Physics 1967

HANS ALBRECHT BETHE

« for his contributions to the theory of nuclear reactions, especially his discoveries concerning the energy production in stars »

Physics 1967

Presentation Speech by Professor O. Klein, member of the Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

This year's Nobel Prize in Physics - to professor Hans A. Bethe - concerns an old riddle. How has it been possible for the sun to emit light and heat without exhausting its source not only during the thousands of centuries the human race has existed but also during the enormously long time when living beings needing the sun for their nourishment have developed and flourished on our earth thanks to this source? The solution of this problem seemed even more hopeless when better knowledge of the age of the earth was gained. None of the energy sources known of old could come under consideration. Some quite unknown process must be at work in the interior of the sun. Only when radioactivity, its energy generation exceeding by far any known fuel, was discovered, it began to look as if the riddle might be solved. And, although the first guess that the sun might contain a sufficient amount of radioactive substances soon proved to be wrong, the closer study of radioactivity would by and by open up a new field of physical research in which the solution was to be found.

While ordinary physics and chemistry could be led back to the behaviour of the electrons which form the outer part of atoms, the new field is concerned with their innermost part, the atomic nucleus. Its discoverer, Rutherford, called it the newer alchemy because nuclear reactions, in contrast to chemical reactions, usually lead to transmutations of the chemical elements-what alchemists wished to produce but could not by their means-the reaction energy being there some million times greater than in chemical reactions.

It soon became clear that the proton, the nucleus of the hydrogen atom, is a common building stone of all atomic nuclei. It is electrically charged. The other building stone, the neutron, being electrically neutral as indicated by its name, was discovered in 1932, twenty-one years later than the nucleus itself. And, in spite of important progress during those years, it may be said that from then on nuclear physics had really started. At that time it was already apparent that Bethe belonged to the smallgroup of young theoretical physicists who through skill and knowledge were particularly qualified for tackling the many theoretical problems turning up in close connection with the rapidly appearing experimental discoveries. The centre of these problems was to find the properties of the force that keeps the protons and neutrons together in the nucleus, the counterpart of the electric force which binds the atomic electrons to the nucleus. Bethe's contributions to the solution of these problems have been numerous and are still continuing. They put him clearly in the first row among the workers in this field-as in several other fields. Moreover, about the middle of the thirties he wrote, partly alone, partly together with some colleagues, what nuclear physicists at the time used to call the Bethe bible, a penetrating review of about all that was known of atomic nuclei, experimental as well as theoretical.

This extensive and profound knowledge of his regarding atomic nuclei together with a rare gift of rapidly grasping the essence of a physical problem and finding ways of solving it explains that Bethe could so swiftly do the work awarded by the Nobel Prize. He started his work after a conference taking place in Washington in March 1938 and the paper containing a thorough description of it was delivered for print at the beginning of September the same year. During that conference and afterwards he seems also to have acquired the necessary astrophysical knowledge. This knowledge depended mainly on a pioneer work by Eddington from the year 1926, according to which the innermost part of the sun is a hot gas mainly consisting of hydrogen and helium. Owing to the high temperature, about 20 million degrees, - these atoms being dissolved into electrons and nuclei-, the mixture, despite the high density - about 80 times that of water - really behaves like a gas. The amount of energy generation necessary to maintain this state was known from measurements of the radiation falling on the earth. Taken as a whole it is enormous, but very slow as compared to the size of the sun. An ordinary 60-Watt electric bulb would correspond to about 300 tons average sun matter. This very slow burning together with the very high energy release from a given weight of fuel gives this source the high durability required by geology and the long existence of life on the earth.

Before coming to the nuclear processes, which according to Bethe's work are definitely the source of the energy generation of the sun and similar stars, a few words should be said about two questions which naturally present themselves in this connection. Why are these nuclear processes so slow in the sun when they are so fast in atomic reactors, not to mention atomic bombs? And why are they non-existent under ordinary conditions? The answer is that nuclei are protected against other nuclei by the repulsion due to their electric charges together with the extremely small range of the nuclear force-which is about as small relative to a midget as a midget is to the sun-implying that a proton must have an extremely high velocity in order to come so close to another nucleus as is necessary for a nuclear reaction to take place. If it were not for the quantum- mechanical tunnel effect studied very closely in this connection by Gamow-who must be considered the main forerunner of Bethe with respect to the application ofnuclear physics to astronomy-even the velocities of the protons at the high temperature of the sun would not be able to produce any such processes. But through this effect the required slow reactions do occur. The case of the atomic reactors is different, because the reactions are there produced by neutrons, which having no charge are not stopped by the electric charge of the nuclei. Fortunately neutrons are short-lived and therefore extremely rare under ordinary circumstances and also in the sun.

Even when Bethe started his work on the energy generation in stars there were important gaps in the knowledge about nuclei which made the solution of the problem very difficult. And it was by a remarkable combination of underdeveloped theory and incomplete experimental evidence, under repeated comparison of his conclusions with their astronomical consequences, that he succeeded in establishing the mechanism of energy generation in the sun and similar stars so well that only minor corrections were needed when many years later the required experimental knowledge had made considerable progress and when, moreover, electronic computers had become available for the numerical calculations.

A very important part of his work resulted in eliminating a great number of thinkable nuclear processes under the conditions at the centre of the sun, after which only two possible processes remained. The simplest of them begins with two protons colliding and forming a nucleus of heavy hydrogen, the surplus of electric charge vanishing in the form of a positive electron. After capturing a few more protons the result of the process is the formation of a helium nucleus from four protons. Thereby the energy release from a given weight of hydrogen is nearly 20 million times greater than that produced by burning the same weight of carbon into carbon dioxide. The second process is more complicated. It requires the presence of carbon which, however, will practically not be consumed but acts as a catalyst, the result being the same as in the former process. It should be mentioned that the first process had been proposed a few years earlier by Atkinson and later discussed by von Weizsäcker, who also considered the second process independently of and at about the same time as Bethe. But none of them had attempted a thorough analysis of these and other thinkable processes necessary to make it reasonably certain that these processes, and only these, are responsible for the energy generation in the sun and similar stars.

Bethe's work constitutes since many years a main foundation for the great development which has taken place of the knowledge of the interior of the sun and the stars. During recent years it has obtained a new actuality through a promising attempt made by a group of astrophysicists to understand what happens when a star has used up its hydrogen, thereby throwing new light on another old riddle, that of the origin of the chemical elements.

Professor Bethe. You may have been astonished that among your many contributions to physics, several of which have been proposed for the Nobel Prize, we have chosen one which contains less fundamental physics than many of the others and which has taken only a short part of your long time in science. This, however, is quite in agreement with the rules of the Nobel Prize and does not imply that we are not highly impressed by the role you have played in so many parts of the development of physics ever since you started doing research some forty years ago. On the other hand your solution of the energy source of stars is one of the most important applications offundamental physics in our days, having led to a deepgoing evolution of our knowledge of the universe around us. On behalf of the Royal Swedish Academy of Sciences I extend to you the most hearty congratulations. And now I have the privilege to ask you to receive the Nobel Prize for Physics from the hands of His Majesty the King.

H.A. BETHE

Energy production in stars

Nobel Lecture, December 11, 1967

History

From time immemorial people must have been curious to know what keeps the sun shining. The first scientific attempt at an explanation was by Helmholtz about one hundred years ago, and was based on the force most familiar to physicists at the time, gravitation. When a gram of matter falls to the sun's surface it gets a potential energy

$$E_{pot} = -GM/R = -1.91 \cdot 10^{15} \text{ erg/g} \tag{1}$$

where $M = 1.99 \cdot 10^{33}$ g is the sun's mass, $R = 6.96 \cdot 10^{10}$ cm its radius, and $G = 6.67 \cdot 10^{-8}$ the gravitational constant. A similar energy was set free when the sun was assembled from interstellar gas or dust in the dim past; actually somewhat more, because most of the sun's material is located closer to its center, and therefore has a numerically larger potential energy. One- half of the energy set free is transformed into kinetic energy according to the well-known virial theorem of mechanics. This will permit us later to estimate the temperature in the sun. The other half of the potential energy is radiated away. We known that at present the sun radiates

$$\varepsilon = 1.96 \text{ erg/g sec} \tag{2}$$

Therefore, if gravitation supplies the energy, there is enough energy available to supply the radiation for about 10 10^{15} sec which is about 30 million years.

This was long enough for nineteenth century physicists, and certainly a great deal longer than man's recorded history. It was not long enough for the biologists of the time. Darwin's theory of evolution had just become popular, and biologists argued with Helmholtz that evolution would require a longer time than 30 million years, and that therefore his energy source for the sun was insufficient. They were right.

At the end of the 19th century, radioactivity was discovered by Becquerel and the two Curie's who received one of the first Nobel prizes for this discovery. Radioactivity permitted a determination of the age of the earth, and more recently, of meteorites which indicate the time at which matter in the solar system solidified. On the basis of such measurements the age of the sun is estimated to be 5 milliards of years, within about 10%. So gravitation is not sufficient to supply its energy over the ages.

Eddington, in the 1920's, investigated very thoroughly the interior constitution of the sun and other stars, and was much concerned about the sources of stellar energy. His favorite hypothesis was the complete annihilation of matter, changing nuclei and electrons into radiation. The energy which was to be set free by such a process, if it could occur, is given by the Einstein relation between mass and energy and is

$$c^2 = 9 \cdot 10^{20} \text{ erg/g} \tag{3}$$

This would be enough to supply the sun's radiation for 1500 milliards of years. However nobody has ever observed the complete annihilation of matter. From experiments on earth we know that protons and electrons do not annihilate each other in 10^{30} years. It is hard to believe that the situation would be different at a temperature of some IO million degrees such as prevails in the stars, and Eddington appreciated this difficulty quite well.

From the early 1930's it was generally assumed that the stellar energy is produced by nuclear reactions. Already in 1929, Atkinson and Houtermans[1] concluded that at the high temperatures in the interior of a star, the nuclei in the star could penetrate into other nuclei and cause nuclear reactions, releasing energy. In 1933, particle accelerators began to operate in which such nuclear reactions were actually observed. They were found to obey very closely the theory of Gamow, Condon and Gurney, on the penetration of charged particles through potential barriers. In early 1938, Gamow and Teller[2] revised the theory of Atkinson and Houtermans on the rate of « thermonuclear » reactions, *i.* e. nuclear reactions occurring at high temperature. At the same time, von Weizsäcker[3] speculated on the reactions which actually might take place in the stars.

In April 1938, Gamow assembled a small conference of physicists and astrophysicists in Washington, D. C. This conference was sponsored by the Department of Terrestrial Magnetism of the Carnegie Institution. At this Conference, the astrophysicists told us physicists what they knew about the internal constitution of the stars. This was quite a lot, and all their results had been derived without knowledge of the specific source of energy. The only assumption they made was that most of the energy was produced « near » the center of the star.

Properties of Stars

The most easily observable properties of a star are its total luminosity and its surface temperature. In relatively few *cases* of nearby stars, the mass of the star can also be determined.

Fig.1 shows the customary Hertzsprung-Russell diagram. The luminosity, expressed in terms of that of the sun, is plotted against the surface temperature, both on a logarithmic scale. Conspicuous is the main sequence, going from upper left to lower right, i.e. from hot and luminous stars to cool and faint ones. Most stars lie on this sequence. In the upper right are the Red Giants, cool but brilliant stars. In the lower left are the White Dwarfs, hot but faint. We shall be mainly concerned with the main sequence. After being assembled, by gravitation, stars spend the most part of their life on the main sequence, then develop into red giants, and in the end, probably into white dwarfs.

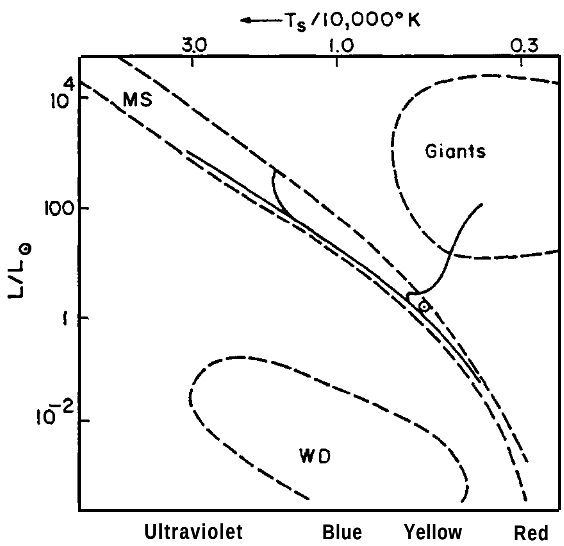

Fig.1. Hertzsprung-Russell diagram. From E.E.Salpeter, in *Apollo and the Universe*, Science Foundation for Physics, University of Sydney, Australia, 1967.

The figure shows that typical surface temperatures are of the order of 10^4°K. Fig. 2 gives the relation between mass and luminosity in the main sequence. At the upper end, beyond about 15 sun masses, the mass determinations are uncertain. It is clear, however, that luminosity increases rapidly with mass.

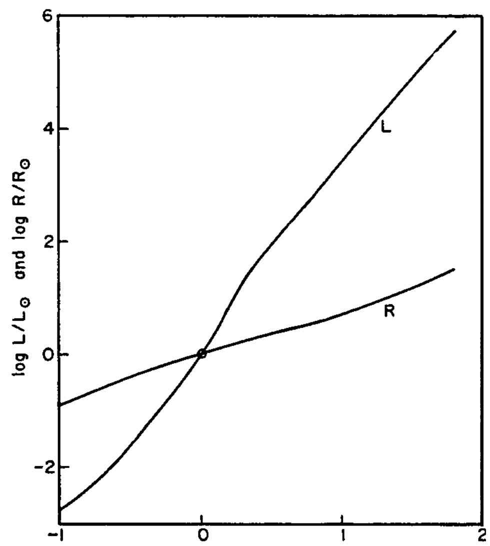

Fig. 2. Luminosity and radius of stars vs. mass. Abscissa is *log* $M/M_\odot$. Data from C. W. Allen, *Astrophysical* Quantities, Athlone Press, 1963, p.203. The curve for log $L/L_\odot$ holds for all stars, that for $R/R_\odot$ only for the stars in the main sequence. The symbol $\odot$ refers to the sun.

For a factor of 10 in mass, the luminosity increases by a factor of about 3000, hence the energy production per gram is about 300 times larger.

To obtain information on the interior constitution of the stars, astrophysicists integrate two fundamental equations. Pioneers in this work have been Eddington, Chandrasekhar and Strömgren. The first equation is that of hydrostatic equilibrium

$$\frac{dP}{dr} = -GM(r)\frac{\varrho(r)}{r^2} \tag{4}$$

in which P is the pressure at distance r from the center, ϱ is the density and $M(r)$ is the total mass inside r. The second equation is that of radiation transport

$$\frac{1}{\varkappa\varrho}\frac{d}{dr}\left(\tfrac{1}{3}acT^4\right) = -\frac{L(r)}{4\pi r^2} \tag{5}$$

Here $\varkappa$ is the opacity of the stellar material for black-body radiation of the local temperature T, a is the Stefan-Boltzmann constant, and $L(r)$ is the flux of radiation at r. The value of L at the surface R of the star is the luminosity.

In the stars we shall discuss, the gas obeys the equation of state

$$P = RT\varrho/\mu \tag{6}$$

where R is the gas constant, while m is the mean molecular weight of the stellar material. If X, Y and Z are respectively concentrations by mass of hydrogen, helium and all heavier elements, and if all gases are fully ionized, then

$$\mu^{-1} = 2X + \tfrac{3}{4}Y + \tfrac{1}{2}Z \tag{7}$$

In all stars except the very oldest ones, it is believed that Z is between 0.02 and 0.04; in the sun at present, X is about 0.65, hence Y= 0.33 and m=0.65. In many stars the chemical composition, especially X and Y, vary with position r. The opacity is a complicated function of Z and T, but in many cases it behaves like

$$\varkappa = C\varrho\, T^{-3\,4} \tag{8}$$

where C is a constant.

The integration of (4) and (5) in general requires computers. However an estimate of the central temperature may be made from the virial theorem which we mentioned in the beginning. According to this, the average thermal energy per unit mass of the star is one-half of the average potential energy. This leads to the estimate of the thermal energy per particle at the center of the star,

$$k\,T_c = \alpha\,\mu\,G\,H\,M/R \tag{9}$$

in which H is the mass of the hydrogen atom, and a is a constant whose magnitude depends on the specific model of the star but is usually about 1 for main sequence stars. Using this value, and (1), we find for the central temperature of the sun

$$T_{6c} = 14 \tag{10}$$

where T_6 denotes the temperature in millions of degrees, here and in the following. A more careful integration of the equations of equilibrium by Demarque and Percy[4] gives

$$T_{c6} = 15.7;\ \varrho_c = 158\ \mathrm{g/cm^3} \tag{11}$$

Originally Eddington had assumed that the stars contain mainly heavy elements, from carbon on up. In this case m= 2 and the central temperature is increased by a factor of 3, to about 40 million degrees; this led to contradictions with the equation of radiation flow, (5), if the theoretical value of the opacity was used. Strömgren pointed out that these contradictions can be resolved by assuming the star to consist mainly of hydrogen, which is also in agreement with stellar spectra. In modern calculations the three quantities X, Y, Z, indicating the chemical composition of the star, are taken to be parameters to be fixed so as to fit all equations ofstellar equilibrium.

Thermonuclear Reactions

All nuclei in a normal star are positively charged. In order for them to react they must penetrate each others Coulomb potential barrier. The wave mechanical theory of this shows that in the absence of resonances, the cross section has the form

$$a(E) = \frac{S(E)}{E} \exp\left(-\sqrt{\frac{E_G}{E}}\right) \tag{12}$$

where E is the energy of the relative motion of the two colliding particles, $S(E)$ is a coefficient characteristic of the nuclear reaction involved and

$$E_G = 2M(\pi Z_0 Z_1 e^2/\hbar)^2 = (2\pi Z_0 Z_1)^2 E_{Bohr} \tag{13}$$

Here M is the reduced mass of the two particles, Z_0 and Z_1, their charges, and E_{Bohr} is the Bohr energy for mass M and charge 1. (13) can be evaluated to give

$$E_G = 0.979\ W \text{ MeV} \tag{14}$$

with

$$W = A Z_0^2 Z_1^2 \tag{14a}$$

$$A = A_0 A_1/(A_0 + A_1) \tag{14b}$$

in which A,, A, are the atomic weights of the two colliding particles. For most nuclear reactions $S(E)$ is between 10 MeV- barns and 1 keV-barn.

The gas at a given r in the star has a given temperature so that the particles have a Boltzmann energy distribution. The rate of nuclear reactions is then proportional to

$$(8/\pi M)^{\frac{1}{2}} (kT)^{-3/2} \int \sigma(E)\, E \exp(-E/kT)\, \mathrm{d}E \tag{15}$$

It is most convenient5 to write for the rate of disappearance of one of the reactants

$$\mathrm{d}X_0/\mathrm{d}t = -[01]\, X_0 X_1 \tag{16}$$

where X_0 and X_1, are the concentrations of the reactants by mass, and

$$[01] = 7.8 \cdot 10^{11} (Z_0 Z_1/A)^{1/3} S_{eff} \varrho T_6^{-2/3} e^{-\tau} \qquad (17)$$

$$\tau = 42.487 (W/T_6)^{1/3} \qquad (17a)$$

Since the reaction cross section **(12)** increases rapidly with energy, the main contribution to the reaction comes from particles which have an energy many times the average thermal energy. Indeed the most important energy is

$$E_0 = (\tau/3) k T \qquad (18)$$

For T= 13 which is an average for the interior of the sun, we have

$$\begin{aligned} \tau/3 = 4.7 &\text{ for the reaction H + H} \\ 19 &\text{ for the reaction C + H} \\ 25 &\text{ for the reaction N + H} \end{aligned} \qquad (19)$$

It is also easy to see from **(17)** that the temperature dependence of the reaction rate is

$$\frac{d \ln [01]}{d \ln T} = \frac{\tau - 2}{3} \qquad (20)$$

Nuclear Reactions in Main Sequence Stars

Evidently, at a given temperature and under otherwise equal conditions, the reactions which can occur most easily are those which have the smallest possible value of W (14a). This means that at least one of the interacting nuclei should be a proton, $A_0 = Z_0 = 1$. Thus we may examine the reactions involving protons.

The simplest of all possible reactions is

$$H + H = D + \varepsilon^+ + \nu \qquad (21)$$

(ε+ = positron, ν = neutrino).

This was first suggested by von Weizsäcker[3], and calculated by Critchfield and Bethe[6]. The reaction is of course exceedingly slow because it involves the beta disintegration. Indeed the characteristic factor S is

$$S(E) = 3.36 \cdot 10^{-25} \text{ MeV-barns} \qquad (22)$$

This has been derived on purely theoretical grounds, using the known coupling constant of beta disintegration; the value is believed to be accurate to 20% or better. There is no chance of observing such a slow reaction on earth, but in the stars we have almost unlimited time, and a large supply of protons of high energy. As we shall see presently, the rate of energy production by this simple reaction fits the observed energy production in the sun very well.

The deuterons formed in (21) will quickly react further, and the end product is ^{4}He. We shall discuss the reactions in more detail later on.

The proton-proton reaction (21), although it predicts the correct energy production in the sun, has a rather weak dependence on temperature. According to (19), (20), it behaves about as T^4. Since central temperatures change only little from the sun to more massive stars, the energy production by this reaction does likewise. However as we have seen in Fig. 2, the observed energy production increases dramatically with increasing mass. Therefore there must exist nuclear reactions which are more strongly dependent on temperature; these must involve heavier nuclei.

Stimulated by the Washington Conference of April 1938, and following the argumentjust mentioned, I examined7 the reactions between protons and other nuclei, going up in the periodic system. Reactions between H and ^{4}He lead nowhere, there being no stable nucleus of mass 5. Reactions of H with Li, Be and B, as well as with deuterons, are all very fast at the central temperature of the sun, but just this speed of the reaction rules them out: the partner of *H* is very quickly used up in the process. In fact, and just because of this reason, all the elements mentioned, from deuterium to boron, are extremely rare on earth and in the stars, and can therefore not be important sources of energy.

The next element, carbon, behaves quite differently. In the first place, it is an abundant element, probably making up about 1% by mass of any newly formed star. Secondly, in a gas of stellar temperature, it undergoes a cycle of reactions, as follows

$$^{12}\mathrm{C} + \mathrm{H} = {}^{13}\mathrm{N} + \gamma \tag{23a}$$

$$^{13}\mathrm{N} = {}^{13}\mathrm{C} + \varepsilon^+ + \nu \tag{23b}$$

$$^{13}\mathrm{C} + \mathrm{H} = {}^{14}\mathrm{N} + \gamma \tag{23c}$$

$$^{14}\mathrm{N} + \mathrm{H} = {}^{15}\mathrm{O} + \gamma \tag{23d}$$

$$^{15}\mathrm{O} = {}^{15}\mathrm{N} + \varepsilon^+ + \nu \tag{23e}$$

$$^{15}\mathrm{N} + \mathrm{H} = {}^{12}\mathrm{C} + {}^{4}\mathrm{He} \tag{23f}$$

Reactions a, c, and d are radiative captures; the proton is captured by the nucleus and the energy emitted in the form of gamma rays; these are then quickly converted into thermal energy of the gas. For reactions of this type, *S (E)* is of the order of 1 keV- barn. Reactions b and e are simply spontaneous beta decays, with lifetimes of 10 and 2 min respectively, negligible in comparison with stellar times. Reaction f is the most common type of nuclear reaction, with 2 nuclei resulting from the collision; *S (E)* for such reactions is commonly of the order of MeV- barns.

Reaction f is in a way the most interesting because it closes the cycle: we reproduce the ^{12}C which we started from. In other words, carbon is only used as a catalyst; the result of the reaction is a combination of 4 protons and 2 electrons* to form one ^{4}He nucleus. In this process two neutrinos are emitted, taking away about 2 MeV energy together. The rest of the energy, about 25 MeV per cycle, is released usefully to keep the sun warm.

Making reasonable assumptions of the reaction strength *S (E),* on the basis of general nuclear physics, I found in 1938 that the carbon-nitrogen cycle gives about the correct energy production in the sun. Since it involves nuclei of relatively high charge, it has a strong temperature dependence, as given in (19). The reaction with ^{14}N is the slowest of the cycle and therefore determines the rate of energy production; it goes about as T^{24} near solar temperature. This is amply sufficient to explain the high rate of energy production in massive stars[9].

Experimental Results

To put the theory on a firm basis, it is important to determine the strength factor *S (E)* for each reaction by experiment. This has been done under the leadership of W. A.Fowler[10] of the California Institute of Technology in a monumental series of papers extending over a quarter of a century. Not only have all the reactions in (23) been observed, but in all cases *S (E) has* been accurately determined.

The main difficulty in this work is due to the resonances which commonly occur in nuclear reactions. Fig. 3 shows the cross section of the first reactions (23a), as a function of energy. The measured cross sections extend over a factor of 10^7 in magnitude; the smallest ones are 10^{-11} barns= 10^{-35} cm^2 and therefore clearly very difficult to observe. The curve shows a resonance at 460 keV. The solid curve is determined from nuclear reaction theory, on the basis of the existence of that resonance. The fit of the observed points to the calculated curve is impressive. Similar results have been obtained on the other three proton-capture reactions in (23).

On the basis of Fig. 3 we can confidently extrapolate the measurements to lower energy. As we mentioned in (18) the most important energy contribut -

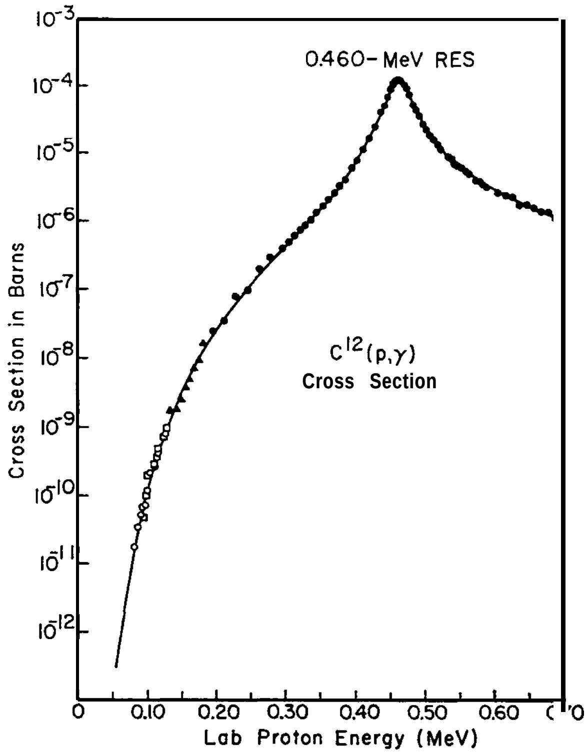

Fig. 3. Cross section for the reaction ^{12}C + H, as a function of the proton energy. From Fowler, Caughlan and Zimmerman[5].

ing to the reaction rate is about 20 *kT*. For T_6= 13, we have *kT*= 1.1 keV; so we are most interested in the cross section around 20 keV. This is much too low an energy to observe the cross section in the laboratory; even at 100 keV, the cross section is barely observable. So quite a long extrapolation is required. This can be done with confidence provided there are no resonances close to *E*= o. Therefore a great deal of experimental work has gone into the search for such resonances.

