FIRST DRAFT

September 21, 1948

"SCIENCE IS MY RACKET"

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"One in eight will die of cancer;" and so you are asked to contribute to the cancer campaign, and you will probably do so, if it so happens that your mother or father or brother died of cancer.

One in eight will die of cancer this year, and most probably also next year, and quite possibly also 10 years hence; and maybe even 20 years hence. ~~But that~~ the day will come through when cancer will be a curable disease is as good as certain, and the day might not even be far off. But will your contribution bring the day nearer? Most probably it will not. Why?

Because the organizations which disburse funds for research, whether they are private or governmental, whether the funds are given for cancer of for polio, whether it is in the field of biology, chemistry or physiology, have not learned how to use funds in a manner that will serve the purpose.

The money spent for research is often wasted, because money can be spent for research wisely only if it is spent under an arrangement that will recognize first that scientists are human beings, and secondly, what kind of human beings they are. Since the arrangements under which the funds are spent do not recognize the human element involved, most of the public support of science does little good, and moreover there are even signs that it may jeopardize the progress of science by putting temptations in the way of scientists, which they find difficult to resist. Science is in danger of becoming a racket, and sometimes it seems to me that it is well on the way of becoming a racket.

~~No one has a right, it seems to me,~~ Since we are born with a right to complain that things done are done badly, unless he is able to say how things could be done better. Because I wish to complain ~~(not to say~~ to accuse) ~~it is my duty to~~ I intend to say "how to do it better" and ~~it is a duty which I wish to discharge first.~~ I shall start out by doing so!

There is an important distinction here that you will have to understand

first of all, and that it the distinction between pure and planned research. The case of atomic energy may serve to illustrate this point, and so may the case of cancer, but the lesson to be learned is not limited to these two fields.

In 1932 I was in Berlin and talking to one of the most interesting men I had ever met. Otto Mandel had been a successful lumber merchant and as a publisher ~~it was~~ he had made was due to him that H. G. Wells ~~became~~ a popular author on the European continent ~~at an early date~~. "Man can not live happily unless he can find an enterprise towards which he can direct his creative abilities" ~~said~~ Mandel to me ~~-~~ "An enterprise that will satisfy the heroic streak in man's ~~xxxxx~~ nature. In a purely idyllic life part of man's nature is starved. Unless ~~our~~ craving for the heroic can be satisfied, mankind will devastate itself in never-ending wars or else will lead a frustrated, dull existence." I did not take down his words, of course, but I have a very vivid memory of ~~this~~ conversation, and I am ~~xxxxxxx~~ reporting it as I remember it. "~~And~~ I know ~~now~~," ~~said~~ Mandel, "what such an enterprise might be that could ~~get~~ hold ~~of~~ man's imagination, an enterprise in which the whole world could unite ~~its efforts~~ and that would satisfy the heroic instinct within us. It is the colonization of other planets. Just as once the species from which we have descended lived in the sea, and then one day began to colonize the land, the time ~~may~~ will come when our species will begin to migrate ~~further~~ forth from the earth. The construction of rockets that are capable of sailing out into space and later the organization of expeditions away from the earth is an enterprise ~~of gigantic proportions;~~ ~~it~~ that might well absorb the surplus energy of mankind for a long time to come."

It was a fantastic thought, bordering on the insane, but thinking back I am glad to ~~record~~ recall that I did not shrug it off with a laugh, but replied in all seriousness "To construct a rocket that will leave the earth might not only be difficult, but it might be impossible. If we could tap nuclear energy, it should not be too difficult to construct such a rocket, but the tapping of nuclear energy might also be an unsolvable problem. ~~However~~ Yet, if you have a million dollars to spare, and want to entrust it to me ~~xxxxxxxxxxxx~~ to promote your project of securing the peace of mankind, I ~~would want~~ shall

to spend it on promoting the progress of nuclear physics. But this trouble which we would be faced. I do not see, nor does anyone else I know of see, any way in which nuclear energy might conceivably be tapped; so all we can do with one million dollars is to go around to the best nuclear physicists, keep my mouth shut about tapping nuclear energy, and merely ask them if money would enable them to make faster progress and if so, under what arrangement they would like to receive it. If they were willing to take the money under some half-way reasonable arrangement, I would just give it to them, without ever mentioning tapping of nuclear energy."

In 1932 , when the conversation took place, Otto Mandel did not have a million dollars to spare, but this is beside the point. The point is that in 1932 the problem of tapping nuclear energy was a case of supporting basic research in nuclear physics, and not a very good case at that. It was not a case of planned research by any stretch of the imagination.

