Dr Howard Jones

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IN MEMORY OF PAUL LANGEVIN

ONE SHILLING

THE ASSOCIATION OF SCIENTIFIC WORKERS, THE SOCIETY FOR VISITING SCIENTISTS and THE WORLD FEDERATION OF SCIENTIFIC WORKERS arranged a Reception and a Public Meeting on May 23rd and May 25th, 1947, in London in Memory of PAUL LANGEVIN.

RECEPTION

held at The Society for Visiting Scientists

5 Old Burlington Street, London, on Friday, 23rd May, 1947, with the Astronomer Royal, Sir Harold Spencer Jones, F.R.S., President of the Society, in the Chair.

After the Proceedings were opened by the President, Professor F. Joliot-Curie, French High Commissioner of Atomic Energy and the Rt. Hon. A. V. Alexander, C.H., M.P., H.M. Minister of Defence, addressed the guests, who included the French Ambassador, the President of the Society and other eminent persons.

PROFESSOR JOLIOT-CURIE (speaking in French) : On several occasions since the death of Paul Langevin, scientists, philosophers and workers have met to pay tribute to his memory. On these occasions, notable people from abroad have associated themselves with the fellow-countrymen of the great scientist, either by their attendance or by sending messages. The Frenchmen who are here today are particularly moved by the idea which has brought us together, and I know I am speaking on their behalf in thanking, through Mr. Alexander, all the people of Great Britain. Moreover, it is the gratitude of Paul Langevin to your great country that I bring here today.

It was at the great University of Cambridge that, after exceptionally brilliant studies, Paul Langevin came to continue his astonishing career. It was not without emotion that some months ago I saw again in the waiting room of the Cavendish Laboratory the figure of this young man with the deep-set eyes who was to become the master of us all, and whom both science and justice can claim as one of their best servants.

One of the former teachers of Paul Langevin was well able to see the considerable influence which his stay in Cambridge had had on the young physicist. On March 2, 1946, Marcel Brillouin, who was at the College of France, one of the teachers of Langevin, wrote him a letter from which I should like to read this short extract :—

> " What most added to your knowledge was the stay which you made, soon after leaving the Ecole Normale Superieurs, at the University of Cambridge. There you found teachers whose background was altogether different from that which the organisation of primary and higher education gave us in France, and this made it necessary for a mind already formed like yours to modify itself in ways of some importance in order to use to the best advantage the presence of eminent scientists such as Larmor and J. J. Thomson, and colleagues from the laboratory, who were to become rapidly famous, such as Rutherford, Wilson and Townsend.
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> On returning to Cambridge you were, in my opinion, obviously destined for a professorship at the College of France, and I had no difficulty in causing to share this opinion—which was supported, moreover, by Lord Kelvin—my father-in-law M. Mascart."

On many occasions Langevin spoke to me of his stay in England ; he often wanted to come back, and I know, notably from Lord Rutherford, that people here regretted that he did not come back more often.

Jean Perrin, whom I always couple with Langevin, liked to say that he would be very happy to come back to this world in a thousand years time, to see what had happened to the science of physics.

The historians of this epoch will no doubt regard the twentieth century as a very stirring period both from the point of view of physics and from that of the social life of men. In this upheaval, of which the two aspects I have mentioned are only in appearance independent, there are men who know how to dominate events and to hold high the torch of truth. Paul Langevin was one of them.

When he tried to place his work as a physicist in its proper perspective in the evolution of science he liked to say that he had lived through all the great revolutionary crises through which the science of physics had passed in the last fifty years. If he spoke at that time of certain difficulties in adapting the mind to the new ways of interrogating nature, we all know that not only did he conquer them, but that in many domains his own contribution was fundamental and his influence essential, as was shown not only by the extraordinary fecundity of his teaching but by the advice which he was always willing to give to the scientists of all countries.

Without wishing to try to give here a summary of his scientific work, I should like simply to recall that in the theory of ions, in the study of dia and para-magnetism, in the theory of electrical and mechanical birefringences, his work was absolutely fundamental.

The great movement of ideas created by relativity, quanta and wave mechanics, found in Langevin not only an adept and not only a magnificent teacher, but also a participator of the first importance, and the celebrated law of equivalence between matter and energy was established by him, independently of Einstein.

Finally, in a domain in which the Minister for Defence must be particularly interested, I would recall that it was Paul Langevin who solved in 1915 the problem of producing and detecting supersonic waves, thus putting into the hands of the Allies a weapon which showed itself during the two wars most useful in the struggle against the German submarines.

A pioneer in scientific work, Paul Langevin brought to the science of physics a contribution of the first order. An incomparable professor, he trained at the College of France, at the School of Physics and Chemistry, and at the Ecole Normale Superieure for Girls, generations of young people, showing to them always the living aspect of science, of science which develops and which seeks to obtain from nature a reply to the questions which we put to her, though always subject to the condition, on which he insisted, that we know what questions to put.

He did not think that he ought to limit his prodigious activity, however, to these tasks. His universal spirit and his precision of judgment, enabled him to analyse profoundly social problems and to adopt with regard to them an attitude which we all admire. Paul Langevin did not wish to be one of a small class of scientists remote from everyday affairs ; it was as a fighter, as a member of the great community of workers, that he interested himself in social problems.

Having fought for peace, for international solidarity and social justice, and having fought against racial theories, Langevin was an obvious choice as a victim for the Nazis. In spite of the risk, in 1940 he came back to Paris, and in October of that year he was arrested and thrown into prison, and afterwards confined at Troyes. His daughter, Hélène was deported, and a relation, the physicist Jacques Salomon, a fervent Communist, was shot.

All these misfortunes deeply wounded Paul Langevin without overthrowing for a single moment his courage and his certainty of the final triumph of justice over barbarism. I know that even those who do not share his opinions will be unable to consider without respect the act of this great scientist in taking his place as a fighter among the workers of his country.

The Secretary of the Academy of Sciences of Paris, the philosopher Fontenelle, had one day to praise Newton, and he wrote : " The English do not honour men of great talent the less for having been born in their country." The English also, as this meeting to-day and the presence of the Minister for Defence show, know how to honour men of great talent born in other countries. Personally, in my quality as a physicist and as a pupil and friend of Paul Langevin, and also as a citizen of France, I want to thank you. The road before us which leads to better knowledge of nature and of justice between men is long. On this road we have paused for a moment to think of one who so courageously and so worthily showed us the way.

THE RT. HON. A. V. ALEXANDER: I feel honoured in having this opportunity of paying tribute to that great French physicist, Paul Langevin, whose life and work are worthy of special notice as an example of co-operation between the scientists of different nations.

It is most appropriate that we in Britain should pay such tribute, both because his association with this country was especially close, particularly in the field of Naval Science, and because of the very friendly relations which happily exist between France and our own country.