The resonances exist of course in the compound nucleus, *i. e.* the nucleus obtained by adding the two initial reactants. To find resonances near the threshold of the reactions (23), it is necessary to produce the same compound nucleus from other initial nuclei, e.g., in the reaction between ^{14}N and H, the compound nucleus ^{15}O is formed. To investigate its levels Hensley[11] at CalTech studied the reaction

$$^{16}O + {}^{3}He = {}^{15}O + {}^{4}He \qquad (24)$$

He found indeed a resonance 20 keV below the threshold for ^{14}N + H which in principle might enhance the process (23d). However the state in ^{15}O was found to have a spin J= 7/2. Therefore, even though ^{14}H has J= 1 and the proton has a spin of 1/2, we need at least an orbital momentum λ= 2, to reach this resonant state in ^{15}O. The cross section for such a high orbital momentum is reduced by at least a factor 10^4, compared toil = o, so that the near-resonance does not in fact enhance the cross section ^{14}N+ H appreciably. This cross section can then be calculated by theoretical extrapolation from the measured range of proton energies, and the same is true for the other reactions in the cycle(23).

On this basis, Fowler and others have calculated the rate of reactions in the CN cycle. A convenient tabulation has been given by Reeves[12]; his results are plotted in Fig.4. This figure gives the energy production per gram per second as a function of temperature. We have assumed X= 0.5, Z= 0.02.

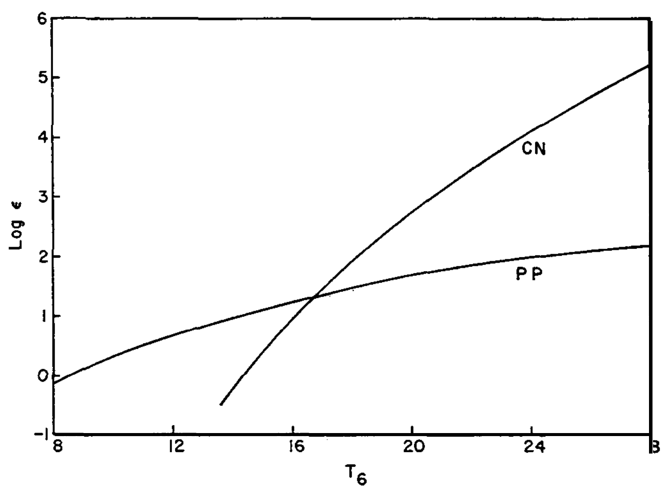

Fig. 4. The energy production, in erg/g sec as a function of the temperature in millions of degrees. For the proton-proton reaction (PP) and the carbon-nitrogen cycle (CN). Concentrations assumed X= Y= 0.5, Z= 0.02. Calculated from Tables 8 and 9 of Reeves[12].

The figure shows that at low temperature the H + H reaction dominates, at high temperatures the C-t- N cycle; the crossing point is at T_6= 13; here the energy production is 7 erg/g sec. The average over the entire sun is obviously smaller, and the result is compatible with an average production of 2 erg/g sec.

The energy production in the main sequence can thus be considered as well understood.

An additional point should be mentioned. Especially at higher temperature, when the CN cycle prevails, there is also a substantial probability for the reaction chain

$$^{16}O + H = {}^{17}F + \gamma \quad (25a)$$

$$^{17}F = {}^{17}O + \varepsilon^{+} + \nu \quad (25b)$$

$$^{17}O + H = {}^{14}N + {}^{4}He \quad (25c)$$

This chain is not cyclic but feeds into the CN cycle. It is customary to speak of the whole set of reactions as the CNO bi-cycle. The effect of reactions (25) is that ^{16}O initially present will also contribute to the reactants available, and thus increase the reaction rate of the CN cycle somewhat. This has been taken into account in Fig. 5.

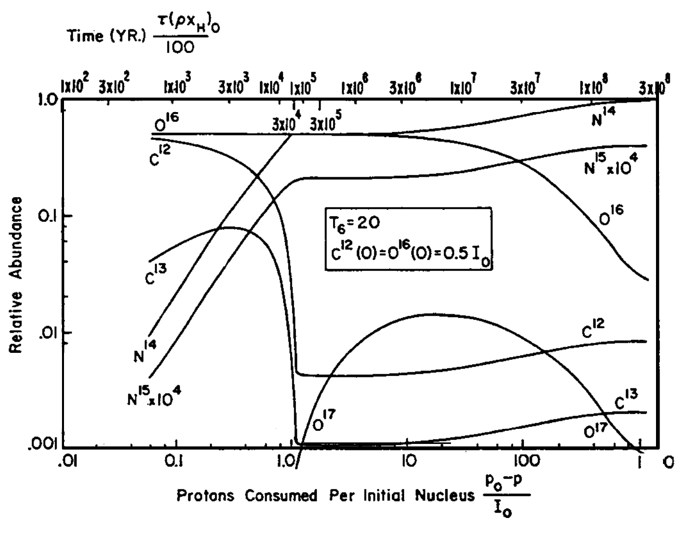

Fig. 5. Variation with time of the abundances of various elements involved in the CNO cycle. It is assumed that initially ^{12}C and ^{16}O have the same abundance while that of ^{14}N is small. From G.R.Caughlan, *Astrophys. J.* (1967).

If equilibrium is established in the CNO bi-cycle, eventually most of the nuclei involved will end up as ^{14}N because this nucleus has by far the longest lifetime against nuclear reactions. There is no observable evidence for this; in fact wherever the abundance can be observed, C and O tend to be at least as abundant as N. However this is probably due to the fact that the interior of a star stays well separated from its surface; there is very little mixing. Astrophysicists have investigated the circumstances when mixing is to be expected, and have found that surface abundances are quite compatible with these expectations. In the interstellar material which is used to form stars, we have reason to believe that C and O are abundant and N is rare. This will be discussed later.

The Completion of the Proton-Proton Chain

The initial reaction (21) is followed almost immediately by

$$^{2}\mathrm{H} + \mathrm{H} = {}^{3}\mathrm{He} + \gamma \tag{26}$$

The fate of ^{3}He depends on the temperature. Below about $T_6 = 15$, the ^{3}He builds up sufficiently so that such nuclei react with each other according to

$$2\ {}^{3}\mathrm{He} = {}^{4}\mathrm{He} + 2\ \mathrm{H} \tag{27}$$

This reaction has an unusually high $S(E) = 5$ MeV- barns[5]. At higher temperature, the reaction

$$^{4}\mathrm{He} + {}^{3}\mathrm{He} = {}^{7}\mathrm{Be} + \gamma \tag{28}$$

competes favorably with (27). The ^{7}Be thus formed may again react in one of two ways

$$^{7}\mathrm{Be} + e^{-} = {}^{7}\mathrm{Li} + \nu \tag{29a}$$

$$^{7}\mathrm{Be} + \mathrm{H} = {}^{8}\mathrm{B} + \gamma \tag{29b}$$

$$^{8}\mathrm{B} = 2\ {}^{4}\mathrm{He} + \varepsilon^{+} + \nu \tag{29c}$$

At about $T_C = 20$, reaction (29b) begins to dominate over (29a). (29b) is followed by (29c) which emits neutrinos of very high energy. Davies[13], at Brookhaven, is attempting to observe these neutrinos.

Evolution of a Star

A main sequence star uses up its hydrogen preferentially near its center where nuclear reactions proceed most rapidly. After a while, the center has lost almost all its hydrogen. For stars of about twice the luminosity of the sun, this happens in less than 10^{10} years which is approximately the age of the universe, and also the age of stars in the globular clusters. We shall now discuss what happens to a star after it has used up the hydrogen at the center. Of course, in the outside regions hydrogen is still abundant.

This evolution of a star was first calculated by Schwarzschild[14] who has been followed by many others; we shall use recent calculations by Iben[15]. When hydrogen gets depleted, not enough energy is produced near the center to sustain the pressure of the outside layers of the star. Hence gravitation will cause the center to collapse. Thereby, higher temperatures and densities are achieved. The temperature also increases farther out where there is still hydrogen left, and this region now begins to burn. After a relatively short time, a shell of *H,* away from the center, produces most of the energy; this shell gradually moves outward and gets progressively thinner as time goes on.

At the same time, the region of the star outside the burning shell expands. This result follows clearly from all the many numerical computations on this subject. The physical reason is not clear. One hypothesis is that it is due to the discontinuity in mean molecular weight: Inside the shell, there is mostly helium, of μ= 4/3, outside we have mostly hydrogen, and μ= 0.65. Another suggestion is that the flow of radiation is made difficult by the small radius

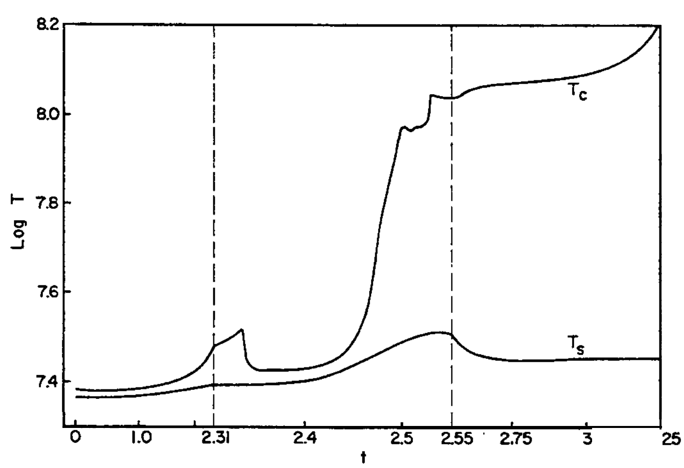

Fig. 6. Evolution of a star of 3 solar masses, according to I. Iben, *Astrophys.J.,* 142 *(1965)* 1447. Abscissa is time in units of 10^8 years (note the breaks in scale at t= 2.31 and 2.55). I. Temperature (on logarithmic scale) : T_c= temperature at center of star, T_s= same at mid-point of source of energy generation, which, after t = 2.48 is a thin shell. T_c, increases enormously, T_s stays almost constant.

of the energy source, and that this has to be compensated by lower density ust outside the source.

By this expansion the star develops into a red giant. Indeed, in globular clusters (which, as I mentioned, are made up of very old stars), all the more luminous stars are red giants. In the outer portion of these stars, radiative transport is no longer sufficient to carry the energy flow; therefore convection of material sets in in these outer regions. This convection can occupy as much as the outer 80% of the mass of the star; it leads to intimate mixing of the material in the convection zone.

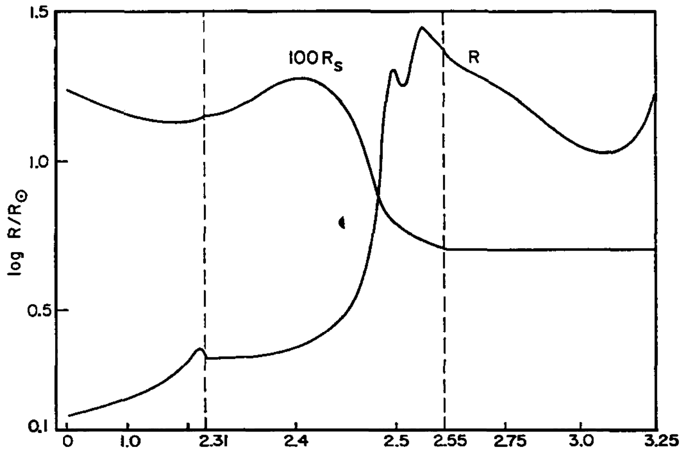

Fig. 7. Evolution of a star, (see caption to Fig. 6). II Radius, in units of that of the sun, on logarithmic scale. R = total radius, 100 R_s = 100 times the radius of mid-point of energy source. R increases tremendously, while R, shrinks somewhat.

Iben[15] has discussed a nice observational confirmation of this convectional mixing. The star Capella is a double star, each component having a mass of about 3 solar masses, and each being a red giant. The somewhat lighter star, « Capella F » (its spectral type is F) shows noticeable amounts of Li in its spectrum, while the somewhat heavier Capella G shows at least 100 times less Li. It should be expected that G, being heavier, is farther advanced in its evolution. Iben now gives arguments that the deep-reaching convection and mixing which we just discussed, will occurjust between the evolution phases F and G. By convection, material from the interior of the star will be carried to the surface; this material has been very hot and has therefore burned up its Li. Before deep convection sets in (in star F) the surface Li never sees high temperature and thus is preserved.

Following the calculations of Iben we have plotted in Figs. 6-g the development of various important quantities in the history of a star of mass= 3 solar masses. The time is in units of 10^8 years. Since the developments go at very variable speed, the time scale has been broken twice, at t= 2.31 and t= 2.55. In between is the period during which the shell source develops.

During this period the central temperature rises spectacularly (Fig. 6) from about T_6= 25 to T_6= 100. At the same time the radius increases from about 2 to 30 solar radii; subsequently, it decreases again to about 15 (Fig. 7). The central density, starting at about 40, increases in the same period to about $5 \cdot 10^4$ (Fig.8). The luminosity (Fig. 9) does not change spectacularly, staying always between 100 and 300 times that of the sun.

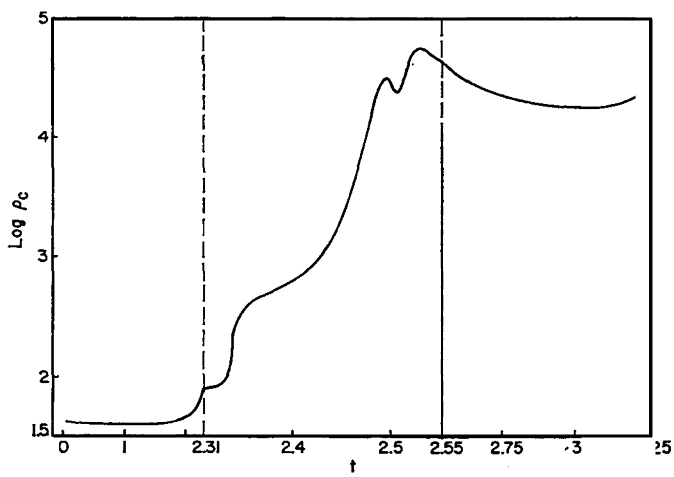

Fig. 8. Evolution of a star (see caption to Fig. 6).111. Density, on logarithmic scale, at the center of the star. This quantity increases about 1000-fold.

While the inside and the outside of the star undergo such spectacular changes, the shell in which the hydrogen is actually burning, does not change very much. Fig. g shows m, the fraction of the mass of the star enclosed by the burning shell. Even at the end of the calculation, t= 3.25, this is only m= 0.2. This means that only 20% of the hydrogen in the star has burned after all this development. Fig.6, curve T_s, shows the temperature in the burning shell which stays near 25 million degrees all the time. Fig. 7, curve R_s, shows the radius of the shell, in units of the solar radius; during the critical time when the shell is formed this radius drops from about 0.15 to 0.07. This is of course the mechanism by which the shell is kept at the temperature which originally prevailed at the center.

In the meantime, the temperature at the center increases steadily. When it reaches about $T_6 = 100$, the ^{4}He which is abundant at the center, can undergo nuclear reactions. The first of these, which occurs at the lowest temperature (about $T_6 = 90$) is

$$^{14}\mathrm{N} + {^4\mathrm{He}} = {^{18}\mathrm{F}} + \gamma \tag{30}$$

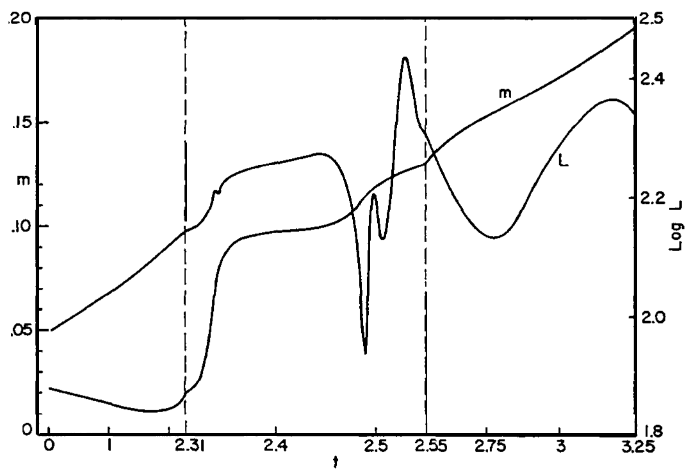

Fig.9. Evolution of a star (see caption to Fig. 6). IV. Curve *L*, luminosity relative to that of the sun, on logarithmic scale. This quantity does not change very much during the life of the star. Curve *m*, fraction of the mass of the star enclosed by energy-producing shell, on linear scale. This fraction increases slowly with time.

While this reaction goes on, the central temperature remains fairly constant. However, there is not much ^{14}N so the reaction soon stops (after about $0.02 \cdot 10^8$ years), and the center contracts further.

The next reaction makes use entirely of the abundant ^{4}He, *viz.*

$$3\,{^4\mathrm{He}} = {^{12}\mathrm{C}} + \gamma \tag{31}$$

This reaction has the handicap of requiring a simultaneous collision of 3 alpha particles. This would be extremely unlikely were it not for the fact that it is favored by a *double* resonance. Two alpha particles have nearly the same energy as the unstable nucleus ^{8}Be, and further ^{8}Be +^{4}He has almost the same energy as an excited state of ^{12}C. This reaction can of course not be observed in the laboratory but the two contributing resonances can be. The importance of the first resonance was first suggested by Salpeter[16], the second by Hoyle[17]. Recent data indicate that (31) requires a temperature of about $T_6 = 110$, at the central densities corresponding to $t = 2.5$, *i.e.* $\varrho_c > 10^4$. Once this reaction sets in, the central temperature does not rise very fast any more.

Reaction (31) is most important for the buildup of elements. Early investigators [3,7] had great trouble with bridging the gap between ^{4}He and ^{12}C. Two nuclei in this gap, mass 5 and mass 8, are completely unstable, the rest disintegrate in a very short time under stellar conditions. Reaction (31) however leads to stable ^{12}C. This nucleus can now capture a further alpha particle

$$^{12}\mathrm{C} + {}^4\mathrm{He} = {}^{16}\mathrm{O} + \gamma \tag{32}$$

the temperatures required for this are about the same as for (31). There is also some capture of alpha particles by ^{16}O leading to ^{20}Ne, but the next step, $^{20}\mathrm{Ne} \rightarrow {}^{24}\mathrm{Mg}$, cannot occur appreciably at these temperatures; instead, the helium gets used up in forming ^{12}C, ^{16}O and some ^{20}Ne.

Helium is depleted first in the center, and now the same process repeats which previously took place with hydrogen. A shell of burning He is formed, at a smaller radius than the H shell, and of course at a higher temperature. The center of the star now contracts further by gravitation and reaches still higher temperatures.

Buildup and Dispersal of Elements

The further developments of a massive star are more speculative. However the theory of Hoyle and collaborator[18] is likely to be correct.

The center of the star heats up until the newly formed carbon nuclei can react with each other. This happens at a temperature of roughly 10^9 degrees. Nuclei like ^{24}Mg or ^{28}Si can be formed. There are also more complicated mechanisms in which we first have a capture reaction with emission of a gamma ray, followed by capture of this gamma ray in another nucleus which releases ^{4}He. This ^{4}He can then enter further nuclei and build up the entire chain of stable nuclei up to the most stable Fe. Not much energy is released in all of these processes.

The center of the star contracts further and gets still hotter. At very high temperatures, several milliards of degrees, thermal equilibrium is no longer strongly in favor of nuclei of the greatest binding energy. Instead, endothermic processes can take place which destroy some of the stable nuclei already formed. In the process, alpha particles, protons and even neutrons may be released. This permits the buildup of elements beyond Fe, up to the top of the periodic table. Because of the high temperatures involved all this probably goes fairly fast, perhaps in thousands of years.

During this stage, nuclear processes tend to consume rather than release energy. Therefore they no longer oppose the gravitational contraction so that contraction continues unchecked. It is believed that this will lead to an unstable situation. Just as the first contraction, at the formation of the H shell source, led to an expansion of the outer envelope of the star, a similar outward expansion is expected now. But time scales are now short, and this expansion may easily be an explosion. Hoyle *et al.* [18] have suggested this as the mechanism for a supernova.

In a supernova explosion much of the material of the star is ejected into interstellar space. We see this, e.g., in the Crab Nebula. The ejected material probably contains the heavy elements which have been formed in the interior of the massive star. Thus heavy elements get into the interstellar gas, and can then be collected again by newly forming stars. It is believed that this is the way how stars get their heavy elements. This means that most of the stars we see, including our sun, are at least second generation stars, which have collected the debris ofearlier stars which have suffered a supernova explosion.

To clinch this argument it must be shown that heavy elements cannot be produced in other ways. This has indeed been shown by Fowler[19]. He has investigated the behavior of the enormous gas cloud involved in the original « Big Bang », and its development with time. He has shown that temperatures and densities, as functions of time, are such that heavy elements beginning with C cannot be produced. The only element which can be produced in the big bang is ^{4}He.

If all this is true, stars have a life cycle much like animals. They get born, they grow, they go through a definite internal development, and finally they die, to give back the material of which they are made so that new stars may live.

I am very grateful to Professor E. E. Salpeter for his extensive help in preparing this paper.

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Biography

Hans Albrecht Bethe was born in Strasbourg, Alsace-Lorraine, on July 2 1906. He attended the Gymnasium in Frankfurt from 1915 to 1924. He then studied at the University of Frankfurt for two years, and at Munich for two and one-halfyears, taking his Ph.D. in theoretical physics with Professor Arnold Sommerfeld in July 1928.

He then was an Instructor in physics at Frankfurt and at Stuttgart for one semester each. From fall 1929 to fall 1933 his headquarters were the University of Munich where he became Privatdozent in May 1930. During this time he had a travel fellowship of the International Education Board to go to Cambridge, England, in the fall of 1930, and to Rome in the spring terms of 1931 and 1932. In the winter semester of 1932-1933, he held a position as Acting Assistant Professor at the University of Tübingen which he lost due to the advent of the Nazi regime in Germany.

Bethe emigrated to England in October 1933 where he held a temporary position as Lecturer at the University of Manchester for the year 1933-1934, and a fellowship at the University of Bristol in the fall of 1934. In February 1935 he was appointed Assistant Professor at Cornell University, Ithaca, N. Y. U. S. A., then promoted to Professor in the summer of 1937. He has stayed there ever since, except for sabbatical leaves and for an absence during World War II. His war work took him first to the Radiation Laboratory at the Massachusetts Institute of Technology, working on microwave radar, and then to the Los Alamos Scientific Laboratory which was engaged in assembling the first atomic bomb. He returned to Los Alamos for half a year in 1952. Two of his sabbatical leaves were spent at Columbia University, one at the University of Cambridge, and one at CERN and Copenhagen.

Bethe's main work is concerned with the theory of atomic nuclei. Together with Peierls, he developed a theory of the deuteron in 1934 which he extended in 1949. He resolved some contradictions in the nuclear mass scale in 1935. He studied the theory of nuclear reactions in 1935-1938, predicting many reaction cross sections. In connection with this work, he developed Bohr's theory of the compound nucleus in a more quantitative fashion. This work and also the existing knowledge on nuclear theory and experimental results, was summarized in three articles in the *Reviews of Modern Physics* which for many years served as a textbook for nuclear physicists.

His work on nuclear reactions led Bethe to the discovery of the reactions which supply the energy in the stars. The most important nuclear reaction in the brilliant stars is the carbon-nitrogen cycle, while the sun and fainter stars use mostly the proton-proton reaction. Bethe's main achievement in this connection was the exclusion of other possible nuclear reactions. The Nobel Prize was given for this work, as well as his work on nuclear reactions in general.

In 1955 Bethe returned to the theory of nuclei, emphasizing a different phase. He has worked since then on the theory of nuclear matter whose aim it is to explain the properties of atomic nuclei in terms of the forces acting between nucleons.

Before his work on nuclear physics, Bethe's main attention was given to atomic physics and collision theory. On the former subject, he wrote a review article in *Handbuch der Physik* in which he filled in the gaps of the existing knowledge, and which is still up-to-date. In collision theory, he developed a simple and powerful theory of inelastic collisons between fast particles and atoms which he has used to determine the stopping power of matter for fast charged particles, thus providing a tool to nuclear physicists. Turning to more energetic collisions, he calculated with Heitler the Bremsstrahlung emitted by relativistic electrons, and the production of electron pairs by high energy gamma rays.

Bethe also did some work on solid-state theory. He discussed the splitting of atomic energy levels when an atom is inserted into a crystal, he did some work on the theory of metals, and especially he developed a theory of the order and disorder in alloys.

In 1947, Bethe was the first to explain the Lamb-shift in the hydrogen spectrum, and he thus laid the foundation for the modem development of quantum electrodynamics. Later on, he worked with a large number of collaborators on the scattering of pi mesons and on their production by electromagnetic radiation.

Bethe is married to the daughter of P. P. Ewald, the well- known X-ray physicist. They have two children, Henry and Monica.

Physics *1968*

LUIS W. ALVAREZ

« *for his decisive contributions to elementary particle physics, in particular the discovery of a large number of resonance states, made possible through his development of the technique of using* hydrogen *bubble chamber and data analysis* >>

Physics *1968*

Presentation Speech by Professor S. von Friesen, member of the Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

The science of physics has as its function the study of energy in all its forms. Einstein observed that matter, or mass, is one of the forms in which energy manifests itself. This fact was established experimentally 35 years ago, when it was discovered that high-energy electromagnetic radiation was capable of producing pairs of electrons, one with positive, the other with negative charges. It has since been possible to produce other similar pairs, for example protons and antiprotons. These newly-created particles are stable and, if left undisturbed, can exist indefinitely. Unstable particles can also be produced, however. These disintegrate rapidly into other particles and, passing through one or several stages, revert to stable forms or develop into other forms of energy. Many such new particles have been discovered and studied during the last two decades. They are so minute that it is impossible to see them; they can only be identified by the tracks they leave behind them as they move. The scientist must behave like the hunter, who determines the identity and behaviour of his quarry by studying tracks left in the snow.

