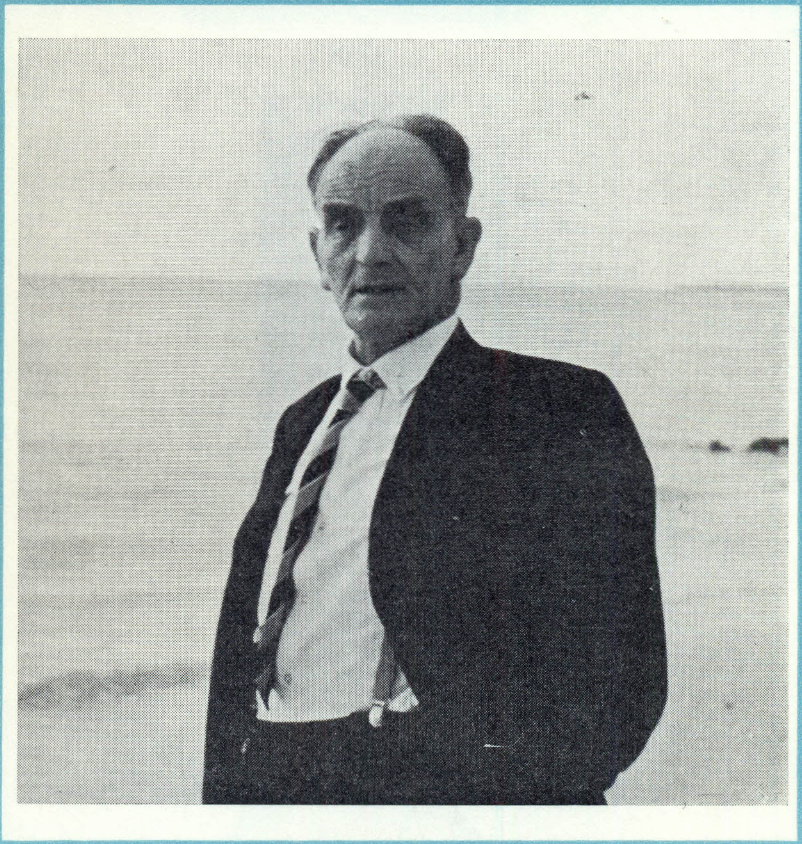
Fra Fysikkens Verden

UTGITT AV NORSK FYSISK SELSKAP

INNHOLD



Professor E. A. Hylleraas

Nr. 2 - 1963
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Utgiver: NORSK FYSISK SELSKAP

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Hylleraas-symposiet

Vi gjengir nedenfor en amerikansk pressemelding fra Hylleraas-symposiet i kvantemekanikk på Sanibel Island 14.–19. januar i år.

Amerika hyller den norske vitenskapsmann professor E. A. Hylleraas ved et internasjonalt symposium i Florida.

Et symposium i kvantemekanikk til ære for den berømte norske fysiker, professor E. A. Hylleraas, har funnet sted ved det velkjente feriested, Sanibel Island, ved den meksikanske gulf.

Deltakerne i konferansen representerte ikke mindre enn 27 land. Blandt de omlag 200 deltakerne var en rekke kjente fysikere fra U.S.A. og andre land.

Symposiet var arrangert av Vinter-Instituttet i Kvantekjemi og Faste Stoffers Fysikk under ledelse av dr. P. O. Løwdin, som er professor ved Florida University i Gainsville, Florida og ved Universitetet i Uppsala.

Professor Hylleraas ga sine meget viktige bidrag til atomfysikken allerede i 1926–28 mens han studerte hos Nobelprisvinneren professor Max Born ved Universitetet i Göttingen. Det var ved denne tiden at de epokegjørende oppdagelser i atomfysikken ble gjort som ga en fullstendig ny forståelse av teorien for atomspektrene og materiens oppbygning.

Grunnlaget for kvantemekanikken som vi kjenner den i dag, ble lagt i løpet av disse få årene som professor Hylleraas ofte kaller atomfysikkens gullalder. For å sitere Hylleraas: «Dette var en vidunderlig periode i bølgemekanikkens utvikling da man ikke kunne unngå å gjøre nyttig arbeide, dersom man

Professor dr. E. A. Hylleraas og frue utenfor hotellet Casa Ybel på Sanibel Island.

var så heldig å være tilstede på det rette sted til den rette tid. Når de store guttene var ute etter storvilt, kunne de mindre guttene lett finne mindre vilt i nærmeste nabolaget».

Det enkleste atomet, Hydrogenatomet, som bare har ett elektron var allerede blitt forklart i 1913 av professor Niels Bohr. I 1925 var der en ny utvikling av kvantemekanikken ved Schrödingers, Heisenbergs og Diracs arbeider. Den generelle gyldighet av denne nye teori, den såkalte bølgemekanikk, kunne bare bevises ved å anvende teorien på mer kompliserte systemer. En avgjørende prøvesten for bølgemekanikken, fremholdt professor Born, var anvendelsen av teorien på Heliumatomet, som med to elektroner er det nest enkleste atom. Flere fysikere hadde forgjeves gjort forsøk på fra bølgemekanikken å beregne den korrekte ioniseringsenergien for Heliumatomet, som er den energi som er nødvendig for å fjerne ett av de to elektronene. En tid så det nærmest ut som om bølgemekanikken ikke skulle være i stand til å forklare dette atoms egenskaper, og at derfor teorien ikke kunne være fullstendig.

Det vakte derfor stor oppsikt da professor Hylleraas fremla et arbeide for de Skandinaviske Naturvitenskapsmenn's møte i København i 1929, der han ut fra bølgemekanikken hadde beregnet Heliumatomets ionisasjonsenergi og funnet perfekt overensstemmelse med den eksperimentelt fundne verdi. Dette ble i vide kretser ansett som det beste bevis for teoriens generelle gyldighet — også ved streng numerisk behandling. Dette ble grunnlaget for en metode som senere er kjent blant fysikere som Hylleraas-metoden, en metode som er og vil være av stor verdi for anvendelser også på tyngre atomer. Dette ble understreket under Sanibel-symposiet, hvor en rekke bidrag basert på Hylleraas-metoden ble lagt fram.

Ved det ovenfor nevnte København-møte ble det klart at Hylleraas-metoden kunne anvendes også på ioniserte atomer. Hylleraas hadde under det videre arbeide som eksperimentell medarbeider den svenske professor Bengt Edlen, den senere så berømte spektroskopist.

Professor Hylleraas har også æren for å ha oppdaget det negative Hydrogen ionet, som er et Hydrogenatom med et ekstra elektron. Den gang var dette ionet ikke blitt observert, men senere — i 1939 — viste astrofysikeren dr. Wildt at dette ion er til stede i himmellegemer og at dette er den vesentlige grunn for opasiteten av solens atmosfære.

Selvsagt blir professor Hylleraas i amerikanske kretser betraktet som Norges ledende fysiker og grunnlegger av teoretisk fysikk i Norge.

Professor Hylleraas's aktive vitenskapelige arbeide har ikke på noen måte avtatt med årene, og han arbeider i dag aktivt med mange forskjellige problemer i teoretisk fysikk.

Professor Hylleraas ga åpningsforedraget ved symposiet på Sanibel Island, og han holdt tilhørerne tryllebundet under hele foredraget som ble framført på hans sedvanlige beskjedne, men ytterst givende, vittige og dyktige måte. Han gjenkalte i minnet tiden fra de vidunderlige dager i Göttingen inntil i dag, og da han avsluttet, ble han begeistret hyllet av sine tilhørere.

Han mottok også en rekke gratulasjoner fra U.S.A. og fra andre steder. Bare for å nevne noen få: Adresser fra Floridas Secretary of State, Tom Adams, fra den norske undervisningsminister ved vitenskapsattasjeen ved den Norske Ambassade i Washington D.C., dr. Erling Christophersen, fra Det Kongelige Norske Videnskabers Selskab og fra fremstående vitenskapsmenn fra alle verdens kanter, og fra mange andre.

Lederen for symposiet, professor Løwdin, uttrykte sin og alle deltageres takknemlighet over at professor Hylleraas hadde kunnet være til stede og således gi inspirasjon og verdifull hjelp til sine yngre kolleger.

«EKSOTISKE» HYPERKJERNAR

I 1953 identifiserte dei polske fysikarane Danysz og Pniewski den første hyperkjernen, dvs. ein atomkjerne som inneheld eit bunde Λ-hyperon i staden for eit nøytron, i ei stjerne produsert av kosmisk stråling i emulsjon.

Nyleg har dei same to fysikarane saman med Zakrzewski identifisert ein dobbel eller «eksotisk» hyperkjerne med to bundne Λ-partiklar, også i emulsjon. Funnet skjedde innafor ramma av det europeiske K-meson-samarbeidet. Eit 1.5 GeV/c K^- frå CERN PS produserte eit Ξ^-, som reagerte med eit proton i ein kjerne. Vanlege konserveringslover (medrekna dei for baryontal og «strangeness») gjer to bundne Λ til dei einast tenkelege reaksjonsprodukta. Danysz og medarbeidere observerte spaltinga av denne «doble» hyperkjernen til ein vanleg hyperkjerne, og igjen spaltinga av denne vanlege hyperkjernen. Sannsynleg skjema er

$$_{\Lambda\Lambda}\mathrm{Be}^{10\text{ eller }11} \to {}_{\Lambda}\mathrm{Be}^{9\text{ eller }10} + p + \pi^-$$
$$_{\Lambda}\mathrm{Be}^{9\text{ eller }10} \to 2\mathrm{He}^4 + \mathrm{H}^{1\text{ eller }2} + \pi^- .$$

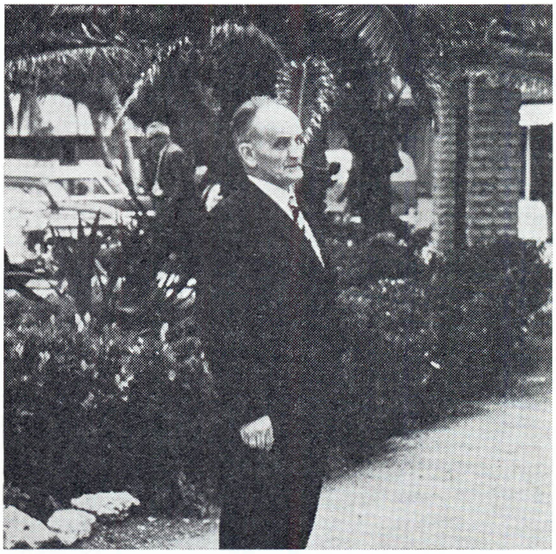
Vanlege hyperkjernar er av stor interesse for studiet av kreftene mellom nukleonar og hyperonar. Doble hyperkjernar er einaste fenomenet som tillet ein å studera kreftene mellom to hyperonar.

H. Øverås, CERN.

REMINICENCES FROM EARLY QUANTUM MECHANICS OF TWO-ELECTRON ATOMS

Egil A. Hylleraas

Professor dr. E. A. Hylleraas' innledningsforedrag ved symposiet i kvantemekanikk på Sanibel Island (Hylleraas-symposiet) 14.–19. januar i år gjengis her på originalspråket. Dette foredrag vil senere også bli trykket i Reviews of Modern Physics. Ved velvillig imøtekommenhet fra redaktøren for Reviews of Modern Physics, professor dr. E. U. Condon, har Fra Fysikkens Verden fått tillatelse til å bringe våre lesere professor Hylleraas' interessante foredrag.



Maybe some of you will be dissatisfied by learning that I am not going to talk in technical terms of the announced helium problem. I am afraid that in that way I should very soon have used up my time in writing equations, which all of you already know, on the blackboard so I have rather chosen to call my lecture: «Reminiscences from early quantum mechanics of two-electron atoms». This sounds like history, and it might well have been a good thing if I had been able to give you a reliable piece of the history of quantum mechanics. But that again is not in accordance with my abilities nor with my aims. I shall prefer to talk quite freely of reminicences coming into my mind. In our language this might be called «å spinne en ende», meaning perhaps something like «spinning yarn», and is a method supposed to be used by old sailors, or stated in another way: What I am going to tell you might be thought of as «nearly true».

