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OBSERVATIONS ON THE PROMOTION OF THE PROGRESS OF SCIENCE

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It is mostly taken forgranted these days that scientific progress is a good thing and that the faster we can make science progress, the better off we shall be. It is not at all sure that this thesis is correct and it is doubtful whether the majority of those who advocate these days the promotion of science are really concerned about science. If you will patiently inquire just why they are so much in favor of accelerating the progress of science, you will frequently get to hear that unless we do so, some other nation will overtake us. What these men are interested in is science as a tool--and particularly as a tool which will improve our position as a nation with respect to other nations. Somehow this answer of theirs fits much better into the general picture which this century presents than does the assumption that their interest in science is genuine. That nationalism is one of the dominant features of this century is certain--whether the pursuit of science falls in this category is doubtful. There are as a matter of fact, some indications that genuine, passionate interest in science has been on the decline in this century and that, generally speaking, it may be in the process of dying out.

Historians, looking back a few hundred years from now to this first half of the 20th century, might find as its most outstanding feature, a conspicuous decline of what might be called the "religious attitude" in the broad sense of the term. The apparent decrease in genuine scientific interest might be traced by them to the same basic cause, since the mainspring of science is a passionate interest in the make-up of the universe and an emotional attitude towards the universe is part of what we term the religious attitude.

Natural science was invented a few centuries ago. It has gathered momentum in the following centuries and is being carried forward now by its momentum. Facilities for scientific work have of course tremendously increased and also the total number of persons who are engaged in what might be called scientific pursuits, but I am not sure that by-and-large, conditions for scientific progress are more favorable to-day than they were fifty years ago. Rather than talk in generalities, let us take one branch of the sciences and then try to discover what conditions are favorable for and what conditions are detrimental to progress. In the case of physics we will have to go even one step further and distinguish between experimental and theoretical physics. What are the conditions necessary for the progress of physics?

After the first World War, Germany was impoverished and a movement got under way to make avaialble large funds for the promotion of scientific research. A reporter, who, in order to be helpful in this drive, wanted to publish an interview with Professor Einstein, and asked Professor Einstein what facilities he would say a theoretical physicist needed for his work. Professor Einstein did not hesitate in naming those facilities: "a theoretical physicist ", so he said, "needs some paper and a pencil." This answer was not of much use to the reporter and so he asked if there are not other tools which theoretical physicists also need. "Oh, yes," said Einstein, "there is one which is most important: the wastepaper basket." I do not know whether the reporter understood what Einstein meant but I am sure that you will have no difficulty in understanding that a theoretical physicist can, if he wishes to do so, sit at his desk and turn out paper after paper which will be printed in the periodicals and will establish his reputation as a sound worker. For this activity, he does not need a wastepaper basket; the scientific periodicals fulfill very well the role of the wastepaper basket. On the other hand, if he is really interested in the mysteries of nature, he will do something different. He will grope around near the edges of the unknown. He will pick up an idea and pursue it to a certain length and he will occasionally be successful, but mostly he will not be successful. If this is the kind of activity he pursues, he will have to watch out. He may build a beautiful theoretical structure but if he will only look closer, he may see a flaw and, if he has the courage to look still closer, he may see that his theory is no good. The less reluctant he is to throw his manuscript into the wastepaper basket, the earlier he will see the flaw, and the better it will be for him and everyone else.

Let us try to pursue the trend of thought of Einstein, and complete the list of those few things which a theoretical physicist needs in order to be successful. Apart from pencil, paper, and wastepaper basket, he needs leisure. He can have leisure only under two conditions. He must have financial security and must not be dependent either financially or morally on obtaining "results". If a theoretical physicist has leisure and the world in which he lives is indifferent to what he does, he can live happilt and now and then take an important step forward. If this material advancement depends on the results which he produces, then his chances for an outstanding discovery are reduced and the same holds if having once achieved fame, the world looks to him with expectation for further advances. To quote Mr. Einstein again, his happiest time was when he was an examiner in the Patent Office at Berne, Switzerland. He had enough leisure to think about what he liked to think about and was under no moral obligation to "lay golden eggs".

There is more than one factor involved in the progress of physics and we shall try to separate them by trying to analyze the picture presented by the development of physics in Germany between 1919 and 1933. One of the factors involved is of course the education of the students of physics and, in this respect, the situation in Germany was rather favorable.