The new particles are normally produced with the help of the great, new accelerators which cause the particles to move at very great speed. This has the advantage that, although the life-span of the particle might be as little as a ten-thousandth part of a millionth of a second, the track acquires a length of several centimetres.

One could, however, suspect the existence of particles with considerably shorter life-spans and with such small track-lengths that they are impossible to measure. In this case one is obliged, instead, to study the tracks of their disintegration products and the tracks of the reactions they produce in collision with other particles. The pattern of tracks thus becomes very complicated; the correct interpretation of what actually occurs requires acute powers of discernment and a particularly advanced experimental technique. It is in this field that Professor Luis Alvarez has made the contributions for which he is today being rewarded.

He has with insight and determination developed the bubble-chamber, invented by the Nobel Prize winner in Physics, Donald Glaser, into an invaluable instrument for this type of investigation. Alvarez' bubble-chamber contains many hundreds of litres of hydrogen, reduced to a temperature of minus 250 °C, which thus becomes fluid. When the particle passes through the liquid, it is warmed to boiling point along the track it leaves. In the wake are a trail of bubbles that can be photographed whilst still very small. The photographs are able, in this way, to reproduce accurately the path of the particle. Because the chamber contains only hydrogen, it is evident that all reactions must occur with hydrogen nuclei, protons. This considerably simplifies the interpretation of the phenomenon, The cost of this instrument, capable of producing about a million photographs annually, was two million dollars.

The photographs must be studied and measured with great accuracy. In order to carry out this enormous task, Alvarez and his assistants have constructed a series of more and more delicate automatic scanning and measuring instruments capable of transferring the information from the photographic film into a state suitable for treatment by computer. In this field, too, Alvarez has made contributions of a pioneering nature.

With the establishment of the hydrogen bubble-chamber, entirely new possibilities for research into high-energy physics present themselves. Results have already been apparent in the form of newly-discovered elementary particles. The first, very short-lived, so called, « resonance particle » was found in 1960. Since then there have been a whole series of discoveries made by Alvarez' group in Berkeley, California and in other laboratories where Alvarez' material is being used or where his methods and programs are adopted. Practically all the discoveries that have been made in this important field of high-energy physics have been possible only through the use of methods originated by Professor Alvarez.

Dr. Alvarez. Your contributions to physics are numerous and important. To-day our attention is focused on the outstanding discoveries which you have made in the field of high-energy physics as a result of your far-sighted and bold development of the hydrogen bubble-chamber into an instrument of great power and high precision and of the means of handling and analysing the large quantities of valuable information which it can produce.

On behalf of the Royal Swedish Academy of Sciences I extend to you our warm congratulations and now ask you to receive the Nobel Prize from the hands of His Majesty the King.

L U I S W . AL V A R E Z

Recent developments in particle physics

Nobel Lecture, December 11, 1968

When I received my B. S. degree in 1932, only two of the fundamental particles of physics were known. Every bit of matter in the universe was thought to consist solely of protons and electrons. But in that same year, the number of particles was suddenly doubled. In two beautiful experiments, Chadwick[1] showed that the neutron existed, and Anderson[2] photographed the first unmistakable positron track. In the years since 1932, the list of known particles has increased rapidly, but not steadily. The growth has instead been concentrated into a series of spurts of activity.

Following the traditions of this occasion, my task this afternoon is to describe the latest of these periods of discovery, and to tell you of the development of the tools and techniques that made it possible. Most of us who become experimental physicists do so for two reasons; we love the tools of physics because to us they have intrinsic beauty, and we dream of finding new secrets of nature as important and as exciting as those uncovered by our scientific heroes. But we walk a narrow path with pitfalls on either side. If we spend all our time developing equipment, we risk the appellation of « plumber », and if we merely use the tools developed by others, we risk the censure of our peers for being parasitic. For these reasons, my colleagues and I are grateful to the Royal Swedish Academy of Science for citing both aspects of our work at the Lawrence Radiation Laboratory at the University of California- the observations of a new group of particles and the creation of the means for making those observations.

As a personal opinion, I would suggest that modem particle physics started in the last days of World War II, when a group of young Italians, Conversi, Pancini, and Piccioni, who were hiding from the German occupying forces, initiated a remarkable experiment. In 1946, they showed[3] that the « mesotron » which had been discovered in 1937 by Neddermeyer and Anderson[4] and by Street and Stevensons, was not the particle predicted by Yukawa[6] as the mediator of nuclear forces, but was instead almost completely unreactive in a nuclear sense. Most nuclear physicists had spent the war years in military-related activities, secure in the belief that the Yukawa meson was available for study as soon as hostilities ceased. But they were wrong.

The physics community had to endure less than a year of this nightmarish state ; Powell and his collaborators7 discovered in 1947 a singly charged particle (now known as the pion) that fulfilled the Yukawa prediction, and that decayed into the « mesotron »,now known as the muon. Sanity was restored to particle physics, and the pion was found to be copiously produced in Ernest Lawrence's 184-inch cyclotron, by Gardner and Lattes[8] in 1948. The cosmic ray studies of Powell's group were made possible by the elegant nuclear-emulsion technique they developed in collaboration with the Ilford laboratories under the direction of C. Waller.

In 1950, the pion family was filled out with its neutral component by three independent experiments. In Berkeley, at the 184-inch cyclotron, Moyer, York *et al.*[9] measured a Doppler-shifted y-ray spectrum that could only be explained as arising from the decay of a neutral pion, and Steinberger, Panofsky and Steller[10] made the case for this particle even more convincing by a beautiful experiment using McMillan's new 300-MeV synchrotron. And independently at Bristol, Ekspong, Hooper and King[11] observed the two-*y* -ray decay of the π^0 in nuclear emulsion, and showed that its lifetime was less than $5 \cdot 10^{-14}$ sec.

In 1952 Anderson, Fermi and their collaborators'2 at Chicago started their classic experiments on the pion-nucleon interaction at what we would now call low energy. They used the external pion beams from the Chicago synchrocyclotron as a source of particles, and discovered what was for a long time called *the* pion-nucleon resonance. The isotopic spin formalism, which had been discussed for years by theorists since its enunciation in 1936 by Cassen and Condon[13], suddenly struck a responsive chord in the experimental physicscommunity. They were impressed by the way Brueckner[14] showed that « *I*- spin » invariance could explain certain ratios of reaction cross sections, if the resonance, which had been predicted many years earlier by Pauli and Dancoff [15], were in the 3/2 isotopic spin state, and had an angular momentum **of 3/2.**

By any test we can now apply, the « 3,3-resonance » of Anderson, Fermi *et al.* was the first of the « new particles » to be discovered. But since the rules for determining what constitutes a discovery in physics have never been codified-as they have been in patent law-it is probably fair to say that it was not customary, in the days when the properties of the 3,3 -resonance were of paramount importance to the high-energy physics community, to regard that resonance as a « particle ». Neutron spectroscopists study hundreds of resonances in neutron-nucleus systems which they do not regard as separate entities, even though their lives are billions of times as long. I don't believe that an early and general recognition that the 3,3 -resonance should be listed in the « table of particles » would in any way have speeded up the development of high-energy physics.

Although the study of the production and the interaction of pions had passed in a decisive way from the cosmic-ray groups to the accelerator laboratories in the late 1940's, the cosmic-ray-oriented physicsts soon found two new families of « strange particles »- the *K* mesons and the hyperons. The existence of the strange particles has had an enormous impact on the work done by our group at Berkeley. It is ironic that the parameters of the Bevatron were fixed and the decision to build that accelerator had been made before a single physicist in Berkeley really believed in the existence of strange particles. But as we look back on the evidence, it is obvious that the observations were well made, and the conclusions were properly drawn. Even if we had accepted the existence-and more pertinently the importance-of these particles, we would not have known what energy the Bevatron needed to produce strange particles; the associated production mechanism of Pais[16] and its experimental proof by Fowler, Shutt *et al.* [17] were still in the future. So the fact that, with a few notable exceptions, the Bevatron has made its greatest contributions to physics in the field of strange particles must be attributed to a very fortunate set of accidents.

The Bevatron's proton energy of 6.3 GeV was chosen so that it would be able to produce antiprotons, if such particles could be produced. Since, in the interest of keeping the « list of particles » tractable, we no longer count antiparticles nor individual members of *I*- spin multiplets, it is becoming fashionable to regard the discovery of the antiproton as an « obvious exercise for the student ». (If we were to apply the « new rules » to the classical work of Chadwick and Anderson, we would conclude that they hadn't done anything either-the neutron is simply another I-spin state of the proton, and Anderson's positron is simply the obvious antielectron!) In support of the non-obvious nature of the Segrè group's discovery of the antiproton[18] I need only recall that one of the most distinguished high-energy physicists I know, who didn't believe that antiprotons could be produced, was obliged to settle a 500-dollar bet with a colleague who held the now universally accepted belief that all particles can exist in an antistate.

I have just discussed in a very brief way the discovery of some particles that have been of importance in our bubble-chamber studies, and I will continue the discussion throughout my lecture. This account should not be taken to be authoritative-there is no authority in this area-but simply as a narrative to indicate the impact that certain experimental work had on my own thinking and on that of my colleagues.

I will now return to the story of the very important strange particles. In contrast to the discovery of the pion, which was accepted immediately by almost everyone-one apparent exception will be related later in this talk- the discovery and the eventual acceptance of the existence of the strange particles stretched out over a period of a few years. Heavy, unstable particles were first seen in 1947, by Rochester and Butler[19], who photographed and properly interpreted the first two « *V* particles » in a cosmic-ray- triggered cloud chamber. One of the V's was charged, and was probably a *K* meson. The other was neutral, and was probably a K^0. For having made these observations, Rochester and Butler are generally credited with the discovery of strange particles. There was a disturbing period of two years in which Rochester and Butler operated their chamber and no more *V* particles were found. But in 1950 Anderson, Leighton *et al.*[20] took a cloud chamber to a mountain top and showed that it was possible to observe approximately one *V* particle per day under such conditions. They reported, « To interpret these photographs, one must come to the same remarkable conclusion as that drawn by Rochester and Butler on the basis of these two photographs, *viz.*, that these two types of events represent, respectively, the spontaneous decay ofneutral and charged unstable particles of a new type. »

Butler and his collaborators then took their chamber to the Pic-du-Midi and confirmed the high event rate seen by the CalTech group on White Mountain. In 1952 they reported the first cascade decay[21]-now known as the Ξ^- hyperon.

While the cloud-chamber physicists were slowly making progress in understanding the strange particles, a parallel effort was under way in the nuclear emulsion-oriented laboratories. Although the first *K* meson was undoubtedly observed in Leprince-Ringuet's cloud chamber [22] in 1944, Bethe[23] cast sufficient doubt on its authenticity that it had no influence on the physics community and on the work that followed. The first overpowering evidence for a *K* meson appeared in nuclear emulsion, in an experiment by Brown and most of the Bristol group[24], in 1949. This so-called τ^+ meson decayed at rest into three coplanar pions. The measured ranges of the three pions gave a very accurate mass value for the τ meson of 493.6 MeV. Again there was a disturb -

inert, nuclearly. The suggestion by Marshak and Bethe[29] that it was the daughter of a strongly interacting particle was published almost simultaneously with the independent experimental demonstration by Powell *et al.*[7] mentioned earlier. Although invoking a similar mechanism to bring order into the strange-particle arena was tempting, Pais[16] made his suggestion that strange particles were produced « strongly » in pairs, but decayed « weakly » when separated from each other.

Gell-Mann[30] (and independently Nishijima[31]) then made the first of his several major contributions to particle physics by correctly guessing the rules that govern the production and decay of all the strange particles. I use the word « guessing » with the same sense of awe I feel when I say that Champollion guessed the meanings of the hieroglyphs on the Rosetta Stone. Gell-Mann had first to assume that the K meson was not an I-spin triplet, as it certainly appeared to bc, but an I-spin doublet plus its antiparticles, and he had further to assume the existence of the neutral Σ and of the neutral E. And finally, when he assigned appropriate values of his new quantum number, strangeness, to each family, his rules explained the one observed production reaction and predicted a score of others. And of course it explained all the known decays, and predicted another. My research group eventually confirmed all of Gell-Mann's and Nishijima's early predictions, many of them for the first time, and we continue to be impressed by their simple elegance.

This was the state of the art in particle physics in 1954, when William Brobeck turned his brainchild, the Bevatron, over to his Radiation Laboratory associates to use as a source of high-energy protons. I had been using the Berkeley proton linear accelerator in some studies of short-lived radioactive species, and I was pleased at the chance to switch to a field that appeared to be more interesting. My first Bevatron experiment was done in collaboration with Sula Goldhaber[32]; it gave the first real measurement of the τ meson lifetime. My next experiment was done with three talented young post-doctoral fellows, Frank S. Crawford Jr., Myron L. Good and M. Lynn Stevenson. An early puzzle in K-meson physics was that two of the particles (the q and τ) had similar, but poorly determined, lifetimes and masses. That story has been told in his auditorium by Lee[33] and Yang[34], so I won't repeat it now. But I do like to think that our demonstration[35], simultaneously with and independently from one by Fitch and Motley[36], that the two lifetimes were not measurably different, plus similar small limits on possible mass differences set by von Friesen *et al.*[37] and by Birge *et a1.*[38], nudged Lee and Yang a bit toward their revolutionary conclusion.

Our experiences with what was then a very complicated array of scintillation counters led me and my colleagues to despair of making meaningful measurements of what we perceived to be the basic reactions of strange particle physics :

$$\begin{array}{ccccccc} \pi^- & + & p & \rightarrow & \Lambda & + & K^0 \\ & & & & \downarrow & & \downarrow \\ & & & & p + \pi^- & & \pi^- + \pi^+ \end{array}$$

The production reaction is indicated by the horizontal arrows, the subsequent decays by the vertical arrows. Fig.1 shows a typical example of this reaction, as we saw it later in the 10-inch bubble chamber. We concluded, correctly I believe, that none of the then known techniques was well suited to study this reaction. Counters appeared hopelessly inadequate to the task, and the spark chamber had not yet been invented. The Brookhaven diffusion-cloud-chamber group[17] had photographed only a few events like that shown in Fig. 1, in a period of two years. It seemed to us that a track- recording technique was called for, but each of the three known track devices had drawbacks that ruled it out as a serious contender for the role we envisaged. Nuclear

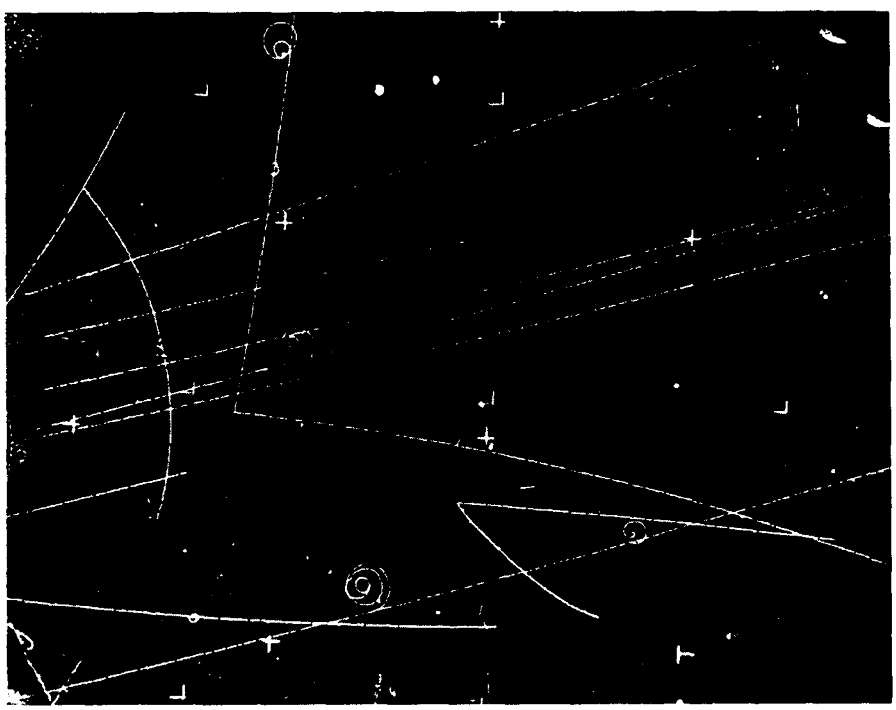

Fig.1. $\pi^- + p \rightarrow K^0 + \Lambda$.

emulsion, which had been so spectacularly successful in the hands of Powell's group, depended on the contiguous nature of the successive tracks at a production or decay vertex. The presence of neutral and therefore nonionizing particles between related charged particles, plus lack of even a rudimentary time resolution, made nuclear-emulsion techniques virtually unusable in this new field. The two known types of cloud chambers appeared to have equally insurmountable difficulties. The older Wilson expansion chamber had two difficulties that rendered it unsuitable for the job: if used at atmospheric pressure, its cycling period was measured in minutes, and if one increased its pressure to compensate for the long mean free path of nuclear interactions, its cycling period increased at least as fast as the pressure was increased. Therefore the number of observed reactions per day started at an almost impossibly low value, and dropped as « corrective action » was taken. The diffusion cloud chamber was plagued by « background problems », and had an additional disadvantage-its sensitive volume was confined in the vertical direction to a height of only a few centimeters. What we concluded from all this was simply that particle physicists needed a track-recording device with solid or liquid density (to increase the rate ofproduction of nuclear events by a factor of 100), with uniform sensitivity (to avoid the problems of the sensitive layer m the diffusion chamber), and with fast cycling time (to avoid the Wilson chamber problems). And of course, any cycling detector would permit the association of charged tracks joined by neutral tracks, which was denied to the user of nuclear emulsion.

In late April of 1953 I paid my annual visit to Washington, to attend the meeting of the American Physical Society. At luch on the first day, I found myself seated at a large table in the garden of the Shoreham Hotel. All the seats but one were occupied by old friends from World War I I days, and we reminisced about our experiences at the M. I. T. radar laboratory and at Los Alamos. A young chap who had not experienced those exciting days was seated at my left, and we were soon talking of our interests in physics. He expressed concern that no one would hear his 10-min contributed paper, because it was scheduled as the final paper of the Saturday afternoon session, and therefore the last talk to be presented at the meeting. In those days of slow airplanes, there were even fewer people in the audience for the last paper of the meeting than there are now -if that is possible. I admitted that I wouldn't be there, and asked him to tell me what he would be reporting. And that is how I heard first hand from Donald Glaser how he had invented the bubble chamber, and to what state he had brought its development. And of course he has since described those achievements from this platform[39]. He showed me photographs of bubble tracks in a small glass bulb, about 1 centimeter in diameter and 2 centimeters long, filled with diethyl ether. He stressed the need for absolute cleanliness of the glass bulb, and said that he could maintain the ether in a superheated state for an average of many seconds before spontaneous boiling took place. I was greatly impressed by his work, and it immediately occurred to me that this could be the « big idea » I felt was needed in particle physics.

That night in my hotel room I discussed what I had learned with my colleague from Berkeley, Frank Crawford. I told Frank that I hoped we could get started on the development of a liquid hydrogen chamber, much larger than anything Don Glaser was thinking about, as soon as I returned to Berkeley. He volunteered to stop off in Michigan on the way back to Berkeley, which he did, and learned everything he could about Glaser's technique.

I returned to Berkeley on Sunday, May 1, and on the next day Lynn Stevenson started to keep a new notebook on bubble chambers. The other day, when he saw me writing this talk, he showed me that old notebook with its first entry dated May 2, 1953, with Van der Waals' equation on the first page, and the isotherms of hydrogen traced by hand onto the second page. Frank Crawford came home a few days later, and he and Lynn moved into the « student shop » in the synchrotron building, to build their first bubble chamber. They were fortunate in enlisting the help of John Wood, who was an accelerator technician at the synchrotron. The three of them put their first efforts into a duplication of Glaser's work with hydrocarbons. When they had demonstrated radiation sensitivity in ether, they built a glass chamber in a Dewar flask to try first with liquid nitrogen and then with liquid hydrogen.

I remember that on several occasions I telephoned to the late Earl Long at the University of Chicago, for advice on cryogenic problems. Dr. Long gave active support to the liquid hydrogen bubble chamber that was being built at that time by Roger Hildebrand and Darragh Nagle at the Fermi Institute in Chicago. In August of 1953 Hildebrand and Nagle[40] showed that superheated hydrogen boiled faster in the presence of a y-ray source than it did when the source was removed. This is a necessary (though not sufficient) condition for successful operation of a liquid hydrogen bubble chamber, and the Chicago work was therefore an important step in the development of such chambers. The important unanswered question concerned the bubble density-was it sufficient to see tracks of « minimum ionizing » particles, or did liquid hydrogen (as my colleagues had just shown that liquid nitrogen did) produce bubbles but no visible tracks?

John Wood[41] saw the first tracks in a 1.5 -inch- diameter liquid hydrogen bubble chamber in February of 1954. The Chicago group could certainly have done so earlier, by rebuilding their apparatus, but they switched their efforts to hydrocarbon chambers, and were rewarded by being the first physicists to publish experimental results obtained by bubble chamber techniques. Fig. 2 is a photograph of Wood's first tracks.

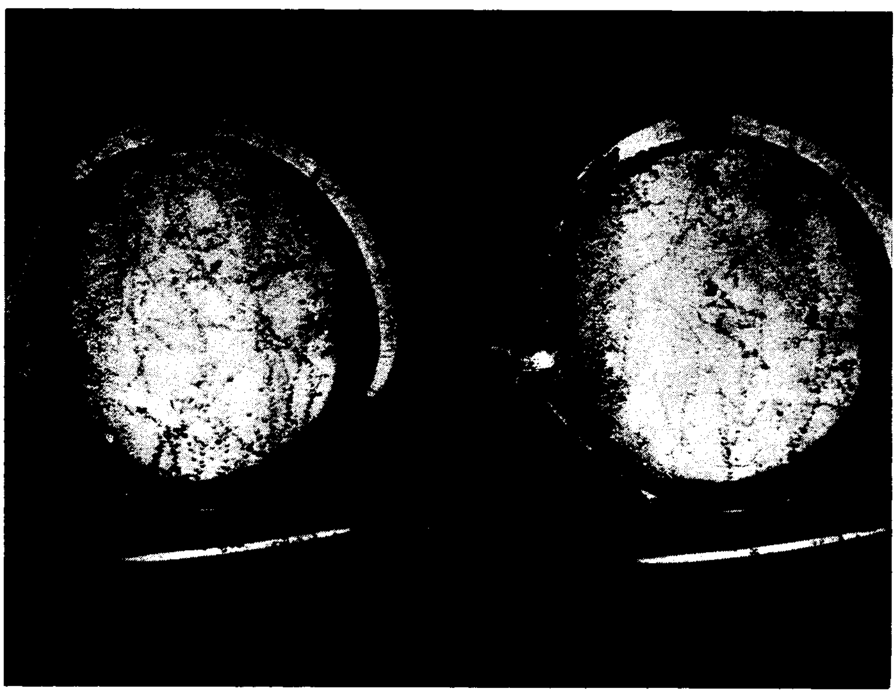

Fig. 2. First tracks in hydrogen.

At the Lawrence Radiation Laboratory, we have long had a tradition of close cooperation between physicists and technicians. The resulting atmosphere, which contributed so markedly to the rapid development of the liquid hydrogen bubble chamber, led to an unusual phenomenon : none of the scientific papers on the development ofbubble-chamber techniques in my research group were signed by experimenters who were trained as physicists or who had had previous cryogenic experience. The papers all had authors who were listed on the Laboratory records as technicians, but of course the physicists concerned knew what was going on, and offered many suggestions. Nonetheless, our technical associates carried the main responsibility, and published their findings in the scientific literature. I believe this is a healthy change from practices that were common a generation ago; we all remember papers signed by a single physicist that ended with a paragraph saying, « I wish to thank Mr. . . . . . . , who built the apparatus and took much of the data ».

And speaking of acknowledgments, John Wood's first publication, in addition to thanking Crawford, Stevenson, and me for our advice and help, said, « I am indebted to A. J. Schwemin for help with the electronic circuits. » « Pete » Schwemin, the most versatile technician I have ever known, became so excited by his initial contact with John Wood's 1.5 -inch- diameter all- glass chamber that he immediately started the construction of the first metal bubble chamber with glass windows. All earlier chambers had been made completely of smooth glass, without joints, to prevent accidental boiling at sharp points; such boiling of course destroyed the superheat and made the chamber insensitive to radiation. Both Glaser and Hildebrand stressed the long times their liquids could be held in the superheated condition; Hildebrand and Nagle averaged 22 sec, and observed one superheat period of 70 sec. John Wood[41] reported, « We were discouraged by our inability to attain the long times of superheat, until the track photographs showed that it was not important in the successful operation of a large bubble chamber. » I have always felt that second to Glaser's discovery of tracks this was the key observation in the whole development of bubble-chamber technique. As long as one ((expanded the chamber » rapidly, bubbles forming on the wall didn't destroy the superheated condition of the main volume of the liquid, and it remained sensitive as a track-recording medium.

Pete Schwemin, with the help of Douglas Parmentier[42], built the 2.5 -inch-diameter hydrogen chamber in record time, as the world's first « dirty chamber ». I've never liked that expression, but it was used for a while to distinguish chambers with windows gasketed to metal bodies from all-glass chambers. Because of its « dirtiness », the 2.5-inch chamber boiled at its walls, but still showed good tracks throughout its volume. Now that « clean » chambers are of historical interest only, we can be pleased that the modern chambers need no longer be stigmatized by the adjective « dirty ».

Lynn Stevenson's notebook shows a diagram of John Wood's chamber dated January 25, 1954, with Polaroid pictures of tracks in hydrogen. A month later he recorded details of Schwemin's 2.5 -inch chamber, and drew a complete diagram dated March 5. (That was the day after the *Physical Review* received Wood's letter announcing the first observation of tracks.)

On April 29, Schwemin and Parmentier photographed their first tracks; these are shown in Fig. 3. (Things were happening so fast at this time that the 2.5-inch system was never photographed as a whole before it ended up on the scrap pile.)