In acting in this way I have, of course, already taken the highest possible advantage of being invited to this symposium as its honorary president. Privately I am thinking that this must have very much to do with my age rather than with my achievements, but officially, of course, I will try to look as though it were otherwise.

For several months now I have delighted myself by being a guest at the University of Wisconsin. The city of Madison should be praised for many things, its beautiful nature and its fairly dense Scandinavian population. But I think that its University of Wisconsin should also be praised for having excellent leaders of scientific research, among which are found Joe Hirschfelder and Julian Mack, whom in addition from personal experience I have found to be quite tolerable masters to work for.

As a last step towards happiness I may mention that professor Löwdin has taken me over to Florida for two months in order to save me from the cold winter up in Wisconsin. And now I think I should do my presidential duty by wishing all of you, in the name of Dr. Löwdin, heartily welcome to this International Symposium on Atomic and Molecular Quantum Mechanics. May you have pleasure of it, and may you learn from it. Personally I have already met a number of colleagues over here who have proved very anxious to come to this meeting or its preceeding courses, as well as many who have expressed their great joy and satisfaction by having had the opportunity of joining corresponding courses or symposia in Sweden. The other way I have also complaints from European colleagues who for some reason or other have been prevented from coming.

For this reason, in addition to his great achievements in the quantum theory of atoms and molecules, we are very proud of Löwdin, not only in Sweden, but all over Scandinavia, a distinction which perhaps means about as much to you as a distinction between Minnesota, Iowa and Wisconsin.

In the time foreseen for this speech I will choose to concentrate upon a certain period, which I like to call the Golden Age of atomic physics and which I place in the years 1925–1930 or maybe a year or two more, until the overwhelming discoveries in nuclear physics began to flow. In particular I shall concentrate upon the Göttingen school under the eminent leadership of Max Born, in a very happy and idyllic period of time, when the disasters of the first World War had begun to be forgotten and a second one was not expected to come.

In this wonderful period of early quantum mechanics, when old bonds were loosening and things had to be put together in a new and different way, there was no escape from doing some piece of work if you just happened to be present at the right place in the right time, and so I myself hit upon the helium atom.

Göttingen was my first visit abroad, where I stayed most of the two years 1926–1928 and a few weeks in the summer of 1931. Gradually I became acquainted with or at least learnt to know a considerable number of already recognized physicists and even more the physicists of the coming age. Some short visits to Berlin or other places extended this personal gallery to scientists like Einstein, Schrödinger, v. Laue and other Berlin physicists, and in Leipzig I met Heisenberg, Hund, whom I already knew, and for the first time also Felix Bloch, now at Stanford. For some foolish reason I never came to Munich until a long time afterwards, a year or two after the unexpected death by an accident of Arnold Sommerfeld, whose famous book: «Atombau und Spektrallinien» had been our student bible with the finest outlook to a new world that could ever be given. On the basis of later private correspondence I had come to think of Arnold Sommerfeld as a personal friend, so I regretted sincerely never to meet him.

To France and England I came only some years later in 1933 on a rather non-scientific trip, although from Holland I remember my friend from years ago H. a. Kramers and also Paul Ehrenfest. In Paris I met both Irene and Frederic Joliot-Curie, whereas Marie Curie excused herself for failing health. In England I met only some few of the Cambridge physicists. However, in 1934 at a Conference in London and Cambridge practically all the English physicists whom I knew by name were present, even the «grand old men» F. F. Thomson and Rutherford, together with Dirac, Darwin, Fowler, G. P. Thomson, the two Braggs, and not to forget D. R. Hartree and J. Lennard-Jones and also a number of French, American and other scientists. There I also heard and saw for the first time young Fermi, speaking a very melodic Italian-English or maybe French. In France in 1936 some acquaintances were renewed and some were new. In particular I remember Bauer, Brillouin, de Broglie, Langevin and his son-in-law, Solomon, who did not survive the war, and finally my half Scandinavian friend, Dr. Rosenblum.

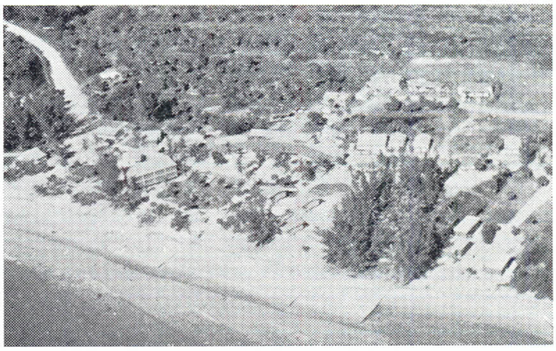
Of course, my best source of knowledge of contemporary atomic scientists was in the early thirties and through the thirties until the war the yearly meetings at Bohr's institute in Copenhagen. In these meetings you were sure to find colleagues from all parts of Europe and even quite a number from America, too many to be mentioned by name, even though they may have played an important role in development of atomic and maybe even more in nuclear physics. Even the most outstanding persons like Kramers, Ehrenfest, Pauli, Lise Meitner and so on did not come there to prove their prominence, but just to listen and learn.

May I — at the expense of your time — mention an event which has forever made a deep impression upon me, because it illustrates so nicely the role of scientific friendship in general and Niels Bohr's role in particular. For a couple of years the meetings ended up with some humorous performances directed preferably by Delbrück and Weisskopf, among which a new edition of Göthes: «Faust», on occasion of the discovery of the neutron, may have been the most successful, but what I am talking of now came a year or two later. In the audience there was one person, who more than any other one heartily enjoyed the entertainment, and this was Ehrenfest. On the final session the next day he gave a serious talk, thanking the Providence that still on earth a man could be found like Niels Bohr. What I did not know and probably very few did know, was that at the bottom of his heart he had a serious grief. Only a few weeks later his life came to an end.

Up to this point, I am sorry, I have been talking of things around myself, as a means of presenting my connection with science, it is true, but still too much of myself. And now I must shock you by telling that I see no other way of expressing what I have in mind than to proceed along the same route and even directly tell of my own life. When I do so, I do it in virtue of my age. I always liked to be young or believe I were so. But now almost all my friends and colleagues at the University of Oslo whom as a student or even as a member of the Faculty I looked upon with reverence have retired and of course many of them have come to life's end. In this country I have been even more struck by learning how a number of scientists well known to me, whom for their early achievements I have thought of as seniors or contemporary of myself, usually prove to be a number of years younger. Looking around in this audience the majority of faces appear so young that quite certainly their owners have their education considerably after the period which I have just baptized the Golden Age and even may be born after that time. Therefore, the few of you who can really claim to have grown up with atomic theory in those years of most intensive new creations are fairly soon counted.

There is also another thing, quite outside of our subject it is true, which on this occasion I should like very much to stress, and that is the unbelievable progress in our century not only in scientific research, but also in cultural life as a whole which of course is mainly a product of scientific progress. For this purpose I want to make use of myself as a medium; if you like you may think of me as «the missing link». In the art of stretching the time coordinate far back into the past I wonder whether anybody here could beat me, and this is of course because I am born in a remote part in the sparsely populated country of Norway. Hence as an artificial effect you may perhaps feel that I am rather talking of things from eighty, ninety or even a hundred years ago. Maybe it is worth while to stress first that in the course of time things have changed very much in direction of the American way of life. And this is caused by the last fourty or fifty years of automobilism and not less by lines of electric power being stretched to every house or hut in the part of our country I am talking of during the last twenty-five years.

However, from my earliest youth I remember the first very modest roads being built to replace the older trails or paths or water ways in order to make useful the 4000 years old invention of the wheel. The mail came once a week, another means for communication, unknown by the former generation, was a primitive telephone line. A railway train I had not seen up to the age of seventeen, and many were those who never came to see it.



Sanibel Island

We lived in a community where nobody was wealthy and, fortunately, very few were on our standard really poor. At the age from ten to fiften youngsters like myself were thought of as useful members of a family, who might for instance in summer time watch the cattle up in the mountains, on «fäbovallen» as it is called in Sweden. For this one should not be pitied. Provided good health, as really appeared a matter of course, I think we were not more and not less happy than young people are to-day. In my mind there even is a rosy colour now over this life in closest contact with nature. Ten hours a day in rain or sunshine as it might come. However we learnt to make a fire for our comfort, and on bright and warm days we were usually not far away from some small and cooling stream.

In winter time, two-thirds of the year, at our homes in the valleys, we went to school, three weeks in freedom and lazyness interrupted by six weeks at home devoted to what was thought of as useful work. We were also completely happy in the sense that the idea of higher education in the modern sense had no actual reality. Our tools were to be the hands, not the head. Of course, we did not estimate at all what we had learnt in our school, because that was common to all. Approaching the age of fiften there might therefore be some bright young boys or even girls who were talking of going beyond the borders of our community to some recognized school on a higher level and there in the course of a year or so become quite learned people or at least entitled to form some sort of brain trust on their return.

However, this was not a static state of affairs in this century of progress. What applies to my earliest childhood does not apply so well at the end. In particular the stress for education was an evergrowing process all over the country. At the beginning of and during the first World War the number of young people having acquired fairly much knowledge beyond what was taught in the elementary public school was steadily increasing, and perhaps it should be said that even our children's school was of a considerably higher quality than you might reasonably guess. And at the end of the war a considerable number of young people from all parts of our country, the coast line and the countryside as well began to invade even our highest centre of education, the University of Oslo. In the years 1918–24 I joined myself this category of students, whose main activum, as a rule, certainly was their optimism, their firm belief in future succes.

Particularly in the field of natural sciences this was a student's life in the old sense, because unlike doctors and priests and lawyers, and more like the humanists, we were not to be cast in the same form. We could do what we chose to do, and noboby had a responsibility for the result, except ourselves. We thought that what we acquired in mathematics, physics, chemistry or other fields was solid knowledge, and partly it also was. But the top aim of our learning usually was to become useful teachers in our high schools, and none of us or at least very few would hit upon the idea of becoming scientists of profession.

Nevertheless we could not avoid becoming a bit acquainted with scientific work and thoughts, as moreover at the end we had to present a written and, as it was called, scientific paper. For my own part, as an assistant at the department of physic, I was taken away from the first planned studies of pure mathematics into applications which were more to my heart and even into matters of pure physical research. In particular my field, in cooperation with Professor Vegard to whom I owe the highest gratitude, became that of the inner structure of crystals as explored by means of x-ray technique. Even with poor equipment for x-ray production and other necessities, as for instance an old dentist high voltage generator and self-made Debye-Scherrer cameras, this activity opened my eyes for the beauty of physical research, and the crystals became my first scientific love, besides, of course, applied mathematics in general.

Therefore, when leaving the university and becoming a school teacher for a year or two, feeling quite happy also there among the young boys and girls, I could not forget about many problems of higher interest which I had become familiar with. Being cut off from experimental work, it had to be replaced by theoretical studies' and here I had the great luck of being acquainted with a wonderful oook by none other but Max Born, Professor of theoretical physics at the University of Göttingen. This book bore the name «Dynamics of Crystal Lattices» or «Atomic Theory of the Solid State» in either of two editions, and I felt that it crowned the knowledge I might already have acquired from books of of authors like Siegbahn, Ewald, Niggli, Wycoff, Sommerfeld and so on. Moreover, this book furnished a basis for actual theoretical work in the field which I loved so much, and being happily free from a too high degree of hypercritisism I even ventured to publish one or two papers based on calculations of double refraction of light in monoaxial crystals in relation to their crystal structures. The basis of this particular part of crystal theory was laid, as you will know, fairly early by P. P. Ewald, still the editor of Acta Crystallographica.