M. Langevin was among the first of the overseas graduates admitted as research students to Cambridge University, and worked in the Cavendish Laboratory under Sir George Thomson. He maintained a close friendship with the Cambridge physicists throughout his life. Lord Rutherford was another overseas research student admitted at this time.

I propose to speak of only one aspect of his scientific work, namely that on the detection of submarines and underwater obstacles during and immediately after the first world war. As First Lord of the Admiralty during the recent war I had every opportunity of studying the results of M. Langevin's work and appreciating the debt we owed to him for the ability and labour he applied in devising the means of detecting a moving submarine and so enabling our Navies to overcome the most deadly menace of the war at sea.

The obvious way to detect a moving submarine is through the noise it makes, by means of a submerged microphone, and this was the method we first adopted in this country. It is not too easy, however, since the noise of the submarine is obscured by the noise of the hunting vessel, although the ear can be trained, to a remarkable extent, to distinguish between the two. This method of detection also has the disadvantage that it cannot detect a stationary submarine, that the range of the submarine cannot be determined with any reasonable size of microphone.

These difficulties can be largely overcome if we transmit sound waves of a frequency above the audible range (so-called supersonic waves) and detect the presence of a submarine or other underwater obstacle by its echo. In this case we have a signal of known frequency which can be amplified selectively against any background due to own ships noise, the range can be measured by the time interval between the transmission of a signal and the reception of the echo, and it is possible to build directive systems for transmission and reception.

These principles are, of course, also the basis of radar, except that electromagnetic waves are used instead of sound waves; the latter are attenuated too rapidly and have too low a velocity, in air.

This was the system on which Professor Langevin worked, originally in conjunction with M. Chilowski, a Pole, and later on his own. The early experiments were not very successful in providing a means of detection, but proved that supersonic waves could be generated in water by vibration of the plates of the condenser of an oscillating electric circuit, and that they could be detected by a carbon microphone.

Real progress was achieved by Professor Langevin in mid-1917 employing the piezo-electric effect in quartz for the generation and detection of the super-sonic waves. This effect was discovered in 1880 by Pierre and Jacques Curie, and is the name given to the property of specially cut sections of certain crystals which develop opposite electric charges on their faces when they are compressed or extended, and conversely, vibrate under the action of an oscillating electric charge.

The piezo-electric effect alone is too small both for the generation of supersonic waves of high power, and for the detection of the small amount of energy reflected by a submarine a long distance away, and, for this reason, had been discarded by workers in this country. Great credit is due to Professor Langevin who overcame all difficulties. A mosaic plate of thin quartz sections was built between two thick sheets of steel, one of which was in contact with the water, and the other free to the air, and the natural period of mechanical vibration of the whole was made equal to the period of the electric circuit used for excitation. This quartz condenser was used for both the generation and the detection of the supersonic waves, and the necessary increase in sensitivity was thus achieved. Use was also made of the sensitive valve amplifiers developed by the French Military Wireless Department.

Close contact was maintained between the British workers in the field of submarine detection, and Professor Langevin, who paid a visit to this country in March, 1917. On a return visit by a scientific mission of the Board of Invention and Research (constituted by the Admiralty in July, 1915), he demonstrated his newly completed equipment using a quartz condenser, and with which he was able to detect a submarine at a distance of 1 kilometre. When we decided to start work with quartz in Britain in August, 1917, Professor Langevin kindly lent us some suitable samples.

Similarly, the Americans were informed of the work in both countries.

Supersonic underwater detection equipment was not used operationally in the first World War, but the work of Professor Langevin laid the foundation for the development, between the two wars, of an effective weapon. In Britain, this work was carried on at the Admiralty Anti-Submarine Experimental Establishment. In this last war it was effective, together with radar, in overcoming the U-boat menace. It is indeed a pleasure to me personally to be able to use this opportunity of acknowledging Britain's debt to Paul Langevin. Operationally, radar, and supersonic detection fulfilled complementary functions, the radar being the more effective against a surfaced submarine, and the latter being necessary when it had submerged.

Professor Langevin opened up the field of supersonics to help his country when she was at war, but his work has also a great value in peace. Not only has his equipment been developed as an aid to marine navigation, both for detecting submerged obstacles and for determining the depth of the sea, but in many respects we are only just beginning to realise the possibilities in this field.

In this story we have two examples. Firstly of the value of research for its own sake in the discovery of the piezo-electric effect by Pierre and Jacques Curie, and secondly, of the value of applied science in this work of Paul Langevin. Both are necessary, more than ever before, in the reconstruction of the world today.

It is with real pleasure that we welcome Professor F. Joliot-Curie, the greatest pupil of a great master. We are delighted to have him with us, but we all deeply regret that, unfortunately, he is far from well. His presence emphasises the effort he has made to pay his tribute to the memory of Paul Langevin and to tell us of his personal recollections of that great man. It is remarkable that the history of Professor Langevin's collaboration with the British in 1914–1918 should, in a way, have repeated itself with Joliot-Curie in 1940, when he sent his co-workers to Cambridge to continue their research on atomic energy.

In paying tribute to-day to the memory of our late French comrade and co-operator, one's mind turns naturally to the vast problems which the world faces to-day as discoveries follow one upon the other indicating the tremendous power which man can wield for good or for ill as science advances. I am reminded of four lines of George Herbert's poem on man :—

" Nothing hath got so far
But man hath caught, and kept it as his prey ;
His eyes discount the highest star ;
He is in little all the sphere."

It is the knowledge we have of such public service as was rendered by Langevin which gives us confidence that whatever dangers threaten us we shall always have men of science devoted to the service of humanity to see us through.

PUBLIC MEETING
at the Beaver Hall, Garlick Hill, London

with Sir Henry Tizard, K.C.B., A.F.C., F.R.S., Chairman of the Advisory Council on Scientific Research, in the Chair.

SIR HENRY TIZARD : We meet tonight in honour of a great scientist and a great Frenchman. There are to be four speakers, among whom we particularly welcome M. Pierre Biquard and are anxious to hear him.

You all know, I expect, that Langevin was one of the famous group of post-graduate students of Cambridge in the days when students who had obtained degrees of other universities were first allowed to obtain a post-graduate degree at Cambridge, much to the horror of the older inhabitants of that great university. It was a very famous group, and some day its history must be written. Langevin was one of them ; Rutherford, as you all know, was the first, and Townsend was another. Before the first world war I worked under Townsend for a short time, until events beyond my control interrupted it. He was a great friend of Langevin and knew his work very well, and when I worked under Townsend I had not only the opportunity but the duty to read Langevin's papers on recombination and mobility of ions. I remember very well the impression that I got in those days of what I always felt was a characteristic of the great French contributions to science ; that is, the papers were remarkably clear, they were short, and they showed a restraint from the use of unnecessary mathematics that a good many English scientists could well follow.

