from a life of physics

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FROM A LIFE OF PHYSICS

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FOREWORD: TWENTY-ONE YEARS AFTER

It is nearly 21 years since the organisation of the lectures on their lives of Physics which are reprinted in the accompanying volume. My first thought is sadness at the number of those who lectured then and who have departed from us (these include Professors Werner Heisenberg, Oscar Klein, Paul Adrian Maurice Dirac and Eugene M. Lifshitz).

Hans Bethe and Eugene Wigner are fortunately with us. May they live very long and active lives, to act as inspiration for us all, as they always have in the past.

These lectures were given within the context of a month-long Conference on Contemporary Physics which assembled to review the whole subject of Physics. It was natural to ask those who had created the subject to speak of their lives of Physics and to list the problems which they felt were still unsolved.

One of the problems which falls in this category is that of the interpretation of Quantum Mechanics – not only for individual particles but also for the universe as a whole. Some of the problems – on the other hand – particularly the problem of infinities which was Dirac's major worry – seem to be near to some sort of resolution by the heterotic superstring, at least in ten dimensions. (Fundamental

Particle Physics has moved in other directions which are concerned with the unification of fundamental forces, on the model of the electroweak unification; leading on to the Grand Unification Theories and culminating in Superstrings which promise to be Theories of Everything.) With the synthesis which phase transitions in the early universe provide us, the Standard Model of Particle Physics and Early Cosmology appear to have converged so that instead of two disciplines, we have just one scientific discipline. Naturally, other new problems have arisen, for example, High-Temperature Superconductivity, a subject ill-understood at the present moment. Large Scale Cosmology and Dark Matter present other unresolved issues.

But I wonder whether there is not a more significant change in the climate of Physics. We, at the Centre, are particularly sensitive to the recent emphasis on the role of Physics in development. In fact, we are now contemplating three new Centres to be set up on the lines of the International Centre for Theoretical Physics. These new Centres will cater particularly to the developing country needs and will consist of 1) an International Centre for High Technology and New Materials, 2) an International Centre for Earth Sciences and the Environment, and 3) an International Centre for Chemistry, Pure and Applied. Unlike the present Centre, these new Centres will undertake also experimental research and training. (The entire complex, consisting of the old and the new centres, will be called the International Centre for Science.)

I wonder whether a Conference organised by the new International Centre for Science will be slanted towards fundamental sciences only, as was the case in 1968 when there was the sense of gratitude and adulation which everyone felt towards the great men of Physics still amongst us.

It is in this context that the appropriateness of this volume being reprinted under the auspices of World Scientific Publishing in Singapore becomes apparent. Singapore was a developing country till recently but has, through its own efforts, based on a purposeful utilisation of modern science and technology, acquired a different status now. I wish to thank Professor K. K. Phua for insisting that this volume must be revised.

Abdus Salam
Trieste, 16 February 1989

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ENERGY ON EARTH AND IN THE STARS

Hans A. Bethe

Professor Salam opened the series of lectures by saying:

One of the purposes of the Symposium now in progress, as we conceived it, was to try to bridge among the many who are here the generation gap; to bring nearer to us the men who have created our subject and whom we have all admired from a distance. So a Life of Physics series was conceived to run coincidently with the Symposium. It provides the opportunity for some of our Grand Old Men to tell us the milieu of *physics* they have helped to create, illustrating it through their own work. We were unfortunate that Professor Weisskopf, who was to have been our first speaker, was prevented by illness from coming. So the starting of the series was delayed.

Tonight we have the privilege and honour of welcoming Professor Hans Bethe. Professor Robert Marshak has kindly agreed to take the chair. Professor Marshak, no stranger to Trieste, was one of the three, not old, but very wise men who selected Trieste in preference to other sites in 1963 for the location of the Centre. You will all agree what a wise choice his committee, which consisted of Professor Van Hove and Professor Tiomno besides Professor Marshak, made. Marshak is a member of the Scientific Council of the Centre.

R.E. Marshak:

Hans Bethe was born in 1906 in Strasbourg, Alsace-Lorraine. His father was a well-known physiologist at the University while his mother was a musician and writer of children's plays. Young Hans attended Goethe Gymnasium, a classical public school, and then left for the University of Frankfurt from which he was graduated in 1926. At the tender age of 22, Bethe gained his Ph.D. from the University of Munich under the famous theoretical physicist, Arnold Sommerfeld. Sommerfeld introduced Bethe to the excitement of modern physics and Enrico Fermi, with whom Bethe worked as a Rockefeller Foundation Fellow in Rome (1930–32), completed Bethe's early training. By 1935, Hans Bethe was settled at Cornell University — a refugee from Nazi Germany — and he has worked there ever since except for his years of war research and numerous visiting appointments.