This activity was to become my fate. The papers were seized by my friend Professor Vegard and attached with some kind acknowledgements from Max Born they provided for me from the always helpful America what was called a Fellowship of the International Education Board. This meant that you need not work any more, might go where you liked and still be paid as though you were of some use. So there was no escape any more. Like Julius Caesar at Rubicon — for nice comparison — I had to say: «Facta est alea» and embark for Göttingen and Max Born. But how insufficiently I was prepared for the scientific life in Göttingen just at this time you soon will learn.

First of all I was heartily disappointed when Max Born told me that he was no more working in the field of crystal lattice theory. His new field of investigation bore the curious name of Matrix Mechanics and, as I understood it, had been invented by himself or rather by some bright fellows by the names of Werner Heisenberg and Pascual Jordan, the latter being seen in Göttingen as Born's most prominent co-worker, the former being away somewhere in Copenhagen or Munich. Moreover, there was much talk of a curious sort of new waves called de Broglie waves. Obviously they did not exist in the sense of the word, since they were running with superlight-speed. Nevertheless people persisted in talking of their wavelengths as something of particular importance and being given by a simple formula reminding of some sort of quantization. Nonexistent waves in quantized form, quite a thrilling idea. Should I prefer them for my real crystals?

One day in the institute's library Dr. Kennard from over here was sitting and reading something which he told me was the Schrödinger wave equation, and he wondered why I was not doing the same, because that was just what people were doing now. So you see, there were plenty of new things to take care of for me, and perhaps you may understand that I had more confidence in the real existence of my dear crystals. The shock I had received from max Born had not quite knocked me down, and so I persisted for some time working in the field of crystal optics. I even succeeded in completing something which I thought of as a very fine piece of work, a calculation of the optical activity of the so-called β-quartz, a high temperature modification of quartz with somewhat simpler crystal structure. A preliminary report, from which I am myself now unable to reconstruct the whole work was forwarded to Zeitschrift für Physik, obviously a compromise in competition with the too fast-running time. The main work, designed for Zeitschrift für Kristallographie, and which I remember was clear in every detail, appears however never to have been forwarded to the Zeitschrift für Kristallographie and the manuscript in the course of time to have disappeared. The real cause for this misfortune must have been my deep absorption in studies of the helium atom, but even with success in this new field I have never felt fully comforted for the loss of this dear work.

So I betrayed my first love and entered onto love with a new one. Later on I have had several loves, but with moderate success. Obviously, in such matters you must be young, or, as the Germans are singing: «Das ist nur einmal, es kommt nicht wieder».

At this time the basic understanding of the helium atom with its two electrons and its double system of spectral lines or its para- and orthostates was already established by Heisenberg. Also a most interesting study of the wave function of the helium atom by Slater appeared fairly early, and I remember learning from it, particularly I think with respect to the mutual polarization effect between the two electronic distributions. Also the wonder of chemical binding forces as produced by the formerly unknown exchange integrals was on the way of being understood, thanks to the application of wave mechanics to the hydrogen molecule by the world-reknowned firm Heitler and London. The former was a Göttingen man acting as Born's assistant and a not too sophisticated person, who was both able and willing to share his knowledge with less experienced people.

You will, I hope, forgive my patriotism when I tell you that Heisenberg's theory of the helium atom was conceived in Norway. Heisenberg, perhaps the most wonderful of Sommerfelds «Wunderkinder» had not succeeded in acquiring a doctor's degree in Munich. The main reason for this misfortune is said to be some unnecessary stubbornness together with a complete lack of knowledge of the theory of the lead accumulator. Max Born obviously did not pay so much attention to the lead accumulator, so in a very short time Heisenberg found himself a doctor of the Georgia Augusta University of Göttingen. Then he felt a deep desire for complete freedom and went to Norway walking in the mountains several weeks entirely alone and with no connection with his family. This might have cost his life. One day when trying to pass a stream he fell into the water and had a very narrow escape. Back into some hotel he wrote his famous paper. How much the cold bath may have contributed to clear his mind I cannot tell.

On the whole the scientific life in Göttingen appeared somewhat frightening to a newcomer like me. Born's seminars were no children's school. To me all his pupils appeared extremely learned men, using methods which I had not heard of and talking in technical terms whose meaning I did not quite realize. Closest to an exception from this rule was perhaps Max Born himself. At least sometimes in the general physical colloquium I remember his proving his brilliant faculty in explaining even the deepest mysteries of quantum mechanics in simple words. As a master of teaching I should also like to remember Friedrich Hund, who was a frequent visiter in Göttingen at that time, and who is now Born's successor there.

As well known, wave mechanics at once reproduced all correct results obtainable from Bohr's theory, and the use of its much more convenient perturbation theory added considerable more, however not always in the strict numerical sense. Now, particularly by Max Born, it was argued that the simplest crucial test of the correctness of wave mechanics in general was to be found in its application to the helium atom, in particular to the ground state.

As well known from Sommerfeld's exposition of the matter in his, «Atombau und Spektrallinien», the Bohr theory, applying a definitely inconsistent ad hoc model of the atom with its two electrons in strictly opposite positions with respect to the nucleus led to a numerical value of about 28 electron volt for the ionization energy of the first electron. On the other hand, a simple perturbation treatment of the Schrödinger equation, as given by Unsöld, led to a much lower value of about 20.3 eV. The true value of 24.46 eV, as known from spectroscopic measurement, was about in the middle in between. Hence there was a broad gap of about four electron volts to be filled up.

The reason for the bad results is easily seen. In the Bohr picture with the electrons held strictly at largest possible mutual distance, the interelectronic energy came out too low. In the wave mechanics picture with independent spherical electronic charge distributions the interelectronic energy would be unreasonably high. About half of the gap might have been filled at once by letting the two electronic distributions expand to a reasonable degree such that enterelectronic repulsion and nuclear attraction would be more balanced. This means a minimization of the total energy and is best known from the use of a scale factor k, which by the minimization process also provides for the fulfillment of the virial theorem, which is no less important in quantum theory than in classical mechanics. Finally if we would think of displacing the electronic charge distribution relatively to each other, a bit to either side of the nucleus, remembering the Bohr picture, we would have what is called a polarization effect or correlation energy. But this requires a much more elaborate mathematical treatment.

As to the question of the scale factor or, what amounts to the same, a shielding constant or an effective nuclear charge, I remember a very early conversation with Dr. Kellner from Berlin and Professor Born where I did not fully understand what was meant with something called an arbitrary nuclear charge. Later on, of course, it became clear to me that Born, in arguing for such a freedom in the procedure, was pointing to the use of the variational principle. Hence, the invention of the scale parameter can be traced back to Professor Born, or rather, as I believe, to Dr. Kellner.

A systematic attack on the ground state problem of the helium atom had been planned by Max Born in cooperation with a pupil Dr. Biemüller, since Born himself had no preference for numerical work. However, the enterprise came to a stop by the failing health of Dr. Biemüller before becoming particularly useful. If brought to an end it would at all events have filled only about 2/3 of the gap between simple perturbation theory and spectroscopic measurement. This is an excellent example of the importance of the so-called completeness relation for system of functions used for the purpose of solving variational problems or equivalent differential equations. Biemüller's use of strict hydrogen wave functions, or rather products of such functions of the two electrons' coordinates, could never have led to a better result. Owing to the existence of a continuous energy spectrum the hydrogenic functions for the discrete spectrum do not form a complete system, and much the less products of such functions for the helium atom.

When Professor Born first suggested to me that — as he said — I was the right one to go on with the helium problem, I felt of course greatly flattered. The real cause for my turning that way was, however, that I felt I had gradually acquired some familiarity with the main principles of wave mechanics and hence was able to deem the problem to take my highest interest, not least because of its apealing «anschaulichen» character.

One thing which I noticed fairly soon was that solutions must exist which depend only on three coordinates instead of the full number of six, and these were the coordinates r_1, r_2, ϑ, defining the shape of the electron-nucleus triangle, leaving its orientation in space out of interest. When confronted with this really useful simplification Born asked: «What does that mean? Let us consult Wigner.»

Eugene Wigner was already at that time a central person among the young Göttingen pioneers. He was suspected to be familiar with some kind of black magic, called group theory. This was several years before the culmination of the so-called «Gruppenpest», when every paper on wave mechanics in order to be taken seriously had to start by stating the «group character» of its subject. Those who know Wigner will not hesitate to guess that he at once gave the correct answer: «Those states», he said, «are the S-states», i.e., with zero angular momentum.

Another and deeper question was that of the completeness of the functional system from which the wave function had to be built up. This problem was easily solved by removing from the argument of the Laguerre functions the main quantum number n, i.e., replacing r/n by r simple. This in fact means a transformation of the discrete eigenvalue spectrum E with series limit at $E = 0$ into that of $1/\sqrt{-E}$ with series limit at infinity. In this way the continuous eigenvalues spectrum is thrown away, and the functional system becomes complete.

In connection with these mathematical aspects of the theory it might be just to mention the valuable support for the whole Göttingen school provided by the two famous Göttingen mathematicians Richard Courant and David Hilbert, occasionally also Herman Weyl from Zürich as a visitor. The excellent book, Courant-Hilbert: «Methoden der mathematischen Physik» may be known to most physicists working in the field of quantum mechanics, and in those early days it was of course more badly needed than ever since.

Courant was an excellent lecturer, playing on his audience like an instrument. Hilbert was quite different. As an emeritus professor and Geheimrat his lectures were given only accidentally and voluntarily and out of pure interest for the new developments in physics. He never was in a hurry, on the contrary he rather seemed to like to taste repeatedly on his own sentences. He was extremely popular, and it was a real pleasure to listen to his mild voice and look into his white-bearded gentle face. To him, the inventor of the Hilbert space, the pathways leading from matrix to wave mechanics, and vice versa, were of course no secret, and this he expressed in the funny way of shaking his head, saying, «Die Nobel-preise liegen ja auf der Strasse». Again, talking of spectra and eigenvalue problems, in particular that of the hydrogen atom, he repeatedly murmured: «Rungs sagte ja immer, die Eigenwerte müssen sich im endlichen häufen. Ja das sagte er, sie müssen sich im endlichen häufen.»

This brings me to a serious question with respect to the mutual interference of physics and mathematics. If the mathematicians of the eighteen-eighties or -nineties had been clever enough, or rather, if they had been much more interested in the physical world, why should they have left us merely with the simple acoustic vibrations of finite bodies? Why not extend their investigations to infinite, maybe artificial, bodies as already indicated by mathematical tools like Bessel-, Hermite- and Laguerre- functions? Finally, why not transform sets of infinite numbers of eigenvalues in a number of different ways and so be able to present before the poor spectroscopists mathematical systems well suited for the classification of spectral lines?

In this way it might well have happened that some bright boy might have hit upon the Schrödinger equation 25 years earlier, and the Thomson and the Bohr theories might never have existed. Admitted, the true nature of atoms and electrons would not have been well understood before the Rutherford disclosure of the smallness of nuclei, but a Schrödinger wave equation might have existed. The conclusion we must draw from this is that the mathematicians as a rule are masters of logic but poor inventors. A mathematician primarily guided by the physical way of thinking, like Erwin Schrödinger, was needed to find the way.

In those days an institute for theoretical physics was not supposed to be in need of any room for its own purpose. It existed as a formal entity in virtue of the presence of a professor or leader, mostly residing at his home. Nevertheless, Born had — apart from his own office — succeded in acquiring a room for his institute in which I had the opportunity to work when needed, and in his humorous way he told me how. One day James Franck came to him to explain how badly he needed a room which was just going to be finished at the institute. But now his senior colleague and Director at Ersten Physikalischen Institut, Professor Pohl, considered himself to posses the priority. Next day

Pohl also came to Born for support, telling how badly he needed this new room, a matter of course which had now been questioned by their dear colleague Professor Franck. Then it was Mrs. Born who hit upon the brilliant idea that Professor Born might also have need for it, and with this solution the two colleagues found themselves rather satisfied, seeing that at least their first competitor did not win the prize.

