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PRIORITIES IN SCIENTIFIC RESEARCH

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PRIORITIES IN SCIENTIFIC RESEARCH

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SVMMARIVM — Auctor exponit et discutit problemata circa generalem ordinem in scientificis investigationibus nostra aetate servandum, quem maxima cura et investigationum duces et ipsi investigatores seligere debent ac definire, cum multa eaque gravissima ex ipso consequantur.

My remarks and observations will concern the problem of judging priorities in research and shall deal chiefly with fundamental research and will touch upon similar questions in applied fields only in as far as they may exert an influence on basic research.

The problems involved are of the greatest interest today both to the individual research worker, to governments and organizations who are to support scientific work and also to those who wish to stimulate individual work by awarding prizes or other distinctions.

I have been involved in such decisions myself during many years and my thoughts are based largely on personal experiences. As I am also a scientist myself I have certain possibilities to view the situation from both sides.

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The dilemma is above all that « free research » can no longer be as free as it used to be, although most responsible people would admit that freedom in basic research is essential for human progress in all fields. The limitations thus put on our freedom are chiefly a consequence of the enormously increasing demands placed on research, both fundamental and applied, which can not be met by a corresponding increase of funds and personnel. There is, however, another equally important factor: the research worker himself in making his own priorities is often aware of his duties to help a suffering world and he may therefore feel inclined to prefer a short-range project of obvious practical importance to a long-range plan which scientifically may seem more significant, and where he may have a greater chance of doing his utmost in his specific capacity.

These problems are often highly personal but it seems worth while to discuss a few points in this context. For example: What are the optimal conditions for original, fundamental research? How do we best judge excellence in scientific work? Can more be done to convince authorities and the general public of the significance of basic work for the development of human welfare? These questions are essential, because quality must always play a prominent role when we discuss priorities.

When great discoveries are published in the scientific literature they are presented in a form which does not tell us very much about now things *really* happened, unless we are so well informed that we can read between the lines. A research worker who has reached a goal will present his results in the most logical and concentrated form, which does not necessarily mean that he will describe the path he actually followed in his work. We all know that this path is mostly rather crooked, full of stumbling blocks, tempting sideways that are « dead ends », wishful thinking and many other human weaknesses. First when the goal is reached one knows what should have been the logical way and this is what is used in the final presentation. This is necessary in order that scientific literature shall not be overloaded, and there is nothing dishonest in this kind of « after-rationalization ». Nevertheless it is to be regretted that in this age of great discoveries so little is written about how great discoveries are made. Although this is mostly a highly personal matter it ought to be possible to obtain and analyze material from personal interviews, autobiographs etc. Much of the work of great scientists in the 18th and 19th centuries, and earlier, is today studied and analyzed by active groups in the history of learning, but it seems to me that we neglect a closer study of today's work. Some material will vanish as scientists pass away. After all it would seem to be a very urgent matter to find out how great science is done, also because of the enormous funds which are today invested to promote research. Much of the « research about research » done deals too much with a comparison in scientific productivity under different conditions or in different countries, measuring the output in number of pages or titles of communications or by other means where quality does not come into the picture.

I am not sure that a penetrating study of the optimal conditions for scientific creativity would lead to practical recommendations to governments, research councils or foundations how they should spend their money. But it might lead to an awareness in the back of their minds which could influence their decisions. And to others it would be fascinating to know more about the act of creation of great science. When discussing such questions with collegues one finds a certain consensus as regards certain factors which seem to have had considerable influence. For example: the privilege of having worked under a great master, to receive inspiration from him and to witness how scientific work is done at its best. This does not involve simply imitation, but sometimes opposition. You know, as well as I do, what a great inspiration a young apprentice may feel when he finds that the professor is wrong. Much of the influence takes the form of criticism — that is positive criticism — but also if on some occasions the senior scientist can explain to his collaborators not only what engages his mind just now, but also what he hopes to do, how he feels about the situation in a certain field, also perhaps his disappointments and sufferings, facing failures or contradictions. This is to « prepare the minds ».