I first came to Cornell in 1937 to do my graduate work under Professor Bethe. I got there rather accidentally by attending a conference on solid state physics which Bethe had organized. Bethe's versatility was already clear when I took up residence as a graduate student since he had just completed his monumental articles on nuclear physics. Within one year, Bethe's scientific activity had moved into the completely alien territory of astrophysics in the form of the epoch-making paper entitled "Energy Production in Stars". This paper was an outgrowth of a small theoretical conference at George Washington University (Washington, D.C.) organized by George Gamow and Edward Teller. Hans Bethe came back from this conference in the spring of 1938, greatly challenged by the problem of the origin of stellar energy. After several months, he had examined every conceivable nuclear reaction which might produce substantial amounts of energy under stellar conditions and had reached the conclusion that the carbon cycle and the proton-proton series of reactions were the two major sources of energy for the common main sequence stars. Bethe's profound analysis of the thermonuclear processes operating in the stars led to his winning the A. Cressy Morrison Astronomical Prize of the New York Academy of Sciences in 1938, the Draper Medal of the National Academy of Sciences in 1947, the Eddington Medal of the Royal Astronomical Society in 1963 and, finally, the Nobel Prize in 1967. Much more could be said about the importance of Hans Bethe's contribution to modern astrophysics but I merely point out that the tremendous effort underway at present to achieve self-sustaining fusion reactions in contained plasmas is a heroic attempt to duplicate on earth the thermonuclear processes so thoroughly analyzed by Bethe for the stars.

When World War II broke out, Bethe demonstrated his ability to apply his physical knowledge to problems of practical importance when the need arises. He first applied his knowledge of electromagnetic theory to radar problems at the M.I.T. Radiation Laboratory and concluded his war work as the Head of the Theoretical Division of the Los Alamos Laboratory. I served under Bethe at both laboratories and can testify to the tremendous energy and understanding which he brought to bear on the most diverse applied problems.

After the end of the war, in the summer of 1946, Bethe and I were both consultants at the General Electric Research Laboratories in Schenectady, trying to communicate to this active laboratory the new wonders of "atomic energy". Some of the "youngsters" we educated at that time in nuclear reactor theory were Harvey Brooks and Henry Hurwitz, destined to become scientific leaders in their own right. And a year later our paths crossed again at the famous Shelter Island Conference where Bethe was inspired to work out the non-relativistic theory of the Lamb shift.

A couple of years ago I decided to arrange for a little volume in honor of Bethe's 60th birthday which would try to reflect and recapture the broad and versatile contributions which he has made to almost every branch of physics. The response was so overwhelming and the coverage of physics and astronomy so complete that I was compelled to use Sam Goudsmit's classification for the *Physical Review* to order properly the various articles in the book!

PLEASURE FROM PHYSICS

Hans A. Bethe:

When Salam asked me to give this talk he wrote that I should talk about the things in physics which I particularly enjoyed. That is exactly what I am going to do.

The first work which I enjoyed very much was the paper about the stopping power of matter. The Born theory of atomic collisions had just appeared and Elsasser had applied it to the scattering of electrons from hydrogen atoms, both elastic and inelastic. Out of that calculation there came rather long and unwieldy formulae, getting worse the higher the quantum number of the excited state, and there was no way to foresee how terrible these formulae would be when you came to the excitation of states in the continuous spectrum. So I thought this was not a way to make a living and one should be able to do it more simply. Essentially I did two things in this paper, one of which was to discover Poisson's equation. The Born approximation tells you that the scattering amplitude is given by

$$\int V(\mathbf{r})\, e^{i\mathbf{q}\cdot\mathbf{r}}\, d^3r.$$

The potential $V(\mathbf{r})$, in the case of elastic scattering, is of course just that made by the charge distribution of the atom, and $\mathbf{q}$ is the change of momentum. As said, I discovered Laplace's equation, by saying that the potential after all is connected with the charge density and thereby the Born approximation result can be transformed into

$$q^{-2} \int \rho(\mathbf{r})\, e^{i\mathbf{q}\cdot\mathbf{r}}\, d^3r.$$

This, of course, gives a very much simpler relation to the actual properties of the atom, since the density is directly given by the wave function.