In this room was installed a 10×10 automatic electric desk computer, an excellent Mercedes Euclid but strong and big as a modern electronic computer and hence with the faculty of giving out not only veritable acoustic waves but even respectable shock waves. Now we all of us know that it does not work very well just to do one's job. In order to gain fame it may be as important to make some noise about what you are doing, and in this respect the Mercedes Euclid helped me quite excellently. Even Herr Wachtmeister honoured me with respect and left me alone in the late afternoons.

The end result of my calculations was a ground state energy of the helium atom corresponding to an ionization energy of 24.35 eV which was greatly admired and thought of as almost a proof of the validity of wave mechanics also in the strict numerical sense. The truth about it, however, was in fact that its deviation from the experimental value by an amount of one tenth of an electron volt was on the spectroscopic scale quite a substantial quantity and might as well have been taken to be a disproof.

The discrepancy continued to bother me for a long time but it was not until a year or so later after my return to Oslo that something began to clear up in my mind; and I think it was the word «completeness» which was constantly ringing in my ears. The interelectronic term in the Schrödinger equation for two-electron atoms reads, if expanded in terms of Laplace angular functions

$$\frac{1}{r_{12}} = \sum_n \frac{r_1{}^n}{r_2{}^{n+1}} \mathrm{P}_n(\cos\vartheta)\,, \qquad r_2 > r_1\,.$$

This expression has no close similarity to the fairly civilized terms which so far had been used in the trial wave function, say powers of

$$2r\,,\, r_2 \cos\vartheta = r_1{}^2 + r_2{}^2 - r_{12}{}^2\,,$$

that is even powers of any of the three metric elements in the eletronic configuration. Why now, I wondered, shall we have to supplement these expressions with odd powers of r_1 and r_2 and not with odd powers of r_{12} which, moreover, possess expansions similar to the above of $1/r_{12}$ and different in the two half-spaces $r_2 \gtrless r_1$. Obviously, therefore, using both odd and even powers of the quantity

$$u = r_{12}$$

the situation would change fundamentally, so why not try. I could not guess at that time that this should be called an invention and thirty years later still should be termed the Hylleraas method. What I really invented I felt was rather the left hand side of the equation, the u, together with the $s = r_1 + r_2$ and $t = -r_1 + r_2$, forming the triple s, t, u, of which I am really proud. No hint of a loan from the velocity triple u, v, w of hydrodynamics or from other sources can be traced. The triple is forever reserved for for atomic research.

To be just the r_{12} had already been used in expressions for the wave function a year before (1928) in two articles in Physical Review by J. C. Slater. Of these articles I may not have been aware, since only his first one from 1927 is cited in my papers.

This change of coordinates had, to my astonishment and to my great satisfaction as well, almost the effect of a miracle. Already in the third approximation, using only the additional terms u and t^2, the troublesome discrepancy already told of, disappeared entirely on the electron volt scale, although still considerable in spectroscopic units. But the tie was loosened and addition of a few more terms made the discrepancy disappear also on the spectroscopic scale within the limits of accuracy of measurements at that time. The rest became a matter of tedious and accurate calculations as improved from time to time by many authors, also myself, and in particular Chandrasekhar and Herzberg and their various co-workers. In the most recent time we have to point to the indeed wonderful calculations as performed by Kinoshita and Pekeris.

The result of the new method was published in the first half of 1929, but I was unaware of its recognition until I had presented it myself before Det Skandinaviske Naturforskermøte in Copenhagen in September the same year. These meetings, at intervals of six years, in 1917 in Oslo, in 23 in Göteborg and the last one in Helsingfors in 35, were of a most venerable kind as instituted long time ago by the Danish physicist Hans Christian Oersted. They were always solemnly opened by some member of the royal family and followed by exquisite celebrations in food and not less speeches. Indeed, they were real events, and in particular for me who happened to participate only in this Naturforskermøte in Copenhagen. There for the first time I obtained the closest contact with most of the physicists and even other scientists from the Nordic coutries. Unfortunately, the second World War caused a stand-still of the meetings, which have never since been renewed. It may be feared that the enormously growing number of scientists in all branches of natural sciences would have them look like a meeting of the American Physical Society, as I remember it from 1947 in Chicago. When going to a lecture I had to stand outside the door and did not hear and much less see the speaker. But this may have been an exception, since the lecturer was Enrico Fermi. At all events even in 1929 since the meeting was held jointly with the Association of Scandinavian Engineers, the number of guests at the celebration dinner, as held in the biggest exhibition hall in Copenhagen, was not less than three thousand.

The number of active physicists, however, was still small enough as to convene in the modest auditory at Bohr's Institute for Theoretical Physics, so familiar to all physicists. I was somewhat struck by the spontaneous acclamation that followed my report, and in a happy mode I returned to my seat at a peaceful place far back. What the next speaker might be going to tell did not take my interest so my thoughts were going wrong and were entirely absent. Then Niels Bohr himself thanking the speaker for his nice performance turned to me and asked for my opinion and wondered whether my method might possibly be applied also to the present case. I felt seriously that my lately so nice position now was at stake and by keeping a bit silent and looking as wise as I possibly could I tried to master the situation. And now I realized the speaker, a very young bright-haired Swede, and gradually also that he had been telling how to strip off electrons from the lithium atom, having thereby obtained the ionization potential of the positive lithium ion. This of course you easily guess was the subsequently so famous spectroscopist Bengt Edlén, whom we are sorry not to see here to-day.

A most exciting cooperation now started between us. No sooner had I found the energy of the lithium ion lying in between his limits of errors than the doubly ionized beryllium ion was on the way, and so it continued with boron and carbon 3 and 4 plus with me always behind. As I suspected there were no limits to Edlén's power of producing ions I decided to overrun him with a good safety margin by introducing even an infinite nuclear charge. This was the origin of the energy formula

$$E = -2Z^2 + \tfrac{5}{4}Z - \varepsilon_2 + \varepsilon_3/Z - \varepsilon_4/Z^2 + \ldots.$$

as counted in Rydberg units.

Now as things appeared well settled for any high nuclear charge, there came a cory from the lower end. Some time before a discussion had arisen between the German physicists Joos and Hüttig on the one side and the Russians Kasarnowsky and Proskurnin on the other with respect to the true value of the lattice energy of lithium hydrid crystals. In these considerations a so-called Born circle process relating to the electron affinity of the hydrogen atom interfered. If that was positive, and hence the negative hydrogen ion was a stable configuration, Joos and Hüttig would be right and Kasarnowsky and Proskurnin would be wrong.

Some short calculation readily proved that such was the case. Putting the results into Zeitschrift für Physik I was shortly afterwards seriously embarrassed by learning that Hans Bethe, the latest wonderchild from Sommerfeld's factory in Munich had several months earlier performed fairly much the same calculations and published them in the very same Zeitschrift für Physik. This I think examplifies neatly the danger of laziness in reading. Of course, I falt ashamed and immediately wrote an article for explanation and excuse together with some more accurate results. But that did not help, so if you read Bethe's article on two-electron atoms in Handbuch der Physik you should not trust him. He — not I — is the father of that curious little child, the strange particle H^-, which for a while appeared to be recognized nowhere, neither in heaven nor on earth.

To be very accurate, it was Linus Pauling who already two years earlier tried to determine the electron affinity of hydrogen atoms by means of some extrapolation formula. His method would, however, correspond to a first order perturbation with scale parameter which is known to be insufficient for producing a binding. A year or two later his method would have been successful.

In spite of thus not having the highest responsibility for this new particle, I did my best for it. I put it into the LiH-lattice together with positive Li ions, both constituents then being of the same two-electron type. Applying the Pauli principle to all electrons, i.e., by antisymmetrization of the wave function of the whole crystal I succeeded in stabilizing the lattice against inner collapse and in this way manufactured a crystal, the LiH, on the pure basis of the Schrödinger wave equation. This is the first crystal produced in that way, and to my knowledge it is so far also the last one. It may be mentioned that the lattice energy came out surprizingly well, and even the lattice constant was not bad, considering that any adjustable parameter was absent. Long time afterwards I planned an attack on the diamond lattice assisted by a clever pupil, but this ended up in difficulties with the atomic wave function of the six-electron carbon atom. Maybe that metallic lithium with three-electron lithium atoms should rather be the next.

The rest of the story of the negative hydrogen atom is brief. In 1938 the astrophysicist Wildt produced a nicer place for this particle in heavenly bodies like the sun, suspecting it to be responsible for the opacity or greyness of the sun's atmosphere in the red and infrared region. From that time our parentless child was well taken care of by Chandrasekhar and other astrophysicists, and so the story had a very happy end.

Now after having tied up your time for quite a while I have scarcely penetrated even the surface of the problem complex I was maybe supposed to explain to you. So I propose that for the time left we proceed on the same path of yearn-spinning. After all the commonwealth of friendship between scientists may sometimes be as important as the scientific problems themselves.

Therefore, and since Göttingen was really an important centre of research a few years of early quantum mechanics, allow me for a short while to return to the idyllic life of this city with its venerable university institutions and traditions. Much may have changed also there in the course of some thirty years, but not at all in a striking way as I have seen with my own eyes, so visitors might well recognize things I am telling. At all events the university has not, like for instance Munich, developed into the size of an American university.

I shall never forget the charming view of the green slopes of the lower Harz as my wife and I with open eyes first approached the city of Göttingen. This was our first journey abroad and a most remarkable journey we thought it. We first found an excellent modern railway station, very much envied by our American friends. It was even whispered that supposed war indemnities went the wrong way to luxury railroad stations in Germany leaving poor legal creditors with their old and ugly ones.

Next to the station at the small river Leine we found a curious little town with a few narrow streets, apparently devoted to the memory of Carl Friedrich Gauss with its Weender- and Prinzenstrasse forming the real and imaginary axis of the Gaussian plane and an idyllic wall around providing for the unit circle. A Town Hall or Rathaus appeared not to have changed in the course of some hundred years, and in the Ratskeller, of course, students might be sitting drinking beer out of huge glass boots, yes boots. In front of the Rathaus the market place with a fine fountain, the Gäuselieselbrunnen, with the charming little Lise feeding the goose and back or to the side old Marien-, Nicolai- and Jacobi- churches counting the time every hour.

Up along the imaginary axis, on the gentle slopes of the Hainberg there was the «Millionärenviertel» with pretty modern houses where the highly distinguished and well-paid professors and Geheimrats of the former Kaiserly German society were supposed to have their homes. In a castle-like building in Merckelstrasse, for instance, James Franck could be found, as easily remembered from an event in a dark evening when the students marched in a torch-light procession, to his house, singing cheerful songs to celebrate Franck's reward by the Nobel prize in physics. In those days the Nobel prizes went to Germany as they are to-day going to America, or rather, half of the prizes in physics and chemistry went there, the other half to England and France.

Around the city at smaller and greater distances there were places of interest and beauty which could be reached by walking on your feet. In extreme cases you might use the railway. Of course, the «Kleinbahns» to the «Dörfer» were already out of date, but on the real railways there was an excellent choice between «Holz oder Poltz», the first, the second, the third and the fourth class, of which the last one for obvious reasons was very much preferred. In the city itself quite frequently some vehicle might be seen drawn by a pair of milk cows at a speed of approximately one mile per hour. Autocars are quite out of my memory, but they may have been appearing at that time at the railway station. Our best friends Dr. and Mrs. Hogness from Berkeley and now in Chicago had two small boys. And whereas the parents neither could nor wanted to care too much about nominative and accusative in the German language, young Johnny, the eldest one, spending much time in the kindergarten, spoke an excellent German. He was lazy too, and walking with his father he might frequently propose: «Vater ich bin so müde, wir nehmen lieber ein Taxi».

