration, throughout the masses of the governed, starting at the top'? This should be done while the other must not be left undone".