Three years later the situation and my own frame of mind were entirely different. What changed the situation was Chadwick's discovery of the neutron. Now I could see how nuclear energy might conceivably be tapped.

In the fall of 1933 Rutherford was reported to have said at the British Association that however talks about the tapping of nuclear energy is talking moonshine. Remarks of this nature, particularly uttered by distinguished scientists, are always annoying. How can anyone say that some one is just "Talking moonshine" before the other fellow had even begun talking! Walking along Southampton Road in London I stopped for a street light and as the light turned green again and I crossed it occurred to me that if any one of the 92 elements would emit neutrons when exposed to a neutron irradiation it might conceivably be possible to set up a nuclear chain reaction and thereby achieve a tapping of nuclear energy.

Later, in spinning this thought out further and trying to visualize the consequence of the existence of such a phenomenon, I arrived by 1935 at a strong subjective

conviction that a systematic survey ought to be made of the 92 elements to see if any of them emitted neutrons when exposed to neutron irradiation. It was difficult to guess whichelement would do the trick, if any. I suspected uranium a little, but not much; I suspected indium more than uranium, and also bromine, but I felt that my reasoning was on shaky grounds, and realized that this was a case where virtue lay in not attempting to be clever, but to be deliberately stupid and the thing to do was to test all of the 92 elements for neutron emission. There was a chance in 100 or so that one of them would do the trick. It seemed that such a survey could be carried out for $10,000, and it seemed that it was worth $10,000. Knowing that I was following a hunch and not a very well substantiated one at that, I felt that I could not afford to devote all my time for a year or two to a survey of this sort, and at that stage, doing so would have indeed jeopardized my career as a physicist. But with $10,000 it would have been possible to obtain technical assistance. I thought the survey ought to be done, and I was willing to spend half of my time at it.

So I turned for help to friends, to raise the $10,000 needed for the survey. First I talked to Professor Charles Singer. He and his wife were very nice to me when I first came to England, and I went to see him to tell them my story. I thought that perhaps if they saw the point, Mrs. Singer's brother, Sir Robert Widdicombe, at that time President of Shell Oil, might also be interested. Charles Singer was Professor of the History of Science at University College, London, and he found it somewhat difficult to visualize what kind of an animal a chain reaction might be, it had played no role in the history of science! So next I thought of going to someone who knew at least what a chain reaction might be. Chain reactions on the molecular level (though, and not on the nuclear level) play a certain role in chemistry. Prof. Chaim Weitzman was a chemist. I thought at least he would know what sort of a thing a chain reaction was. So I went to talk to Prof. Weitzman. He was very nice about it, and told me he would see if he could get me the $10,000 which the survey I proposed would cost. I was not sure, though, if he wasn't just polite about it, and if he didn't secretly think that the strain was just too much for me, and that I had cracked. To make sure, I asked Michael Polanyi, an old

friend of mine, and at that time head of the Department of Chemistry at the University of Manchester, to talk to Weitzman again on my behalf, and to testify to my sanity, which he did.

After the war I met Weitzman by chance in Washington, and he reminded me of our conversation in '35. He told me that he really tried to get me the ~~$10,000~~ for which I had asked, and he found that he couldn't get it. The point that I ~~am making~~ is that in 1935, in contra-distinction to 1932, the problem of tapping nuclear energy had become a problem of planned research, because in 1935 ~~it was~~ possible to state that a survey of the elements for neutron emission ought to be made; i.e, ~~it was possible to~~ indicate a specific avenue of approach to the problem of tapping nuclear energy. But if in '35 the problem of tapping nuclear energy ~~had become~~ a problem of planned research it was not a very good problem, for it was not possible to give any valid reason for why any of the 92 elements should emit neutrons if exposed to neutron irradiation. ~~The fact~~ that my faith in the possibility of such a phenomenon was very strong in 1935 ~~is not relevant, for it was faith based on hunches, and as a matter of fact, this faith~~ itself ~~failed~~ by 1938, ~~to the extent that~~ by that time I was no longer willing to spend even half my time on such a wild goose chase as this survey would have represented. And even in 1935, when my faith was strongest, I ~~would have been~~ unwilling to risk my scientific reputation by appealing to any of the foundations for financial support, and chose rather to pin my hope on some individual, either sufficiently enlightened to see the justification for such a survey, or sufficiently ignorant to be unaware of ~~how~~ slim the chances of success for the survey were, according to current scientific opinion.