We all know that in the first world war it was Langevin who was chiefly responsible for the development of methods for detecting and following submarines under water. There are some details of the history of that which are interesting, and I do not know that they are universally known.

The first suggestion for detecting under-water ships or obstacles was made by an Englishman after the *Titanic* disaster in 1912, but I do not think that it came to very much. Then, in the first world war, Chilowski came to Paris with some ideas and was sent to Langevin, who saw the opportunity and rapidly developed the subject. We knew nothing about it in this country until some time in 1916, when we were first informed officially by the French, and shortly afterwards Langevin brought to this country his first experimental work, in which he made use of the Maxwellian stresses in mica as a method for generating very high frequency sound waves which he then detected by means of a microphone. He then went back to France, and we started here quite early on this work with quartz crystals, suggested, I believe, independently in this country—although Langevin took it up—by Rutherford. The results which we obtained—I say " we " as meaning the English scientists who were working on the problem at the time ; I was not doing so myself—were what were expected, but they were much too small to be of any practical use. The reflections obtained from a submarine, that is, were far too small, and we did not know at that time that the French military department for the development of wireless had succeeded in making 6-valve and 8-valve amplifiers which were able to amplify a very small current up to an amount which would make it of practical use in the detection of submarines. That fact was conveyed to us by Langevin in 1917, and transformed the whole subject from a practical point of view.

Langevin then succeeded in going very much faster than we did in this country in the development of the piezo-electric method, which became the basis of all the methods used and has been so since ; and towards the end of the war he was able to locate and follow submarines on their course under water at a distance of something like a thousand yards. It was a great achievement, and there is absolutely no doubt that Langevin himself must have the chief credit ; but it was too late for use in the first world war. It was, however, very rapidly developed in between the two wars, and was, as you all know, of incalculable value in the war which has just ended.

That was a very fine example of international co-operation in matters of defence, and it was more than that. It is interesting to remember, and I think that we ought to remember, that Asdic—a great invention, and, like radar, an invention of this war—was based on developments in pure science. The piezo-electric effect was first discovered in 1880 by Pierre and Jacques Curie. There is an interesting piece of family history about that, because Langevin himself succeeded Pierre Curie as Director of the School of Physics and Chemistry in 1925, and Professor Joliot-Curie—whom we are very sorry not to see here today—he has not been well and has had to return to Paris—was, as is well known, Langevin's most famous pupil.

This was, as I say, a fine example of international co-operation, and its development was based on pure science; but the development would not have been possible unless good work had been done in applied science within the French military service, so that I think that there are two morals here which we can draw. One is that whatever we do in the interests of the defence of the country in peacetime we must never forget that the scientific strength of a country and its greatness depend on the support given to the free pursuit of knowledge without any idea of application. Do not let us forget also the constant interaction between pure and applied science. It is from the pure science that the applied science starts, and the applied science itself has its reaction on the pure science.

The last years of Langevin's life were typical of his greatness. He refused to be overwhelmed by the tragic death of his son-in-law and by the deportation of his daughter to Germany. We were very glad to hear from M. Biquard that she returned to France in good health. Langevin took a great part in the resistance movement in France.

The life of a man such as Langevin is really an inspiration. He was born of humble parentage and was raised by his own genius and industry. He was engaged throughout life in the sheer pursuit of truth and of high ideals. The influence of a man like that stretches far beyond the confines of his own country, and we do well to honour him tonight.

I shall first ask Sir Harold Spencer Jones to speak to us, I think this time in his capacity as President of the Society for Visiting Scientists.

SIR HAROLD SPENCER JONES, F.R.S. (Astronomer Royal, President of the Society for Visiting Scientists): I do not propose to say anything about the life and work of Paul Langevin; that will be dealt with much better than I could do it by Sir George Thomson, M. Biquard and Professor Bernal. I do want to say, however, on behalf of the Society for Visiting Scientists how pleased the Society is to be associated with the Association of Scientific Workers and the World Federation of Scientific Workers in paying this tribute this evening to Paul Langevin, a great Frenchman and a great scientist, whose work, as Sir Henry Tizard has told you, was of incalculable value in anti-submarine warfare, a man who exercised a profound stimulus on those who worked with him and who founded a lasting school of physics.

It is, I think, appropriate that the Society for Visiting Scientists should be associated with this gathering. The Society was founded under the auspices of the British Council during the war at a time when many scientists from the Dominions and from Allied nations were in London or passing through London on scientific missions of one sort or another, and when many scientists who had escaped from the occupied countries were reaching this country either to work here or on their way to the United States. The Society for Visiting Scientists provided a meeting-ground for them where they could receive help and advice and where they could meet one another.

The Society has, we feel, a permanently useful field in providing such a meeting-ground, and particularly in these days for the younger scientists who are coming to England without knowing many men of science in this country, and who want advice as to whom they can best see and with whom it would be most useful for them to come into contact.

I think that men of science have a very great part to play, through their common pursuit of knowledge, in promoting that better understanding between nations which is so important at the present time. The Society for Visiting Scientists, through the contacts and the help which it is able to give to scientists who visit this country, has, I feel, an important part to play in that respect. I think that the aims of the Society are such as would have appealed very greatly to Paul Langevin himself, and therefore I am very glad, as President of the Society, to be associated with this meeting tonight.

SIR GEORGE THOMSON, F.R.S. (Professor at the Imperial College of Science and Technology) : The Chairman has told us how Langevin came to Cambridge among the first group of workers under a new statute which enabled them to dispense with three years' residence. He was, I believe, actually the very first foreigner to take advantage of this statute. He came to Cambridge in October, 1897, to work in the Cavendish Laboratory under J. J. Thomson. The Chairman has told you that this was the period when atomic physics, as we now call it, was beginning and when a group of men, of whom Rutherford, Townsend and C. T. R. Wilson are perhaps the best known, was already starting to work upon it.

The impulse which had been given by Rontgen's discovery of X-rays two years before, in 1895, was producing a rapid forward movement in physics. Almost at once the ionization effect of these rays was discovered in Cambridge, and formed the basis of a number of researches, of which those of Langevin are amongst the most notable. This idea, the idea of gaseous ionization, which had been suggested some fifteen years before Giese, was made a working theory which was most fruitful both in the suggestion of experiments and in their interpretation.

It now seems to us so natural, but if we look back and try to put ourselves in the position in those times we shall realise that it required considerable boldness to get rid of the idea that ionization implied electrolosis and that electrolosis implied some kind of chemical separation. However, once the idea of the separation of charges from a single atom was established, progress became rapid. This, of course, was helped by my father's measurement of e x m for cathode rays early in 1897, the very year in which Langevin came to Cambridge, and made it extremely probable, of course, that negatively-charged bodies existed much lighter than atoms and common to all kinds of atoms. When Langevin arrived the value of e, the charge of the electron, had still to be determined, but the work of Townsend and his fellow-workers in the Cavendish Laboratory on the charges which are carried by the drops which are formed by gases released by electrolosis had been an important indication or suggestion that this was the same as the charge carried by electrolosis and had about the value that we know now.