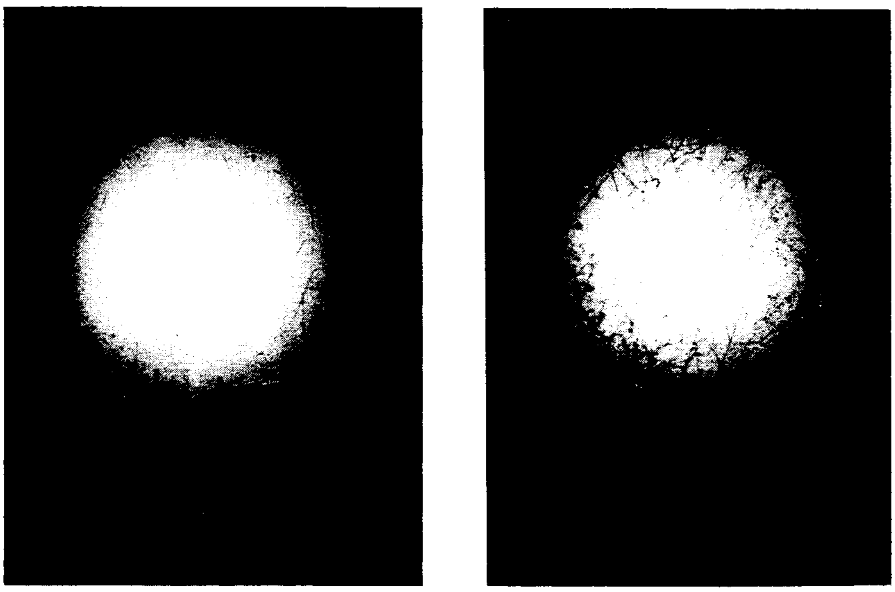

Fig. 3. Tracks in 2.5 -inch chamber: neutrons (left) ; y-rays (right).

In August, Schwemin and Parmentier separately built two different 4-inch-diameter chambers. Both were originally expanded by internal bellows, and Parmentier's 4-inch chamber gave tracks on October 6. Schwemin's chamber produced tracks three weeks later, and survived as *the* 4-inch chamber (see Fig. 4). The bellows systems in both chambers failed, but it turned out to be easier to convert Schwemin's chamber to the vapor expansion system that was used in all our subsequent chambers until 1962. (In that year, the 25-inch chamber introduced the « Ω -bellows » that is now standard for large chambers.)

Fig. 5 shows all our chambers displayed together a few weeks ago, at the request of Swedish Television. As you can see, we all look pretty pleased to see so many of our « old friends » side by side for the first time.

Fig. 6 shows an early picture of multiple meson production in the 4-inch chamber. This chamber was soon equipped with a pulsed magnetic field, and in that configuration it was the first bubble chamber of any kind to show

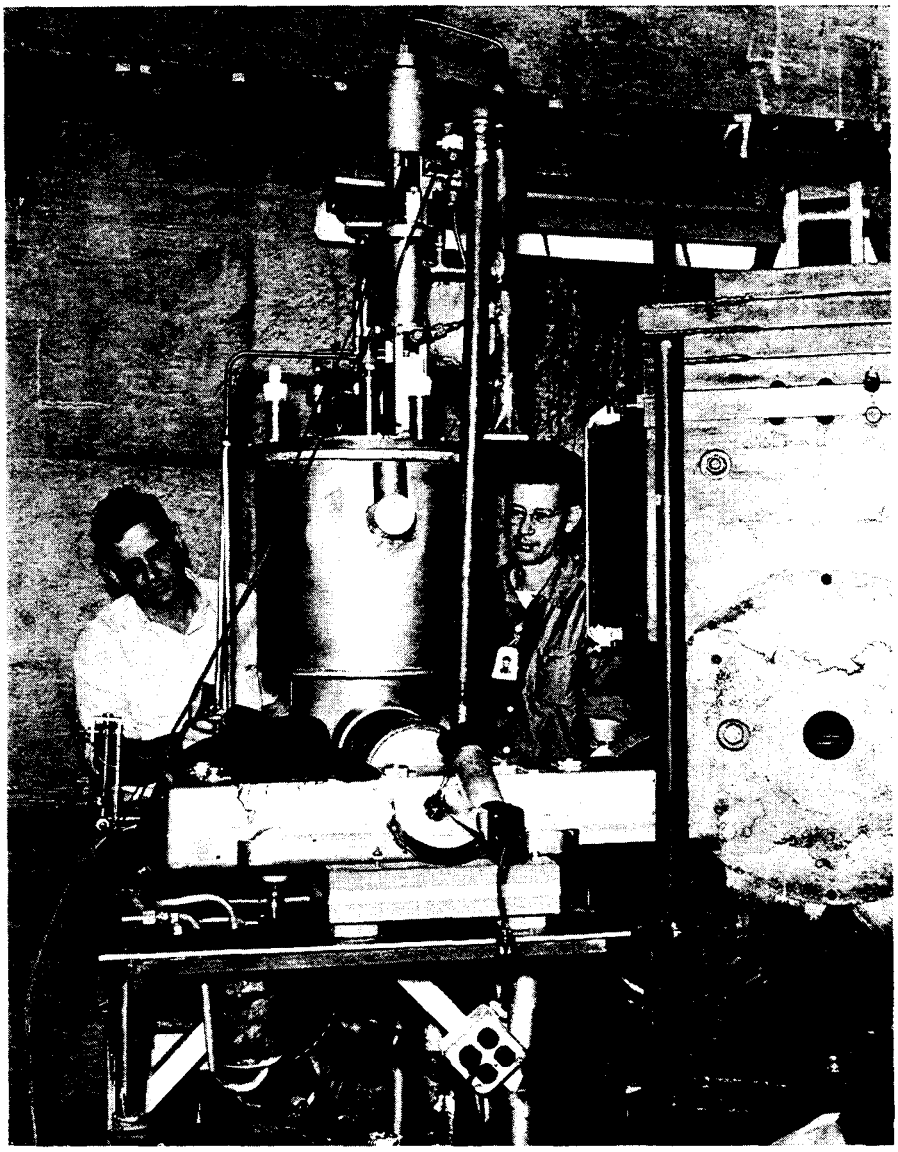

Fig. 4. The 4-inch chamber. D.Parmentier (left), A. J. Schwemin (right).

magnetically curved tracks. It was then set aside by our group as we pushed on to larger chambers. But it ended its career as a useful research tool at the Berkeley electron synchrotron, after almost two million photographs of 300-MeV Bremsstrahlung passing through it had been taken and analyzed by Bob Kenney *et al.*[43].

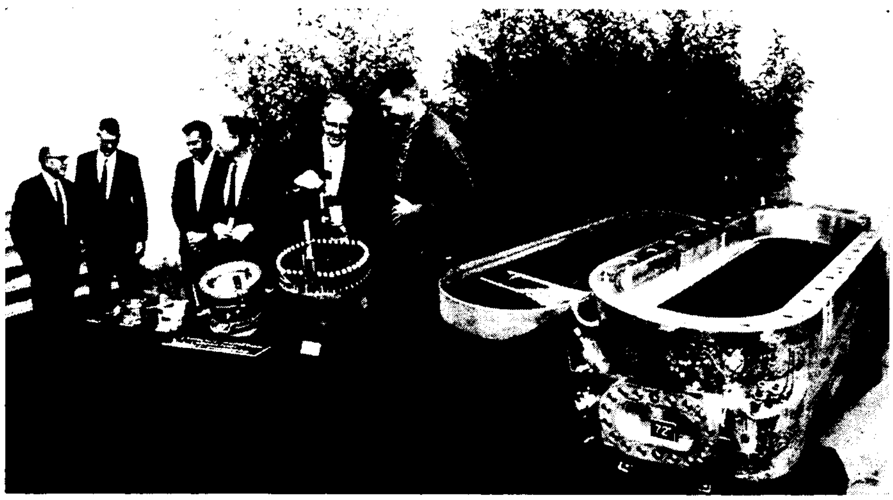

Fig. 5. Display ofchambers, November 1968. From left to right, 1.5, 4, 6, 10, 15 and 72 inch chambers ; Hernandez, Schwemin, Rinta, Watt, Alvarez and Eckman.

In the year 1954, as I have just recounted, various members of my research group had been responsible for the successful operation of four separate liquid hydrogen bubble chambers, increasing in diameter from 1.5 inches to 4 inches. By the end of that eventful year, it was clear that it would take a more concerted engineering- type approach to the problem if we were to progress to the larger chambers we felt were essential to the solution of high-energy physics problems. I therefore enlisted the assistance of three close associates, J. Donald Gow, Robert Watt and Richard Blumberg. Don Gow and Bob Watt had taken over full responsibility for the development and operation of the 32-MeV linear accelerator that had occupied all my attention from its inception late in 1945 until it first operated in late 1947. Neither of them had any experience with cryogenic techniques, but they learned rapidly, and were soon leaders in the new technology of hydrogen-bubble chambers. Dick Blumberg had been trained as a mechanical engineer, and he had designed the equipment used by Crawford, Stevenson and me in our experiments, then in progress, on the Compton scattering of *y* -rays by protons[44].

Wilson Powell had built two large magnets to accommodate his Wilson Cloud Chambers, pictures from which adorned the walls of every cyclotron laboratory in the world. He very generously placed one of these magnets at our disposal, and Dick Blumberg immediately started the mechanical design of the 10-inch chamber - the largest size we felt could be accommodated in the well of Powell's magnet. Blumberg's drafting table was in the middle of the single room that contained the desks of all the members of my research group. Not many engineers will tolerate such working conditions, but Blumberg was able to do so and he produced a design that was quickly built in the main machine shop. All earlier chambers had been built by the experimenters themselves. The design of the 10-inch chamber turned out to be a much larger job than we had foreseen. By the time it was completed, eleven members of the Laboratory's Mechanical Engineering Department had worked on it, including Rod Byrns and John Mark. The electrical engineering aspects of all our large chambers were formidable, and we are indebted to Jim Shand for his leadership in this work for many years.

Great difficulty was experienced with the first operation of the IO-inch

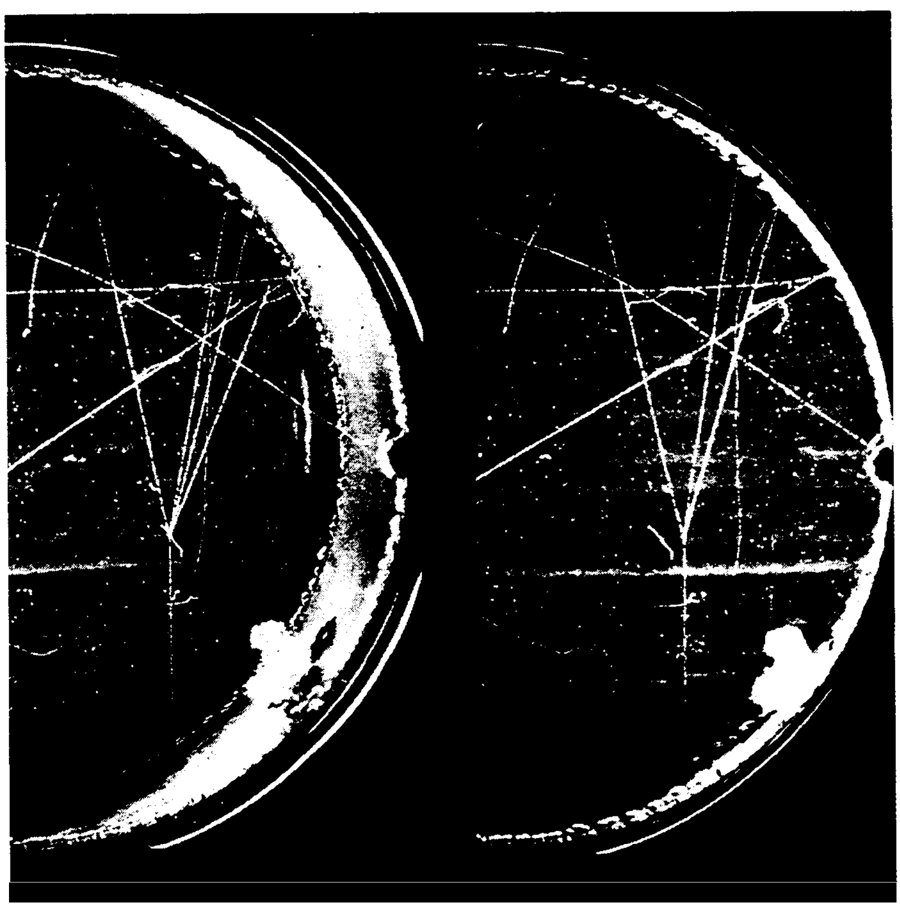

Fig. 6. Multiple meson production in 4-inch chamber.

chamber: too much hydrogen was vaporized at each « expansion ». Pete Schwemin quickly diagnosed the trouble and built a fast-acting valve that permitted the chamber to be pulsed every 6 sec, to match the Bevatron's cycling time.

It would be appropriate to interrupt this description of the bubble-chamber development program to describe the important observations made possible by the operation of the 10-inch chamber early in 1956, but instead, I will preserve the continuity by describing the further development of the hardware. In December of 1954, shortly after the 4-inch chamber had been operated in the cyclotron building for the first time, it became evident to me that the 10-inch chamber we had just started to design wouldn't be nearly large enough to tell us what we wanted to know about the strange particles. The tracks of these objects had been photographed at Brookhaven[17], and we knew they were produced copiously by the Bevatron.

The size of the « big chamber » was set by several different criteria, and fortunately all of them could be satisfied by one design. (Too often, a designer of new equipment finds that one essential criterion can be met only if the objects is very large, while an equally important criterion demands that it be very small.) All « dirty chambers » so far built throughout the world had been cylindrical in shape, and were characterized by their diameter measurement. By studying the relativistic kinematics of strange particles produced by Bevatron beams, and more particularly by studying the decay of these particles, I convinced myself that the big chamber should be rectangular, with a length of at least 30 inches. This length was next increased to 50 inches in order that there would be adequate amounts of hydrogen upstream from the required decay region, in which production reactions could take place. Later the length was changed to 72 inches, when it was realized that the depth of the chamber could properly be less than its width and that the change could be made without altering the volume. The production region corresponded to about 10% of a typical pion-proton mean free path, and the size of the decay region was set by the relativistic time-dilated decay lengths of the strange particles, plus the requirement that there be a sufficient track length available in which to measure magnetic curvature in a « practical magnetic field » of 15 000 gauss. In summary, then, the width and depth of the chamber came rather simply from an examination of the shape of the ellipses that characterize relativistic transformations at Bevatron energies, plus the fact that the magnetic field spreads the particles across the width but not along the depth of the chamber.

The result of this straightforward analysis was a rather frightening set of numbers: The chamber length was 72 inches; its width was 20 inches, and its depth was 15 inches. It had to be pervaded by a magnetic field of 15 000 gauss, so its magnet would weigh at least 100 tons and would require 2 or 3 megawatts to energize it. It would require a window 75 inches long by 23 inches wide and 5 inches thick to withstand the (deuterium) operating pressure of 8 atmospheres, exerting force of 100 tons on the glass. No one had any experience with such large volumes of liquid hydrogen; the hydrogen-oxygen rocket engines that now power the upper stages of the Saturn boosters were still gleams in the eyes of their designers- these were pre- Sputnik days. The safety aspects of the big chamber were particularly worrisome. Low temperature laboratories had a reputation for being dangerous places in which to work, and they didn't deal with such large quantities of liquid hydrogen, and what supplies they did use were kept at atmospheric pressure.

For some time, the glass-window problem seemed insurmountable - no one had ever cast and polished such a large piece of optical glass. Fortunately for the eventual success of the project, I was able to persuade myself that the chamber body could be constructed of a transparent plastic cylinder with metallic end plates. This notion was later demolished by my engineering colleagues, but it played an important role in keeping the project alive in my own mind until I was convinced that the glass window could be built. As an indication of the cryogenic ,« state of the art » at the time we worried about the big window, I can recall the following anecdote. One day, while looking through a list of titles of talks at a recent cryogenic conference, I spotted one that read, « Large glass window for viewing liquid hydrogen.» Eagerly I turned to the paper-but it described a metallic Dewar vessel equipped with a glass window 1 inch in diameter !

Don Gow was now devoting all his time to hydrogen bubble chambers, and in January of 1955 we interested Paul Hernandez in taking a good hard engineering look at the problems involved in building and housing the 72-inch bubble chamber. We were also extremely fortunate in being able to interest the cryogenic engineers at the Boulder, Colorado, branch of the National Bureau of Standards in the project. Dudley Chelton, Bascomb Birmingham and Doug Mann spent a great deal of time with us, first educating us in large-scale liquid-hydrogen techniques, and later cooperating with us in the design and initial operation of the big chamber.

In April of 1955, after several months of discussion of the large chamber, I wrote a document entitled « The Bubble Chamber Program at UCRL ».

This paper showed in some detail why it was important to build the large chamber, and outlined a whole new way of doing high-energy physics with such a device. It stressed the need for semiautomatic measuring devices (which had not previously been proposed), and described how electronic computers would reconstruct tracks in space, compute momenta, and solve problems in relativistic mechanics. All these techniques are now part of the ((standard bubble-chamber method)), but in April of 1955 no one had yet applied them. Of all the papers I have written in my life, none gives me so much satisfaction on rereading as does this unpublished prospectus.

After Paul Hernandez and Don Gow had estimated that the big chamber, including its building and power supplies, would cost about 2.5 million dollars, it was clear that a special AEC appropriation was required; we could no longer build our chambers out of ordinary laboratory operating money. In fact, the document I've just described was written as a sort of proposal to the AEC for financial support-but without mentioning money! I asked Ernest Lawrence if he would help me in requesting extra funds from the AEC . He read the document, and agreed with the points I had made. He then asked me to remind him of the size of the world's largest hydrogen chamber. When I replied that it was 4 inches in diameter, he said he thought I was making too large an extrapolation in one step, to 72 inches. I told him that the 10-inch chamber was on the drawing board, and if we could make it work, the operation of the 72-inch chamber was assured. (And if we couldn't make it work, we could refund most of the 2.5 million.) This wasn't obvious until I explained the hydraulic aspects of the expansion system of the 72-inch chamber; it was arranged so that the 20-inch wide, 72 -inch long chamber could be considered to be a large collection of essentially independently expanded 10- inch square chambers. He wasn't convinced of the wisdom of the program, but in a characteristic gesture, he said, « I don't believe in your big chamber, but I do believe in you, and I'11 help you to obtain the money. » I therefore accompanied him on his next trip to Washington, and we talked in one day to three of the five Commissioners: Lewis Strauss, Willard Libby (who later spoke from this podium), and the late John Von Neumann, the greatest mathematical physicist then living. That evening, at a cocktail party at Johnny Von Neumann's home, I was told that the Commission had voted that afternoon to give the laboratory the 2.5 million dollars we had requested. All we had to do now was build the thing and make it work!

Design work had of course been under way for some time, but it was now rapidly accelerated. Don Gow assumed a new role that is not common in ing period of more than a year and a half before another τ meson showed up.

In 1951, the year after the τ meson and the I/particles were finally seen again, O'Ceallaigh[25] observed the first of his kappa mesons in nuclear emulsion. Each such event involved the decay at rest of a heavy meson into a muon with a different energy. We now know these particles as $K+$ mesons decaying into $\mu^+\pi^0+\nu$, so the explanation of the broad muon energy spectrum is now obvious. But it took some time to understand this in the early 1950's, when these particles appeared one by one in different laboratories. In 1953, Menon and O'Ceallaigh[26] found the first $K_{\pi 2}$ or θ meson, with a decay into $\pi^++\pi^0$. The identification of the θ and τ mesons as different decay modes of the same K meson is one of the great stories of particle physics, and it will be mentioned later in this lecture.

The identification of the neutral Λ emerged from the combined efforts of the cosmic-ray cloud-chambers groups, so I won't attempt to assign credit for its discovery. But it does seem clear that Thompson *et al.*[27] were the first to establish the decay scheme of what we now know as the K_{I}^0 meson : $K_{\mathrm{I}}^0 \rightarrow \pi^+ + \pi^-$. The first example of a charged Σ hyperon was seen in emulsion by the Genoa and Milan groups[28], in 1953. And after that, the study of strange particles passed, to a large extent, from the cosmic-ray groups to the accelerator laboratories.

So by the time the Bevatron first operated, in 1954, a number of different strange particles had been identified: several charged particles and a neutral one all with masses in the neighborhood of 500 MeV, and three kinds of particles heavier than the proton. In order of increasing mass, these were the neutral Λ, the two charged Σ's (plus and minus), and the negative cascade (E-), which decayed into a Λ and a negative pion.

The strange particles all had lifetimes shorter than any known particles except the neutral pion. The hyperons all had lifetimes of approximately 10^{-10} sec, or less than 1% of the charged pion lifetime. When I say that they were called strange particles because their observed lifetimes presented such a puzzle for theoretical physicists to explain, I can imagine the lay members in this audience saying to themselves, « Yes, I can't see how anything could come apart so fast. » But the strangeness of the strange particles is not that they decay so rapidly, but that they last almost a million million times longer than they should-physicists couldn't explain why they didn't come apart in about 10^{-21} sec.

I won't go into the details of the dilemma, but we can note that a similar problem faced the physics community when the muon was found to be so physics laboratories, but is well known in military organizations ; he became my « chief of staff ». In this position, he coordinated the efforts of the physicists and engineers; he had full responsibility for the careful spending of our precious 2.5 million dollars, and he undertook to become an expert second to none in all the technical phases of the operation, from low- temperature thermodynamics to safety engineering. His success in this difficult task can be recognized most easily in the success of the whole program, culminating in the fact that I am speaking here this afternoon. I am sorry that Don Gow can't be here today; he died several years ago, but I am reminded of him every day-my three-year-old son is named Donald in his memory.

The engineering team under Paul Hernandez's direction proceeded rapidly with the design, and in the process solved a number of difficult problems in ways that have become standard « in the industry ». A typical problem involved the very considerable differential expansion between the stainless steel chamber and the glass window. This could be lived with in the 10-inch chamber, but not in the 72-inch. Jack Franck's « inflatable gasket » allowed the glass to be seated against the chamber body only after both had been cooled to liquid hydrogen temperature.

Just before leaving for Stockholm, I attended a ceremony at which Paul Hernandez was presented with a trophy honoring him as a « Master Designer » for his achievements in the engineering of the 72-inch chamber. I had the pleasure of telling in more detail than I can today of his many contributions to the success of our program. One of his associates recalled a special service that he rendered not only to our group but to all those who followed us in building liquid hydrogen-bubble chambers. Hernandez and his associates wrote a series of ((Engineering Notes », on matters ofinterest to designers of hydrogen-bubble chambers, that soon filled a series of notebooks that spanned 3 feet of shelf space. Copies of these were sent to all interested parties on both sides of the Atlantic, and I am sure that they resulted in a cumulative savings to all bubble-chamber builders ofseveral million dollars; had not all this information been readily available, the test programs and calculations of our engineering group would have required duplication at many laboratories, at a large expense of money and time. Our program moved so rapidly that there was never time to put the Engineering Notes into finished form for publication in the regular literature. For this reason, one can now read review articles on bubble-chamber technology, and be quite unaware of the part that our Laboratory played in its development. There are no references to papers by members of our group, since those papers were never written -the data that would have been in them had been made available to everyone who needed them at a much earlier date.

And just to show that I was also deeply involved in the chamber design, I might recount how I purposely « designed myself into a corner » because I thought the results were important, and I thought I could invent a way out of a severe difficulty, if given the time. All previous chambers had had two windows, with « straight through » illumination. Such a configuration reduces the attainable magnetic field, because the existence of a rear pole piece would interfere with the light-projection system. I made the decision that the 72-inch chamber would have only a top window, thereby permitting the magnetic field to be increased by a lower pole piece and at the same time saving the cost of the extra glass window, and also providing added safety by eliminating the possibility that liquid hydrogen could spill through a broken lower window. The only difficulty was that for more than a year, as the design was firmed up and the parts were fabricated, none of us could invent a way both to illuminate and to photograph the bubbles through the same window. Duane Norgren, who has been responsible for the design of all our bubble-chamber cameras, discussed the matter with me at least once a week in that critical year, and we tried dozens of schemes that didn't quite do the job. But as a result of our many failures, we finally came to understand all the problems, and we eventually hit on the retrodirecting system known as coat hangers. This solution came none too soon; if it had been delayed by a month or more, the initial operation of the 72-inch chamber would have been correspondingly delayed. We took many other calculated risks in designing the system; if we had postponed the fabrication of the major hardware until we had solved all the problems on paper, the project might still not be completed. Engineers are conservative people by nature; it is the ultimate disgrace to have a boiler explode or a bridge collapse. We were therefore fortunate to have Paul Hernandez as our chief engineer; he would seriously consider anything his physics colleagues might suggest, no matter how outlandish it might seem at first sight. He would firmly reject it if it couldn't be made safe, but before rejecting any idea for lack of safety he would use all the ingenuity he possessed to make it safe.

We felt that we needed to built a test chamber to gain experience with a single- window system, and to learn to operate with a hydrogen refrigerator ; our earlier chambers had all used liquid hydrogen as a coolant. We therefore built and operated the 15 -inch chamber in the Powell magnet, in place of the 10-inch chamber that had served us so well.

The 72-inch chamber operated for the first time on March 24, 1959, very nearly four years from the time it was first seriously proposed. Fig. 7 shows it at about that time. The « start- up team » consisted of Don Gow, Paul Hernandez and Bob Watt, all of whom had played key roles in the initial operation

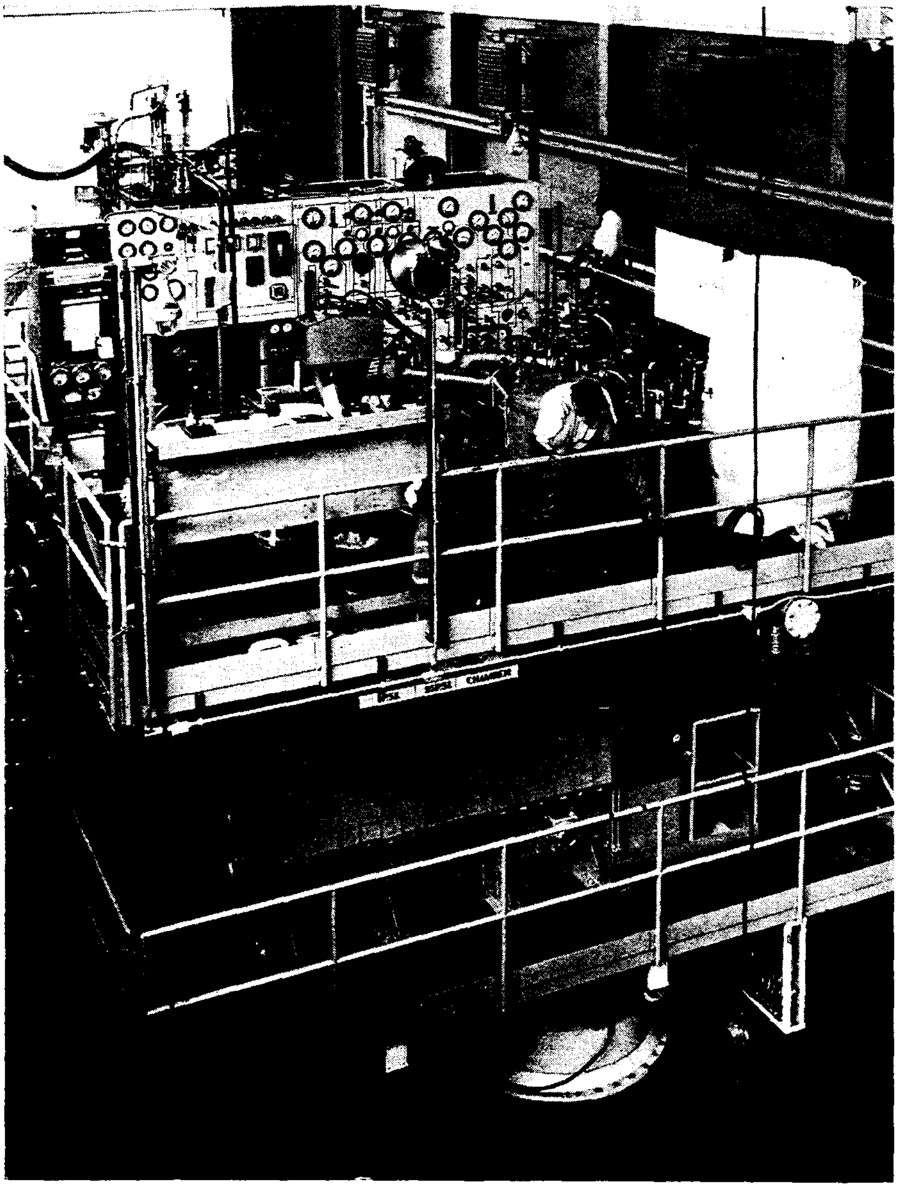

Fig. 7. The 72-inch bubble chamber in its building.

of the 15 -inch chamber. Bob Watt and Glenn Eckman have been responsible for the operation of all our chambers from the earliest days of the 10-inch chamber, and the success of the whole program has most often rested in their hands. They have maintained an absolutely safe operating record in the face of very severe hazards, and they have supplied their colleagues in the physics community with approximately ten million high-quality stereo photographs. And most recently, they have shown that they can design chambers as well as they have operated them. The 72-inch chamber was recently enlarged to an 82-inch size, incorporating to a large extent the design concepts of Watt and Eckman.

