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PHYSICAL SCIENCE AND ITS APPLICATIONS TO INDUSTRY.

By Professor W. H. BRAGG, F.R.S.

I HAVE been asked to say something about physical science and the way in which physical science and other sciences have an effect on industry. It is a general topic of discussion, now-a-days, this matter of science and what people call its neglect. It is pretty easy to bring forward a number of instances in which it is clear we have not done all that we might. We can see in many cases that we might have been much better prepared for the war; in aeroplane construction and in many other things we certainly had not developed ourselves—our own forces—as much as the Germans had done. We have heard great tales of what the Germans had ready for the war and how they thought out all its possibilities, and how the things which to us were utter surprises were to them no surprises at all. That is quite true. We are told, too, that we have been neglecting science in the matter of manufactures. I think there is truth in that also. The fact is, I believe, that in England we have been so well off that we have not worried. We have been good workmen for many years, our wares were good, we had our own ships, and our trade extended, and our factories were full. There was no need why changes should be made, particularly when, without much trouble, the money kept coming in sufficiently well at least. Therefore, in this matter we have shown up badly compared with other nations—America and Germany, for instance, who had to fight hard and put in their best work and employ all available means if they were going to get any trade at all. Then, again, we have allowed certain industries to be wholly captured by foreign nations. Take the glass industry and the patent medicines which, we discovered when the war began, had been made in Germany. It was a serious hindrance to us that nobody here could produce the things we were wanting.

But whilst you try to consider what there is in all this, do not let us assume that things are worse than they really are. First of all, it is only fair to say we have in England quite sufficient ability to produce inventions and quite sufficient scientific achievement, to justify us in believing we have all the science at our disposal we really want. I do not want to say this in any spirit of boasting, but it is only fair, if we are proceeding to think whether we have been doing wrong and might have done better, that we should look on all sides, and we must not make the mistake of blaming ourselves for sins we have not got. If we take the scientific side, in the matter of scientific discovery England has done more than her share, and has been doing it steadily for years past. It is a fair test to take some new science—something which is new to the world. The study of radio-activity and radium has become a science of its own, and it is a very big subject. All the nations started on the same line in respect to it. As a matter of fact, France got perhaps a little start in that Madame Curie was the first person to isolate radium at all. But if you go through the text-books on radio-activity you will find that by far the greater portion of the actual experimental discoveries were made in the British Empire. I think that is a very fair test, and it does not seem to me that you

can get a very much better one. It shows that we have in the Empire people who, taking up a scientific subject, can develop it as well as any other nation. Neither have we any right to blame ourselves for want of practical skill—though that is a subject on which you know more than I do. Of what fault then may we be accused? It is difficult to put the whole case in a nutshell, but probably we come near to the truth when we admit that though we have inventive ability and practical skill, we are not adepts at bringing the two together. I might mention one instance of the way we are found to be possessed of the necessary skill as soon as we are called upon to employ it. It is the case of glass. You know the Germans have been masters of the glass industry for many years past, but since the beginning of the war many of our own people have been working steadily to make up lost ground. I was told the other day by a man largely employed in glass production that we are doing extremely well, and that we have already in certain lines of glass manufacture outstripped the product on which we wholly depended upon Germany before the war; and, as a matter of fact, we have in England better manufacturers of glass than in Germany in one way at least. There are in the German catalogues an enormous number of receipts for glass, and I was told the reason for this is that they often fail get two meltings alike. We have almost neglected this industry, and if it had not been for Chance, of Birmingham, and one or two other makers, we should have been without the skilled labour to go on with the production of glass and glass instruments.

We have now all become suddenly anxious lest we are not doing the proper thing in encouraging scientific research, and various committees have been formed in England for the purpose of remedying the defect as far as the war is concerned. It is a little bit late, because it is like putting your fertiliser on your acres of land just when you expect to reap the crop. Nevertheless, we are very glad to see it done, because it will come to fruit in time, and there will be no harm done; in fact, there will be a certain amount of good. Of course, you can put it baldly, and say what we are all trying to do is to bring the inventor in touch with the practical man; but much depends on what you mean by an inventor and what you mean by a practical man. You can have extremes in both cases. If I may speak of the extremes of the practical man, there is a man whose faith is that he practises what he has been told, and what he does not practice is not practical. There is also a certain kind of inventor against whom I can understand the grudge of the practical man, because he simply collects stray facts from books and from other people's sayings and brings them together and then says: "Here is a beautiful idea. I ask £100,000, but I will be content with £10,000." It is most difficult to get into such people's heads the important fact that it is a very long way between an idea and its application. It is all very well to say—"Why cannot they bring those Zeppelins down? All you need to do is to take two stations a mile apart and direct a searchlight from each of them and you can then, by a calculation in trigonometry, find where it is." I suppose hundreds of people have thought that; but to get from that to a real instrument which actually does the trick and enables the artilleryman to hit the Zeppelin requires hard work and high skill. However, I imagine that ideas and work and skill have all been forthcoming if we may judge by recent events. No, a certain kind of inventor is a very trouble-