The prepared mind is no doubt an essential background but it is not enough. A deep personal involvement is necessary, one has to live with one's problem and one even may have to suffer with it. It is interesting that the deeper one penetrates this subject, the more obvious is the conclusion that creativity is basically the same in science as it is in the arts and in music. This may seem strange to some laymen who tend to believe that science is only cold logics and thus a rather inhuman occupation.

We must however also count with another factor, namely the « triggering » mechanism which works on the prepared mind. There are many anecdotes about this — Newton's apple for example — and some of them may be true. Studies of optimal conditions for creativity have given amusing results. For example: a Danish philosopher found that his most productive time for new ideas was when he was brushing his teeth. Because while doing so he felt no obligations to the rest of the world. The essence of this and of similar observations is probably the « anti-stress » condition produced by a break in an attitude of a too conscious effort to produce something.

Now to return to the dilemma in the selection of priorities in the world as it is to-day. An increasing number of scientists are becoming aware of the sufferings of the world to the extent that they suffer themselves. And they know — perhaps better than most people — which enormous risks we are running as our capacity to make use of our discoveries has outgrown our ability to prevent the misuse. I can very well imagine that in this situation a scientist would be willing to sacrifice his long-range plans for some more immediate goal, if this appears particularly desirable in the present situation. And I can also imagine that now the optimal conditions for creativity — as I have just discussed, would apply to such cases even to people who earlier would never have thought of sacrificing some of their so-called freedom.

I have experienced in my life both the « nuclear age » and the « space age » but I would hope to live to see also a « cleaning up » age. Cleaning up after a development which in certain respects has been recklessly fast. To clean up does not seem particularly fascinating as compared to research into atomic nuclei or into the outer space. This has to be weighed against the increasing degree of personal involvement, of which I have spoken. And even if the atoms and the space may have to wait, they will still be there, waiting for future explorers coming from a cleaner world.

To decide about priorities in research is, as we all know, extremely difficult. It requires an almost prophetic vision. In the early days of the atomic bomb some biologists complained that life sciences, although they appear to be of greater importance to the immediate needs of mankind, came too much in the background as compared to nuclear research. Now we can see the benefit to mankind of results coming out of the work on the atomic bomb, for example the immense benefit to medicine by the use of isotopes, and the great possibilities in agriculture which may come out of the possible use of nuclear energy in de-salination of sea-water. The contribution of leading scientists to the immediate needs of mankind may also take the form of suggesting or urging priorities — they should have the vision, more than anybody else, what the future perspectives and possibilities are.

I have, since many years, been active in various functions in the Nobel Foundation and I have had certain responsibilities in decisions about the award of the scientific Nobel prizes. This involves every year to find among about 100 candidates who is the most worthy. As you can imagine this is no easy task. It is, however, encouraging to find that there seems to be a surprisingly uniform opinion among scientists all over the world, irrespective of nationality, as regards, let me say, ten out of the one hundred being the worthiest. But even so, the problem is to select one, or two or perhaps three among these ten. According to Alfred Nobel's will the prizes should be awarded to those who have done the greatest benefit to mankind. This is again a priority problem, although of a somewhat different kind — rather a value problem. I have sometimes been asked by colleagues who have followed our activities, who know the rules laid down in Nobel's will, why scientific Nobel prizes so often are awarded for achievements which do not at present seem to be of immediate importance for the well-being of man. This is no doubt the case, and reflects a strong belief among those responsible for the decisions in the value of basic research, but it also seems to be in accordance with Alfred Nobel's views. He wished to give support to those who needed it best, which he believed to be rather the far-sighted dreamers who were not shrewd enough to benefit financially from their discoveries, in contrast to those who made obviously and immediately explorable inventions. To-day it would seem that such a distinction between long-range and short-range usefulness is less realistic, and this is perhaps also the case with the profit-making as the prime motivation for serving mankind. Nevertheless, basic research has always had problems in finding resources as compared to applied fields, and it seems to me that this policy for the Nobel Foundation is justified even today. It may help to focus the attention of authorities and of the general public to the value of basic research, and also to listen to advice coming from leading scientists as to how their results should be used and how a misuse should be avoided.