The second trick I used in the paper was a sum rule. Of course this was not the first time that sum rules were used. The Kuhn-Reiche-Thomas sum rule had in fact been the basis of quantum mechanics, a couple of years earlier, but sum rules had not yet been thoroughly exploited for the simplification of results in quantum mechanics. Now, it is simple to get a sum rule for the total cross section. In the case of inelastic scattering, of course, $\rho(r)$ is replaced by

$$\Psi_n^*(\mathbf{r})\ \Psi_0(\mathbf{r}),$$

the final state times the initial state wave function. If you sum the cross section over all the excited states you essentially get the absolute square of the operator

$$\sum_i e^{i\mathbf{q}\cdot\mathbf{r}_i},$$

which is unity in the case of hydrogen; so you can tell the total cross section for excitation of all states.

EXPRESSING ENERGY LOSS

The sum rule which was not quite so trivial was that which corresponds to the energy loss of a particle. The quantity I have discussed is the scattered amplitude $F_n(g)$, its absolute square is the cross section. If you now multiply this square by the energy loss of the incident particle and sum over all excited states, this gives you the probability of scattering, multiplied by the average energy loss; it essentially gives you the energy loss per atom traversed. Fortunately, I found that there is a very simple sum rule for this: in fact it is even simpler than the one giving the total cross section. If q is very small, the matrix element reduces to q times the dipole moment, and hence the energy loss sum becomes just the Thomas-Reiche-Kuhn rule, which gives the number of electrons. But the interesting fact was that also in the case where q is not small, this energy loss sum also gives just the number of electrons. This, I still think, is very remarkable and it certainly was very pleasant to me at the time. So I was able, starting from the theory of Born, to arrive at a closed expression for the energy loss, per unit length of path, for the charged particle traversing matter.

I should mention that I used this paper to become a privatdozent in Munich. As you know, this is a special ritual in Germany. Having a doctor's degree does not entitle you to be a teacher at a German University, you have to pass a second examination which consists of writing an acceptable paper and presenting a certain number of "thesis", i.e., one has to make a number of claims and the whole faculty of science can come and attack the candidate and prove that his claims are wrong. Of course, this is only a formality and the faculty is very gentle!

After a year or two, I met Professor Blackett at Cambridge and he told me: "Now look here, you have made a theory of the energy loss of charged particles, but your qualitative results are no good to me, I really want to know this energy loss quantitatively, and I want to know it so accurately that I can measure the range of a particle and from that range deduce the energy of the particle." At that time, it was quite difficult to have a good enough electrical apparatus to measure the energy of a particle by electric and magnetic deflection. The range was then the most accessible measurement of the energy of a particle. So Blackett said, "Well, there is a paper by Mr. Duncanson who has calculated the range on the basis of the old Bohr theory which we know is not very good, why don't you do it again on the basis of quantum theory?" So I was led into generalizing my theory to the case of complex atoms. This introduced the average excitation potential of the atom which was still an empirical constant which had to be determined from the measurement of one range for a certain energy; from that, the range-energy relation for all energies could be deduced. This scheme worked surprisingly well and kept me busy for many years refining it, that is, my students and I put in corrections for the fact that some of the electrons have strong binding energy.

Then, a few years later, Moller's theory of relativistic collisions came along. One of the things I did was to apply this to the stopping power problem, and I found that it worked out pretty much in the same way as in the non-relativistic case. I found out just a few days ago that almost at the same time, Oppenheimer did the same calculation. Well this much for the stopping power; it was a most satisfactory field because you could come from first principles to something which could really be compared with experiment, and could actually be helpful to the experimentalist in his interpretation.

SOLVED IN THE SUBWAY

The most satisfactory period of my life was in the 1930's, the development of nuclear physics. This started in Manchester when I lived and worked together with Peierls and we were both very interested in the deuteron. This was being investigated experimentally at the time by Chadwick and Goldhaber. We considered especially the relation of the binding energy of the deuteron to the scattering of neutrons by protons. We found at that time (this was mostly Peierls' suggestion) a very close relation between the scattering cross section and the deuteron binding energy, essentially the scattering cross section was

$$\sigma = \frac{4\pi\hbar^2}{M} \cdot \frac{1}{E + \epsilon},$$

i.e., a constant, divided by the energy of the system of the neutron and the proton in the centre-of-mass system, plus the binding energy. Well our theory was very nice, but it did not agree with experiment! The better the experiments the worse the discrepancy. Finally, the solution to this was told to me in 1935 in a subway train in New York by Eugene Wigner. I don't know, I must have been able to hear much better than I do now, which I know is true, and probably Wigner spoke louder than he does now! At any rate, I was able to hear him in the subway, and he said, "Now look here, all that the deuteron tells you is the interaction of neutrons and protons in the triplet state: How do you know how they interact in the singlet state? Probably they interact quite differently." So he solved this problem between Columbia University and Pennsylvania station, but he never published it. I published it, giving him credit for it, in one of the three *Reviews of Modern Physics* articles, but I am very sorry that he never wrote it up.