Although I haven't done justice to the contributions of many close friends and associates who shared in our bubble-chamber development program, I must now turn to another important phase of our activities-the data-analysis program. Soon after my 1955 prospectus was finished, Hugh Bradner undertook to implement the semiautomatic measuring machine proposal. He first made an exhaustive study of commercially available measuring machines, encoding techniques, etc., and then, with Jack Franck, designed the first « Franckenstein ». This rather revolutionary device had been widely copied, to such an extent that objects of its kind are now called « conventional » measuring

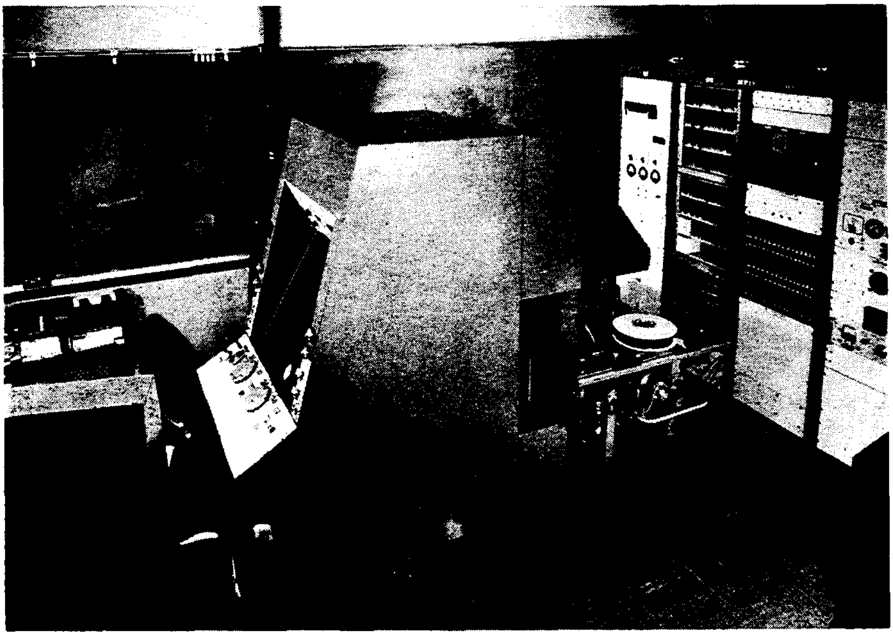

Fig. 8. « Franckenstein ».

machines (Fig. 8). Our first Franckenstein was operating reliably in 1957, and in the summer of 1958 a duplicate was installed in the U. S. exhibit at the « Atoms for Peace » exposition in Geneva. It excited a great deal of interest in the high-energy physics community, and a number of groups set out to make similar machines based on its design. Almost everyone thought at first that our provision for automatic track following was a needless waste of money, but over the years, that feature has also come to be « conventional ».

Jack Franck then went on to design the Mark II Franckenstein, to measure 72-inch bubble-chamber film. He had the first one ready to operate just in time to match the rapid turn-on of the big chamber, and he eventually built three more of the Mark II's. Other members of our group then designed and perfected the faster and less expensive S MP system, which added significantly to our « measuring power ». The moving forces in this development were Pete Schwemin, Bob Hulsizer, Peter Davey, Ron Ross and Bill Humphrey[45]. Our final and most rewarding effort to improve our measuring ability was fulfilled several years ago, when our first Spiral Reader became operational. This single machine has now measured more than one and a half million high-energy interactions, and has, together with its almost identical twin, measured one and a quarter million events in the last year. The SAAB Company here in Sweden is now building and selling Spiral Readers to European laboratories.

The Spiral Reader had a rather checkered career, and it was on several occasions believed by most workers in the field to have been abandoned by our group. The basic concept of the spiral scan was supplied by Bruce McCormick, in 1956. Our attempts to reduce his ideas to practice resulted in failure, and shortly after that, McCormick moved to Illinois, where he has since been engaged in computer development. As the cost of transistorized circuits dropped rapidly in the next years, we tried a second time to implement the Spiral Reader concept, using digital techniques to replace the analog devices of the earlier machine. The second device showed promise, but its ((hard-wired logic » made it too inflexible, and the unreliability of its electronic components kept it undergoing repair most of the time. The mechanical and optical components of the second Spiral Reader were excellent, and we hated to drop the whole project simply because the circuitry didn't come up to the same standard. In 1963, Jack Lloyd suggested that we use one of the new breed of small high-speed, inexpensive computers to supply the logic and the control circuits for the Spiral Reader. He then demonstrated great qualities of leadership by delivering to our research group a machine that has performed even better than he had promised it would. In addition to his development of the hardware, he initiated POOH, the Spiral Reader filtering program, which was brought to a high degree of perfection by Jim Burkhard. The smooth and rapid transition of the Spiral Reader from a developmental stage into a useful operational tool was largely the result of several years of hard work on the part of Gerry Lynch and Frank Solmitz. Fig. 9, from a talk I gave two and a half years ago[46], shows how the measuring power of our group has increased over the years, with only a modest increase in personnel.

According to a simple extrapolation of the exponential curve we had been

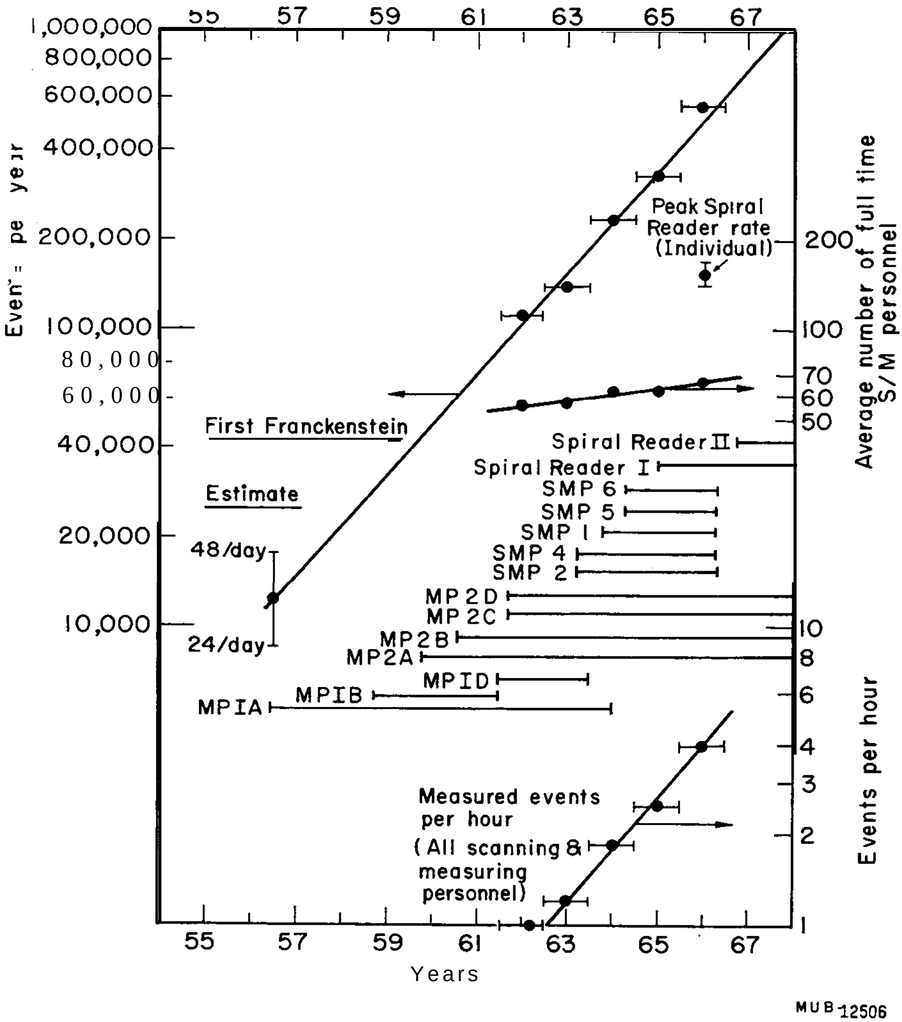

Fig. 9. Measuring rates.

on from 1957 through 1966, we would expect to be measuring 1.5 million events per year some time in 1969. But we have already reached that rate and we will soon be leveling off about there because we have stopped our development work in this area.

The third key ingredient of our development program has been the continually increasing sophistication in our utilization of computers, as they have increased in computational speed and memory capacity. While I can speak from a direct involvement in the development of bubble chambers and measuring machines, and in the physics done with those tools, my relationship to our computer programming efforts is largely that of an amazed spectator. We were most fortunate that in 1956 Frank Solmitz elected to join our group. Although the rest of the group thought of themselves as experimental physicists, Solmitz had been trained as a theorist, and had shown great aptitude in the development of statistical methods of evaluating experimental data. When he saw that our first Franckenstein was about to operate, and no computer programs were ready to handle the data it would generate, he immediately set out to remedy the situation. He wrote HYDRO, our first system program for use on the IBM 650 computer. In the succeeding twelve years he has continued to carry the heavy responsibility for all our programming efforts. A major breakthrough in the analysis of bubble-chamber events was made in the years 1957 through 1959. In this period, Solmitz and Art Rosenfeld, together with Horace Taft from Yale University and Jim Snyder from Illinois, wrote the first « fitting routine », GUTS, which was the core of our first ((kinematics program)), KICK. To explain what KICK did, it is easiest to describe what physicists had to do before it was written. HY D R O and its successor, PANG, listed for each vertex the momentum and space angles of the tracks entering or leaving that vertex, together with the calculated errors in these measurements. A physicist would plot the angular coordinates on a stereographic projection of a unit sphere known as a Wolff-plot. If he was dealing with a three-track vertex-and that was all we could handle in those days-he would move the points on the sphere, within their errors, if possible, to make them coplanar. And of course he would simultaneously change the momentum values, within their errors, to insure that the momentum vector triangle closed, and energy was conserved. Since momentum is a vector quantity, the various conditions could be simultaneously satisfied only after the angles and the absolute values of the momenta had been changed a number of times in an iterative procedure. The end result was a more reliable set of momenta and angles, constrained to fit the conservation laws of energy and momentum. In a typical case, an experienced physicist could solve only a few Wolff-plot problems in a day. (Lynn Stevenson had written a specific program,C O P LAN, that solved a particular problem of interest to him that was later handled by the more versatile GUTS.)

GUTS was being written at a time when one highly respected visitor to the group saw the large pile of P A N G printout that had gone unanalyzed because so many of our group members were writing GUTS-a program that was planned to do the job automatically. Our visitor was very upset at what he told me was a ((foolish deployment of our forces ». He said, « If you would only get all those people away from their program writing, and put them to work on Wolff-plots, we'd have the answer to some really important physics in a month or two ». I said I was sure we'd end up with at lot more physics in the next years if my colleagues continued to write GUTS and KICK. I'm sure that those who wrote these pioneering « fitting and kinematics programs » were subjected to similar pressures. Everyone in the high-energy physics community has long been indebted to these farsighted men because they knew that what they were doing was right. KICK was soon developed so that it gave an overall fit to several interconnected vertices, with various hypothetical identities of the several tracks assumed in a series of attempts at a fit. The relationship between energy and momentum depends on mass, so a highly constrained fit can be obtained only if the particle responsible for each track is properly identified. If the degree of constraint is not so high, more than one « hypothesis » (set of track identifications) may give a fit, and the physicist must use his judgment in making the identification.

As another example in this all- too- briefsketch of the computational aspects of our work, I will mention an important program, initiated by Art Rosenfeld and Ron Ross, that has removed much of the remaining drudgery from the bubble-chamber physicists' life. S UMX is a program that can easily be instructed to search quickly through large volumes of ((kinematics program output », printing out summaries and tabulations of interesting data. (Like all our pioneering programs, S UMX was replaced by an improved and more versatile program - in this case, KIOWA. But I will continue to talk as though SUMX were still used.) A typical SUMX printout will be a computer-printed document 3 inches thick, with hundreds of histograms, scatter plots, etc.

Hundreds of histograms are similarly printed showing numbers of events with effective masses for many different combinations of particles, with various « cuts » on momentum transfer, etc. What all this amounts to is simply that a physicist is no longer rewarded for his ability in deciding what histograms he should tediously plot and then examine. He simply tells the computer to plot all histograms of any possible significance, and then flips the pages to see which ones have interesting features.

One of my few real interactions with our programming effort came when I suggested to Gerry Lynch the need for a program he wrote that is known as GAME. In my work as a nuclear physicist before World War II, I had often been skeptical of the significance of the « bumps » in histograms, to which importance was attached by their authors. I developed my own criteria for judging statistical significance, by plotting simulated histograms, assuming curves to be smooth; I drew several samples of « Monte Carlo distributions », using a table of random numbers as the generator of the samples. I usually found that my skepticism was well founded because the « faked)) histograms showed as much structure as the published ones. There are of course many statistical tests designed to help one evaluate the reality of bumps in histograms, but in my experience nothing is more convincing than an examination of a set of simulated histograms from an assumed smooth distribution.

GAME made it possible, with the aid of a few control cards, to generate a hundred histograms similar to those produced in any particular experiment. All would contain the same number of events as the real experiment, and would be based on a smooth curve through the experimental data. The standard procedure is to ask a group of physicists to leaf through the 100 histograms -with the experimental histogram somewhere in the pile - and vote on the apparent significance of the statistical fluctuations that appear. The first time this was tried, the experimenter-who had felt confident that his bump was significant-didn't know that his own histogram was in the pile, and didn't pick it out as convincing ; he picked out two of the computer- generated histograms as looking significant, and pronounced all others-including his own - as of no significance! In view of this example, one can appreciate how many retractions of discovery claims have been avoided in our group by the liberal use of the GAME program.

As a final example from our program library, I'll mention FAKE, which, like S UMX, has been widely used by bubble-chamber groups all over the world. FAKE, written by Gerry Lynch, generates simulated measurements of bubble-chamber events to provide a method of testing the analysis programs to determine how frequently they arrive at an incorrect answer.

Now that I have brought you up to date on our parallel developments of hardware and software (computer programs), I can tell you what rewards we have reaped, as physicists, from their use. The work we did with the 4-inch chamber at the 184-inch cyclotron and at the Bevatron cannot be dignified by the designation « experiment », but it did show examples of π–μ–e decay and neutral strange-particle decay. The experiences we had in scanning the 4-inch film merely whetted our appetite for the exciting physics we felt sure would be manifest in the 10-inch chamber, when it came into operation in Wilson Powell's big magnet.

Robert Tripp joined the group in 1955, and as his first contribution to our program he designed a « separated beam » of negative K mesons that would stop in the 10-inch chamber. We had two different reasons for starting our bubble-chamber physics program with observations of the behavior of K-mesons stopping in hydrogen. The first reason involved physics: The behavior of stopping π^- mesons in hydrogen had been shown by Panofsky[47] and his co-workers to be a most fruitful source of fundamental knowledge concerning particle physics. The second reason was of an engineering nature: Only one Bevatron « straight section » was available for use by physicists, and it was in constant use. In order not to interfere with other users, we decided to set the 10-inch chamber close to a curved section of the Bevatron, and use secondary particles, from an internal target, that penetrated the wall of the vacuum chamber and passed between neighboring iron blocks in the return yoke of the Bevatron magnet. This physical arrangement gave us negative particles (K^- and π^- mesons) of a well- defined low momentum. By introducing an absorber into the beam, we brought the K^- mesons almost to rest, but allowed the lighter π^- mesons to retain a major fraction of their original momentum. The Powell magnet provided a second bending that brought the K^- mesons into the chamber, but kept then- mesons out. That was the theory of this first separated beam for bubble-chamber use. But in practice, the chamber was filled with tracks of pions and muons, and we ended up with only one stopped K^- per roll of 400 stereo pairs. It is now common for experimenters to stop one million K^- mesons in hydrogen, in a single experimental run, but the 137 K^- mesons we stopped in 1956[48] gave us a remarkable preview of what has now been learned in the much longer exposures. We measured the relative branching of $K^- + p$ into

$$\Sigma^- + \pi^+ : \Sigma^+ + \pi^- : \Sigma^0 + \pi^0 : \Lambda + \pi^0$$

And in the process, we made a good measurement of the Σ^0 mass. We plotted the first decay curves for the Σ^+ and Σ^- hyperons, and we observed for the first time the interactions of Σ^- hyperons and protons at rest. We felt amply rewarded for our years of developmental work on bubble chambers by the very interesting observations we were now privileged to make.

We had a most exciting experience at this time, that was the result of two circumstances that no longer obtain in bubble-chamber physics. In the first place, we did all our own scanning of the photographic film. Such tasks are now carried out by professional scanners, who are carefully trained to recognize and record ((interesting events)). We had no professional scanners at the time, because we wouldn't have known how to train them before this first film became available. And even if they had been trained, we would not have let them look at the film-we found it so completely absorbing that there was always someone standing behind a person using one of our few film viewers, ready to take over when the first person's eyes tired. The second circumstance that made possible the accidental discovery I am about to describe was the very poor quality of our separated K^- beam-by modern standards. Most of the tracks we observed were made by negative pions or muons, but we also saw many positively charged particles-protons, pions and muons.

At first we kept no records of any events except those involving strange particles; we would look quickly at each frame in turn, and shift to the next one if no « interesting event » showed up. In doing this scanning, we saw many examples of $\pi^+–\mu^+–e^+$ decays, usually from a pion at rest, and we soon learned about how long to expect the μ^+ track to be-about 1 centimeter. I did my scanning on a stereo viewer, so I probably had a better feeling for the length of a μ^+ track in space than did my colleagues, who looked at two projections of the stereo views, sequentially. Don Gow, Hugh Bradner, and I often scanned at the same time, and we showed each other whatever interesting events came into view. Each of us showed the others examples of what we thought was an unusual decay scheme: $\pi^-\to\mu^-\to$ e-. The decay of a μ^- at rest into an e-, in hydrogen, was expected from the early observations by Conversi et *al.*[3], but Panofsky[47] had shown that a π^- meson couldn't decay at rest in hydrogen. Our first explanation for our observations was simply that the pion had decayed just before stopping. But we gradually became convinced that this explanation really didn't fit the facts. There were too many muon tracks of about the same length, and none that were appreciably longer or shorter, as the decay-in-flight hypothesis would predict. We now began to keep records of these « anomalous decays », as we still called them, and we found occasional examples in which the muon was horizontal in the chamber, so its length could be measured. (We had as yet no way of reconstructing tracks in space from two stereo views.) By comparing the measured length of the negative muon track with that of its more normal positive counterpart, we estimated that the negative muons had an energy of 5.4MeV, rather than the well-known positive muon energy (from positive pion decay at rest) of 4.1 MeV. This confirmed our earlier suspicion that the long primary negative track couldn't be that of a pion, but it left us just as much in the dark as to the nature of the primary.

After these observations had been made, I gave a seminar describing what we had observed, and suggesting that the primary might be a previously unknown weakly interacting particle, heavier than the pion, that decayed into a muon and a neutral particle, either neutrino or photon. We had just made the surprising observation, shown in Fig.10, that there was often a gap, measured in millimeters, between the end of the primary and the beginning of the secondary. This finding suggested diffusion by a rather long-lived negative particle that orbited around and neutralized one of the protons in the liquid hydrogen. We had missed many tracks with these « gaps » because no one had seen such a thing before; we simply ignored such track configurations by subconsciously assuming that they were unassociated events in a badly cluttered bubble chamber.

One evening, one of the members of our research team, Harold Ticho from our Los Angeles campus, was dining with Jack Crawford, a Berkeley astrophysicist he had known when they were students together. They discussed our observations at some length, and Crawford suggested the possibility that a fusion reaction might somehow be responsible for the phenomenon. They calculated the energy released in several such reactions, and found that it agreed with experiment if a stopped muon were to be binding together a proton and a deuteron into an HD μ^- molecular ion. In such a « molecule » the proton and deuteron would be brought into such close proximity for such a long time that they would fuse into ^{3}He, and could deliver their fusion energy to the muon by the process of internal conversion. However, Ticho and Crawford couldn't think of any mechanism that would make the reaction happen so often - the fraction of deuterons in liquid hydrogen is only 1 in 5000. They had, however, correctly identified the reaction, but a key ingredient in the theoretical explanation was still missing.

The next day, when we had all accepted the idea that stopped muons were catalyzing the fusion of protons and deuterons, our whole group paid a visit to Edward Teller, at his home. After a short period of introduction to the observations and to the proposed fusion reaction, he explained the high probability of the reaction as follows : the stopped muon radiated its way into

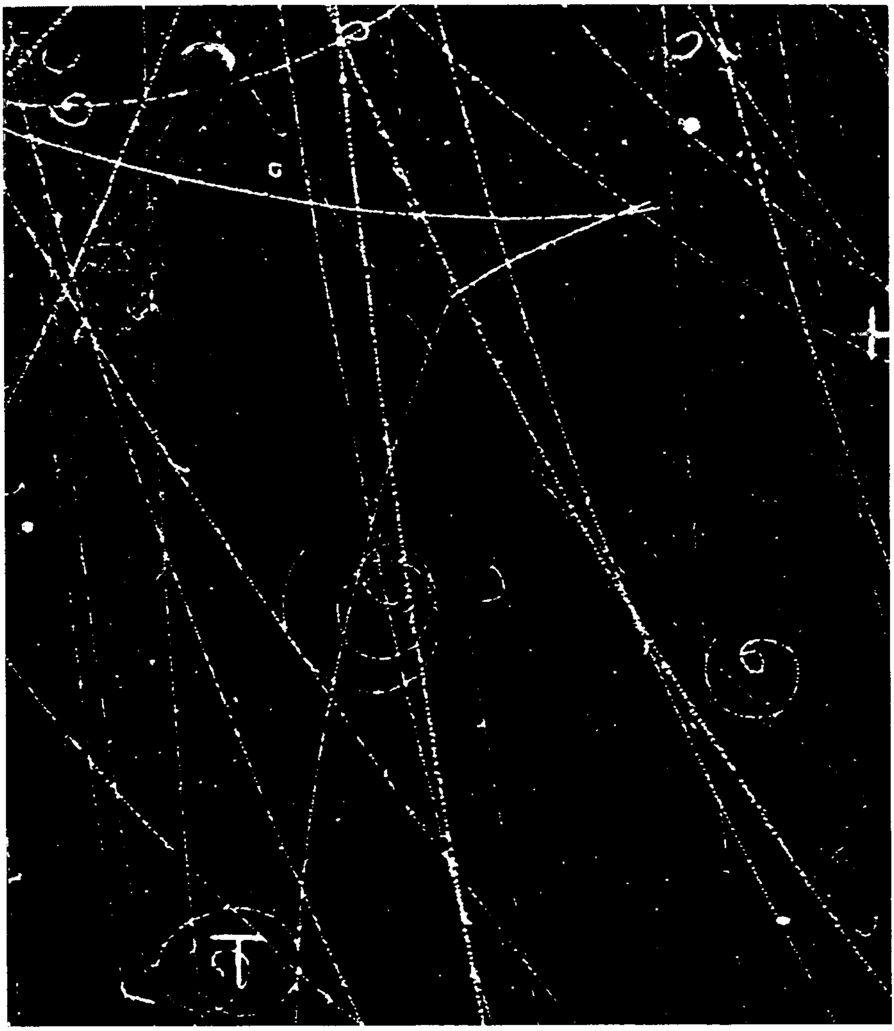

Fig. 10. Muon catalysis (with gap).

the lowest Bohr orbit around a proton. The resulting muonic hydrogen atom, $p\mu^-$, then had many of the properties of a neutron, and could diffuse freely through the liquid hydrogen. When it came close to the deuteron in an HD molecule, the muon would transfer to the deuteron, because the ground state of the $\mu^- d$ atom is lower than that of the $\mu^- p$ atom, in consequence of « reduced mass » effect. The new « heavy neutron » $d\mu^-$ might then recoil some distance as a result of the exchange reaction, thus explaining the « gap ». The final stage of capture of a proton into a $pd\mu^-$ molecular ion was also energetically favorable, so a proton and deuteron could now be confined close enough together by the heavy negative muon to fuse into a ^{3}He nucleus plus the energy given to the internally converted muon.

We had a short but exhilarating experience when we thought we had solved all of the fuel problems of mankind for the rest of time. A few hasty calculations indicated that in liquid HD a single negative muon would catalyze enough fusion reactions before it decayed to supply the energy to operate an accelerator to produce more muons, with energy left over after making the liquid HD from sea water. While everyone else had been trying to solve this problem by heating hydrogen plasmas to millions of degrees, we had apparently stumbled on the solution, involving very low temperatures instead. But soon, more realistic estimates showed that we were off the mark by several orders of magnitude-a « near miss » in this kind of physics!