In February 1939 the case for tapping nudear energy had suddenly become a very good case for planned research. In January news reached us that uranium is split by neutrons in two about equal halves. I could see very good reasons why neutrons should come off in the process also, and that there was a good possibility that it might be feasible to set up a chain reaction on the basis of this neutron emission. It seemed necessary, and

not only necessary but also urgent, to test whether or not neutrons were in fact emitted from uranium which underwent fission, and again I was turning to my friends for help to raise the necessary funds. Just as I was a newcomer in England, now I was a newcomer in this country, but a friend of mine loaned my $2000 so that I might rent a gram of radium for a year, and having done this with the help of this gram of radium I we were able to show within a few days that neutrons were indeed emitted from uranium. By August of 1939 the case for planned research became a very good one indeed, for by that time I was able to say specifically that what we want to do is to try to set up a chain reaction in the graphite - uranium system. But even though by September of that year I was allied with Fermi and had the support of Dean Pegram of Columbia University and the support of such men as Albert Einstein and Prof. E. P. Wigner in Princeton, and even though we succeeded in getting the President to appoint a committee through which we could deal with the government, the support which was forthcoming amounted to $6000 in March of 1940 and $40,000 in November of 1940. This is not the place to analyze the reasons for this exceptionally bad fundraising performance; they were manifold, but lack of showmanship on my part was undoubtedly part of it. It should be remembered however that scientific ability and showmanship do not always go hand in hand, and that if the handling of funds is guided more and more by showmanship, as seems to be the rule, rather than the exception, in the present post-war period, the results will not be healthy for the progress of science.

If we now take the case of cancer as another important example, I should be inclined to say that this is a case for planned research a little more than atomic energy was a case for planned research in 1935, but perhaps not quite as much as it was in 1939.

It is quite possible that none of those avenues of approach which anyone today can name will actually lead to the solution of the cancer problem. It is quite possible, and some of my friends would say that it is likely, that the solution of the cancer problem will come through some unforeseen advance in physiology or immunology or biochemistry -

some unforeseen advance in some of the branches of biology. If that were really so, then the best course we could adopt in relation to the cancer problem would be the course which I proposed in 1932 to Otto Mandel in relation to the problem of tapping nuclear energy. But those who contribute to the cancer campaign today would probably feel cheated, and perhaps with some justification, if all the funds collected in the campaign were spent for the support of free research in the general field of biology. If these funds were all spent for free research, and if they were well spent, we would make progress, even though the progress might be slow.

But spending funds well for free research is a job even more difficult than spending funds well for planned research. Let us then turn now to the easier task of the two.

THE CASE OF PLANNED RESEARCH

When the war was over, I did not continue work on atomic energy. I had contemplated turning to biology in 1933, but the exciting possibilities which I saw opening up in physics deterred me from switching then. Now, after the war, a combination of my own inclinations and forces of circumstance led me to begin to work in the field of biology. My work does not lie in the field of cancer; there are more interesting things in the field of biology than the problem of curing cancer, just as there are more interesting things in physics than work on atomic energy.

But I am beginning to see certain conceivable approaches to the problem of cancer, and by the time this appears in print, I might be as far advance in the realm of pure though towards the solution of the cancer problem as I had advanced by 1939 towards the tapping of nuclear energy. If this should in fact happen, and let us assume for the sake of argument that it does, I would probably consider devoting 6 months out of the year to this problem. It is not what interests me most, but members of my faimily have died of cancer, and I am myself at an age where cancer begins to take its toll, and perhaps I am also influenced by the pronouncement of the cancer campaign blaring at me every so often over the radio. Whatever the reason, I am not unwilling

Please remember though that 3% chance of success means 97% of failure. ~~Don't~~ So do not expect me to ask any of these men to give up their own line of work and to come (unconnected with cancer) along with me to face ~~the~~ almost certain failure. ~~Never mind that~~ I want to ~~have~~ a team of scientists not a suicide squad, build,

I ~~am~~ shall not attempt to offer any of these men jobs ~~at~~ in my laboratory or try to license them jobs at my university

to do something about cancer, if I see my way clear to it.

As long as my ideas remain on paper, I could never be sure that they will in fact work out, and I would hardly be willing to stake on these ideas my entire scientific life, however good these ideas may look to me. In the most favorable case, that is at all likely, I might see the path leading to the solution of the cancer problem in the manner in which one sees from the valley the path leading up to the top of a mountain. One sees where one has to start and one sees it go up the slope for a stretch, then it disappears from sight, and one has to guess which way it may turn. Not till you get up there are you able to see which way to go on, and it may take some probing and guessing even then. In the most favorable case that is at all likely, if I should succeed in mapping an approach to the problem of cancer which subsequently might prove to be the correct one, the proof will not be forthcoming until there has been further work and exploration by biochemists, immunologists, etc., and not just by any biochemists and immunologists, but by very good men (who are rarer than the public realizes and the professionals care to admit) who are deeply interested in the problem, who see eye to eye with each other on the specific approach chosen, and who are willing to collaborate with each other on this specific approach.