In the German University, a student who studied science usually decides very early whether he wants to become a teacher and stand for the appropriate examinations, or whether he wants to get a doctor's degree, which was at most universities in Germany a degree in "philosophy". If he decided to study for the doctor's degree, there were no examinations of any kind or description from the day he entered the university, usually at the age of 18, until the day when he submitted his thesis, three, four, or five years later. There were no required classes, and if during his studies, he wanted to pay the tuition fee and miss all his classes, he was welcome to do so. The thesis which was submitted was scrutinized very carefully, (with the exception of a few universities of low reputation) and if it was found that the student really did an original sound piece of work, he was given an oral examination that was rather perfunctory. The only thing the universities were interested in was whether the man was capable of original and reliable research work. They were not concerned whether there were large gaps in his knowledge in his chosen field, that is, they were more interested to find out what he knew than what he did not know. With such a system in operation you can hardly say that a man who has a doctor's degree in physics can be expected to acquire the knowledge which he needs for the work which he undertakes. Clearly, the German system is not to be recommended if our aim is to have a universally high level of training for students who emerge with a degree from a university. But the system might have been in part responsible for the high productivity displayed at an early age by a number of German theoretical physicists.

We may contrast this with training of the student in the United States who wants to obtain a Ph.D degree in physics. The major American universities offer excellent graduate courses and give graduate students a very thorough knowledge of their field. A thorough training for graduate students undoubtedly produces thorough workers, but it is by no means sure that it accelerates the progress of science. There seem to be a number of factors involved and at best one of them can be illustrated by going from one extreme to another and comparing conditions in the United States with the conditions as they existed in Hungarian universities towards the end of the first World War.

In Hungary there was practically no industry which needed physicists and, therefore, a young man interested in physics would hardly go and study physics, unless he wanted to become a school teacher. He would rather study either engineering or if it had to be science he would study chemistry, since chemists were needed even in Hungary. If he really was interested in physics, he would occasionally get some glimpses of the subject in the course of his university studies, pick up, in discussion with others, some elementary facts, and occasionally, get hold of a book dealing with electro-dynamics or thermo-dynamics, or some of the other classical branches of physics. His hunger awakened and short of food, he would set out to solve the problems which interested him, and occasionally he would find certain results which would invariably turn out to have been produced by others earlier. If his passion for physics was really great, he would leave chemistry, or engineering, or medicine, or whatever else he studied, and try to become a physicist. Which incidentally almost invariably meant that he had to leave Hungary, and most of the Hungarian physicists who are internationally known, were associated after the First World War with some of the German universities. Whether the advantages of this type of education outweighs its disadvantages is difficult to say. It cannot be decided by comparing the average contibution of Hungarian physicists to that of other physicists groups and obviously in the case of Hungary, there is a strong factor of selection which enters. Only the most passionately interested students will overcome the difficulties of changing over from engineering, medicine, or chemistry, to physics, a fact which makes direct comparison with other physicists groups difficult.

In contrast to this, an American boy who is interested in physics, can get excellent and thorough training at a much earlier age and will know much more than his Hungarian colleague, but he will rarely have the opportunity to discover and solve a problem before the solution of the problem is presented to him on the blackboard. His spiritual food supply will be so abundant that at no time will he feel the pangs of hunger, or have an opportunity to make a "great discovery" (made previously by others). Having been gently led through the known territories to the fringes of present knowledge, he is finally told to go ahead and be from there on onhis own, when he has never been on his own before.

His education prepares him more for solving problems which have been formulated rather than for formulating problems which have remained unrecognized.

Between the two extremes represented by the physicless universities of Hungary and the overorganized curricula in physics in the United States, the German universities occupied a happy middle-of-the-road position and this might very well have been a factor in bringing about the high standard of theoretical physics which was maintained in Germany throughout the years from 1919 to 1933.

But while theoretical physics was flourishing, experimental physics was in a stage of stagnation in Germany during the same period. A number of fundamental discoveries were made in the world during this period but they were made in the United States, England, and France. None of them was made in Germany.