To restore the dignity of the family Hogness I should like to add that, on occasion of the Davisson-Germer discovery of electron diffraction in crystals, Dr. Hogness was the first I ever heard to form the historical sentence: «Das Elektron is eine Welle».

If the word students may be used for those staying for a while, there were quite a number of American students and visitors there. From France I remember Brillouin coming repeatedly giving his lectures in German, and from over here I particularly remember outstanding persons like Irving Langmuir and K. T. Compton. The latter, in congratulating James Franck for his Nobel prize, nicely added that he would not have felt more satisfied «if it had been one of our own». The other Compton, the A. H. or the *hv*-Compton as Onsager would call him, I met much later, in London 1934.

Our Chairman to-day also was a Göttingen man, as may be inferred from the early establishment of the famous Franck-Condon principle. Closing my eyes I believe to see Edvard Condon speaking at some colloquium, but that is all, so he may have left too early for me.

Even Enrico Fermi is said to have studied in Göttingen, however, not with full satisfaction. He may have been too early out, or he had problems of his own to be solved somewhere else.

Among the short-visitors I remember a Hungarian nobleman, in wisdom comparable to Wigner, and a very nice-looking man. It was a real pride to see him walking on the Weenderstrasse in an elegant summer suit, so I guess this must have been in the summer time in 1928 or in 1931 on my shorter visit there. In 1936 I met him in Paris where we were to give some lectures at the Institut Henri Poincaré, and again he impressed me enormously by his ability in French. With an accurate manuscript supervised by Professor Bauer and backed by my dear French-Scandinavian friend Dr. Rosenblum for whispering purposes, I put my tongue in the correct position and received at the end a heartily acclamation from our always gallant French friends. The other lecturer of course did not need a manuscript at all and still gave his talks with an eloquence which our friends here Mr. and Mrs. Pulman might nearly have denied him. So you may well guess that was John v. Neumann.

Another quite young man of a slender shape and with a goodlooking face was frequently seen walking on the wall and sometimes pulling up his pipe. I might sometimes shake hands with him but, I am sorry, did never come in closer contact. That was because of my great respect for those belonging to the brain trust, as he obviously was, as inferred from his frequent private conferences with Born. Twenty years later I had for a shorter time the privilege of hospitality at his institute in Princeton. But now he had heavy burdens on his shoulders, absorbed in problems outside of my sphere and even well guided, so you may guess that this was Robert Oppenheimer. Moreover at that time I was looking more to the side of Wigner and Wheler at the Princeton University. It was however of great interest to meet at the institute mathematicians like v. Neuman, Weyl, Siegel and Oswald Veblen as well as famous visitors like Niels and Harald Bohr and Dirac. Even Albert Einstein might sometimes be seen walking over the green fields between the Institute and his home and always in a nonconventional suit much more in harmony with his prophetic appearance. He also gave a lecture putting, as it appeared to me, a new asterisk to some of his for me still incomprehensible tensors. Very modestly he declared himself unable to draw the conclusions except by extensive calculations, it was merely a suggestion he said. Once I met him more head on an shaking hands with him I had a most friendly, however absent look, and I have never guessed what it meant, whether he thought of me as a distinct person or rather only as one of the citizens of the world. And this was my last sight of Albert Einstein.

Back to Göttingen I see an energetic young man with black hairs and mighty black eyebrows, also a Hungarian, so I need not hesitate to tell that this was Edward Teller, a pupil of Heitler for his doctor thesis on the hydrogen molecular ion. At that time I was considered to know a little even of the hydrogen molecule and for that reason was taken along for joint discussions with both of them. This I tell in order that you may think I have made a little contribution even to the hydrogen bomb.

Finally — and now I mean finally — there was a man from Russia a stout man with black moustaches and black hairs — apart from Edlén it appears that black hairs are a good thing for atomic scientists. Even Weisskopf you know is brilliantly black-haired. I looked up to this distinguished Russian scientist, taking him to be at the age of Professor Born, a recognized colleague from Russia on leave of absence. It was not until two years ago on his visit to Oslo and having much the same appearence as in the Göttingen days that he revealed to me being a year younger than myself.

For quite a period of the thirties we had a nice correspondence and I was frequently pleased by learning of progress in his work although a little worried for his modest supply of paper. He is however now, and maybe since long ago, on the highest rank as an Academy Professor of Moscow with his home and main duties in Leningrad. On presenting me a nice book he told me that he would be happy to have the privilege of calling me «his old friend», and to this I consented on the condition that this be a reciprocal sort of privilege.

This was the second father of the famous Hartree-Fock method, whose range is far beyond the two-electron problem. The former one, Douglas R. Hartree, I shall never forget, being of the kindest persons I ever met, and whose premature death I sincerely regretted. The words he used of his own father, the other Harthree whose name appears in some joint publications, that he was the most wonderful artist in numerical calculations he ever knew, may well be turned toward himself.

It is a sad thing to observe friends and colleagues and pioners of the Atomic Age passing away. Among the nearly half a hundred persons I have touched upon in this review half of them are no more alive and quite a number of them did not reach the normal length of a life. The latest, fairly normal cases, I know of are those of Niels Bohr and Charles Darwin. In this sense my review, although unintentionally, may still be called a little piece of history.

Above all we have to remember the giants of early atomic research in our century like Planck, Einstein, Rutherford, Bohr and Sommerfeld, to mention only a few and to these I should like to add as one of the most venerable representatives still alive from that time Max Born of Göttingen now after twenty years of exile, living peacefully in the nearby Bad Pyrmont. His eightieth birthday was lately celebrated at the Physics Institute in Göttingen, on which occasion I have had letters from him. Although his health is maybe not of the strongest kind his mind is unusually active, and in his memory as I personally have learnt from him he keeps an amount of valuable reminiscences, particularly from early days in Berlin together with Albert Einstein.

You will forgive me that, if my lecture has turned too much towards early days in Göttingen, it has been rather much to the honour of my dear friend and first teacher in theoretical atomic physics, Max Born.

Forandring av strålefølsomheten av enzymer og bakterier ved hjelp av H_2S og NO Del 2

Tor Brustad

Går vi nå tilbake til vår tidligere omtale av H_2S, så fant vi at det var liten eller ingen effekt av H_2S på enzymet bestrålt i tørr form. Med andre ord: H_2S har ikke nevneverdig innflytelse på den direkte effekt. Ved bestråling ved værelsestemperatur av den vanndige løsning gjennomboblet med ren H_2S gass, fant vi jo en meget markert reduksjon av strålefølsomheten med en faktor på ca. 1/130. (Fig. 4). Det ser derfor ut til at vi kan trekke den viktige konklusjon at H_2S i tilstrekkelig konsentrasjon, tilveiebringer meget nær en komplett beskyttelse mot de indirekte inaktiveringsmekanismer.

Vi står altså her foran et strålebeskyttende kjemikalium hvis effektivitet langt overstiger det som er vist for noe annet strålebeskyttende stoff, og som i sin virkning på enzymer hovedsakelig influerer på de indirekte inaktiveringsmekanismer. Aksepterer vi nå det syn at den indirekte effekt skyldes radikaler eller toksiske produkter dannet i vannkappen omkring target, som ved diffusjon kan nå inn til target, reagere med dette og forårsake den observerte effekt, er det mulig ved hjelp av en analyse som først ble beskrevet av Zirkle og Tobias og senere av Hutchinson, å beregne den gjennomsnittelige diffusjonslengde av disse radikaler. De fant ut at den forøkede strålefølsomhet på grunn av indirekte effekter, som vi tidligere har betegnet med V', kunne uttrykkes slik:

$$V' = 4\pi\, Y\, q\, (\rho r^2 + r\rho^2) \qquad (4)$$

ρ er den gjennomsnittlige diffusjonslengde av radikalene, Y er antall radikaler eller toksiske produkter dannet per primær ionization, q er sannsynligheten for reaksjon mellom target og radikalet når dette har nådd frem til target. q kan i de fleste tilfelle, i følge Hutchinson, settes lik 1. r er den gjennomsnittlige radius av target og er for lysozym, som allerede nevnt, lik ca. 20 Å. Y er blitt bestemt for rent vann ved kjemiske midler, samt ved hjelp av elektron-spinn-resonans studier av is, selv om en ikke sikkert vet om alle typer av toksiske produkter inngår i disse målinger. Likningen kan da løses med henblikk på ρ som i det foreliggende tilfellet viser seg å bli ca. 50 Å. Vi får derfor følgende tilnærmede bilde, nemlig at vi kan betrakte target som en kule med radius ca. 20 Å, omgitt av en vannkappe av tykkelse bestemt ved at radikalenes gjennomsnittelige diffusjonslengde er ca. 50 Å. Strålingsinduserte radikaler og toksiske produkter dannet innenfor dette skall kan diffundere inn til target og påvirke dette.

Vi har nå sett litt på hvordan H_2S influerer på strålefølsomheten av lysozym. Ganske analoge resultater får en også for andre enzymer, så som trypsin. La oss derfor nå gå over til en langt mer komplisert prosess, nemlig inaktivering av bakterier. På grunnlag av det som er utledet vedrørende effektiviteten av H_2S til å beskytte enzymer, la oss først forsøke å spå hvor stor strålebeskyttelse vi kan vente å finne av H_2S på bakterier i suspensjon. Etterpå vil vi så sammenholde dette med hva mine eksperimentelle undersøkelser har vist.

Det er i cellulær radiobiologi et generelt fenomen at cellekjernen er langt mer strålefølsom enn det omkringliggende cytoplasma,

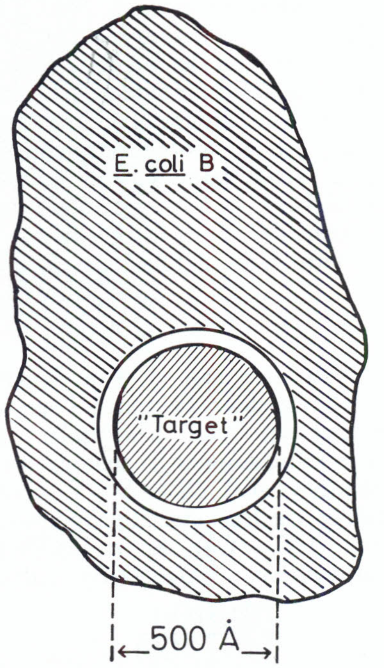


Fig. 8. Skjematisk fremstilling av en bakterie hvor «inaktiverings target» er avmerket, omgitt av et «skall» hvor strålingsinduserte radikaler ved diffusjon kan nå inn til target.

når det gjelder inaktivering. Uten å komme inn på hva som egentlig er target når det gjelder inaktivering av mikroorganismer, ganske enkelt fordi dette er ukjent, skal vi derfor nøye oss med å slå fast at det på en eller annen måte er knyttet til cellekjernen. Således har en indisk forsker fra bestrålings-studier av tørkede bakterier av *E. coli* funnet at target for inaktivering kan antas innesluttet i en kule av radius ca. 250 Å. Med andre ord: Det er meget stort sammenliknet med hva vi tidligere fant for enzymer. Vi har derfor følgende bilde (fig. 8): Target har en gjennomsnittlig radius på ca. 250 Å. Det medium som omgir target vil i kontrast til ved enzymundersøkelsene, tildels være cytoplasma, som inneholder en rekke organiske forbindelser, ja til og med svovelholdige forbindelser som godt kan virke som radikalscavangers. Vi vil derfor vente at diffusjonslengden av de strålingsinduserte radikalene under disse forhold ikke vil bli lengre enn hva vi tidligere fant for enzymer i trippel-destillert vann.