Langevin, therefore, came into a very exciting atmosphere. That he enjoyed his stay there is indicated by an inscription which he wrote in a beautifully bound copy of his doctor's thesis, the thesis that he made in the University of Paris in the year 1902, and which, to my father's great pride, he dedicated to him. I should like to read you the inscription which he wrote in it. It is true that in intention it was a tribute to my father, but it shows very clearly the generosity and warm-heartedness of Langevin, so that I think that it is almost more a tribute to Langevin's goodness than to my father. He wrote : " In memory of the delightful year that I passed at Cambridge and of the great hospitality that I received, I offer to Professor J. J. Thomson this modest contribution to the work which he, more than almost any other, contributed to initiate. Very affectionately, Paul Langevin."

Langevin's own work at Cambridge was on the ionization produced when X-rays fall on a metal surface. He was able to show that this was in fact a volume effect. We should now describe it as the ionization produced by the secondary electrons released by the photo-action of the X-rays on the metal, but they could not put it quite in those terms in those days. He was able to show that it extended for a matter of a centimetre or two from the surface at atmospheric pressure, and was not, as had been supposed, a purely contact effect. He was able to make use of this source of ionization to make what, as far as I can discover, were the first measurements of the relative ease of ionization of different gases, the relative amount of work which has to be performed to produce a given amount of ionization in different gases ; because here there was an ionization which was completely absorbed, so that by completely absorbing it in different gases and measuring the amount of ionization he could determine the amount of energy required for that purpose.

While at Cambridge he also started his work on the measurement of the coefficient of recombination. The earliest measurements of this had in fact been made by Rutherford, but they were rather rough in character. Langevin actually completed this work after his return to France in the Sorbonne, and I shall refer to that a little later.

One is perhaps inclined to think that it must have been extremely easy to make important discoveries in those days, when, as we now know, there was so much to be discovered, but I do not think that it was. Apart altogether from the difficulty of thinking clearly in a world of revolutionary ideas and of rapidly accumulating results—some of which, of course, were erroneous—the technical difficulties were enormous, and I think that few of the present generation can realise them. The evacuation of potentially-leaking apparatus—I say " potentially " but in fact it always did leak—by, if you were very lucky, a Toepler pump worked by hand, was certainly a test of patience, if nothing else, and also required almost superhuman acuity as a finder of faults, because if you could not spot the leak you were done for. Although I first worked at the Cavendish Laboratory in 1915, fifteen years after Langevin, I started on a Toepler pump, and I doubt whether Langevin had one as good. The only instrument available to Langevin, and which was used in all his researches, for measuring the small ionization currents was a quadrant electro-meter of the old-fashioned Kelvin type, and even Kelvin himself could not make that work very often. The X-ray tubes of the period were weak, fickle and fragile ; they could not be trusted for steady running even for very short periods, and if you read Langevin's experiments you will see that he was careful never to trust them ; he always arranged his experiment so that the particular ionization which he was measuring was balanced as nearly as possible against another ionization somewhere else produced simultaneously by the same X-ray tube, so that he was as nearly as possible independent of fluctuations in the intensity of the tube.

It was a triumph of which he was justly proud that he was able to make measurements with a single flash of X-rays produced by a single interruption of the primary of his induction coil—because, of course, an induction coil was used in those days, not a transformer.

Langevin's thesis for his Paris degree in 1902 is a magnificent exposition of the state of knowledge of atomic physics at that time. It is very remarkable. I read it through a few days ago again, and it is very remarkable to see how much had been achieved in seven years. Langevin's own work on the experimental side, apart from that which he had done at Cambridge, was his measurement of the coefficient of recombination and of the mobilities of gaseous ions. He used methods which were at that time much the most accurate, and which are still quoted in text-books and must be regarded with respect. They are not those used nowadays, because they are not appropriate to the extreme degree of purity in the gas which we now know is necessary in order to get theoretically interesting results on mobilities.

On the theoretical side he developed a theory of ionic recombination which is valid for high pressures and is still that used. He fully realised its limitations and their reason, and his own experiments illustrated very beautifully the way in which as you gradually increase the pressure the experimental results approach asymptotically to those predicted by his theory. It is a most beautiful and a very finished piece of work.

His experimental work on mobilities was done on the same apparatus, but used in a different fashion. He added to that in 1905 a most important theoretical paper in which he investigated on the basis of the kinetic theory the exchange of momentum between two streams of gas of different characteristics passing through one another. He arrived at a theoretical result which is susceptible of calculation in the case of certain types of force, notably when the force is due to the effect produced by a charge inducing polarity in molecules combined with a finite radius—quite a difficult and complicated matter. That theory is still the dominating theory in the study of the mobility of gaseous ions. It is given to few men to say at the same time the first and the last word on a scientific question.

He was particularly interested in the question of the size of these ions, in how far molecules clustered round them to make an ion bigger than a simple molecule, and so on. He was fully aware of the important effect of what we would now describe as polar molecules, which he described as molecules of substances of high dielectric constant. That work, which now perhaps belongs to the classical side of physics, showed him as both an experimenter and a theoretician of the highest order.

I think that for most of those of us who knew him, however, our feelings towards him are quite as much those of affection as those of the respect that one feels for a great scientist. He had an extraordinarily human side. Although much of what I have to say is hearsay, my father spoke of him so often that, although I only met him perhaps half a dozen times, I feel that I knew him very well. He was one of the group who started the Cavendish Dinner, a meeting of the research students of the Cavendish, shortly before breaking up for the Christmas holidays, which has endured from that time to this. It is recorded that at the first of those dinners, at which there were always songs, some of them written for the occasion, notably by the late Dr. Rodd of St. John's, Langevin was asked to oblige and sang the Marseillaise with so much fervour and emotion that one of the waiters, who was a Frenchman, could not be restrained from falling on his neck and embracing him.

Perhaps I may mention one personal matter. Langevin who did not very often travel, took the trouble to come to this country on the occasion of my father's seventieth birthday and proposed his health in English in what was one of the two or three really notable after-dinner speeches to which it has ever been my pleasure to listen. I feel that we must regard Langevin not only as a great scientist, but, as all who knew him will agree, as a supremely good man.

THE CHAIRMAN : I will now ask M. Biquard to speak to us. He is one of Langevin's most loved pupils, and from the time he joined Langevin was his assistant and right-hand man. We also honour him for the distinguished part which he took in the French resistance movement during the war.

M. PIERRE BIQUARD (of the French Commissariat of Atomic Energy and the Paris School of Physics and Chemistry) : On the third of March, 1945, the great Amphitheatre of the Sorbonne was overcrowded with thousands of people eager to show their appreciation of Paul Langevin, just returned from Switzerland after the liberation of France.