How does such a team come into existence? Clearly, it has to be created but how can it be created?

If I were faced with this situation I would go around to foundations and to private individuals, and I would try to speak to them in this manner: "Would it be worth your spending 20 million if you were certain that we could produce a cancer cure within 7 years? If so, don't you think you should be willing to invest half a million dollars for an approach that has a chance of 3% of leading to the cure of cancer? If you are willing to go along and let me have 75 thousand dollars per year for 5 to 7 years, I shall take the next 6 months off to see if I can interest really first class people in the various aspects of the particular specific approach which I have chosen, which have to be worked out in detail. I am not going to attempt to offer these people jobs in my

He ought to be compensated. But no ~~however we may~~ sum that I may be able to allocate for this purpose but at a 50-to-75 000 dearly point will fully compensate him in case of the loadline which more latterly than not we ~~shall~~ are facing; He will shall have to take some risk but ~~with~~ good men time to take risks, within reason that is. —

laboratory. If I did that, a half million dollars would not go a long way, and moreover the men I would want I probably could not get at any price. Most of them are quite happy were they are. The work they are doing is interesting to them, they have security and a salary on which they can live. Maybe they do not have quite enough salary, maybe they have a little more teaching than they would like to have, maybe they do lack some equipment or technical assistance which would speed up their regular work.

I would try to interest them in the general problem, and in the specific approach proposed. Some of them might see it as I see it. Of those who think that the approach I porpose has sufficient promise, some may be willing to spend 6 months out of the year on this kind of work, if I could arrange to free them from their teaching duties for a year or two, if I could provide them with additional equipment they would need for this special work, and perhaps some technical assistance to help with special work and their regular work. If it is a case of a younger man, and in most cases it would be, perhaps an Assistant Professor who has a salary of $6000, I would want to supplement his salary for the 6 months which he spent on this special research by 2 or 3 thousand dollars, which would bring up his annual income maybe to $9000, if in his opinion and he was entitled to such additional compensation.

The organization which provides the fund can well argue that if it is worth spending 20 million dollars for a certain cure for cancer, it is worth half a million for a cure which has a 3% chance of success. But an Assistant Professor cannot very well afford to spend 6 months in a year for a number of years on a cure for cancer that has a 97% chance of failure, yet this is what we are asking him to do. More likely than not, the time he devotes to this reseach project will slow down his career, his promotions, will be delayed, his reputation will not be enhanced. It is true though that by leaving him in his own laboratory, in his own institution, he will get credit for his contribution if any progress is made by the team. His own institution will see to it that his contribution should not reamin unnoticed by the world. Moreover, he will retain a feeling of independence. If I had asked him to move to my laboratory, he would have lost his feeling of independence and he would have received little credit for his contri-

bution, for in that case, whether I wanted it or not, most if not all the credit would ~~have gone~~ to me. Leaving a man in his own institution has many advantages. ~~My fund~~ The grant may have to provide for all special equipment, but all the regular equipment it is at his disposal already. His own work is not disrupted. and he remains free to pursue it during 6 months of the year.

He would retain his feeling of independence and get all the credit for his contribution, which he deserves. His institution would be proud of his achievement and he could expect promotion if he made good in this collaboration, as well as in his individual line of work which he could still pursue for 6 months of the year. On this basis excellent men might be willing to collaborate on a project provided they would feel that it was a good project, with an appreciable chance for success, small though the chance might be. In team work organized on this basis, assuming that there is some compensation for the man involved above his regular salary, assuming that some compensation will have to go to the institution if the man is freed from his teaching duties, assuming that some special equipment is involved which will have to be provided, and some technical assistance, we may assume that the cost for each collaborator of this type on the team may amount to 10-15 thousand dollars. 50-75 thousand dollars a year will ~~go a long way is spent on this basis.~~ maintain a team of 5 first class men.

If on the other hand, I attempt to bring a man to my own laboratory, he would lose his feeling of independence, he would not look towards a career at my institution, he would have no security, and he would get little credit for his contribution to the work of the team. He would lack the equipment for his special line of work, all the regular equipment which he would need for his work on the project would have to be purchased, the man would have a housing problem to face, his wife might be unhappy, in her new surroundings, space would have to be provided for him. Since space would have to be provided for him, which means that funds would have to be spent on enlarging the laboratory, and even so it would remain overcrowded, if the man were good and had

any sens, he would not come, and if he had no sense he would be of little help.

This then now in my opinion ~~is not~~ planned research should be carried out in those fields where it is possible to have team work within geographically separated groups.

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