Ved bestråling av bakterier i suspensjon vil vi vente en indirekte effekt fra radikaler og toksiske produkter dannet innenfor et «skall» omkring target med en tykkelse som er bestemt ved at disse reaktive enheter har en gjennomsnittlig diffusjonslengde sannsynligvis litt i underkant av 50 Å. Ved å benytte formel (4) og her sette inn $r = 250$ Å og $\rho = 50$ Å, kan vi beregne det ventede (maksimale) bidrag til strålefølsomheten fra indirekte effekter. Vi kan videre beregne forholdet mellom den totale, ventede strålefølsomhet i suspensjon, altså summen av direkte og indirekte effekt $(V + V')$, og strålefølsomheten i tørr tilstand (V), som altså skyldes direkte effekter. Dette forhold blir i det foreliggende tilfelle:

$$(V + V')/V = 4{,}1$$

På den annen side kan vi si at dersom H_2S også når det gjelder bakterier i suspensjon, tilveiebringer en total beskyttelse mot de indirekte strålingsskader i analogi med våre enzymestudier, skal $(V + V')/V$ også være lik forholdet mellom strålefølsomheten i O_2-atmosfære, $1/D_{37}(O_2)$ og strålefølsomheten i H_2S-atmosfære, $1/D_{37}(H_2S)$, altså som

$$\frac{1/D_{37}(O_2)}{1/D_{37}(H_2S)}$$

Hvis denne antagelse er riktig, skulle vi altså vente en maksimal strålebeskyttelse av H_2S svarende til en faktor på 4,1, et forhold som altså er langt mindre enn tilfellet var for enzymer, ganske enkelt fordi target for inaktivering av bakterier er så mye større enn for inaktivering av lysozym.

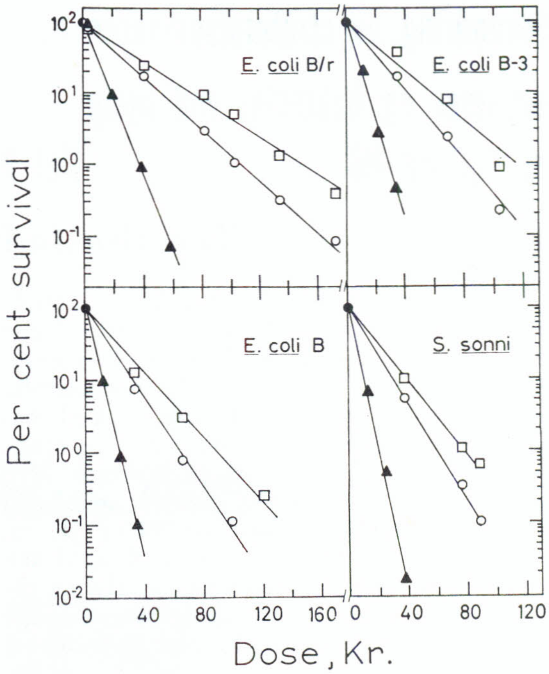


Fig. 9. Dose-effekt kurver av fire forskjellige typer av bakterier, bestrålt i atmosfærer av: ▲ O_2, ○ N_2, □ H_2S.

La oss nå gå over til de eksperimentelle data.

Figur 9 viser endel dose-effekt kurver for inaktivering av fire forskjellige typer av bakterier som alle er behandlet på nøyaktig samme måte. Kriteriet på stråleskade er inaktivering av bakterienes evne til å danne synlige kolonier når de såes ut på «nærings» agar. Bakteriesuspensjonen er under bestrålingen gjennomboblet med henholdsvis O_2, N_2 og H_2S. Som en ser er strålefølsomheten i alle tilfelle størst i O_2-atmosfære, betraktelig mindre i N_2-atmosfære og ytterligere mindre i H_2S.

Tabell I.

	Strålesensitivitet, $1/D_{37}\ 10^{-5}\ rad^{-1}$			$\frac{1/D_{37}(O_2)}{1/D_{37}(H_2S)}$
	O_2 atmosfære	N_2 atmosfære	H_2S atmosfære	
E. coli B/r	12.3	4.3	3.1	4.0
E. coli B–3	15.7	5.6	4.0	3.9
E. coli B	19.3	7.2	5.2	3.8
S. sonni	22.0	7.7	5.8	3.8

Tabell I viser strålefølsomhetene som er avledet av de foregående dose-effekt kurver. Angitt er også de observerte strålefølsomheter i O_2-atmosfære relativt til det man finner i H_2S-atmosfære, et forhold vi er interessert i å se om tilsvarer forholdet mellom beregnet total effekt og direkte effekt, som omhandlet ovenfor. Vi ser at det er ganske likt for alle de fire organismene, beliggende i området fra 3,8–4,0. Dette må i sannhet sies å være svært nær hva vi tidligere resonnerte oss fram til, basert på enzymstudiene. Antar en at gjennomsnittelig diffusjonslengde i bakterieeksperimentene er litt i underkant av 50 Å, kan en faktisk få en nesten perfekt overensstemmelse.

Det ser derfor ut til at en har meget god grunn til å trekke den samme konklusjon som vi tidligere kom fram til for enzymer: H_2S er en meget effektiv strålebeskytter. I tilstrekkelig konsentrasjon tilveiebringer denne forbindelse en komplett beskyttelse mot indirekte stråleskade. Strålingsinduserte radikaler og toksiske produkter ser ut til også i dette system å ha meget kort gjennomsnittlig diffusjonslengde, av størrelse litt i underkant av 50 Å.

Et annet resultat er det også verdt å bemerke. Det er ingen tvil om at strålefølsomheten i H_2S-atmosfære er betraktelig mindre enn i nitrogenatmosfære. En kan derfor utelukke den forklaring som er fremsatt for så mange strålebeskyttende svovelforbindelser, nemlig at de utelukkende tilveiebringer en beskyttelse mot den forøkede strålefølsomhet som tilstedeværelse av surstoff forårsaker.

Vi har i denne framstillingen lagt hovedvekten på å vise at H_2S er en meget effektiv strålebeskytter. Spede forsøk er også gjort når det gjelder å påvise hvilke hovedmekanismer H_2S influerer på. Går man imidlertid ett skritt videre og spør hvilke spesielle kjemiske reaksjoner H_2S deltar i og som resulterer i denne utpregede strålebeskyttelse, er man henvist til mer eller mindre fri gjetning og spekulasjon, som en ikke ønsker å komme inn på her, da moderne radiobiologi allerede er rik nok på dette, noe som kan være bra, men ikke alltid leder våre tanker i de riktige tankebaner. La meg bare illustrere dette ved hjelp av noen eksperimenter utført med NO.

Det vakte atskillig interesse da Howard-Flanders for noen få år siden i eksperimenter på bakterier påviste at ved å gjennomboble en bakteriesuspensjon med NO under bestrålingen økte strålefølsomheten på samme måte som om den ble gjennomboblet med oxygen. Howard-Flanders konkluderte derfor med at NO strålebiologisk sett var ekvivalent med oxygen, med andre ord at molekyl for molekyl var de to gasser like effektive til å forøke strålefølsomheten. Senere ble det vist av Powers at strålefølsomheten av bakteriesporer bestrålt i *tørr* form i NO atmosfære bare var omtrent halvparten så stor som ved bestråling i O_2 atmosfære. Dette førte til at det ble vanlig i radiobiologiske arbeider å anta at ekvivalensen mellom NO og O_2 som strålesensitiviserende forbindelser måtte innskrenke seg til å gjelde bare for de indirekte inaktiveringsmekanismene. At generaliseringer kan være risikable og at radiobiologi fremdeles er en ung vitenskap hvor man stadig kan vente overraskelser, vil jeg illustrere med et annet eksperiment jeg har gjennomført på en vanndig løsning av lysozym:

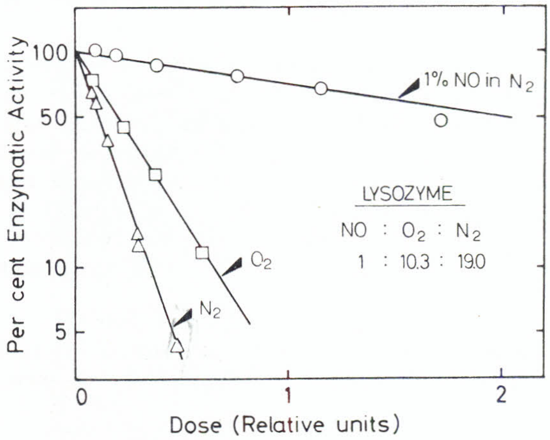


Fig. 10. Dose-effekt kurver av lysozym i vanndig løsning (1 mg/ml), gjennomboblet under bestrålingen med 100 % N_2, 100 % O_2 og 1 % NO i N_2.

Figur 10 viser tre dose-effekt kurver. I det ene tilfellet er løsningen gjennomboblet med N_2 under bestrålingen, i det andre tilfellet med O_2 og i det tredje tilfellet med en 1 % blanding av NO i N_2. Jeg tror en trygt kan si at resultatet av dette eksperiment er høyst uventet. Som vi ser, har vi her nemlig et eksempel på en, la oss kalle det «invertert oxygeneffekt». Med andre ord: Sammenholdt med strålefølsomheten observert under anoxia virker både oxygen og nitrogenoksyd som strålebeskyttere. Enn videre, da konsentrasjonen av løst NO i enzymløsningen på molar basis er mindre enn 1/60 av oxygenkonsentrasjonen, kan en også konkludere med at i det foreliggende tilfelle er O_2 og NO heller ikke likeverdige overfor indirekte mekanismer. Preliminære undersøkelser har vist at graden av invertert O_2 og NO effekt er sterkt avhengig av suspensjonens surhetsgrad, et forhold som må klargjøres i fremtidige eksperimenter.

Jeg har i denne framstillingen lagt hovedvekten på å vise at både H_2S og NO har utpregede egenskaper til å modifisere strålesensitiviteten av biologisk materiale. Det er klart at ved utarbeidelse av framtidige teorier for virkningen av ioniserende stråling på biologisk materiale, må det også gjøres rom for slike uortodokse effekter som invertert oxygeneffekt og de meget høye strålebeskyttelsesfaktorer som her er omhandlet. Det ser for meg ut til at vi som arbeider innen feltet radiobiologi og strålingsbiofysikk ikke behøver nære nevneverdig engstelse for å bli arbeidsledige i den nærmeste tid ..... årsaken kan i så fall neppe begrunnes med mangel på uløste, fundamentale problemer.

Gnistkammeret og dets anvendelse

Del 2

Trygve Holtebekk

Å gi en fullstendig oversikt over gnistkammerets mange anvendelsesmuligheter lar seg ikke gjøre her. Kammeret har enda bare vært i drift i kort tid og mulighetene er langt fra uttømt, men selv om en skulle holde seg til det som er gjort, ville en slik oversikt bli for lang. Jeg vil isteden beskrive kort to eksperimenter som illustrerer anvendbarheten meget bra.

Det første, foreslått av professor H. L. Anderson ved University of Chicago [6] skal nyttes for å studere desintegrasjonsprosessen:

$$\mu^+ \rightarrow e^+ + \gamma$$

en prosess som kan være mulig, men iallfall forekommer sjeldent sammenliknet med den vanlige:

$$\mu^+ \rightarrow e^+ + \nu + \bar{\nu}\,.$$

Det nyttes her to gnistkamre, (fig. 9), det ene for å registrere elektroner fra desintegrasjonsprosessen, det andre for elektronpar produsert av γ-kvantet ved pardannelse. I elektronkammeret sitter først noen gap skilt med tynne aluminiumplater, så kommer en scintillasjonsteller innebygd i en av platene, deretter grafittplater belagt med sølv eller aluminium. Disse siste bremser ned elektronet og nyttes til å bestemme energien ut fra rekkevidden. γ-detektoren består av en scintillasjonsteller som er koplet i antikoinsidens og garanterer at ingen ladde partikler som kommer inn i dette kammeret, blir registrert.