In his reply to those who had told how much everyone in the world was indebted to him, Paul Langevin said that he felt himself specially grateful towards three scientists whom he called his masters. Two of them were French : Pierre Curie and Marcel Brillouin, the third was the late Sir J. J. Thomson who, said Langevin, " welcomed me, forty eight years ago in Cambridge and whom I was able to embrace in January, 1940, just a few months before he died."

In the same amphitheatre of the Sorbonne, not long ago, we were again assembled but Langevin was not there. And amongst those who came to celebrate his fame we were happy to see Professors Bernal, Blackett and my friend, J. G. Crowther.

Tonight I feel myself deeply moved, and proud, too, to be able to tell you, here in London, on behalf of all French Scientists and especially of my friend Joliot-Curie, how much we appreciate all that you, in Great Britain, have done to honour our greatest physicist, Paul Langevin.

Born in Paris on the twenty-third of January, 1872, Langevin was the son of a Parisian workman who had been unable, for lack of money, to carry on his studies.

Paul was sixteen years old when he entered, the first of his year, the Ecole de Physique et de Chimie. Then, after three years, he went to the Ecole Normale Superieure, always at the top of his class, and was granted by the city of Paris a scholarship to go to Cambridge. That was in 1897, the very beginning of his brilliant scientific career. Back in Paris he succeeded Mascart at the Collège de France and Pierre Curie at the Ecole de Physique et de Chimie. From then onwards he devoted a great deal of his activity to this school, of which he became Director in 1925.

The Vichy government dared not immediately dismiss him in 1940, but awaited his imprisonment by the Gestapo to drive him away.

I do not intend to give here the full list of his various distinctions, but I know by him personally that he was specially proud of having been elected, in 1928, Foreign Member of the Royal Society.

To give an exact idea of the scientific work of Langevin, and to place it in the general evolution of physics is a rather difficult task, and the very distinguished scientists who are present tonight would do it much better than I can myself.

When Langevin was working in Cambridge, as Sir George Thomson has explained, among the big problems for the physicists were the electric discharge in gas at low pressure, and the ionisation produced in gases by Roentgen-rays. Beautiful experiments led him to the discovery of the " big ions " which exist normally in the air and he was able to explain why only the ordinary little ions and the very big ones can exist. As a consequence he provided theoretical explanation of cloud formations whose layers are always vertically distant at several kilometres. He gave at that time new developments to the Kinetic theory of gases.

During the four years after 1891, Pierre Curie had carried on very difficult experiments on the variations of magnetic properties with temperature up to fourteen hundred degrees centigrade.

The result of this famous work was the Curie Law : The paramagnetic moment is inversely proportional to the absolute temperature.

Ten years later, in 1905, Langevin was able to explain theoretically this important phenomenon. Ascribing permanent magnetism to the revolutions of electrons, Langevin was able to explain diamagnetism and paramagnetism. In the latter case the thermal agitation would realise an isotropic distribution of all the elementary magnetic moments while the external magnetic field tends to orientate them in its own direction, and to compute the equilibrium Langevin made use of the Boltzmann statistical laws. He stated in this way the well-known Langevin's law and cleared the way to the researches of Pierre Weiss on ferromagnetism.

By the same theory of molecular orientation Langevin explained the electric and magnetic biréfringences. The same approach to the problems allowed Debye to give the theory of the electric moments of molecules.

It is worth while mentioning here that Langevin's thermodynamical theory of diamagnetism and paramagnetism enabled the physicist de Haas to obtain temperature as low as one-fourth of a degree absolute.

Langevin used to say that his life as a physicist had been rather difficult due to the successive crises that physics has passed through in forty years. And he added that it is really tiresome to be obliged three times in a life to change's one's conception of time, space, mechanics, the structure of matter and radiations.

After having contributed to the Lorentz theory, today called " classical " theory, Langevin was among the very few physicists who, since the beginning, understood and fostered the theory of relativity.

He demonstrated that conservation of energy and relativity imply a new dynamics in which the most important affirmation is the inertia of energy. Of course, and specially since recent years, we are accustomed to this idea, but when Langevin stated it, and explained through it the discrepancies of the atomic masses from integers that was a glorious step for physics.

In the quantum crisis too, Langevin gave important contributions to problems involving equilibrium between matter and radiations. Even during the war when he was obliged by the Nazis to live in the small town of Troyes, he produced theories on collisions between neutrons and nucleus and on the slowing down of neutrons.

I come now to one part of Langevin's work which is better known not only to scientists but also to engineers : ultrasonic waves. Various attempts had been made by physicists to extend to higher frequencies the generation of elastic waves, and Lord Rayleigh had stated the great importance of the subject.

After the sinking in 1912 of the ocean liner *Titanic* some experiments were made in Great Britain by Richardson and Sir Charles Parsons, but without success. During the first world war, Langevin asked by Chilowski, found the way of converting electric oscillations into acoustic ones by means of the piezo-electric phenomenon discovered by Pierre and Jacques Curie.

A quartz plate coated with thin metallic layers and set in vibration by an electric field, oscillating at its resonance frequency produces a beam of ultrasonic waves. The wave length being very short it was easy to obtain a directed beam and thence to get an echo from a submarine obstacle. So was born an effective weapon for the struggle against the German U-boats and it is just to say that Langevin largely contributed by this accomplishment to the two victories over the German Navy. When foreign scientists came to visit his laboratory

Langevin was always very happy to show the small kitchen sink in which he was able, for the first time, to detect the radiation pressure of ultrasonics by means of a torsion vane.

It is not necessary to recount the details here of the prodigious and very quick developments in this branch of physics : velocity and absorption of sound in liquid and gases, scattering of light by ultrasonics in liquids and solids, and all the application not only in warfare but also in television in chemistry, in fisheries, in the recording of blood pressure, and so on . . .

I will just say that ultrasonics provide a remarkable example of what pure science is able to give to mankind. We must not forget, when enumerating all these developments of the ultrasonic technique that they are the result of considerations made by Pierre Curie on the symmetry in natural phenomena.

Not only science and the applications of science, but the philosophy of science occupied the thought of Paul Langevin. It would take too long to make a survey of this aspect of his work and I should like, with your permission, to read some parts of a lecture given by him in Warsaw in 1938.

Speaking of the so-called crisis of physics, Langevin said :—

> " The profound changes which have accompanied these crises have obliged physicists to consider more carefully the manner in which they work and the philosophy of their science, to reflect upon the way the structure of their theory evolves by contact with facts, so as to notice the answers given by Nature to the questions our theories ask her."

And further :—

> " In order to describe this enrichment of the facts it has been necessary to pursue at the same time an enrichment of the language to enable us to classify, represent and explain these new facts within the framework of our theories, suitably transformed and renewed . . . We must reconstruct more closely that grammar of science, represented by epistomology, and reflect like Monsieur Jourdain, upon the way in which we speak although we have done it for a long time."