some man. A friend of mine tells me that he was apprenticed to a patent agent who had a long experience of such, and his final conclusion was this: that every inventor, as soon as he has made his invention, ought to be drowned.

I say these things because I would like it to be clear that there is no reason why we should look after these people with stray ideas, or help them to worry practical men. We have a work to do which is much finer and more full of method and purpose. We have to show that industry is indebted to science, not for accidental and undeserved discoveries, but for patient and organised research into the laws and facts of nature. Lest it be forgotten how great is the debt, let us take one or two illustrative cases. On Christmas Day, 1821, Faraday called his wife down into his laboratory to look at a magnet going round a current of electricity. That was the first time anybody had seen an electric motor. Now, think what the electric motor industry is! That was pure science, because Faraday was experimenting purely with the purpose of getting knowledge. He had not the faintest idea what his experiment was going to turn into. How shall we put it into figures? I can take some from an authority, an old student of mine, who was speaking recently in America on this subject. He pointed out that a few years ago the light and power stations in America were valued at 200 million pounds sterling and the telephone industry in the United States was valued at 180 million pounds sterling; and between them they had an annual revenue of 30 million pounds—not dollars. And all this actually flowed directly from that little magnet going round the electric current on that Christmas Day. That was pure science, and that is the result. You could not have had that prodigious industry without that experiment. We may take another example. The waterfalls in the United States are supposed to be running 150 million horse power to waste, and every one could possibly be harnessed by electricity. I am dealing with big figures. Pasteur investigated the action of certain baccilli, and found the cause of anthrax and chicken cholera and the silk worm disease; by this last remedy alone it is said that he saved to France an annual figure equal to the whole indemnity that France paid to Germany at the conclusion of the war in 1870. That was a piece of pure scientific research, and that was the result. If it is a case of considering whether scientific research does lead to pounds, shillings and pence, well, it certainly does. A little while ago, I was asked to give a lecture in the United States at the research laboratories of the General Electric Company. At that laboratory they concern themselves largely with improvements in electric lamps, chiefly the filaments and the construction of the bulb. They run the laboratory for that purpose and they spend £20,000 a year on it. About 100 people came to hear what I had to say, and every one of them was engaged on experimental work; nobody was making anything for sale, and the £20,000 was spent wholly on research work. The Director, Mr. Whitney, estimates that, as a result of the investigations on the methods of the preparation of the bulb, 50 million pounds have been added to the value of electric light. Or, again, to come closer home, when Sir William Perkin did his famous experiments at Manchester—"pure science" experiments—he created the value represented in the enormous German industry in aniline dyes.

If anyone asks "What does science do for industry?" our answer is, "There would not be any industry at all if it was not for science."

In order to put clearly what is the relation between science and applied science, there are three principles which you may lay down. One of them is this: no one can foretell what scientific research will enable us to do; it is all hidden in the future, and you cannot, not even the most imaginative man, cast your ideas forward and say what is to come. It is a very common and fascinating exercise on the part of many clever writers to write a book about the year 1950, and try and describe all the wonderful things that will be happening. If you look carefully at such books you always find they never forecast anything but extensions of such things as we already have. The human mind cannot do it; it cannot throw itself forward; it is beyond us. Jules Verne wrote about the submarine, but he had seen fishes going under water, and he simply thought men might do what fishes do. It was a very fine exercise, and the book was very charming. But he never wrote a book about men flying in the air; we have had fables about men flying, but Jules Verne was too practical a man to think that could be done. When he wrote about going round the moon—that was a flight of the imagination; but it has not come off yet.