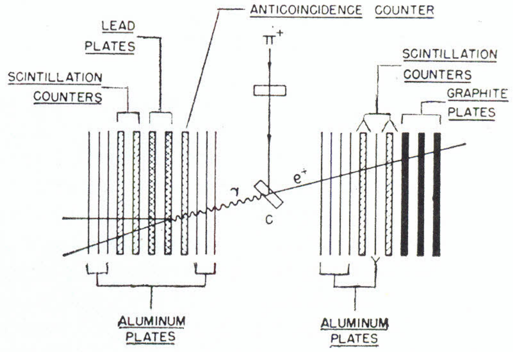


Fig. 9. Oppstilling for å studere prosessen $\mu^+ \rightarrow e^+ + \gamma$. (Fra ref. 6).

Deretter følger to blyplater for konvertering av γ-kvantene til elektronpar. Så kommer to koinsidenskoplete tellere og noen tynne aluminiumplater.

Eksemplet skulle vise noen av de rike variasjonsmuligheter en har både når det gjelder bruken av koinsidens og antikoinsidenstellere, og når det gjelder å velge plater av egnet materiale inne i kammeret.

Det andre eksemplet som skal omtales, er nøytrinoeksperimentet som ble utført ved ved 30 GeV maskinen i Brookhaven sommeren 1962, der det ble vist at det finnes to slags nøytrinoer. Dette eksperimentet, regnet for meget betydningsfullt når det gjelder å forstå elementærpartiklenes natur, hadde neppe vært mulig uten en hurtigarbeidende spordetektor,

Nøytrinoet har i en årrekke spilt omtrent samme rolle for elementær-partikkel-fysikken som eteren i sin tid gjorde for elektronmagnetismen, som noe som må være der for å skape orden i systemet, nærmere bestemt, som det som blir til overs når et nøytron desintegrerer i et proton og et elektron. For ca. 10 år siden klarte en imidlertid forholdsvis direkte å påvise nøytrinoets eksistens, idet en registrerte nøytrinoinduserte kjernereaksjoner. I mellomtiden hadde en måttet

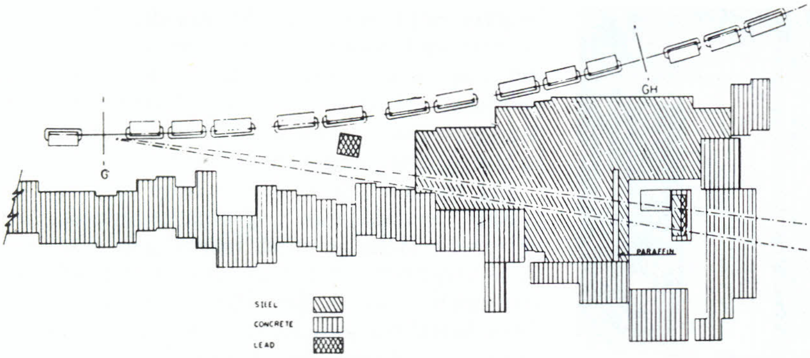


Fig. 10.
Skisse av nøytrino-eksperimentet i Brookhaven. (Fra ref. 10).

nytte nøytrino-hypotesen også andre steder, blant annet for å forklare π-mesonets overgang til et myon. Spørsmålet meldte seg da naturlig nok om disse nøytrinoer alle var like, eller de hadde forskjellige egenskaper. Enkelte karakteristiske størrelser er ens. F. eks. har de ingen hvilemasse, de har spinn 1/2, men muligheten av at partiklene likevel var forskjellige på en eller annen måte, kunne ikke utelukkes. En følge av måten de er dannet på er at et nøytrino dannet ved β-emisjon — et elektron assosiert nøytrino — kan reagere med et proton og gi et nøytron og et positron, mens et som er dannet ved desintegrasjon av et π-meson — et myon assosiert nøytrino — må kunne gi et myon når det reagerer med et proton. Hvis nå de to nøytrinoene er ens, skal de når de reagerer med protoner, produsere omtrent like mengder myoner og elektroner uavhengig av hvordan de selv er dannet, bare energien er tilstrekkelig. Dette gir en problemstilling som prinsipielt sett er enkel å besvare. Vanskeligheten er at reaksjonstverrsnittet for nøytronoet er så lite at prosessen lett blir skjult av andre uønskede prosesser.

Den måten en gruppe ved Columbia University og Brookhaven National Laboratory [10] etter forslag av professorene Schwartz og Lederman valgte å utføre eksperimentet på, er følgende: En stråle av hurtige π-mesoner desintegrerer under flukten. De frigjorte nøytrinoene passerer et gnistkammer som registrerer hurtige ioniserende partikler oppstått inne i kammeret. Disse partiklene må enten være myoner eller elektroner. I kammeret kan energien bestemmes ved rekkeviddemålinger. Ved å studere partiklenes vekselvirkning med materien i kammeret kan det avgjøres om de produserte partiklene er myoner eller elektroner.

Vi skal se litt på selve eksperimentets utførelse. På fig. 10 ser vi en skisse av hele oppstillingen. Protonstrålen fra synkrotronen treffer et berylium-target, og vil etterpå inneholde en betraktelig mengde π-mesoner. En del av disse desintegrerer under en 21 m lang flukt i myoner og nøytrinoer. Alle de ladde partiklene og nøytrale partikler som har sterk vekselvirkning med materien, f. eks. gammastråling og nøytroner, stoppes effektivt i en 13,5 meter jernvegg. Nøytrinoene — både langsomme og hurtige — fortsetter inn i detektoren.

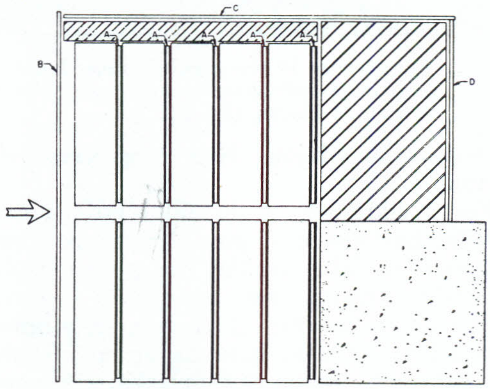


Fig. 11. Gnistkammer og teller arrangement. Koinsidens signal fra to av tellerne A utløser kammeret. B, C og D er antikoinsidenskoblede tellere. (Fra ref. 10).

Denne består (fig. 11) av et gnistkammer bygget i 10 seksjoner og skjermet på alle kanter med antikoinsidenstellere, slik at kamrene ikke kan utløses av ladde partikler som kommer inn utenfra, fra maskinen eller fra kosmisk stråling.

Selve kammeret må for å gi et rimelig antall nøytrino-reaksjoner, være svært stor. Det er (fig. 12) bygget i 10 seksjoner, hver med 8 gnistgap på 3 mm mellom aluminiumsplater 1 tomme tykke og 110 × 110 cm i flate. Mellom seksjonene sitter tellere, koinsidenskoplet slik at alle seksjonene i kammeret utløses hvis en ioniserende partikkel går

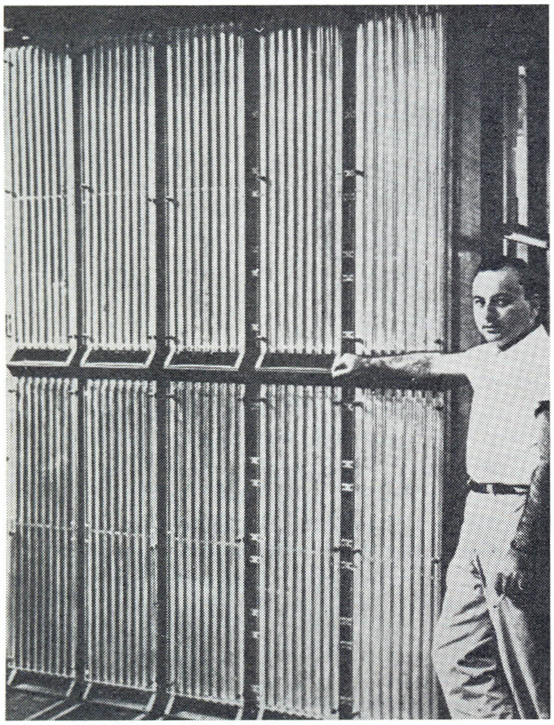


Fig. 12. Fotografi av 10 tonns gnistkammer brukt for nøytrinoeksperimentet med professor Schwartz stående ved siden. (Fra ref. 7).

gjennom én seksjon. Hele kammeret veier 10 tonn.

For å sikre seg at de observerte sporene virkelig har noe med partikler fra maskinen å gjøre og ikke skyldes nøytrinoer i den kosmiske stråling, har en ved koinsidens-koplinger sørget for at høyspenningspulsen som utløser gnisten bare dannes når strålen treffer target. Strålen ut fra maskinen kommer i pulser på 20–30 mikrosekunder med repitisjonsperiode på 1,2 sekunder. Hver av disse pulsene blir ved en deflektor kuttet opp og slipper inn på target i 20 nanosekunders pulser med en avstand på 200 nanosekunder. Dette betyr at strålen treffer target i ca. 2 mikrosekunder hvert sekund, bare i disse 2 mikrosekundene har en muligheter for å registrere noe i kammeret, og bakgrunnen er derfor neglisjerbar. I løpet av ca. 500 timers operasjonstid med maskinen, slapp maskinen fram protoner til target i ca. 5 sekunder i en total mengde av $8{,}5 \cdot 10^{17}$ protoner (20 millicoulomb). I løpet av disse 5 sekundene som ble igjen av 6 måneders målinger, observerte en 56 begivenheter hvor en mente å kunne påvise at det var dannet høyenergetiske myoner, mens en ikke kunne påvise noen begivenheter som sikkert skyldtes høyenergetiske elektroner. En konkluderte med at det var mye større sannsynlighet for å få produsert et myon enn et elektron med et myon-assosiert nøytrino, og at en derfor har to forskjellige slags nøytrinoer.

Jeg har forsøkt å trekke fram noen av de typiske egenskapene ved gnistkammeret. Det er ytterst sjeldent at et instrument viser seg så anvendbart etter bare et par års tid. Instrumentet skal heller ikke i sin nåværende form betraktes som ferdig. Det kan fremdeles være muligheter for forbedring både når det gjelder tids- og romoppløsning. Forskningen drives intenst på disse feltene. Hvorvidt den vil gi resultater vet vi selvsagt ikke. Sikkert er det at gnistkammeret alt i sin nåværende form har gjort det mulig å komme et skritt videre på vei i vår forståelse av noe fundamentalt i materien, elementærpartiklene, deres slektskap og vekselvirkning.

[1] *J. W. Keuffel:* Rev. Sci. Instr. *20,* 202 (1949).
[2] *F. Bella* and *C. Franzinetti,* Nuovo Cimento *10,* 1335 (1953).
[3] *P. G. Henning:* Upubl. avhandl. Hamburg (1955).
[4] *T. E. Cranshaw* and *J. F. de Beer:* Nuovo Cimento *5,* 1107 (1957).
[5] *S. Fukui* and *S. Miyamoto:* Nuovo Cimento *11,* 113 (1959).
[6] Spark Chamber Symposium, Rev. Sci. Instr. 32, 480 (1961).
[7] *J. W. Cronin:* I. R. E. Trans. *9,* 247 (1962).
[8] *J. G. Rutherglen:* Progr. Nucl. Phys. *9* (1962).
[9] *G. K. O'Neill:* Scientific American *207 no. 2,* 36 (1962).
[10] *Danby, Gaillard, Guolianos, Lederman, Mistry, Schwartz, Steinberger:* Phys. Rev. Letters *9,* 36 (1962).

Brev fra leserne

Denne spalte er åpen for korte innlegg fra leserne. Redaksjonen håper på denne måte å skape en nærmere kontakt mellom leserne og mellom leserne og redaksjonen.

Måleenheter i fysikkundervisningen.

Herr redaktør.