He insisted very often on his faith in determinism in spite of some interpretations given to the Heisenberg principle. I quote once more :—

> " I should like to insist upon the point that we do not limit determinism when we make it more explicit and more adequate for the conditions in which physicists work up to the limitations of their information, which do not allow them to arrive at uncertainty.
>
> " In my opinion quantum physics does not represent a failure of determinism, which idea it makes more humane, more concrete and more precise, but of mechanics. This has become progressively exhausted ever since it was raised upon a pedestal above the other sciences."

And reading this we may not forget that Langevin began to devote his life to science at a time when it seemed that the electro-magnetic theory was really the key to explain all the universe.

On several occasions when talking to me of the overwhelming tasks which faced him, Langevin told me : " I am afraid I will not have time to write a book I have been thinking of for many years : a book on the mutual reactions of scientific discoveries and human philosophy."

He did not realise this dream, and we must regret it very much. But he explained many times how acting led to thinking and conversely.

But, indeed, I will not deal with this, seeing here Professor Bernal and having still in memory the last lecture given by Langevin, in which he read his own translation of an article written by Bernal whom he called on that occasion " un grand citoyen du monde."

The social relations of science were deeply felt by Langevin and his social responsibility as a scientist made him take an active part in the political struggle in France.

Like his scientific activity, his political activity began when he was in the Cavendish Laboratory. A letter from the poet Charles Peguy asking him to sign a manifesto on the " Dreyfus affair " reached him in Cambridge, and he agreed immediately. That was in 1898 in " that happy time," says Langevin, " when one single man's fate was very valuable and could excite the whole of mankind."

Since that time Langevin was a very active member of the " Ligue des droits de l'Homme " created to protect the citizen against abuses from collective power.

It is worthy of notice that the Dreyfus affair greatly influenced political life in France for many decades, and too, that since the beginning of this affair many leading scientists have fought for the truth against the prejudices of conservatism.

He succeeded, as chairman of the league, Victor Bach when this eighty years old professor and his wife were assassinated near Lyons by French fascists acting on behalf of the Nazis.

I heard the name of Langevin for the first time during a lecture when our professor of physics complained that this great scientist had not yet been made a member of the French Academy of Sciences. Some years later, in 1920, during the big strikes which happened in France, I was able to read in the socialist paper Humanité, an open letter of Professor Langevin raising a strong protest against the recruiting of young students as strike-breakers. At this very moment I realised that Langevin was really a " Man " in the full sense of the word, a man because of his powerful brain and his human heart. And I must say that the greatest luck of my life has been to be his student, his assistant and friend (I remember how happy and proud I was when the first time he wrote me using the words : Mon cher ami).

Through science mankind harnesses at full speed natural forces, but at the same time men seem to be unable to promote justice at the same rate. And there lies the drama of our present time. This gives to the scientists a special responsibility and creates for him the duty to take some part in social affairs. Langevin was fully conscious of this duty. Improvements in the social system, removal of the causes of war, fighting against racialist theories always found him on the correct side of the battle front.

In 1931 he called attention to the aggression of Japan against China as being the first failure of the League of Nations.

In 1935 during the Italian aggression against Ethiopia he asked firmly for international action.

But above all he was strongly opposed to the voluntarily blind policy of France during the war in Spain, war which was not a " So-called Civil War " but the first act in the fascist attempt to dominate Europe.

After the 6th February, 1934 riots, Langevin and some friends created the " Comité de Vigilance des Intellectuels antifascistes " and he opposed very firmly the Munich capitulation in 1938.

The war came. Langevin, who had been ignored, for political reasons, by the French Admiralty, was asked to look for improvements in the ultrasonics detecting systems. The collapse of France in June, 1940, prevented him from continuing these researches, and after some months he came back to Paris in order to direct again the school of physics and chemistry.

I have kept in my memory this afternoon of October, 1940, when going to my laboratory I passed in the street a car, driven by German officers. Inside was a man who, seeing me, took his hat off and waved it towards me in sign of friendship. That was Professor Langevin just arrested by the Gestapo.

He remained in the " Santé jail " for six weeks, just treated as any criminal.

After two weeks he was allowed the great favour of being given paper and pencil to carry on calculations he had already began with burnt matches. As he told me later, he had in jail all the quietness necessary to think and to work. I must add too, that the Gestapo officer in charge of him, Colonel Boemelburg, formerly a professor of history, I believe, confiscated all the sheets of paper covered with Langevin's calculations. That was for him, doubtless, a collector's item, as was the letter from Einstein to Langevin, which inspired the comment " dirty Jew," but which he never gave back !

After ten days imprisonment he was called for examination. Boemelburg was waiting for him in a little room full of tobacco smoke. Opening suddenly the window the Nazi officer turned to Langevin and said : " We, uncivilized people, like fresh air." And that was the only time Langevin was able to agree.

He was told he was put on arrest by virtue of the "Fuhrerprinzip" as he was the most representative of all intellectuals who had fought against Hitler. The main charges against him were his membership of the Ligue des Droits de l'Homme, and above all because "he had tried to prevent Germany from ridding itself of the Jews."

Great indignation arose in scientific circles in Paris because of this imprisonment and great numbers of letters were sent to Langevin, because we were sure that the Germans would read them. Joliot-Curie began a strike, refusing to appear in his laboratory as long as Langevin was in jail. And he announced this in public during a short lecture at the College de France. I can see now Joliot-Curie leaving the theatre in tears.

After six weeks Langevin was liberated but compelled to stay in Troyes. His daughter, Hèléne, was arrested and deported to Auschwitz, his son-in-law, the young and brilliant physicist Salomon was shot, because he was a communist leader.

It became more and more dangerous for Langevin to remain in France and the Resistance arranged for him to travel under a false name to the Swiss border, where the Swiss Intelligence service enabled him to escape. He travelled by train in a compartment full of German officers.

Coming back to France after the liberation he took in the ranks of the French Communist Party the place set free by the death of Jacques Salomon, and he had the great consolation of seeing his daughter come back from the hell of concentration camps.

At that time he happened to tell me : "I am rather old and tired now, but there is a task I must still perform : to promote a considerable reform in the French way of teaching."

He had thought about this problem all his life long and he was specially gifted to solve it. So he was given the chairmanship of the so-called "Langevin Commission."

He built the framework in which, I hope, France will renew completely her methods of education.

The duty of Society is to enable everyone, whatever his origin and social situation, to develop his qualities, and that means both free instruction and the possibility at any time to correct by a suitable change a false orientation given to a student.

During all the course of a scholar's life, study and activity should be adapted to the individual spirit, and Langevin insisted very firmly on the beautiful example given in this way by the "Central Schools" created in France by the Convention and which Napoleon suppressed for reasons very easy to understand.