ATOMIC MASSES

In addition to the binding energy and the scattering, we also did the photoelectric disintegration of the deuteron which just at that time had been observed by Chadwick and Goldhaber. It was Goldhaber who led me to the second subject in nuclear physics, by pointing out that the atomic masses were in a terrible mess. This was especially true of ^{9}Be. According to the published masses, ^{9}Be should really not exist, because it had a mass bigger than a neutron plus two alpha particles! So the next thing I did was to look at the atomic masses and try to use as much as possible, energies which came from nuclear disintegrations, which by that time had been observed in large numbers, and to take only those mass spectrograph measurements which seemed very reliable. Essentially only the measurements made at Cambridge and at Harvard fell into this category. Combining these with disintegration data I was able to construct a table of atomic masses. This was about my first work at Cornell in 1935, and this table no longer gave to ^{9}Be too large a mass, but made it perfectly stable. The deuteron also was in this category. The mass spectrograph data had indicated that the deuteron also ought to disintegrate spontaneously into a neutron and a proton. In the course of time, the mass spectrograph people changed their data and confirmed that really the nuclear disintegration values were the correct ones.

At Cornell I was in contact with experimentalists. We had a cyclotron, the second one ever built, which had been built by Livingston who had been a very close collaborator of Ernest Lawrence. Our cyclotron was very small because the department could only afford, I think, two or three thousand dollars to build it. For many years we were very proud that we had the smallest cyclotron in operation. We claimed that per dollar and per kilowatt, we have produced more research than any other cyclotron.

Well I found that the experimentalists didn't know much about nuclear physics and that it was very laborious to explain the same things to one experimenter after another. So I decided it would be much easier to write it down, and so I wrote the three articles in the *Reviews of Modern Physics* which Segrè called the "Mattoncino", (a small rock I think). In these I put down most of the things that were then known about nuclear physics. This was lots of fun; I had two collaborators, Konopinski and M.E. Rose who sat in an office together and did all the calculations that I needed done. We would only interrupt this work when Konopinski came to tell me it was time to eat; so we would go to eat, as Marshak described it. We were able to fill in a great number of gaps in the then existing knowledge, like excitation functions, something about the diffusion of neutrons, and quite a bit about binding energies of light nuclei. Also at that time people began to play with the shell model. The shell model worked fine up to 40 Ca but not beyond.

It was very satisfactory to describe all this in the *Reviews* article, and it was even more satisfactory that many experimentalists were interested in confirming some of the theories that we had put down. There was an especially close collaboration with the University of Rochester which had a bigger cyclotron, and therefore could test quite a number of the theories of excitation functions that we had developed. At about that time, Bohr invented the compound nucleus, Breit and Wigner found the dispersion formula, and in connection with the resonances predicted by these theories I did the only experiment which I ever had done personally. There was an experimental graduate student at Cornell who measured the radioactivity produced in silver by slow neutrons and our problem was to prove that the energy of these neutrons was different from the thermal energy. We did this with the help of the boron absorption which was simultaneously used for the same purpose in England by Moon and others. In this case, I really sat at night in one of the laboratories and counted the number of counts made by these neutrons.

SOLID STATE

Marshak has mentioned solid state as the reason why I was not hired by Pauli. I worked on solid state but I must say at the time it was a far less satisfactory pursuit than nuclear physics. It was really much too early to do solid state seriously. My ambition at the time was to calculate such things as the shape of the Fermi surface for the electrons in silver or at least in sodium, and then to have some experimental confirmation for this. By now we know the shapes of the Fermi surfaces of these substances, but at that time there was absolutely no way of doing it, all you could measure was conductivity and a few thermoelectric and magnetic effects, gross numbers which certainly would never give you any information of the kind I wanted. Wigner and Seitz, at the end of the period I worked on solid state physics, invented their very powerful method to determine theoretically the allowed bands of electrons in metals. This was obviously the way to do it, but at least I lacked the mathematical power to put this method really to work and to get out of it the information which you need to get, let us say, the shape of the Fermi surface.

There were two other things which I did on solid state theory. One was the splitting of atomic energy levels when the atom is inserted into a crystal, into a site of given symmetry. I did that essentially only because I had studied a book on group theory, and you can't really understand something unless you apply it and work with it yourself.