I fysikkundervisningen spiller målsystemene en ikke uvesentlig rolle. Dessverre har dette felt ikke vært preget av den enhetlig og klarhet som er ønskelig. Situasjonen er imidlertid etter hvert bedret slik at man i dag kan se bort fra andre systemer enn det nye internasjonale enhetssystem (SI) og det tekniske system. Et vesentlig fremskritt betyr det også at kraftenheten i det sistnevnte har fått et annet navn (kilopond) enn masseenheten i det førstnevnte.

I denne forbindelse kan det være av interesse å nevne de anbefalinger som er utgitt av International Organization for Standardization, ISO Recommendation R 31, Part I til IV. (Disse publikasjoner er til salgs gjennom Norges Standardiserings-Forbund). Part I foreligger forøvrig allerede i norsk utgave som del 1 av Norsk Standard NS 1020.

Arbeidet med opprydding i målsystemene bør fortsette, med sikte på å nå frem til det færrest mulige antall grunnenheter for hver størrelse. Det vil i praksis si at enheter som kalori, hestekraft, og mm kvikksølv konsekvent erstattes av joule, watt og millibar. Likedan erstattes atmosfære i betydningen 760 mm Hg av bar. At liter avskaffes som eksakt volumenhet, er en selvfølge.

Den praktiske gjennomføring må først skje i tabellverk og lærebøker. Er ikke tiden moden for denne reform nå?

L. P. Buseth.

Ad. Feilen i Isaachsens lærebok i fysikk, side 64.

Herr Redaktør:

Undertegnede har med stor interesse lest innleggene fra H. Sollie og E. Schreiner (F. F. V. nr. 2 og 4) angående «feilen» i formelen $K = \mu_0 HIl$ i oven nevnte lærebok. Jeg er enig med Red. i hans kommentarer, men tillater meg likevel å komme med noen bemerkninger.

I læreboken er det uttrykkelig sagt fra at $H = NI_{sp}/L$ er den magnetiske feltstyrke i det homogene feltet *inne i* spolene. Lærebokforfatteren tilføyer her med petit: «Definisjonen er ikke helt riktig i denne form, men er som regel tilstrekkelig nøyaktig når spolen er lang i forhold til diameteren.» Det skulle være unødvendig å si at denne forenkling er gjort ut fra pedagogiske hensyn, da våre gymnasiaster i flg. pensum kun skal kjenne formelen for feltstyrken i en lang solenoide.

La oss så ut fra dette gå formel (2) nærmere etter i sømmene. Hvis vi forutsetter at tverrsnittet i luftgapet er like stort som i jernkjernen, vil induksjonen i jernet Bj være lik induksjonen i luftgapet B_0. Kaller en den magnetiske feltstyrken i jernkjernen for H og i luftgapet for H_0, er $\mu_0 H_0 = \mu_j H$. Dette gir så kraften på strømførende leder $K = \mu_0 H_0 Il = \mu_j HIl$. Formel (2) i læreboken ville da bli tilnærmet riktig om permeabiliteten for vakuum μ_0 ble erstattet med jernets permeabilitet μ_j. Ut fra disse betraktninger synes det for meg å være her den vesentligste feilen i lærebokens formelverk ligger.

Bergen den 27/2–63.

Med hilsen

Øystein Falch.

Hr. redaktør.

I «Fra Fysikkens Verden» har det i de siste heftene vært flere interessante innlegg om «en grov feil» i fremstillingen i Isaachsens fysikk av kraften på en elektrisk strømleder i et magnetfelt og sammenhengen mellom B og H. Forklaringen på at feilen er kommet inn er at teksten opprinnelig var skrevet til et apparat som i fig. D10–1 med luftfylte spoler. Forsøket i fig. D10–3 som jeg selv har brukt til demonstrasjon i en årrekke, ble siden satt inn fordi det gir meget tydelig kvalitativ virkning; men det er selvsagt ikke brukbart til å finne sammenhengen mellom B og H. Dessverre ble dette ikke presisert i teksten, så man får inntrykk av at strømmen i magnetspolene, I_{sp}, betyr strømmen i viklingen i elektromagneten fig. D10–3. At en elev ikke på egen hånd skjønner den egentlige sammenheng er nokså rimelig, men for en som kjenner betydningen av ligningene

(1) $H = \dfrac{nI_{sp}}{L}$ og (2) $K = \mu_0 HIl$, skulle det

synes klart hva som er ment, særlig når man er oppmerksom på henvisningen til fig. D3–1 som viser feltet i en luftfylt spole, og fotnoten om at spolene må være lange i forhold til diameteren. Å definere H ved hjelp av en elektromagnet I_{sp} er ikke brukbart, man måtte jo da foregripe det som først senere kan forklares om jern i magnetfelt, og det blir allikevel ikke noen eksakt definisjon. Logisk best er vel definisjonen ved hjelp av den magnetomotoriske kraft i et lukket felt, som en torus. Danskene er her i en heldigere stilling, fordi de ikke helt underslår Maxwells ligninger, som vi er nødt til.

Jeg vil gjerne tilføye at jeg har skrevet nytt hele avsnittet D 10 til neste utgave.

J. Holtsmark.

Bøker

Ronald W. Clark: «Da bomben ble til» («Birth of the Bomb»), med et forord av Sir George Thomson, FRS. Oversatt og tilrettelagt av Ragnar Wold. Aschehoug, Oslo 1962. (223 sider, hf. kr. 25,—, ib. kr. 32,—).

Denne bok skal være «beretningen om hvordan Storbritannia bidrog til å skape det våpen som forandret hele vår verden». Vi har nu efterhånden fått en ganske rikholdig litteratur av dokumentarisk eller halv-dokumentarisk natur omfattende utviklingen av de kjernefysiske våben, og som de fleste bøker som omhandler dette emne, er også denne en blanding av rent referatstoff, sitater fra vitenskapelige og politiske kilder, samt enkelte pussige anekdoter.

Forfatterens hensikt er først og fremst å klargjøre og fremheve Storbritannias andel i den kjernefysiske forskning i tiden like før og i de første par år efter annen verdenskrigs utbrudd, og da spesielt med tanke på forskningen omkring atombombe-problemet. Det er nok riktig når forfatteren hevder at man i almindelighet er tilbøyelig til å gi U. S. A. en vel stor del av «æren» for utviklingen av de første atomvåben, og at der tidligere ikke er offentliggjort en såvidt bred fremstilling av de britiske anstrengelser på dette område.

Av de mer interessante iakttagelser boken fremfører, er at på den tid britene var blitt klare over at et kjernefysisk våben virkelig kunne konstrueres, og også hadde besluttet å fremstille et slikt våben, hadde amerikanerne ingen bestemte planer om å utnytte de kjernefysiske processer i krigsøyemed. Frykten for at det nazistiske Tyskland skulle vinne «kappløpet» var reell nok dengang, selv om den senere skulle vise seg å være ubegrunnet, og i den situasjon England befant seg ved krigens begynnelse, måtte tanken om et tysk atomvåben fortone seg som et mareritt. Efter hvert som de britiske forskningsprojekter skred frem, begynte imidlertid også den amerikanske holdning å forandre seg, og da det efter den første utviklingsfase ble klart hvilken enorm industriell innsats som måtte til, ble videreutviklingen og selve produksjonen overført til U. S. A. Spesiell interesse for det videre samarbeide på dette område fikk den såkalte Quebec-avtalen, og forfatteren diskuterer også kort hvilke konsekvenser denne avtalen medførte for den senere utveksling av opplysninger og forskningsresultater.

Boken inneholder forøvrig en hel del interessant og for mange sikkert nytt stoff, men er svært springende i fremstillingen. Dette skyldes muligens det faktum at den norske utgave er noe forkortet hva angår spesielle forhold og hendelser i Storbritannia, mens beretningen om tungtvannsproduksjonen i Norge og de tilhørende sabotasjeaksjoner er blitt supplert og korrigert. Det er ingen lettlest bok vi her har foran oss, men den viser bare hvor vanskelig der er å lave god journalistikk, når et dokumentarmateriale av sådant omfang skal få plass i en bok av relativt beskjedent format. Man taper lett tråden ved stadig å møte disse «Meanwhile — back on the Ranch»-konstruksjoner. Et navneregister ville ha vært en stor hjelp.

At boken representerer «en redelig og nøktern fremstilling av hva som egentlig foregikk», medfører sikkert riktighet hva redeligheten og nøkternheten angår, men det vil vel ennu gå noen tid før vi helt får vite «hva som egentlig foregikk».

Richard R. Solem.

AHA-spalten*

Problem:

En partikkel beskriver en sirkelformet bane rundt Solen. Hvorledes vil Solens strålingstrykk innvirke på banen?

Løsning av Problem i nr. 4, 1962:

På grunn av friksjonseffekten vil kraftvektorene fra de to tidevannsbølger, sett fra Månen, danne en liten vinkel med hverandre, foruten at de er ulike store. Dette bevirker en kraftkomponent i Månens bevegelsesretning, hvorfor den vil aksellerere i denne retning, og således langsomt fjerne seg fra Jorden langs en spiralbane.

Herr *Olav Hagelund*, Kile i Stokke, får en liten oppmuntring for sin besvarelse av problemet.

Errata.
Regneautomaten ved N.T.H. W. Romberg (Fra Fysikkens Verden Nr. 1, 1963).
For Züse, les Zuse.
På side 7, 5. linje nedenfra i høyre spalte: For 100 ord, les 1000 ord.
Professor Romberg opplyser for øvrig at maskinen allerede nå er i bruk i gjennomsnitt ca. 12 timer pr. døgn.

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* Aktiviserer Hjemlige Autoriteter!

INNHOLD

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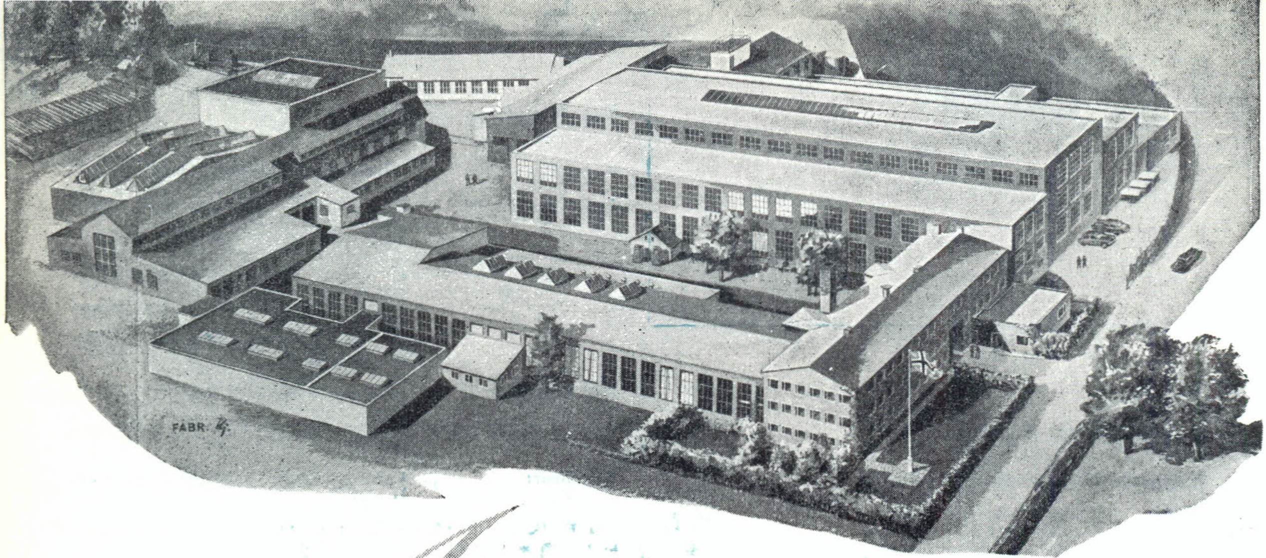

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