Many possibilities must be offered to the child, manual, intellectual, artistic, and this can be attained only if culture is not irrelevant to life. In the school, points out Langevin, the child has to make his apprenticeship of social life, of democratic life, and, as far as possible, self-government should be experienced.

For man to find his real place in society he must learn both individual and collective work.

And let me quote again :—

> "The final objective would be to find for the individual in human society his rightful place in every respect. Society will then appear to every one of its members as a living entity in which each of us is entrusted for a while, with a treasure of civilisation acquired by our ancestors at the price of innumerable hardships and pains, which it is our duty to pass on after enriching it according to the extent of our ability.
>
> "Let us hope that every child, on leaving our schools of tomorrow, will be convinced that the two mortal sins of conformity and selfishness respectively oppose the double imperative duty of personal achievement and social solidarity."

And to attain these aims Langevin thought it especially important to get a suitable way of teaching science.

He insisted upon the necessity of not giving to students dogmatic and utilitarian instruction in science.

Science is not a dead body and, particularly in the first years of study, it is very important not to give the impression that science has solved in a definitive way the problems of Nature. Recognition must be given to the great importance of the history of science, the history of discoveries, the history of the development of science ana to, as suggested once by Professor

Bernal, the history of circumstances which have often slowed down the progress of science. Among these Langevin thought that very often in the past followers have misinterpreted the spirit and intentions of authors of great theories, putting them in a definitive form, and requiring later the greatest effort to abandon them.

And he gave many examples : the famous Euclid postulate the Newton theory were only, in the minds of their authors, possible assumptions. They experienced quick success and, as always, men thought that definite truth was known.

Once, coming back home after his lecture in the College de France and speaking partly to Joliot who was with him, and partly to himself, Langevin asked : " Was I not wrong in devoting too much of my time to social affairs ? "

The answer to this question was given when thousands and thousands of workers went to his funeral, some, like the coal miners, in their working clothes to pay him the respect he would have loved most, to testify that one of them had disappeared and that they will forget neither him nor the example of his life.

The answer to this question has been given recently to Louis Saillant, general secretary of the World Federation of Trade Unions when speaking of Langevin in a meeting somewhere in Manchuria he noticed a gleam of proudness and of gratefulness in the eyes of his hearers.

The answer is given here, tonight. Since life appeared on our planet, mankind has lived a wonderful story full of defeats but full also of splendid victories. And we are here because we know that among millions and millions of men doing their best in this prodigious evolution some of them have done so much more than the others that we have to thank them and to follow their example.

And Langevin was of those few.

The answer to the question, I must add, was also given by Langevin himself a few minutes after having put it, when he said with his so warm and so human voice : " After all, I am sure I was right."

THE CHAIRMAN : You have already shown by your applause your appreciation of M. Biquard's brilliant speech. I hope that it will be published in full in this country as a great record of a great life. I should like on your behalf, to wish M. Biquard a happy journey back. I will ask him to carry with him our warmest wishes to Professor Joliot-Curie, in particular, and to all our French colleagues in the scientific world. M. Biquard will always be a very welcome guest here and a very welcome visitor to England.

PROFESSOR J. D. BERNAL, F.R.S. (President of the British Association of Scientific Workers, Vice-President of the World Federation of Scientific Workers) : I feel, as I think all of you do, after listening to what we have just heard, that there is very little that I can add to what has been said by M. Biquard. We have, however, some task in saying what Langevin has meant to us here, and I think that I have been chosen to do that because I had the good fortune of knowing Langevin and of meeting him very frequently not in those early heroic days of science, before I was born, but in the days when the stress of affairs began to dominate the scene of science.

I think that, apart from certain scientific conferences, I met Langevin first in 1934, and from then on I saw him both in Paris and on his fairly frequent visits to this country in connection with the work about which M. Biquard has just told you. The importance of Langevin to me, as I think to many other people in this country, was that he had already demonstrated in his person and by his life that it was possible, and indeed that it was necessary, for the great scientist to undertake in the full sense his social and political responsibilities.

Langevin had already, as you have heard, while he was at Cambridge, taken on such responsibility in the Dreyfus affair. I had the privilege of listening to the speeches made in Paris at the meeting held to commemorate him earlier this year, and there we heard from one speaker after another the important part that he had played in the political life of France ever since that time. In that work he always kept in mind the importance of the actual relation between his scientific work and his political aims. I have had myself a conversation with him along the lines of which M. Biquard spoke at the very end of his speech, regarding this question which he put to himself on the rightness of spending so much time and energy on social questions. On that occasion he gave me an answer which was perhaps a little more explicit than the one we have been given tonight. He said : " The scientific work which I can do can be done and will be done by others, possibly soon, possibly not for some years ; but unless the political work is done there will be no science at all." That, I think, was the atmosphere in which he lived and in which he taught us to live in the 'thirties.

Langevin was the kind of person, as you have already heard, who would be subsequently described as "a premature anti-fascist." There were many people in the 'thirties, very respectable and very important people, and to a certain extent very well-meaning people, who considered from 1932 onwards that it was absurd to worry about the political prospects of the world ; there might be a certain amount of trouble in distant countries, there might be certain readjustments of frontiers, there might be certain economic consequences, but it was no affair of the scientists, and they should not turn from their work to deal with it.

Now Langevin, as you have heard, never held that view, and he was very distinctly instrumental in changing that view over here. As I say, I met him first in 1934, and at that time he had just founded the Comité de Vigilance des Intellectuels Anti-Fascists. Of course, it was a good deal easier in France to see Fascism in action in 1934 than it was in this country. In 1934 in France you had Fascists bands coming up the Place de la Concorde trying to besiege the Chamber of Deputies, and you had the friends of Hitler parading openly—as they paraded in this country, though we tend to forget it, not very much later under Sir Oswald Mosley. In France, it was a very serious threat, and it needed the efforts of all kinds of people to deal with it. On the intellectual front Langevin was very important, and important sections of French scientists were willing not only to put their names to protests and letters on this subject but to be active about it, to speak and organise and arouse the conscience of the nation against Fascist danger.

At that time we also were beginning to feel this, and it was a letter written by Langevin himself to certain people here—I think it was actually written to Mr. Forster—which started a corresponding movement among writers and scientists in this country. We are apt to forget it now ; it was a long time ago, and in a sense the war has made a larger gap than many years would have done, but that body, which was then called " For Intellectual Liberty " was closely associated with the Committee with which Langevin was associated and to which I have referred.

We followed step by step the tragic events of that period. It began with the Anglo-German naval treaty at that time, which permitted the Germans to build those submarines which we afterwards had to use Asdic to sink. There was the occupation of the Rhineland, the step by step movements in Abyssinia, in Spain, in Austria and in Czechoslavakia. In every one of these we attempted to protest and to react. At the time it seemed to us that this type of movement, whether among intellectuals or in the wider popular sphere of the international peace campaign which Langevin himself took a very large part in founding, was to a large extent beating the air. It seemed as if the mechanisms of government were determined to go on making things easier and easier for Mussolini and Hitler.