Take, on the other hand, an actual invention like wireless telegraphy. Above the roof of Whitehall there stands a mast, and connected with it is a network of wires; and from those wires spread messages from the Lords of the Admiralty to ships all over the world, and messages come back in return. No one ever foretold wireless telegraphy; no one ever had the faintest idea that we should be able to do things like that. The original things science discovers are utter surprises, and you never find anybody able to anticipate them. Who anticipated X-rays? No one. I am glad that it is not possible to anticipate the discoveries of science; it makes scientific research so much the more interesting. That is one principle that can be laid down. Another one, I think, is this: that a problem cannot be solved before its time. Suppose, for example, in 1850 or '60, or '70, you could have looked into the future and seen that the Admiralty was talking to its ships without wires, and suppose you had called together the scientific men of that day and said to them, "How is this going to be done?" there would not have been the faintest movement towards a solution; nobody would have known what step to take. The reason is that the discoveries which make these expansions of our powers possible are all made bit by bit, each discovery arising out of something going before, and until you have made one step you do not know how to make the next.

Consider the wireless telegraphy process and the principles upon which it depends. The essentials are, in the first place, some source of electric power, an induction coil, and a long aerial wire, upheld by a mast. Through the agency of the coil, surges of electricity are forced into the aerial, where they rock to and fro, generating electric waves which travel away over the earth's surface. At the receiving end is a similar wire, perhaps on land, perhaps attached to the mast of a ship. When the waves reach the wire, responsive surges of electricity move up and down it. At the lower end of the wire is the delicate receiver. In the early days this was a small tube full of iron filings, through which a battery was trying to drive a current of electricity. But because the contacts between the filings were not sufficiently close the battery was not very successful. When the surges of electricity passed

through the coherer the filings were slightly welded together, with the immediate result that the battery was now able to send through the tube a far larger current than before. Thus the manipulation of a key at the sending station caused a change at the receiving station, a change which could instantly be reversed by tapping the coherer and breaking up the abnormal contacts of the iron filings.

The whole process was made possible by the discovery of the coherer by Branly, in Paris; and had to wait until that discovery was made.

And, again, the process depended on the fact that for many decades men had been extremely interested in the phenomena of the electric spark. In the seventies, Maxwell urged the concentration of scientific thought on the spark, for, as he said, electricity was so intangible a thing that its visible effects in the spark must surely afford the best chance of an insight into its nature. As regards the induction coil, you must go back still further, to the time of Faraday in England, Rhumkorff in Germany, and Henry in America.

Unless these various discoveries had been made in their due course wireless telegraphy could not have been devised. Not one of them was hurried into the world in order to be useful for wireless telegraphy. Nor could the final step have been made until each previous step had been accomplished.

Thus it is not only true that a great discovery is never anticipated; it is also true that even if the result were foretold the process would remain unknown.

There is another point of considerable importance which is not always realised. Scientific research is not a mere turning over of the ground in the hope of finding new and important things. On the contrary, it is an ordered progress, moving step by step along certain lines from which it is difficult to break away. Think, for example, of the long sequence of investigations which led to wireless telegraphy, which I have already referred to in illustration. Faraday showed the behaviour of induced currents of electricity. Maxwell laid down the laws of the phenomena of Faraday, and was able to predict certain consequences. Hertz first realised those consequences, and men like Fitzgerald, Righi, Lodge, and others pushed the advance a stage further. Marconi was a student under Righi, and carried the principles of the lecture-room out into the open field. In all this work, occupying the greater part of a century, one step led to another, and the following step could not be made until that which preceded it had indicated the next advance. The number of lines along which such advances are being made is quite limited. They are familiar to all investigators of physical science. There are certain obvious routes along which we may advance into the country of the unknown, and we have not much choice of the ways along which we move forward.

To change the analogy, when you are unravelling a tangled skein you take one thread at a time and pull away until you come to a knot that you cannot undo. Then, perhaps, you start on another thread, hoping to join on again to the first, in time. But for long periods you keep to one thread only, following it wherever it takes you. In scientific work it is just the same.

As the student of science moves step by step in this way, he can only go whither he is led. It is not in his power to forecast the result which he will obtain, nor can he shape his work by any particular

practical result. He cannot, for example, make up his mind that it would be very useful to enable the Admiralty to speak to ships wherever they may happen to be, and then proceed to invent a method. On the contrary, the actual process is that an army of investigators marching along a certain well-understood route discovers all sorts of things by the way; and among their discoveries are some which turn out to be applicable to the purpose just mentioned. While experiments are in progress there is no particular practical result in view. If there were, there would be no progress to begin with, and no practical result to follow.