What were the significant advances made in physics during the period in which we are interested? There was a discovery by Rutherford in Cambridge, that atoms can be disintegrated by shooting alpha particles on them. This was found by looking at ~~tracts~~ tracks of alpha particles in air made feasible by using a Wilson cloud chamber. There was a discovery by ~~Eston~~ Aston in Cambridge, that the stable elements of the periodic system are composed of isotopes which opened up a new chapter in physics. There was a discovery early in the 20's by A. H. Compton of an entirely unexpected phenomena displayed by x-rays, the so-called Compton effect. There was the discovery in America of a positive electron, found by Carl Anderson, in Milliken's laboratory at Pasadena. These positive electrons were found to be present in large numbers in the cosmic radiation and could be easily observed. There was a discovery by Joliot in France, of the emission of positive electrons by bombarding light elements with alpha particles and this led Joliot and his wife in 1933 to the discovery of artificial radioactivity which marks the beginning of modern nuclear physics. Artificial radioactivity was one of the two most important factors leading to the liberation of atomic energy which was achieved during the war. The other important factor was the discovery of the neutron. And again, the most important step which led to the discovery of the neutron was made by Joliot who published a striking phenomenon that could obviously not be explained on the basis of the previously known articles. The actual realization that neutrons existed and were responsible for the phenomena observed by Joliot was made by Chadwick in the Cavendish Laboratory in England. Another experiment leading to an entirely unexpected result was performed by Ellis in Cambridge, England and led to the conception of the nutrino (neutrino).

The German advances, as you see, are conspicuously missing from this list. Yet facilities for scientific work in Germany were good and there were a number of very good physicists installed in well-equipped laboratories. The students of experimental physics benefited from the education they received at the universities. There was vigorous and well organized scientific life with frequent informal meetings among scientists making the exchange of ideas and experiences easy and pleasureable.

In addition there was a much larger number of able men in Germany active in the field of physics than in France and England taken together. And yet none of the fundamental advances in physics came from Germany but a number of them came from France and England.

If we want to understand the lack of productivity in experimental physics, we must look for causes which do not lie in the education of the German physicists but rather in the social pressure to which he was exposed in the years after he completed his studies. Most young German physicists were dependent for their career on establishing a reputation rather early and in maintaining the reputation by publishing papers at not too distant intervals. In experimental physics, as long as you move near the main roads of development, where there is a theoretical conception to guide you, it is quite easy to pick up a problem and you can be sure that you will emerge with a sound paper which either fits in with the theory upon which stimulated it--which is nice--or it may prove that the theory has certain defects--which is also nice and might even be interesting.

Some physicists might have felt tempted to pick on a problem which is not so near the main road of development and to follow some dark hunch which might have led to a great discovery, but had one of them yielded to such an impulse, after two or three years work, he might have found himself in a dead-end street. He would not have kept up his reputation and even his financial security might have been placed in jeppardy if he did not have tenure and few men below 40 had tenure. As such a man grows older, acquires financial security, and more and more facilities available for his work, he finally becomes "free" to choose problems according to his heart's desire. But by that time, he usually had become a man of "regular habits" and the chances were that he would go on putting out papers at the accustomed rate.

I have seen at close distance this influence at work in Germany and I was convinced rather early in the '20's that it would kill experimental physics in Germany in a very short time. I believe that experimental physics was in fact dead in Germany well before 1933. In a sense you might say that experimental physics in Germany was killed by the public appreciation of the value of scientific results which led to a system that rewarded scientific results, by promoting those physicists who produced a steady out-put of scientific results, as demonstrated by their papers. It is quite likely that if the ~~xxxxxx~~ public attitude had been different--if they considered the publication of scientific papers as an unnecessary luxury and did not permit anyone to publish, say more than one paper in two years, and if in addition they would have made everyone pay for the cost of having his paper printed, we might have seen discoveries made in Germany which were made elsewhere, or which were perhaps made nowhere.

It is doubtful that the objective validity of these views can be really convincedly demonstrated just by looking at the results. The number of people who are responsible for the major advances in physics in France and England was very small and it is impossible to apply statistical considerations to such a small number. Only if one was in close touch with the scientists in the different countries can one interpret the objective data on the basis of the psychological insight which will be derived from such close contact. Obviously, conditions in France and in England were rather different from conditions in Germany. In France there were only a handful of good -to first-class- physicists active. They formed a rather closely knit group and were reinforced by intermarriage between the different families. Positions were hardly given on the basis of family relationships unless the men met the standards required, yet in filling positions there was hardly much weight given to the number of papers which anyone had published.

In England much stress was laid on personality and less stress on achievement. Many physicists in England had a sense of security which might have originated earlier when they used to have private incomes and the feeling of security may have survived the source of income.

It is difficult to be too definite about these things as far as England or as far as France is concerned because of the fact that the men who were responsible for the important advances are so few in number and therefore what you might call the statistical error is rather formidable. In France practically all the significant steps were taken by one man, namely, Joliot, frequently working in collaboration with his wife, Irene Curie. In England practically all of the major important advances originated with three or four men, all of them from Cavendish Laboratory in Cambridge, and for all we know, they may all have originated with one man, Rutherford, who was at that time in charge of the Cavendish Laboratory.