I think that we can say now, however, that Langevin was right, that it was necessary to protest, however uselessly ; it was necessary to arouse opinion ; it was necessary to build up an intellectual resistance before there could be a physical resistance. From what you have heard and from what we know happened in France and from what happened in this country we can see that that work was not in vain, that it built up, as the surrender proceeded step by step, a determination on the part of larger and larger numbers of people to react against it, and in time, when things went too far, that reaction became the dominant feature. In France I think that it was also the basis of the resistance movement, in which scientists like Biquard, Joliot-Curie and Langevin played such a prominent part. They played that part because they had been conditioned to it beforehand by the kind of work which Langevin had been doing through all that period.

I should like to refer to another and more technical aspect of his influence. Langevin, on account of his interest in the processes of warfare in the first world war studied and had fought against the tendencies which had been prevalent in the interval. One of those tendencies was to rely on the terrorisation of civil populations through the threat of air bombardment, and particularly the threat of gas. It was from Langevin that in 1935 I first became aware of the technical side of that question. I have a certain diffidence in mentioning this in the presence of Sir Henry Tizard, but it was that influence which started a very curious body, the Cambridge Scientists' Anti-War Group, in which Dr. Wooster among others played a very notable part. It was, I think, due to Dr. Wooster that we started being scientists in a real sense—that is, in actually carrying out experiments—and there we had very considerable help and suggestions from Langevin.

What we did not understand at the time, but only found out later, was that the experiments which we were carrying out in a purely voluntary way were on the whole rather better experiments, and certainly carried out by more, and better qualified, people than those of the Government of the day. We could not believe that, because we could not believe that a Government which was managing to spend so much on science was in fact using it so little. In a curious way, the Cambridge Scientists Anti-War Group became, as it were, a place of apprenticeship for a number of scientists who afterwards came to play important parts in military science.

I remember discussing with Langevin the concept of the radius of destruction of any particular military weapon, such as a bomb, and this statistical theory of bombing was made use of in the actual struggle against Fascism when it came out into the open in a military way. Perhaps, therefore, in a technical as well as in an ideological way " premature anti-Fascism " paid a dividend. It was there also, I think, that we had the beginnings of a really close and effective scientific liaison between the two countries.

This went on into the early days of the war. I remember very well a very sad occasion of a dinner in Langevin's own house with his family, his daughter and Saloman, at which I was present, towards the end of April, 1940. I was there on some kind of official mission in connection with explosive phenomena. I remember Langevin then being in an extremely sad and depressed mood. He felt, and we were beginning to feel very much from our reception by the French military, that there was something very bad about the state of France at that time. We did not know how close the danger was, but we felt it to be imminent, and he felt it particularly, because he felt that at that time the country had already been betrayed, although the Vichy government existed only in embryo, but in very important political and military positions. It was, I think, the lowest point in his life.

I was very glad, however, to have the opportunity of seeing him again, while the war was still going on, but after the liberation, and of meeting him in Paris and finding that in spite of sufferings which he had gone through and of the losses which he had suffered he now felt that his work was really justified. He even told me that his imprisonment and his questioning by the Nazis had been to him a very valuable experience. The reason why it was a valuable experience was that after he had been questioned he said : " I now know what I have always believed, that there is just nothing intellectual behind this Nazi idea ; there is nothing but the old and sterile ideas against which I have fought all my life. There is no mystery or secret of power in the Nazis." " That hope," he added, " held me up for the latter part of the war." He found then what we have ourselves learnt since, though not all of us and not sufficiently ; we now know from the various studies which have been made of the Nazi war machine that, excellent as it was in detail, perfect as it was in certain respects in discipline, it suffered from all the evils of the normal capitalist regime of pre-war Germany, or in fact anywhere else. It was riddled with corruption ; it was full of inefficiencies which were all the greater because they were covered up. He realised that in fighting against the Nazis we were fighting against all the forces which had held up the national application of science in the past, and by the defeat of the Nazis we were at the beginning of being able to make possible the utilisation of science for the ends which we have always had in view.

That was the task which Langevin set himself after the war. M. Biquard has spoken of his educational work, and I think we should recall also the important work which he did in helping the general organisation of science in France, and the work which he did, politically and scientifically, in building up the National Centre of Research. The French situation, so far as science is concerned, has not been a happy one, because France was the last great scientific country to stand for absolute individuality, and what went with it, the lack of support for those individuals. The greatest of all French scientists, Louis Pasteur, who contributed so enormously to the whole of our present-day life—most of us would not be alive today if it were not for Louis Pasteur—suffered all his life from inadequate support. He suffered that quite consciously and making violent protests against it—because he was a violent creature. It was not until the Popular Front Government came in in the 'thirties that a real attempt was considered for giving adequate support to French science. The National Centre of Scientific Research has served as a useful model of how to couple full respect of individual achievements in science with the necessary organisation and support of buildings, apparatus and assistance that are needed. We have made use of that to a certain extent consciously in the remodelling of our own organisation of science, of which Sir Henry Tizard is now our director, and I hope that it will now be possible not only to exchange ideas on how to manage science but to build up in the future a very much closer and more organised relation between science in this country and science in France.

One of the most useful things that we could do for the memory of Paul Langevin is to see that his work is carried on, and his work was not limited to the boundaries of France. He considered himself, and he was himself very much more truly than the attribution he gave to me, a citizen of the world. I feel that we should be carrying on his work if we could build a really international scientific organisation to promote the application of science to general human betterment.

We are beginning to do that, and we have at any rate the governmental framework of that in UNESCO. We have to facilitate organisation on the governmental plane. We have also —and I want to mention this, because this is the origin of this meeting—in the World Federation of Scientific Workers the beginning of a rank and file organisation which will put energy behind the government organisation for the international utilization of science. I hope that the World Federation, which has made this its first international meeting, will continue in that way and bring out the value of the contributions of great men of science to the world outside their own countries. I think that I should announce here that the next meeting of the World Federation will take place in the autumn in Paris to celebrate another great scientist, Lord Rutherford. In doing so we hope to bring out to this age, which owes so much to that band of workers at the Cavendish Laboratory in 1897–8, to J. J. Thomson, to Rutherford, to Langevin, to Wilson and the others, the part which must be played to turn that revolution of science into a revolution for human welfare.

Message from Professor C. T. R. WILSON, F.R.S., Nobel Laureate

I greatly regret not being able to attend the meeting in memory of Paul Langevin, whom I had known and admired for fifty years.

I have the happiest memories of our friendship, begun when he came to the Cavendish Laboratory at the outset of his scientific career. I remember how he impressed me then as overflowing with life and energy and kindliness—and this is the picture of him which I have always retained.

I should very much have liked to be present at the memorial meeting.

C. T. R. WILSON.

AVERTON,
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SCOTLAND.

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