If, then, there are to be practical applications of science to industry, there must be scientific research to begin with, in which the domain of knowledge is increased without concern as to what use is to be made of it. Men must be encouraged to proceed with researches which do not seem at the time to have necessarily any practical bearing, but which are by no means arbitrary because they will be proceeding along the natural lines of which I have spoken. If this were not done the supply of ideas would fail, and the stream of vivifying thought on which successful industry really depends.

It is fair, no doubt, to expect that the State, which can afford to take the longest views, should be primarily responsible for the encouragement of pure scientific research. The State may subsidise Universities and Institutes where such work is done. But it is not unfair, I hope, to lay these considerations before you who are manufacturers on a large scale and managers of great industrial concerns. You will recognise how much has flowed from pure scientific discovery in the past, and how meagrely the debt has been repaid: and you are directly—nay, vitally—interested in the continuance of pure research in the future. You may think it right to take some of this burden on yourselves as private contributors, though you cannot expect exclusive returns. It is an old-established British practise to which we owe some of our finest institutions and some of our best work.

Apart from that, it is becoming more and more the custom for great firms to build and equip their own scientific laboratories. The General Electric Company has, at Schenectady a laboratory on which it spends £20,000 a year: where the physicists and chemists work on lines which are only broadly defined. The men are not there to make so many useful inventions, but to explore a field of which it is desirable that the Company should know all that can be discovered for the proper and vigorous prosecution of its business. Already, its profits—speaking from a purely financial standpoint—have far exceeded the expenditure. Think of the power which it gives to a firm like this to know through its scientific workers of all that is going on in the scientific world which may be of use to itself, and even to take a lead in discovery in certain directions. Whole new lines of business suggest themselves, and the old are continually improved and re-directed.

Let me once more try to state my point concisely. All the great increases of knowledge are in the beginning the acquisitions of pure science: pure in the sense that they were acquired in the course of the steady advance of scientific research. The advance takes place along certain well-defined and well-known lines. Applied science charges itself simply with the application of discoveries as they are made to aid industries and manufactures: and the discoveries come at their own time; they are not made to order. There is no spasmodic hunt-

ing for odd things that may be useful. There is a noble and dignified progress of knowledge and scientific research which should have the sympathy of all of us and receive our encouragement, and from which the nation, as a whole, and we individually, will reap benefit.

VOTE OF THANKS.

The CHAIRMAN: I am going to ask Mr. Warner to propose a vote of thanks to Professor Bragg for the most inspiring words he has said to us.

Mr. FRANK WARNER: I rise with very great pleasure, although I was unaware I was to be called upon, to propose a very hearty vote of thanks to Professor Bragg for his intensely interesting lecture. Professor Bragg has given us, in homely terms, very wide views of science, and I feel sure that we business men will go away to-night much better informed and having acquired a more appreciative view of pure science than we ever held. We, as members of the Textile Institute, have recently been considering, and have even gone further, and have taken up the question of research in connection with the textile industries. That in itself is a most useful work, and its object, I may say, is to find out something we do not know, but which we feel we ought to know. The object of pure science, which Professor Bragg has described, has a wider aim, and we have to realise that its study must come before everything else and must be continued and pushed with all the energy we have, from the Government downwards. I am very glad that Professor Bragg, at the outset, discouraged anything like a condemnation of what we have done in the past. It is no use looking back and saying we did not go in for scientific research when we should have done. The fact remains, and stands out boldly, that our textile industry has held its own, and more than held its own, against the competition of the world. But we come now to a great awakening. The war has made us look around and has made us realise that we have something more to do than go along the old paths. Amongst the things coming to the front is the idea that each industry, by joint action, can advance its manufactures by means of scientific research. It is a very healthy movement, but one thing we have to avoid at the present moment is overlapping in our efforts. Beyond that there is no need to utter a word of warning because the whole movement is in its infancy. We have to go further, and encourage those in trade and manufacture to do far more than they have done yet, in coming together to air their difficulties with a view to putting before the Government such schemes of research as should be tackled without delay. We who have already taken in hand some research have been much encouraged by the sympathetic assistance we have received from the University of Leeds, and our debt of gratitude will be greatly increased by the reception we have met with here, and also by the enlightening lecture Professor Bragg has given us to-night. I want to say one word to advocate that, whilst we are doing something to clear up difficulties we are all aware of, we should not lose sight of another side of the question of the production of textile fabrics, and that is their beautifying, or, to put it another way, the improvement of our fabrics by a higher grade of artistic taste. If we are to get the same standard of artistic taste in design and colour as in other qualities of production, it must come from a more thorough study of art, and I was

much encouraged when I came into the room to-night to be asked by Dr. Sadler if I would take an opportunity to talk over with him the question of what can be done to improve our designing for those trades that require it. In the highest grades of goods—figured goods for dress purposes and materials of that character—the foreigner has been leading the way, and we have some leeway to make up to get on a level with him. When I see the enormous Board of Trade returns of our exports, I want to know something more about them and as to what the figures actually represent. I want to feel that when we export goods we are exporting articles into which we have put the utmost possible in the processes of production and manufacture, and that we are not simply spinning threads for the manufacturers of other countries to weave into beautiful fabrics. In proposing this vote of thanks, I should like to say how much, personally, I have enjoyed the lecture.