Just before we published our results[49], we learned that the « μ^--catalysis » reaction had been proposed in 1947 by Frank[50] as an alternative explanation of what Powell *et* al. had assumed (correctly) to be the decay of π^+ to μ^+. Frank suggested that it might be the reaction we had just seen in liquid hydrogen, starting with a μ^-, rather than with an+. Zel'dovitch[51] had extended the ideas of Frank concerning this reaction, but because their papers were not known to anyone in Berkeley, we had a great deal of personal pleasure that we otherwise would have missed.

I will conclude this episode by noting that we immediately increased the deuterium concentration in our liquid hydrogen and observed the expected increase in fusion reaction, and saw two examples of successive catalyses by a single muon (Fig. 11). We also observed the catalysis of $D + D \rightarrow {}^3H + {}^1H$ in pure liquid deuterium.

A few months after we had announced our μ-catalysis results, the world of particle physics was shaken by the discovery that parity was not conserved in β-decay. Madame Wu and her collaborator+, acting on a suggestion by Lee and Yang[53], showed that the p-rays from the decay of oriented ^{60}Co nuclei were emitted preferentially in a direction opposite to that of the spin. Lee and Yang suggested that parity nonconservation might also manifest itself in the weak decay of the Λ hyperon into a proton plus a negative pion. Crawford *et* al. had moved the 10-inch chamber into a negative pion beam, and were analyzing a large sample of Λ's from associated production events. They looked for an « up- down asymmetry » in the emission of pions from Λ's, relative to the « normal to the production plane », as suggested by Lee and Yang. As a

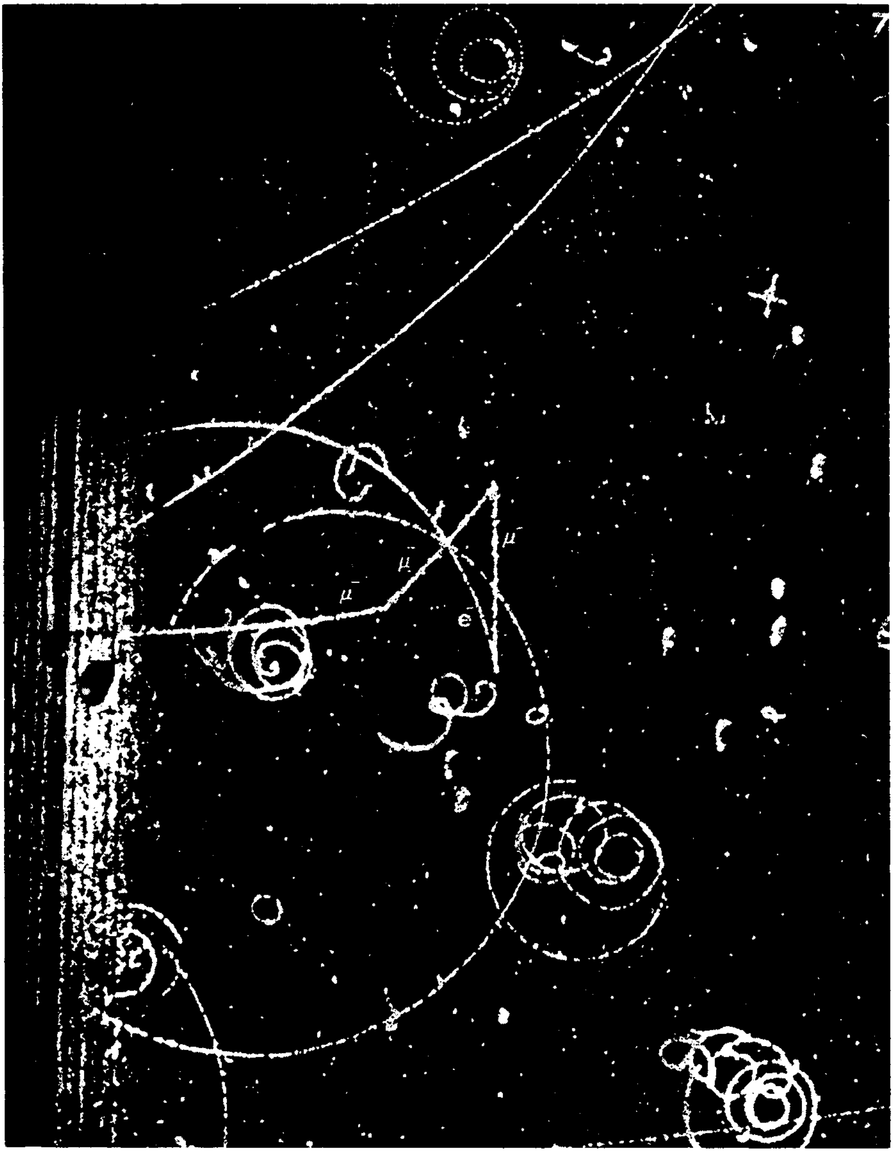

Fig. 11. Double muon catalysis.

result, they had the pleasure of being the first to observe parity nonconservation in the decay of hyperons[54].

In the winter of 1958, the IS-inch chamber had completed its engineering test run as a prototype for the 72-inch chamber, and was operating for the first time as a physics instrument. Harold Ticho, Bud Good and Philippe a

b

c

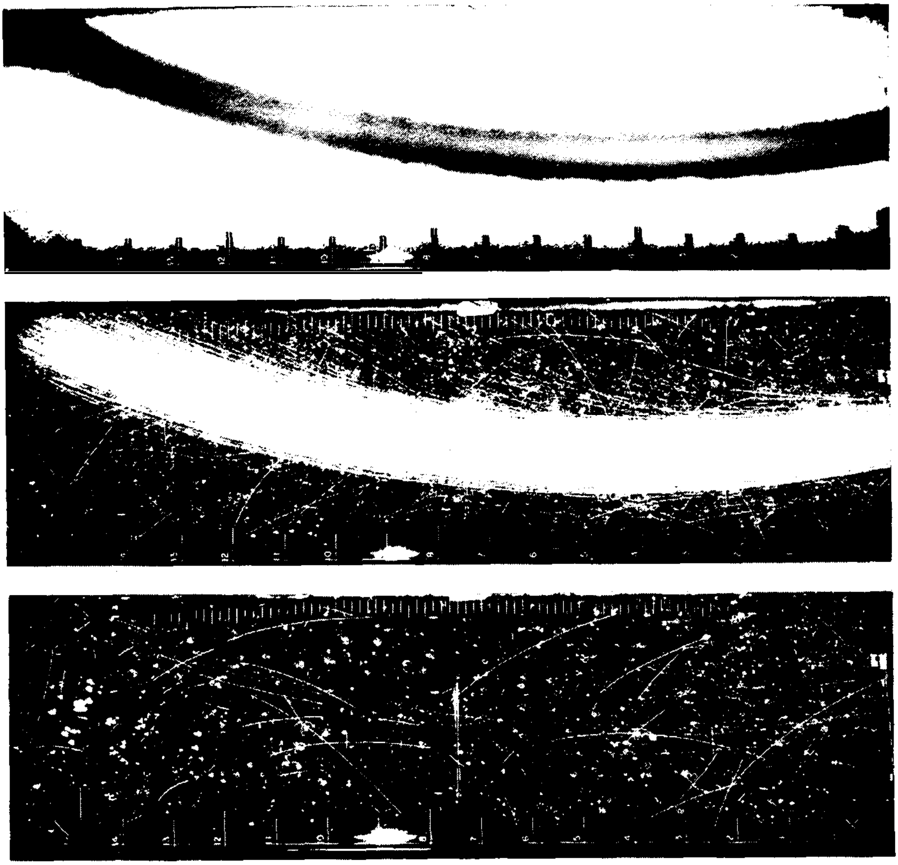

Fig.12. K- beam in 72-inch bubble chamber. (a) No spectrometers on; (b) one spectrometer on; (c) two spectrometers on.

Eberhard[55] had designed and built the first separated beam of K- mesons with a momentum of more than 1GeV / c. Fig.12 shows the appearance of a bubble chamber when such a beam is passed through it, and when one or both of the electrostatic separators are turned off. The ingenuity which has been brought to bear on the problem of beam separation, largely by Ticho and Murray, is difficult to imagine, and its importance to the success of our program cannot be overestimated[55]. Joe Murray has recentlyjoined the Stanford Linear Accelerator Center, where he has in a short period of time built a very successful radiofrequency- separated K beam and a backscattered laser beam.

The first problem we attacked with the IS-inch chamber was that of the Ξ^0. Gell-Mann had predicted that the Ξ^- was one member of an I-spin doublet, with strangeness minus 2. The predicted partner of the Ξ^- would be a neutral hyperon that decayed into a Λ and a π^0- both neutral particles that would, like the Ξ^0, leave no track in the bubble chamber. A few years earlier, as an after-dinner speaker at a physics conference, Victor Weisskopf had « brought down the house » by exhibiting an absolutely blank cloud-chamber photograph, and saying that it represented proof of the decay of a new neutral particle into two other neutral particles! And now we were seriously planning to do what had been considered patently ridiculous only a few years earlier .

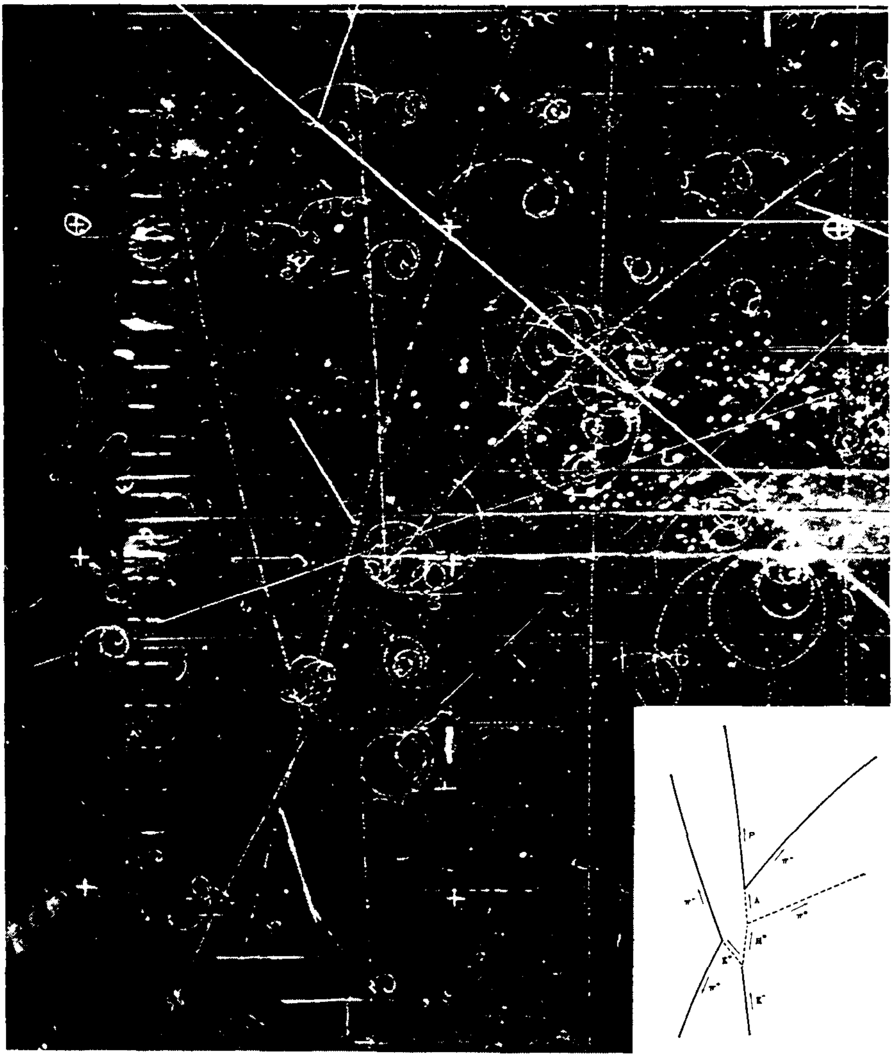

Fig. 13. Production and decay of a neutral cascade hyperon (Xi zero).

According to the Gell-Mann and Nishijima strangeness rules, the Ξ^0 should be seen in the reaction

$$\begin{array}{ccccccc} K^- + p \rightarrow & \Xi^0 & & + & K^0 & & \\ & \downarrow & & & \downarrow & & \\ & \Lambda & + \pi^0 & & \pi^- + \pi^+ & & \\ & \downarrow & & & & & \\ & \pi^- + \mathrm{p} & & & & & \end{array}$$

In the one example of this reaction that we observed, Fig.13, the charged pions from the decay of the neutral K^0 yielded a measurement of the energy and direction of the unobserved K^0. Through the conservation laws of energy and momentum (plus a measurement of the momentum of the interacting *K*-track) we could calculate the mass of the coproduced Ξ^0 hyperon plus its velocity and direction ofmotion. Similarly, measurements of then- and proton gave the energy and direction of motion of the unobserved Λ, and proved that it did not come directly from the point at which the *K*- meson interacted with the proton. The calculated flight path of the Λ intersected the calculated flight path of the Ξ^0, and the angle of intersection of the two unobserved but calculated tracks gave a confirming measurement of the mass of the Ξ^0 hyperon, and proved that it decayed into a Λ plus a π^0. This single hard-won event was a sort of tour de force that demonstrated clearly the power of the liquid hydrogen bubble chamber plus its associated data-analysis techniques.

Although only one Ξ^0 was observed in the short time the 15 -inch chamber was in the separated *K*- beam, large numbers of events showing strange-particle production were available for study. The Franckensteins were kept busy around the clock measuring these events, and those of us who had helped to build and maintain the beam now concentrated our attention on the analysis of these reactions. The most copious of the simple ((topologies)) was $K^-p\rightarrow$ two charged prongs plus a neutral V-particle. According to the strangeness rules, this topology could represent either

$$\begin{array}{c} K^- + \mathrm{p} \rightarrow \Lambda + \pi^+ + \pi^- \\ \qquad\qquad \downarrow \\ \qquad\qquad \pi^- + p \end{array}$$

or

$$\begin{array}{c} K^- + p \rightarrow \overline{K}^0 + p + \pi^- \\ \downarrow \\ \pi^- + \pi^+ \end{array}$$

The kinematics program KICK was now available to distinguish between these two reactions, and to eliminate those examples of the same topology in which an unobserved π^0 was produced at the first vertex. SUMX had not yet been written, so the labor ofplotting histograms was assumed by the two very able graduate students who had been associated with the K- beam and its exposure to the 15 -inch chamber since its planning stages : Stanley Wojcicki and Bill Graziano. They first concentrated their attention on the energies of the charged pions from the production vertex in the first of the two reactions listed above. Since there were three particles produced at the vertex-a charged pion of each sign plus a Λ -one expected to find the energies of each of the three particles distributed in a smooth and calculable way from a minimum value to a maximum value. The calculated curve is known in particle physics as the « phase-space distribution ». The decay of a τ meson into three charged pions was a well known ((three-particle reaction)) in which the dictates of phase space were rather precisely followed.

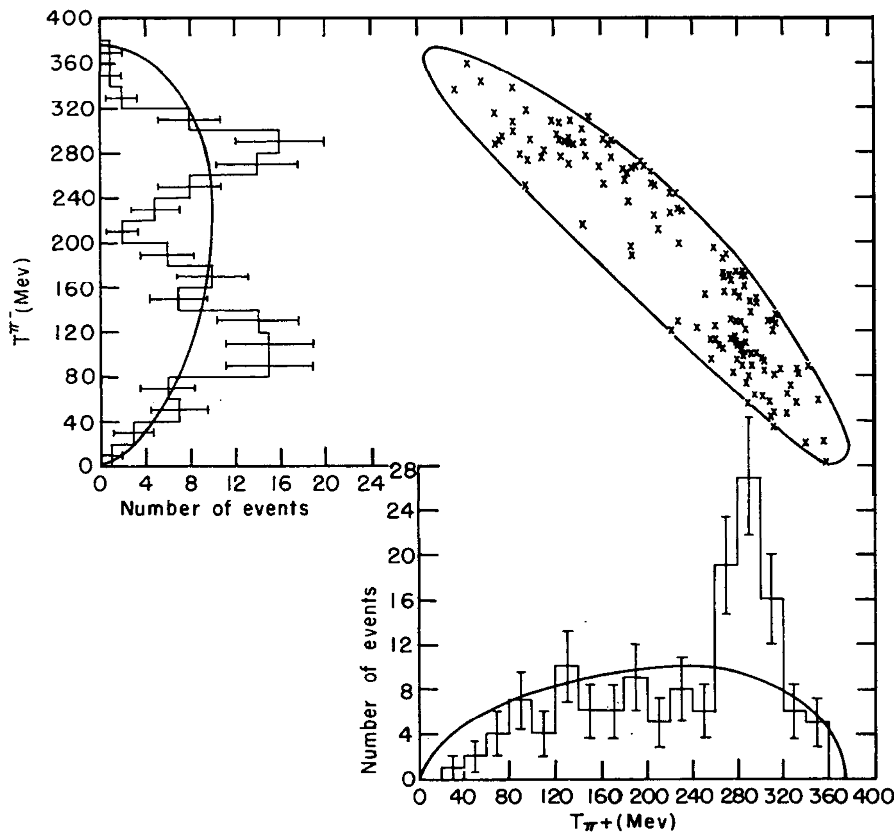

Fig. 14. Discovery of the Y_1^* (1385).

But when Wojcicki and Graziano finished transcribing their data from KICK printout into histograms, they found that phase-space distributions were poor approximations to what they observed. Fig. 14 shows the distribution of energy of both positive and negative mesons, together with the corresponding « Dalitz plot », which Richard Dalitz[56] had originated to elucidate the « τ–θ puzzle », which had in turn led to Lee and Yang's parity-nonconservation hypothesis.

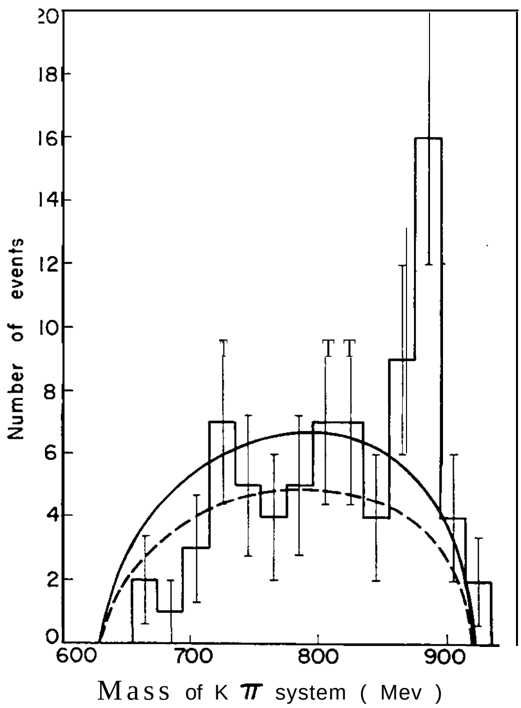

Fig. 15 Discovery of the K^* (890).

The peaked departure from a phase-space distribution had been observed only once before in particle physics, where it had distinguished the reaction $p+p\rightarrow\pi^{+}+d$ from the « three-body reaction » p+p-+n+ $+p$ + n. (Although no new particles were discovered in these reactions, they did contribute to our knowledge of the spin of the pion[57].) But such a peaking had been observed in the earliest days of experimentation in the artificial disintegration of nuclei, and its explanation was known from that time. Oliphant and Rutherford[58] observed the reaction p + ^{11}B + 3 ^{4}He. This is a three-body reaction, and the energies of the α particles had a phase-space-like distribution except for the fact that there was a sharp spike in the energy distribution at the highest α - particle energy. This was quickly and properly attributed[58] to the reaction

$$p + {}^{11}\mathrm{B} \rightarrow {}^{8}\mathrm{Be} + {}^{4}\mathrm{He}$$
$$\downarrow$$
$${}^{4}\mathrm{He} + {}^{4}\mathrm{He}$$

In other words, some of the reactions proceeded via a two-body reaction, in which one α particle recoiled with unique energy against a quasistable ^{8}Be nucleus. But the *Be nucleus was itself unstable, coming apart in 10^{-16} sec into two α particles of low relative energy. The proof of the fleeting existence of *Be was the peak in the high energy a-particle distribution, showing that initially only two particles, *Be and ^{4}He, participated in the reaction.

The peaks seen in Fig. 14 were thus a proof that the $\pi^{\pm}$ recoiled against a combination of $\Lambda+\pi^{\mp}$ that had a unique mass, broadened by the effects of the uncertainty principle. The mass of the $\Lambda\pi$ combination was easily calculable as 1385 MeV, and the I-spin of the system was obviously **1, since** the I-spin of the Λ is 0, and the I-spin of the π is 1. This was then the discovery of the first « strange resonance », the Y_1^*(1385): Although the famous Fermi 3,3-resonance had been known for years, and although other resonances in the $\pi^{\pm}$ nucleon system had since shown up in total cross-section experiments at Brookhaven and Berkeley, CalTech and Cornell[59], the impact of the Y_1^* resonance on the thinking of particle physicists was quite different- *the* Y_1^* *really acted like a new particle,* and not simply as a resonance in a cross section.

We announced the Y_1^* at the 1960 Rochester High Energy Physics Conference[60], and the hunt for more short-lived particles began in earnest. The same team from our bubble- chamber group that had found the Y_1^* **(13 85) now** found two other strange resonances before the end of 1960-the K^* (890)[61], and the Y_0^*(1405)[62].

Although the authors of these three papers have for years been referred to as « Alston et al. », I think that on this occasion it is proper that the full list be named explicitly. In addition to Margaret Alston (now Margaret Garnjost) and Luis W. Alvarez, and still in alphabetical order, the authors are: Philippe Eberhard, Myron L. Good, William Graziano, Harold K. Ticho, and Stanley G. Wojcicki.

Figs. 15 and 16 show the histograms from the papers announcing these two new particles; the K^* was the first example of a « boson resonance » found by any technique. Instead of plotting these histograms against the energy of one particle, we introduced the now universally accepted technique of plotting

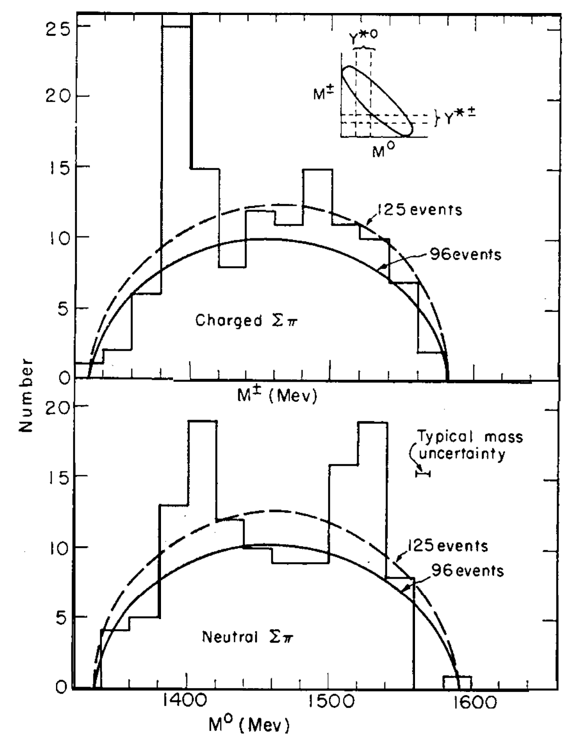

Fig. 16. Discovery of the $Y_0{}^*$ (1405).

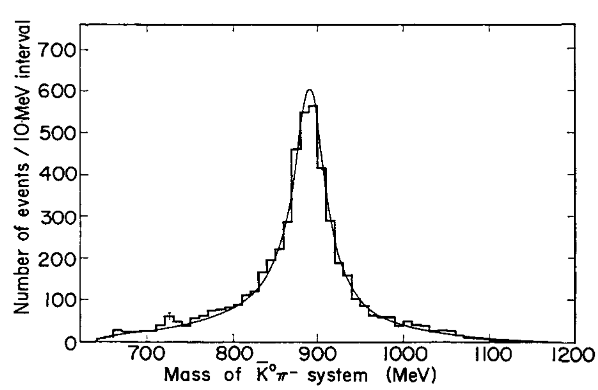

Fig. 17. Present-day *K* (890).*

them against the effective mass of the composite system: $\Sigma+\pi$ for the $Y_0{}^*$ **(1405)** and $K+\pi$ for the K^* (890). Fig. 17 shows the present state of the art relative to the K*(890) ; there is essentially no phase-space background in this histogram, and the width of the resonance is clearly measurable to give the lifetime of the resonant state via the uncertainty principle.

These three earliest examples ofstrange-particle resonances all had lifetimes of the order of 10^{-23} sec, so the particles all decayed before they could traverse more than a few nuclear radii. No one had foreseen that the bubble chamber could be used to investigate particles with such short lives; our chambers had been designed to investigate the strange particle swith lifetimes of 10^{-10} sec- 10^{13} times as long.

In the summer of 1959, the 72-inch chamber was used in its first planned physics experiment. Lynn Stevenson and Philippe Eberhard designed and constructed a separated beam of about 1.6- GeV/c antiprotons, and a quick scan of the pictures showed the now famous first example of antilambda production, via the reaction

$$\begin{array}{ccccc} \bar{p} + p \rightarrow & \bar{\Lambda} & + & \Lambda \\ & \downarrow & & \downarrow \\ & \pi^+ + \bar{p} & & \pi^- + p \end{array}$$

Fig.18 shows this photograph, with the antiproton from the antilambda decay annihilating in a four-pion event. I believe that everyone who attended the 1959 High Energy Physics Conference in Kiev will remember the showing of this photograph-the first interesting event from the newly operating 72-inch chamber.

Hofstadter's classic experiments on the scattering of high-energy electrons by protons and neutrons[63] showed for the first time how the electric charge was distributed throughout the nucleons. The theoretical interpretation of the experimental results[64] required the existence of two new particles, the vector mesons now known as the ω and the ϱ. The adjective « vector » simply means that these two mesons have one unit of spin, rather than zero, as the ordinary π and K mesons have. The ω was postulated to have I-spin = o, and the ϱ to have I- spin = 1; the ω would therefore exist only in the neutral state, while the ϱ would occur in the + , - , and o charged states.