Another consideration should remind us of the need for caution. Theoretical physicists were exposed in Germany by-and-large to the same social pressures as experimental physicists. Yet theoretical physics flourished in Germany as well as in the rest of Europe. Unless we can explain this satisfactorily, our interpretation for the stagnation of experimental physics will have to be abandoned.

It is our contention that the theoretical physicists were less sensitive to the social pressure which was mentioned earlier, although they were not entirely to them. A certain element of selection may have been involved. Theoretical physics was no practical profession with which a man could count upon earning a living. It was an addiction, rather than a profession. The men who chose it as a vocation were the few who were so passionately interested in the field that they brushed aside the practical considerations which prevented others from going into this field. These men were willing to take risks, but on the other hand, the risks which they had to take were considerably smaller than those which their colleagues in experimental physics would have had to take if they had tried to follow their example. It is easy to understand why this is so. As long as a theoretical physicist has leisure, and leisure is nowhere entirely in disrepute in Europe, it will be possible for him to pursue any idea his heart desires, at least for a few hours, days, weeks, and even months. Ideas may come to him in the morning when he wakes up.

He can certainly pursue them while he dresses, or in his bath, as long as he is willing to permit himself the luxury to let himself go, rather than try to meet schedules. After a few hours or days of thought, he will know whether he is on the right track. He can follow a hunch without investing two or three years of experimentation and ten or twenty thousand dollars in equipemnt as his colleague in experimental physics may have to do. Moreover a theoretical physicist can work alone. All he has to risk is his own time and advancement. In experimental physics nowadays many problems require the work of a group of people and this means that a young experimental physicist would have to persuade a number of his colleagues to take the same risks which he might be willing to take. Even if the others might be willing, the decision to sk others to take risks is a very difficult one and I know of no one who feels that he can make such a decision on the basis of a hunch. Unless he is in the position to give financial compensation to his collaborators. That is, unless he has a means to work with paid assistants. It would be a rather fascinating and instructive task to compare the advances made during the war in England and the United States in the field of radar and other fields of war applied xix science, but the history of wartime research cannot yet be written. Yet, the balance is clear even with the details still shrouded in the official fog. And the highly organized research under the OSRD in the United States was by-and-large an efficient machine. It was efficient in solving problems which were assigned to the various laboratories by committees set up for that purpose. Problems were solved speedily and efficiently kxxxxxxxxxxxxx but recognition of the problems came almost invariably from men who were in the scientists organiation. A great majority of them came from England and originated with just a few men who worked, you would say, under much less favorable conditions, less favorable in every respect, except from the psychological point of view.

What practical conclusions should we draw from these considerations--if we have funds available and want to use them for the promotion of science. If a National Science Foundation were set up as proposed in a bill now pending in Congress, the Government would probably provide fairly large amounts annually for this avowed purpose. As far as physics and biology is concerned, I believe that the best use of these funds would consist in putting into effect a scheme of the following type: first of all, we would have to pick out, as early as possible, somewhere between the ages of 26 and 30, each year a number of men who have shown that they are capable of independent thinking and have the stuff to be good research workers. These men could then be appointed for life as Fellows of the Foundation. Each Fellow would draw a salary of perhaps $12,000.00, generously corrected upward or downward, according to the number of his wives and children. Each of these men would be perfectly free to pick any problem he wants to work on and, if he is willing to invest out of his salary a sum up to $5,000.00, the Foundation would match each $1,000.00 he invested with another $4,000.00. Thus, if a Fellow is willing to live on $7,000.00 he would have $25,000.00 (a maximum under this scheme) available for his research work. He could go to any university or research institute, present his problem and state what fraction of his salary he is going to invest, which according to the above determines the total budget that he is bringing along. It may very well be that quite a number of these Fellows would rather loaf, retire to California, or go big game hunting, and live on their full salary, rather than spend a cent for their research work. If so, they are good riddance. The fact that they will not clutter up the laboratories and fill up the magazines with worthy, but unnecessary, papers, will improve the general quality and spirit of the scientific community. Surprisingly, some of them might even produce extraordinary results, and after many years of gentlemanly leisure suddenly open up somenew field of knowledge which previously did not even have a name.

All the present schemes which were so far discussed in connection with the National Science Foundation have the shortcoming of promoting the solution of recognized problems, rather than promoting the recognition of unrecognized ones. It is the last type of activity, though, which controls, under modern conditions, in my opinion, the speed at which science progresses. Once a problems is recognized there under modern conditions any number of people available to work on it.