MR. GEO. GARNETT : It is with very great pleasure that I respond to an invitation to second this vote of thanks. On this occasion I must for the moment dissociate myself as a member of Mr. Lowden's Committee of this University, and speak as a member of the Textile Institute. I do not think we could have met under more happy auspices, and had a subject that could have energised us more and given us a vision of the greatness of our possibilities. We have had most happy illustrations, and we have had shown to us clearly not only the beauty and delight that can come from study of this kind, which is the first essential of all great progress, but we have also seen the £ s. d. side in the illustrations given, and Professor Bragg has proved to us beyond all question how the great developments of the future rest upon the great, pure science discoveries from time to time. When I saw the programme of this Institute Congress I had a thrill of joy, because I felt we were going to appreciate and understand some of the great work this University has done, is doing, and will do in the future. We have had put before us on two memorable occasions how the Textile Institute could provide the nucleus of a great work. Those observations, first by Sir William M'Cormick, at Manchester, and more recently by Mr. Sadler, at Wakefield, were much appreciated, and it made us feel that we were the trustees of an Institute that could do great work if we only rose to the occasion. I have had dreams of what this Institute could do, having heard from Dr. M. O. Forster of the Institute of Chemistry which the chemists have built for themselves, furnished, and equipped with everything that could be a stimulus to scientific research. And I have been hoping the day is not far distant when this Institute will be so strong that we may have our own home in the same way. The chemists have had to wait a long time before they could achieve what they have done, and we should not think of doing anything at present; but I believe there is a power, a policy, and a spirit amongst the members of the Institute that can achieve something creditable to the textile trade, and it is at this time when we meet at the shrine of a great educational institution like this that we feel we are being influenced in a most magnetic way to achieve something in the future. The address we have had to-night will help us in to-morrow's programme, and give us most pleasant recollections of this gathering. I have great pleasure in seconding this vote of thanks to Professor Bragg for the delightful, instructive, and inspiring address he has given to us.

The CHAIRMAN : Before putting this vote of thanks, I should like

to give you a bit of history which the address has called to my mind. You all know that this University grew out of the Yorkshire College, and that it was part of Victoria University. Circumstances led to the application of this College for a charter of its own as a University. You all know how much we have been indebted, from the first, to the Clothworkers' Company, who founded and endowed at the very beginning the textile and dyeing departments. Well, when the Yorkshire College was drafting the suggested charter, our then Chancellor, Lord Ripon, pointed out that the first thing we had to do must be to consult the Clothworkers' Company. With great care, a scheme—a somewhat elaborate scheme—was drawn up which preserved their personal touch, and it was carried, so far as the textile department was concerned, to them for their approval. We flattered ourselves this was a most admirable scheme, and we went up with light hearts. Lord Ripon explained it all to a meeting of the Clothworkers' Company, and they listened. They were men thoroughly able to understand a big scheme, and they listened in silence, and then said they would talk it over. We went into the next room to wait. When we returned to the room the Chairman said they had considered it very carefully and they would have nothing to do with it, and if we persisted in that scheme they would hand the Department over to some other body. He said, "We are quite prepared to hand over the whole of the buildings we have built, to the future University to carry on the work it has been doing. We will endow it with what has been spent on it annually, £4,000 a year, and all we demand is that it shall be held in trust to carry out the purposes for which it was founded. We have come to the conclusion that any reference to us in the way you suggest would be hampering, and we believe as an integral part of the University and in close touch with the scientific work it will go to a higher level than it can possibly do if it is kept as a separate department." In reply, Lord Ripon said, "I think there is nothing more to be said except that we accept your proposal in every possible way." We afterwards learnt that Lord Kelvin thought out this course, and induced his colleagues to take his view. I feel that the address we have had to-night is a proof that Lord Kelvin was right.

The vote of thanks was carried with loud applause.