Many experimentalists, using a number of techniques, set out to find these important particles, whose masses were only roughly predicted. The first success came to Bogdan Maglić, a visitor to our group, who analyzed film from the 72-inch chamber's antiproton exposure. He made the important decision to concentrate his attention on proton-antiproton annihilations into five pions- two negative, two positive, and one neutral. KICK gave him a selected sample of such events; the tracks of the π^0 couldn't be seen, of course, but the constraints of the conservation laws permitted its energy and direction

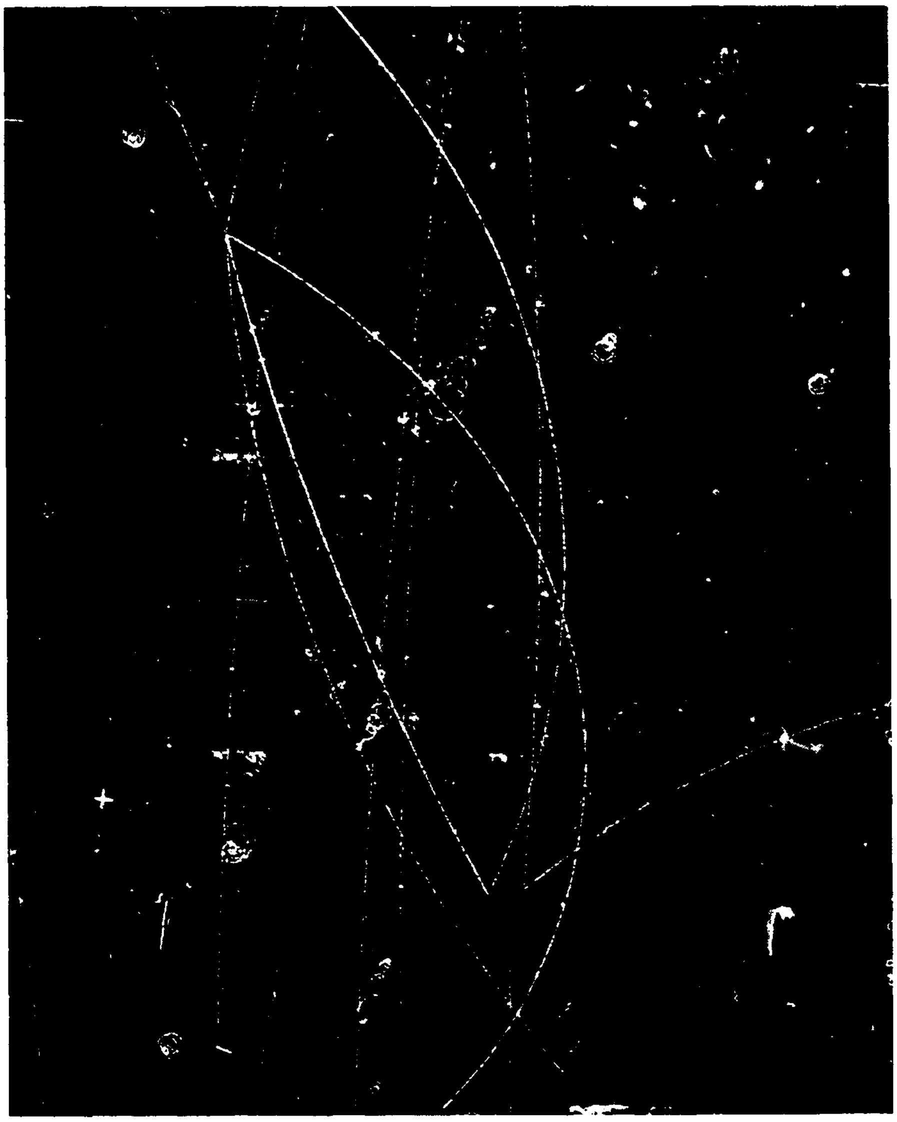

Fig. 18. First production of anti-lambda.

to be computed. Maglid then plotted a histogram of the effective mass of all neutral three-pion combinations. There were four such neutral combinations for each event; the neutral pion was taken each time together with all four possible pairs of oppositely charged pions. SUMX was just beginning to work, and still had bugs in it, so the preparation of the histogram was a very tedious and time-consuming chore, but as it slowly emerged, Maglid had the thrill of seeing a bump appear in the side of his phase-space distribution. Fig.19 shows a small portion of the whole distributions, with the peak that signaled the discovery of the very important ω meson.

Although Bogdan Maglić originated the plan for this search, and pushed through the measurements by himself, he graciously insisted that the paper announcing his discovery[65] should be co-authored by three of us who had developed the chamber, the beam, and the analysis program that made it possible.

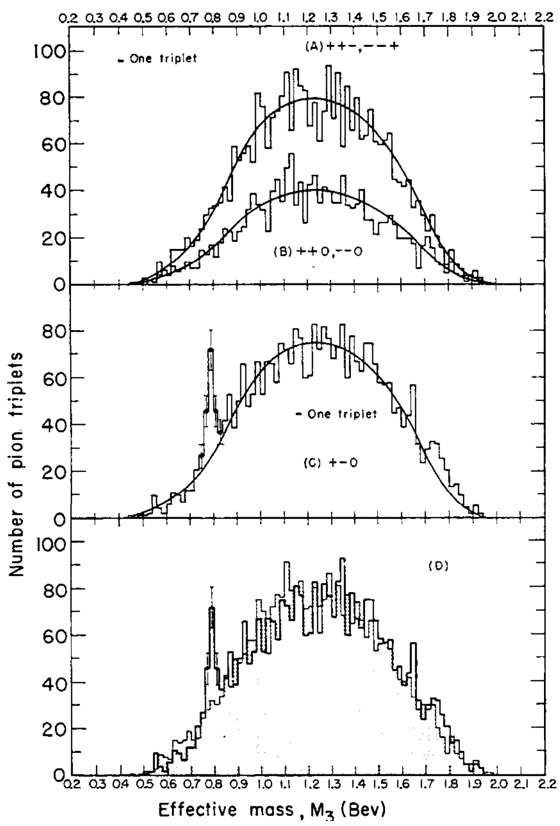

Fig. 19. Discovery of the ω meson.

The ϱ meson is the only one from this exciting period in the development of particle physics whose discovery cannot be assigned uniquely. In our group, the two Franckensteins were being used full time on problems that the senior members felt had higher priority. But a team of junior physicists and graduate students, Anderson et *al.*[66], found that they could make accurate enough measurements directly on the scanning tables to accomplish a ((Chew-Low extrapolation ». Chew and Low had described a rather complicated procedure to look for the predicted dipion resonance now known as the ϱ meson. Fig. 20 shows the results of this work, which convinced me that the ϱ existed and had its predicted spin of 1. The mass of the ϱ was given as about 650 MeV, rather than its now accepted value of 765 MeV. (This low value is now explained in terms of the extreme width of the ϱ resonance.) The evidence for the ϱ seemed to me even more convincing than the early evidence Fermi and his co- workers produced in favor of the famous 3,3 pion-nucleon resonance.

But one of the unwritten laws of physics is that one really hasn't made a discovery until he has convinced his peers that he has done so. We had just persuaded high-energy physicists that the way to find new particles was to look for bumps on effective-mass histograms, and some of them were therefore unimpressed by the Chew-Low demonstration of the ϱ. Fortunately, Walker and his collaborators67 at Wisconsin soon produced an effective-mass ideogram with a convincing bump at 765 MeV, and they are therefore most often listed as the discoverers of the ϱ.

Ernest Lawrence very early established the tradition that his laboratory would share its resources with others outside its walls. He supplied short-lived

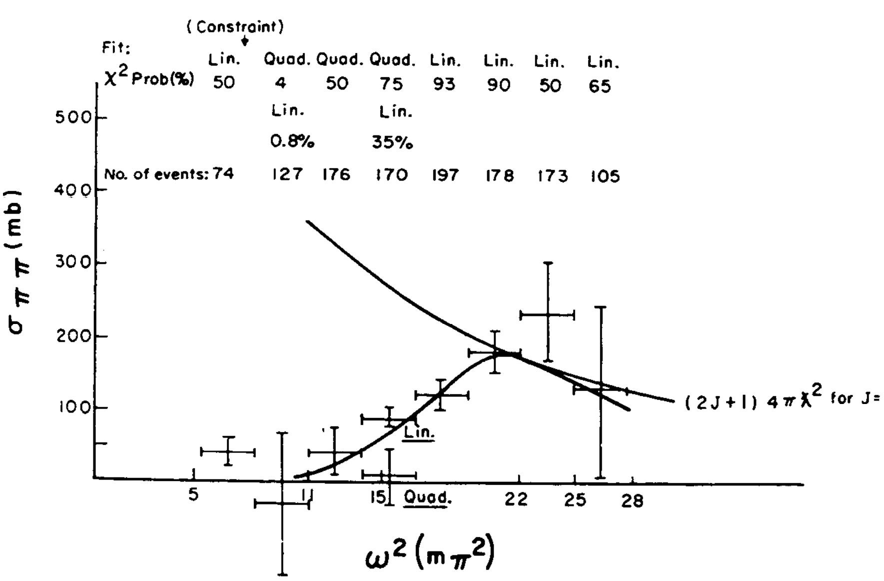

Fig. 20. First evidence for the ϱ meson.

radioactive materials to scientists in all departments at Berkeley, and he sent longer- lived samples to laboratories throughout the world.The first artificially created element, technetium, was found by Perrier and Segrè[68], who did their work in Palermo, Sicily. They analyzed the radioactivity in a molybdenum deflector strip from the Berkeley 28-inch cyclotron that had been bombarded for many months by 6-MeV deuterons.

We followed Ernest Lawrence's example, and thus participated vicariously in a number of important discoveries of new particles. The first was the η found at Johns Hopkins, by a group headed by Aihud Pevsner[69]. They analyzed film from the 72-inch chamber, and found the η with a mass of 550 MeV, decaying into $\pi^+\pi^-\pi^0$. Within a few weeks of the discovery of the η,

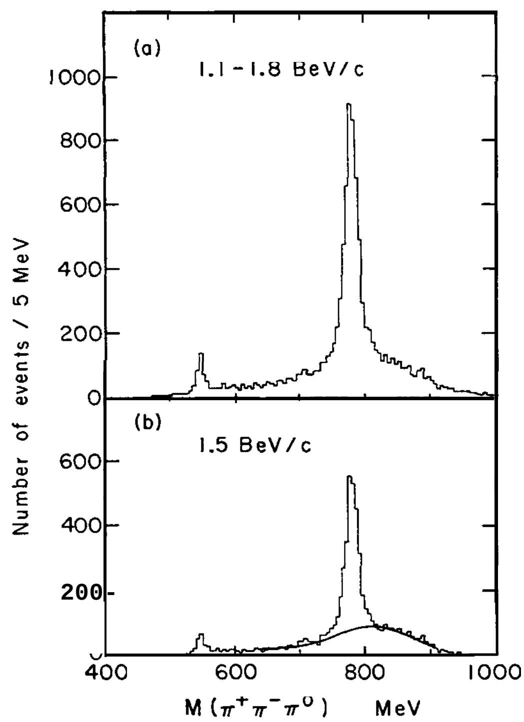

Fig.21. Present-day histogram showing ω and η mesons.

Rosenfeld and his co- workers[70] at Berkeley, who had independently observed the η, showed quite unexpectedly that I spin was not conserved in its decay. Fig. 21 shows the present state of the art with respect to the ω and η mesons; the strengths of their signatures in this single histogram is in marked contrast to their first appearances in 72-inch bubble-chamber experiments.

In the short interval of time between the first and second publications on the η, the discovery of the $Y_0^\star$ (1520) was announced by Ferro-Luzzi, Tripp, and Watson[71], using a new and elegant method. Bob Tripp has continued to be a leader in the application of powerful methods of analysis to the study of the new particles.

The discovery of the $\Xi^\star$(1530) hyperon was accomplished in Los Angeles by Ticho and his associates[72], using 72-inch bubble-chamber film. Harold Ticho had spent most of his time in Berkeley for several years, working tirelessly on every phase of our work, and many of his colleagues had helped prepare the high-energy separated K- beam for what came to be known as the K72 experiment. The UCLA group analyzed the two highest-momentum K- exposures in the 72-inch chamber, and found the $\Xi^\star$(1530) just in time to report it at the 1962 High Energy Physics Conference in Geneva. (Confirming evidence for this resonance soon came from Brookhaven[73].)

Murray Gell-Mann had recently enunciated his important ideas concerning the « Eightfold Way »[74], but his paper had not generated the interest it deserved. It was soon learned that Ne'eman had published the same suggestions, independently[75].

The announcement of the $\Xi^\star$ (1530) fitted exactly with their predictions of the mass and other properties of that particle. One of their suggestions was that four I-spin multiplets, all with the same spin and parity, would exist in a « decuplet » with a mass spectrum of « lines » showing an equal spacing. They put the Fermi 3,3-resonance as the lowest mass member, at 1238 MeV. The second member was the $Y_1^\star$ (1385), so the third member should have a mass of (1385) +(1385-1238) = 1532. The strangeness and the multiplicity of each member of the spectrum was predicted to drop 1 unit per member, so the $\Xi^\star$ (1530) fitted their predictions completely. It was then a matter of simple arithmetic to set the mass, the strangeness, and the charge of the final member -the W-. The realization that there was now a workable theory in particle physics was probably the high point of the 1962 International Conference on High Energy Physics.

Since the second and third members of the series-the ones that permitted the prediction of the properties of the W- to be made-had come out of our bubble chambers, it was a matter of great disappointment to us that the Bevatron energy was insufficient to permit us to look for the W-. Its widely acclaimed discovery[76] had to wait almost two years, until the 80-inch chamber at Brookhaven came into operation.

Since the name of the W had been picked to indicate that it was the last of the particles, the mention of its discovery is a logical point at which to conclude this lecture. I will do so, but not because the discovery of the W signaled the end of what is sometimes called the population explosion in particle physics -the latest list[77] contains between 70 and 100 particle multiplets, depending upon the degree of certainty one demands before « certification ». My reason for stopping at this point is simply that I have discussed most of the particles found by 1962 -the ones that were used by Gell-Mann and Ne'eman to formulate their S U (3) theories-and things became much too involved after that time. So many groups were then in the ((bump-hunting business)) that most discoveries of new resonances were made simultaneously in two or more laboratories.

I am sorry that I have neither the time nor the ability to tell you of the great beauty and the power that has been brought to particle physics by our theoretical friends. But I hope that before long, you will hear it directly from them.

In conclusion, I would like to apologize to those of my colleagues and my friends in other laboratories: whose important work could not be mentioned because of time limitations. By making my published lecture longer than the oral presentation, I have reduced the number of apologies that are necessary, but unfortunately I could not completely eliminate such debts.

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Biography

Luis W. Alvarez was born in San Francisco, Calif., on June 13, 1911. He received his B. SC. from the University of Chicago in 1932, a M. SC. in 1934, and his Ph.D. in 1936. Dr. Alvarezjoined the Radiation Laboratory of the University of California, where he is now a professor, as a research fellow in 1936. He was on leave at the Radiation Laboratory of the Massachusetts Institute of Technology from 1940 to 1943, at the Metallurgical Laboratory of the University of Chicago in 1943-1944, and at the Los Alamos Laboratory of the Manhattan District from 1944 to 1945.

Early in his scientific career, Dr. Alvarez worked concurrently in the fields of optics and cosmic rays. He is co-discoverer of the « East-West effect, in cosmic rays. For several years he concentrated his work in the field of nuclear physics. In 1937 he gave the first experimental demonstration of the existence of the phenomenon of K-electron capture by nuclei. Another early development was a method for producing beams of very slow neutrons. This method subsequently led to a fundamental investigation of neutron scattering in ortho- and Pam-hydrogen, with Pitzer, and to the first measurement, with Bloch, of the magnetic moment of the neutron. With Wiens, he was responsible for the production of the first ^{198}Hg lamp ; this device was developed by the Bureau of Standards into its present form as the universal standard of length. Just before the war, Alvarez and Cornog discovered the radioactivity of ^{3}H (tritium) and showed that ^{3}He was a stable constituent of ordinary helium. (Tritium is best known as a source of thermonuclear energy, and ^{3}He has become of importance in low temperature research.)

During the war (at M. I. T.) he was responsible for three important radar systems- the microwave early warning system, the Eagle high altitude bombing system, and a blind landing system of civilian as well as military value (G C A, or Ground- Controlled Approach). While at the Los Alamos Laboratory, Professor Alvarez developed the detonators for setting off the plutonium bomb. He flew as a scientific observer at both the Almagordo and Hiroshima explosions.

Dr. Alvarez is responsible for the design and construction of the Berkeley

40-foot proton linear accelerator, which was completed in 1947. In 1951 he published the first suggestion for charge exchange acceleration that quickly led to the development of the « Tandem Van de Graaf accelerator ». Since that time, he has engaged in high-energy physics, using the 6 billion electron volt Bevatron at the University of California Radiation Laboratory. His main efforts have been concentrated on the development and use of large liquid hydrogen bubble chambers, and on the development of high-speed devices to measure and analyze the millions ofphotographs produced each year by the bubble-chamber complex. The net result of this work has been the discovery by Dr. Alvarez' research group, of a large number of previously unknown « fundamental particle resonances ». Since 1967 Dr. Alvarez has devoted most of this time to the study of cosmic rays, using balloons and superconducting magnets.

Professor Alvarez is a member of the following societies: National Academy of Sciences, American Philosophical Society, American Physical Society (President 1969), American Academy of Arts and Sciences, and National Academy of Engineering. In 1946 he was awarded the Collier Trophy by the National Aeronautical Association for the development of Ground-Controlled Approach. In 1953 he was awarded the John Scott Medal and Prize, by the city of Philadelphia, for the same work. In 1947 he was awarded the Medal for Merit. In 1960 he was named « California Scientist of the Year » for his research work on high-energy physics. In 1961 he was awarded the Einstein Medal for his contribution to the physical sciences. In 1963 he was awarded the Pioneer Award of the AIEEE; in 1964 he was awarded the National Medal of Science for contributions to high-energy physics, and in 1965 he received the Michelson Award. He has received the following honorary degrees: Sc.D., University of Chicago, 1967; Sc.D., Carnegie-Mellon University, 1968; Sc.D., Kenyon College, 1969.

Physics 1969

MURRAY GELL-MANN

« for his contributions and discoveries concerning the classification of elementary particles and their interactions »

Physics 1969

Presentation Speech by Professor Ivar Waller, member of the Nobel Committee for Physics

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

Elementary particle physics which is now so vigorous was still in its infancy when Murray Gell-Mann in 1953 published the first of the papers which have been honoured with this years Nobel prize in physics.

The physicists were, however, already then aquainted with a rather large number of particles which apparently were indivisible and therefore elementary building stones of all matter. The elementary particle known for the longest time was the electron.

New particles were added when the atomic nuclei were explored. It was found that the atomic nuclei consist of positively charged protons and electrically neutral neutrons. These particles are held together in the atomic nuclei by enormously strong forces called nuclear forces which do not distinguish between protons and neutrons. This symmetry of the nuclear forces was expressed by saying that the nuclear forces are charge-independent. A proton and a neutron have further very nearly the same mass. They form a doublet of particles and have been given the common name of nucleons.

An increase already expected and desired occurred in the family of elementary particles at the end of the 1940's, when new particles called pi-mesons were discovered. They were named mesons because they have a mass between the electron and the nucleon masses. The pi- mesons had been predicted by the Japanese physicist Yukawa. They form a triplet of particules having nearly the same mass but different charges which are + 1, 0 and - 1 in units of the proton charge. Their interaction with the nucleons is strong and charge- independent. Their most important task is to be an intermediary agent for the strong interactions between the nucleons.

A very remarkable discovery which marked a new area in particle physics was made by the British physicists Rochester and Butler about the same time. They found new unstable particles which did not fit in with the theoretical ideas developed so far. Some of the new particles are heavier than the nucleons and were grouped together with them under the common name of baryons. The others were lighter than the nucleons but heavier than the electrons and were called K- mesons. The new particles were copiously produced when high-energy pi-mesons collide with nucleons and were therefore assumed to interact strongly with other particles. But they had such a long lifetime that some law must exist which prevent the strong forces to act when they disintegrate into other particles. Gell-Mann discovered this law after some preliminary results had been found by Pais.

It had been assumed earlier that the new baryons from doublets like the nucleons and that the K- mesons form triplets like the pi-mesons. Gell-Mann made the fundamental new assumption that the new baryons instead form a singlet, a triplet and a doublet, the latter being different from the nucleon doublet, and that the new mesons form two kinds of doublets, one consisting of the antiparticles of the other. Gell-Mann assumed further that the principle of charge-independence was generally valid for strong interactions. He could thereby explain the mysterious properties of the new particles. He introduced a new fundamental characteristic of a multiplet called its hypercharge. This is defined as twice the mean value of the charges in the multiplet. Gell-Mann's proposed the new rule : Elementary particles can be transformed in others by the strong and the electromagnetic interactions only if the total hypercharge is conserved. This rule reminds of the law of conservation of the electric charge. It should be remarked that Gell-Mann initially used instead of the hypercharge a closely related number called the strangeness.

This discovery by Gell-Mann was admirable considering in particular the very meagre experimental material available to him. In the predicted baryon multiplets there occurred empty places. Gell- Mann could on this ground predict two new baryons. One of them was soon discovered but the other not until six years later.

This classification of the elementary particles and their interaction discovered by Gell-Mann has turned out to applicable to all strongly interacting particles found later and these are practically all particles discovered after 1953. His discovery is therefore fundamental in elementary particle physics.

It should be added that two Japanese physicists, Nakano and Nishijima, published a similar classification some months later than Gell-Mann.

Many theoretical physicists tried during the following years to find new symmetries which should give relations between the particle multiplets. Initiated by Sakata a series of papers were published in particular by Japanese physicists. They indicated that a certain kind of symmetry could be of interest. Gell-Mann showed in a new fundamentally important paper of 1961 that this symmetry which had since long been studied in pure mathematics could be used for the classification of all strongly interacting particles. Assuming the validity of the new symmetry which includes the symmetry corresponding to charge-independence, Gell-Mann found that his earlier multiplets could be brought together into larger groups called supermultiplets each containing all baryons or all mesons which have the same spin and the same parity, *i.e.* have the same measure for their rotation around their axes and are transformed in the same way by reflections. Gell-Mann called this classification « The Eightfold Way ». The nucleons were found to belong to a supermultiplet of eight particles *i.e.* an octet. For the mesons an octet was proposed were the pi- and *K*- mesons filled seven places. Because one place was empty a new meson was predicted. Its existence had been suspected already by some of the Japanese physicists mentioned above. It was soon discovered which meant that Gell-Mann's theory was strongly supported. Still more famous is Gell-Mann's prediction in 1962 of a new baryon called omega minus.

A similar classification was proposed by Y. Ne'eman somewhat later than Gell-Mann.

Gell-Mann has also found that « The Eightfold Way » can be described very simply by assuming that all particles which interact strongly with each other are composed of only three kinds of particles which he called quarks and of the corresponding antiparticles. The quarks are peculiar in particular because their charges are fractions of the proton charge which according to all experience up to now is the indivisible elementary charge. It has not yet been possible to find individual quarks although they have been eagerly looked for. Gell-Mann's idea is none the less of great heuristic value.

And interesting application of « The Eightfold Way » is the so-called current algebra which was founded by Gell-Mann. It has *e.g.* made evident that there are important connections between the different kinds of elementary particle interactions.

Gell-Mann has given many fundamental contributions to the theory of elementary particles besides those which have been mentioned here. He has during more than a decade been considered as the leading scientist in this field.

Professor Gell-Mann. You have given fundamental contributions to our knowledge of mesons and baryons and their interactions. You have developed new algebraic methods which have led to a far-reaching classification of these particles according to their symmetry properties. The methods introduced by you are among the most powerful tools for further research in particle physics.

On behalf of the Royal Swedish Academy of Science, I congratulate you on your successful work and ask you to receive your Nobel Prize from the hands of His Majesty the King.

MURRAY GELL-MANN

Symmetry and currents in particle physics

Nobel Lecture, December 11, 1969

[Professor Gell-Mann has presented his Nobel Lecture, but did not submit a manuscript for inclusion in this volume]

Biography

Murray Gell-Mann was born on 15th September 1929, in New York City. He obtained his B.Sc. at Yale University in 1948, and his Ph.D. in 1951 at the Massachusetts Institute of Technology. In 1952 he became a member of the Institute for Advanced Study, during 1952-1953 he was instructor at the University of Chicago, from 1953 to 1954 he was Assistant Professor, in 1954 he was appointed Associate Professor for research on dispersion relations. In this period he developed the strangeness theory and the eightfold way theory. In 1956 he was appointed Professor, his research then turned more to the theory of weak interactions.

In 1959 Professor Gell-Mann was awarded the Dannie Heineman Prize of the American Physical Society. He is a Fellow of this society and a member of the National Academy of Sciences.

Murray Gell-Mann was in 1955 married to J.Margaret Dow; they have a daughter, Elizabeth, and a son, Nicholas.

Physics 1970

HANNES ALFVÉN

« for fundamental work and discoveries in magneto- hydrodynamics with fruitful applications in different parts of plasma physics»

LOUIS NÉEL

« for fundamental work and discoveries concerning antiferromagnetism and ferri-magnetism which have led to important applications in solid state physics >>

Physics 1970

Presentation Speech by Professor Torsten Gustafsson, member of the Royal Swedish Academy of Sciences

Your Majesty, Your Royal Highnesses, Ladies and Gentlemen.

From the sun, there blows a wind so hot that its atoms are split into electrically-charged particles, electrons and ions. They are attracted by the earth's magnetic field and the electrons follow the lines of force and produce the aurora borealis. This wind is one example of a plasma, an electrically-conducting gas with such remarkable properties that one, in addition to the well-known states of matter, solid, liquid and gaseous, has now, in the last fifty years, recognised it as a fourth. It is the most common state of matter in the universe. It was the most important state at the time of the creation of the solar and planetary systems; it is found in interstellar space, in fusion reactors and in welding apparatus.

Alfvén introduced into discussion of the aurora the fundamental idea that plasma, even in space, has a magnetic field associated with it.

In this way, he was led to study the general question of the significance of magnetic fields in the movements of plasmas. The magnetic field forces the positive and negative charges to move in different directions, giving rise to electric currents. The interaction of these currents produces mechanical forces, which can completely change the plasma's direction and speed. In particular, Alfvén discovered the existence of hitherto unsuspected magneto-hydrodynamical waves, the so-called Alfvén waves.

In cosmic physics, Alfvén's fundamental contribution has been the introduction of the magnetic field of force and the application of magneto - hydro - dynamics. Prior to his work, one simply did not take these forces into consideration: through him, they have found widespread application in astrophysical problems, particularly in studying that phase of the development of the solar system in which the planets and satellites were created. Thus, the sun's rotation and the regular pattern of the planetary orbits can be explained by the idea that hydro-magnetic waves from the sun flowed along magnetic lines of force and transferred rotational energy to the planets when they were in the early stages of formation.

Furthermore, magneto- hydrodynamics is important in discussing the problem of how the central body in a plasma cloud can develop into a sun and system of planets, or in investigations of stability conditions for a plasma consisting of electrons and ions moving at relativistic velocities interacting with cosmic fields. This is of interest in connection with both supernovae and the powerful eruptions recently found to occur in the centre of the Milky Way.

Alfvén's contributions to clarify the physical properties of plasmas have been considerable. Particularly important have been those works which form the bases of fusion research in different parts of the world. These works are important independently of how a fusion reactor can be built. The problem of containing a plasma at temperatures of millions of degrees in a magnetic field is related to Alfvén's concept of frozen lines of magnetic force. The plasma flowing in the bottle must not collapse like a breaking wave. Knowledge of the properties of Alfvén waves has been of extreme assistance in finding currents with the stability required.

Professor Alfvén. You have created magneto-hydrodynamics. Its development, in which you have played the major role, has shown the significance of this new branch of physics, both on the cosmic scale as well as here on earth.

On behalf of the Royal Academy of Science, it is my pleasure to congratulate you on your Nobel Prize in Physics.

About two thousand years ago, the first magnetic compass was made in China by stroking a piece of iron with a lump of magnetite. Such a compass always arouses much surprise, from the child who asks about the invisible force which aligns it along the north-south axis, to the scientist, who here confronts one of the very difficult problems of physics.

Three states of magnetism have long been recognised, dia-, para- and ferromagnetism. In the two former, the elementary magnets of the atoms behave independently of one another when subjected to a magnetic field. However, in ferromagnetism, which is many times stronger, they are aligned collectively, which makes the understanding of the physics much more difficult.

The first scientist who tried to explain magnetism was Ampère with his hypothesis about elementary currents. In 1907, Pierre Weiss found that there must be a special kind of force which aligned the elementary magnets, although he could not identify it. In his doctor's thesis in 1911. Niels Bohr showed that magnetism could not be caused by currents originating from the classical motion of electrical charges, but that something completely new was needed. Using the new ideas of atomic physics, Heisenberg in 1928 was able to give a qualitative explanation of the aligning force occurring in ferromagnetics.

To these three types of magnetism, Néel in 1932 added a fourth, anti-ferromagnetism. He found that for certain crystals adjacent elementary magnets could align themselves anti-parallel and not parallel as in ferro-magnetic materials. He deduced the existence of a new variant of the force postulated by Weiss and presented a model for crystals which are built up from two interlaced lattices with equally strong magnetic fields acting in opposite directions. Anti-ferromagnetism is an ordered state with important properties. Thus, Néel showed that the magnetic state should disappear at a temperature now known as the Néel point, in analogy with the Curie point. Similarly, other remarkable observations in the physics of the solid state were explained.

In 1948, Néel made another fundamental discovery with his explanation of the strong magnetism found in the ferrite materials, of which magnetite is one. He generalized his earlier assumption by assuming that the lattices could be of different strengths and could produce external fields. In magnetite, with three atoms of iron and four of oxygen, the effects of two of the iron atoms cancel out while the third gives rise to the magnetic field. It is remarkable that magnetite which in the hands of the Chinese was used to produce the first compass, is in fact not ferromagnetic, but, in Néel's terminology, ferrimagnetic.

Néel could present an accurate description of the behaviour of the new synthetic magnetic materials and so explain hitherto puzzling experimental observations. These developments have been of considerable technical importance, *e.g.* in computer memories and in high-frequency techniques.

Néel has made many other contributions, such as investigations in the theory of magnetic domains and the discovery of the effect found in small particles, called super-paramagnetism.

Professor Néel. I have attempted to describe your major discoveries which follow in the great French tradition of studies of magnetic phenomena.

I have particularly emphasised your discoveries of anti-ferro- and ferrimagnetism which have been of such importance in the shaping of modem theories of magnetism.

I have the pleasure and the honour to convey to you the most sincere congratulations of the Royal Academy of Science.

Professor Alfvén, Professor Néel. I invite you to receive the Nobel Prize in Physics from the hands of His Majesty the King.

HANNES ALFVÉN

Plasma physics, space research and the origin of the solar system

Nobel Lecture, December 11, 1970

1. Science and instruments

The center of gravity of the physical sciences is always moving. Every new discovery displaces the interest and the emphasis. Equally important is that new technological developments open new fields for scientific investigation. To a considerable extent the way science takes depends on the construction of new instruments as is evident from the history of science. For example after the development of classical mechanics and electromagnetism during the 19th century, a new era was started by the construction of highly developed spectrographs in the beginning of this century. For its time those were very complicated and expensive instruments. They made possible the exploration of the outer regions of the atom. Similarly, in the thirties the cyclotron - for its time a very complicated and expensive instrument -was of major importance in the exploration of the nucleus. Finally, the last decade has witnessed the construction of still more complicated and expensive instruments, the space vehicles, which are launched by a highly developed rocket technology and instrumented with the most sophisticated electronic devices. We may then ask the question: What new fields of research - if any - do these open for scientific investigation ? Is it true, also in this case, that the center of gravity of physics moves with the big instruments?

2. Scientific aims of *space research*

The first decade of space research mainly concentrated on the exploration of space near the Earth: the magnetosphere and interplanetary space. These regions earlier were supposed to be void and structureless but we now know that they are filled with plasmas, intersected by sheathlike discontinuities, and permeated by a complicated pattern of electric currents and electric and magnetic fields. The knowledge gained in this way is fundamental to our general understanding of plasmas, especially cosmic plasmas. Indirectly it will hence be important to thermonuclear research, to the study of the structure of the galaxy and the metagalaxy, and to cosmological problems. Our advancing knowledge in cosmical electrodynamics will make it possible to approach these fields in a less speculative way than hitherto. The knowledge of plasmas is also fundamental to our understanding of the origin and evolution of the Solar System, because there are good reasons to believe that the matter which now forms the celestial bodies once was dispersed in a plasma state.

The second decade of space research seems to display a different character, at least to some extent. As several of the basic problems of the magnetosphere and interplanetary space are still unsolved, one can be sure that these regions will still command much interest. However, the lunar landings and also the deep-space probes to Venus and Mars have supplied us with so many new scientific facts that the emphasis in space research is moving towards the exploration of the Moon, the planets, and other celestial bodies in the Solar System.

The first phase of this exploration is necessarily of a character somewhat similar to the exploration of the polar regions and other regions of the earth which have been difficult to reach: a detailed mapping-out combined with geological, seismic, magnetic, and gravity surveys and an exploration of the atmospheric conditions. However, when applying this research pattern to the Moon and the planets one is confronted with another problem, *viz.* how these bodies were originally formed. In fact many of the recent space research reports end with speculations about the formation and evolution of the solar system. It seems that this will necessarily be one of the main problems-perhaps the main problem-on which space research will center in the near future. Already at an early data NASA stated that the main scientific goal of space research should be to clarify how the solar system was formed. This is indeed one of the fundamental problems of science. We are trying to write the scientific version of how our Earth and its neighbors once were created. From a - shall we say-philosophical point of view, this is just as important as the structure of matter, which has absorbed most of the interest during the first two thirds of this century.

3. Plasma physics and its applications

Before we concentrate on our main topic: how the solar system originated, we should make a brief summary of the state of plasma physics. As you know, plasma physics has started along two parallel lines. The first one was the hundred years old investigations in what was called electrical discharges in gases. This approach was to a high degree experimental and phenomenological, and only very slowly reached some degree of theoretical sophistication. Most theoretical physicists locked down on this field, which was complicated and awkward. The plasma exhibited striations and double-layers, the electron distribution was non-Maxwellian, there were all sorts of oscillations and instabilities. In short, it was a field which was not at all suited for mathematically elegant theories.

The other approach came from the highly developed kinetic theory of ordinary gases. It was thought that with a limited amount of work this field could be extended to include also ionized gases. The theories were mathematically elegant and when drawing the consequences of them it was found that it should be possible to produce a very hot plasma and confine it magnetically. This was the starting point of thermonuclear research.

However, these theories had initially very little contact with experimental plasma physics, and all the awkward and complicated phenomena which had been treated in the study of discharges in gases were simply neglected. The result of this was what has been called the thermonuclear crisis some 10 years ago. It taught us that plasma physics is a very difficult field, which can only be developed by a close cooperation between theory and experiments. As H.S. W-Massey once said (in a somewhat different context): « The human brain alone is not able to work out the details and understanding of the inner workings of natural processes. Without laboratory experiment there would be no physical science today. »

The cosmical plasma physics of today is far less advanced than the thermonuclear research physics. It is to some extent the playground of theoreticians who have never seen a plasma in a laboratory. Many of them still believe in formulae which we know from laboratory experiments to be wrong. The astrophysical correspondence to the thermonuclear crisis has not yet come.

I think it is evident now that in certain respects the first approach to the physics of cosmical plasmas has been a failure. It turns out that in several important cases this approach has not given even a first approximation to truth but led into dead-end streets from which we now have to turn back.

The reason for this is that several of the basic concepts on which the theories are founded, are not applicable to the condition prevailing in cosmos. They are « generally accepted » by most theoreticians, they are developed with the most sophisticated mathematical methods and it is only the plasma itself which does not « understand », how beautiful the theories are and absolutely refuses to obey them. It is now obvious that we have to start a second approach from widely different starting points.

4. Characteristics of first and second approach to cosmic plasma physics

The two different approaches can be summarized in Table 1.

If you ask where the border goes between the first approach and the second approach today, an approximate answer is that it is given by the reach of spacecrafts. This means that in every region where it is possible to explore the state of the plasma by magnetometers, electric field probes and particle analyzers, we find that in spite of all their elegance, the first approach theories have very little to do with reality. It seems that the change from the first approach to the second approach is the astrophysical correspondence to the thermonuclear crisis.

Table 1

Cosmical electrodynamics

First approach	*Second approach*
Homogeneous models	Space plasmas have often a complicated inhomogeneous structure
Conductivity $\sigma = \infty$ Electric field $E_\parallel = 0$	σ depends on current and often suddenly becomes 0, $E_\parallel$ often $\neq 0$
Magnetic field lines are « frozen in » and « move » with the plasma.	Frozen-in picture often completely misleading.
Electromagnetic conditions illustrated by magnetic field line picture.	It is equally important to draw the current lines and discuss the electric circuit.
Electrostatic double layers neglected.	Electrostatic double layers are of decisive importance in low density plasmas.
Filamentary structures and current sheets neglected or treated inadequately.	Currents produce filaments or flow in thin sheets.
Theories mathematically elegant and very well developed.	Theories still not very well developed and partly phenomenological.

5. *The origin of the solar system*

From what has been said it is obvious that astrophysics runs the risk of getting too speculative, unless it tries very hard to keep contact with laboratory physics. Indeed it is essential to stress that astrophysics is essentially an application to cosmic phenomena of the laws of nature found in the laboratory. From this follows that a particular field of astrophysics is not ripe for a scientific approach before experimental physics has reached a certain state of development. As a well-known historic example, before the advance ofnuclear physics the attempts to understand how the stars generated their energy could not possibly be more than speculations without very much permanent value.

The problem of how the solar system originated has been the subject of a large number of highly divergent hypotheses. The reason for this has been that there was not enough basic knowledge of physics in the fields essential for the understanding of the phenomena and for a decision about which processes were possible.

However before we discuss any details of a theory of the origin and evolution of the solar system, it is essential to define what general character such a theory should have. In the past too much attention has been concentrated on the formation of planets around the sun. One of the unfortunate results of this is that many theories of the origin of the solar system have been based on theories of the early history of the sun. This is a very shaky basis because the formation of the sun (and other stars) is a highly controversial subject. Recognizing that the satellite systems of Jupiter, Saturn, and Uranus are very similar to the planetary system, and at least as regular as this system, it seems now more appropriate to aim at a general theory of the formation of secondary bodies around a central body, regarding the formation of the planetary system as only one of the applications of such a general theory.

The study of the sequence of processes by which the solar system originated has often been called *cosmogony,* a term which, however, is used in many other connections. As the origin of the solar system is essentially a question of the repeated formation of secondary bodies around a primary body, the term *hetegony* (from Greek *hetairos* or *hetes* = companion) has been suggested.

It seems likely (and is fairly generally agreed) that the sequence of events leading to the formation of the solar system is likely to have been as shown in Fig. 1(we are here following what has been called the « planetesimal » approach). A primeval plasma was concentrated in certain regions around a central body, and condensed to small solid grains. (Even the primeval plasma

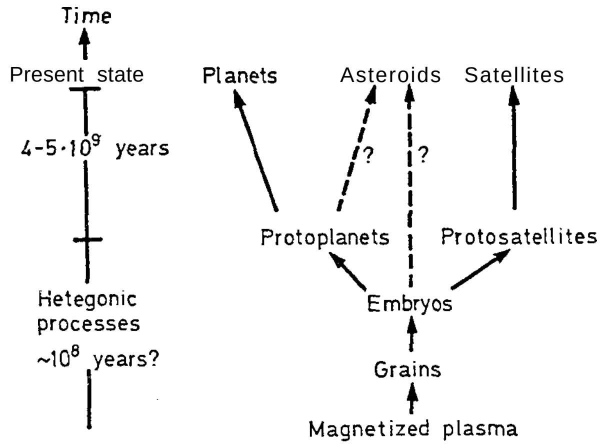

Fig. 1.

may have contained grains.) The grains accreted to what have been called embryos and by further accretion larger bodies were formed: planets if the central body was the sun, and satellites if it was a planet. The place of the asteroids in the hetegonic diagram is controversial. They have formerly been generally considered to be fragments of a broken-up planet, but there are now an increasing number of arguments for the view that they represent - or at least are similar to - an intermediate state in the formation of planets. A clarification of these two alternatives is important.

Even if the diagram of Fig. 1 is fairly generally accepted as it stands, this does not mean that the different processes are clarified. To a high degree they are still of a hypothetical character. Up to rather recently this has necessarily been the case because the basic processes have not been known very well. To some extent we have been in the same situation as the astrophysicists trying to clarify the energy generation in stars before the advent of nuclear physics. However, the situation seems now to be changing so that there is a good hope to bring the whole field of research from the state of a discussion of more or less bright hypotheses to a systematic scientific analysis.

6. Basic knowledge for the reconstruction of the hetegonic processes

Besides plasma physics, which we have already discussed, there are a number of other fields of research which are basic for the reconstruction of the hetegonic processes.

(1) Plasma chemistry means the field of research concerned with chemical reactions in a plasma. These are basically different from the reactions in non-ionized gases. It should also be considered to include the separation of different elements which takes place in an inhomogeneous plasma due to, *e.g.* temperature gradients and electric currents. Furthermore, the interaction between a plasma and a solid grain condensing from it is highly dependent on the state of ionization. The laboratory results and their application to cosmic conditions are relevant for the understanding of the different chemical composition of the celestial bodies.

For the next process in our evolutionary diagram, *viz.* the accretion of larger bodies from the initial condensation, the following fields of research are essential.

(2) Solid-body ***collisions.*** The grains which are the primary result of the condensation will move in Kepler orbits around the central body, but their motion will be disturbed by several effects. One of them is due to the mutual collisions. The relative velocities at these collisions may have any value from zero up to some 10 km/sec. This means that in many cases we are in the region of « hypervelocity » collisions. This is a field which is not yet understood very well. Available laboratory results seem to be scarce, and their application to cosmic conditions is uncertain because we know very little about the structure of the grains. Collisions between bodies with fluffy shock-absorbing surface layers are likely to differ from collisions between hard « marbles ». Meteorite studies are supplying us with some information. The Apollo results about meteoroid impact on the lunar surface are another important source of knowledge. In these cases, however, we do not gain very much information about the structure of the grains in space, because the particles we recover have either passed the terrestrial atmosphere or been destroyed by impact on the lunar surface.

(3) The study *Kepler motion in a viscous medium* is essential for our understanding of the evolution of the orbits of the grains and the embryos. From a formal point of view this problem is similar to some basic problems in plasma physics, which are also concerned with a large number of interacting particles. It turns out that in the neighborhood of a central body the condensed grains have a tendency to move in similar orbits, thus forming what have been called « jet streams » in space.

(4) Celestial mechanics serves of course as a general background for the whole hetegonic process. This field has been rejuvenated by the application of computer analysis to many of the problems which were formerly impossible to

handle. Connected with this is the discovery of the importance of *resonance phenomena* in the present structure of the solar system. It seems likely that at hetegonic times also resonances played a decisive role.

(5) The hetegonic processes took place 4 to 5 billion years ago. The evolution of the primary product of these processes into our present-day solar system has consisted of a number of relatively slow changes : geological forces have transformed the structure of the planets, tidal effects have braked the spins of some of the bodies (especially of the satellites), collisions have taken place in the asteroidal belt, and there have been meteor impacts on the planetary surfaces, etc. All these effects are important for the reconstruction of the state of the system immediately after the hetegonic processes ended. It is only after « correcting » for them that the solar system data we observe today are of value for the reconstruction of the hetegonic processes.

7. Space observations relevant to the hetegonic problem

From the analysis we have made it is evident that the background knowledge necessary for the understanding of the hetegonic processes is rapidly increasing through advances in several different fields of research. We shall now discuss the question of what sort of space missions are of special value for the study of the hetegonic problem.

Let us first state that many of the space missions which are carried out today or planned for the future give valuable contributions. Increased knowledge of the behavior of cosmic plasmas is gained by spacecraft carrying out plasma and particle measurements in the magnetosphere and interplanetary space. Further, meteor impacts on spacecraft supply us with information of the very small bodies in our environment, which are probably related to those small bodies out of which our present planets once accreted. Particularly important is the study of meteor impacts on the Moon (and on Mars). Hence these and other investigations « automatically » contribute to the background knowledge necessary for the solution of the hetegonic problem. But although this is satisfactory there are a number of crucial problems which cannot be solved unless space research is purposely directed towards solving them. We shall now discuss how this could be done.

8. Big bodies versus small bodies

It is usually thought that after the lunar landings the most important missions will be those to Venus, Mars, and the other planets. This is not necessarily true, because missions to asteroids and comets would be at least as interesting from a scientific point of view. As some asteroids are the closest neighbours of the Earth-Moon system, this would also be the easiest from a technical point of view.

Our analysis has indicated which fields of research will contribute to the clarification of different phases of the hetegonic processes. Plasma physics and plasma chemistry are important for the first phase, including the condensation of small grains. The study of meteorite and asteroid sized bodies will have bearing on the accretion. We can state as a general rule that the smaller the body the further will the study of it bring us back in time. Thus small bodies will be relevant to earlier periods more than large bodies. This means that it is essentially through studying the properties of small bodies in space that we can hope to understand the crucial phase in the formation of the solar system when most of the matter, which later formed the planets and satellites, was still dispersed.

There is evidence that during the formation of the planets and satellites a great deal of information about the formation processes was stored in them. However, to a large extent this information is either obliterated or inaccessible. The planets are likely to have accreted from « planetesimals ». The earliest phase of this accretion produced a small body, the matter of which may today be in the core of the planet, which means that it is inaccessible even if a manned spacecraft should land on the surface of the planet. There is also a possibility that, for example, convection in the interior of the planet has more or less completely obliterated the information once stored there. Concerning the surface layers, geological processes, including atmospheric effects, have mostly wiped out the surface traces of the hetegonic processes in Earth and probably also in Venus. In other bodies like the Moon and Mars, and probably also Mercury, there seems to be considerable information left, but only referring to the very last phase of the hetegonic processes.

Hence our conclusion is that studies of large bodies like the planets has only a limited value for the study of the origin of the solar system.

Asteroids, comets and meteoroids are different in this respect. Even if some of these bodies are fragments produced at collisions in space it is very likely that also these fragments contain considerable information about the condensation and the accretion processes. Because of the smallness of the bodies there is no heating or convection in their interior which can obliterate the information stored from the time when they were produced, and at least in the very small bodies and by the fragmented bodies their « interior » is accessible. Furthermore, a study of them will give us knowledge of the behavior of small bodies in space which will be valuable for the clarification of the hetegonic processes in general. We study in them intermediate products in the manufacturing of planets. They give us, so-to-say, snapshots showing the sequence of events when a planet like the Earth once was created.

9. Old and new fields of science

We shall now return from our odyssey in both space and time to our starting point - how new technologies displace the center of gravity of the physical sciences. The great revolution in physics which took place in the beginning of this century meant that classical mechanics and classical electrodynamics were considered to be more or less obsolete as fields of research. The new fields which attracted the interest were the theory of relativity and quantum mechanics and the experimental work was largely concentrated on the exploration of the electron shells of the atom. The advance of nuclear physics marked another step in a similar direction.

The new trend which is introduced by the rise of plasma physics and space research is to some extent opposite. In these fields quantum mechanics and the theory of relativity are not very important. Instead, classical mechanics has become rejuvenated and is essential not only for calculating the trajectories of spacecraft but also for the study of the motion of the natural celestial bodies during their evolutionary history. Also classical electromagnetism is of decisive importance to the theory of magnetized plasmas, which is basic both for thermonuclear research and for astrophysics in general. This does not mean that we should make the mistake-similar to what was made 50 years ago - of declaring the atomic and nuclear physics to be obsolete. They are not. They have an enormous inertia which will keep them moving, and they will produce many new and interesting results. But they have got very serious competitors, and remarkably enough these are the fields which earlier were declared dead that are now being resurrected.

It is possible that this new era also means a partial return to more understandable physics. For the non-specialists four-dimensional relativity theory, and the indeterminism of atom structure have always been mystic and difficult to understand. I believe that it is easier to explain the 3 3 instabilities in plasma physics or the resonance structure of the solar system. The increased emphasis on the new fields mean a certain demystification of physics. In the spiral or trochoidal motion which science makes during the centuries, its guiding center has returned to those regions from where it started. It was the wonders of the night sky, observed by Indians, Sumerians or Egyptians, that started science several thousand years ago. It was the question why the wanderers- the planets - moved as they did that triggered off the scientific avalanche several hundred years ago. The same objects are now again in the center of science - only the questions we ask are different. We now ask how to go there, and we also ask how these bodies once were formed. And if the night sky on which we observe them is at a high latitude, outside this lecture hall - perhaps over a small island in the archipelago of Stockholm - we may also see in the sky an aurora, which is a cosmic plasma, reminding us of the time when our world was born out of plasma. Because in the beginning was the plasma.

Biography

Hannes Olof Gösta Alfvén was born in Norrköping, Sweden, in 1908. His parents Johannes Alfvén and Anna-Clara Romanus were both practising physicians. Hannes Alfvén studied at Uppsala University from 1926, he obtained the degree of doctor of philosophy in 1934, in this same year he was appointed lecturer in physics at Uppsala University. In 1937 he became research physicist at the Nobel Institute for Physics in Stockholm, in 1940 he was appointed Professor in the Theory of Electricity at the Royal Institute of Technology in Stockholm, Professor of Electronics in 1945, and Professor of Plasma Physics in 1963. Since 1967 he is visiting professor of Physics at the University of California at San Diego.

In 1935 Hannes Alfvén married Maria Erikson Kerstin, they have five children : Cecilia, Inger, Gösta, Reidun and Berenike.

Professor Alfvén published a number of papers in physics and astrophysics, and the following monographs : Cosmical Electrodynamics, 1948 ; Origin of the Solar System, 1956; and together with C.-G. Fälthammar, Cosmical Electrodynamics, Fundamental Principles, 1963.

LOUIS NÉEL

Magnetism and the local molecular field

Nobel Lecture, December 11, **1970**

1. The Weiss molecular field

It has long been known that ferromagnetism originates from interactions between the atomic magnetic moments which tend to align them parallel to one another in spite of thermal motion. In order to obtain a quantitative explanation of the experimental facts, Pierre Weiss[1] assumed that a ferromagnetic behaved as a pure paramagnetic, *i.e.* a paramagnetic with carriers of independent moments and magnetization given by

$$J = f(H/T) \tag{1}$$

and that the field of the interactions was equivalent to that of an imaginary magnetic field *k,,* called the *molecular field,* which was proportional to the magnetization :

$$k\,, = n\,/ \tag{2}$$

and which added to the applied field H. One thus obtains, if H is sufficiently the weak, so-called Curie-Weiss law of magnetization:

$$J = \frac{CH}{T - 8} \quad \text{with } 0 = nC \tag{3}$$

If θ is positive, the susceptibility J/H becomes infinite when the temperature falls below the Curie point θ. From this temperature θ to absolute zero the substance assumes under the effect of its molecular field alone $h_m = nJ$ a certain *spontaneous magnetization,],.*

In this conception, the molecular field is considered to be a *uniform field* inside the ferromagnetic specimen. Furthermore, to give his theory a more complete nature, P. Weiss distinguished *the energy molecular field,* defined starting from the internal energy U by the relationship :

$$H_m = -(\partial U/\partial J) \tag{4}$$

from the *corrective molecular field of the equation of state* h_m defined, as stated above, by :

$$J = f\left(\frac{H + h_m}{T}\right) \tag{5}$$

A thermodynamic argument shows that the following relationship exists between these two fields:

$$H_m = h_m - T\frac{dh_m}{dT} \tag{6}$$

This manner of explanation, theoretically very satisfactory, enables the energy properties of ferromagnetic substances to be treated and explained elegantly and simply, but it has the disadvantage of leading one to admit as a dogma the uniformity of the molecular field and all that follows therefrom, in particular the linear variation of the reciprocal susceptibility with temperature above the Curie point. This certainly retarded progress in the theory.

2. *The local molecular field*

On the other hand, P. Weiss was unable to give a satisfactory solution to the problem of the origin of the molecular field. It was only in *1928* that Heisenberg found an interaction mechanism giving a satisfactory order of magnitude. From the point of view of interest to us, it is essential only to point out that this involved very short-range interactions, preponderant between first-neighbour atoms and negligible beyond the second or third neighbours.

If, then, we consider an alloy composed of two kinds of randomly distributed atoms A and B, the surroundings of the atoms may be very different and the approximation of a unique molecular field representing for all sites the action of the surroundings must be very poor. The theoretical problem of the rigorous treatment of such interactions is still far from being solved but, while retaining the simplicity of the theories based on the molecular field, we can improve them considerably by introducing what I have called local molecular fields.

Weiss' hypothesis amounts to writing that the energy E_c of the system of A and B atoms is expressed in the form:

$$E_c = -\tfrac{1}{2}n(J_A + J_B)^2 \tag{7}$$

where J_A and J_B denote the magnetizations of the A and B atoms respectively. Actually, since their energy is the sum of the contributions made by pairs of close-neighbour atoms, A-A, A-B, and B-B, we should rather write:

$$E_c = -\tfrac{1}{2}(n_{AA} J_A^2 + 2\, n_{AB} J_A J_B + n_{BB} J_B^2) \tag{8}$$

This amounts to abandoning the notion of a general molecular field and to introducing *local molecular* fields, *hA* = nAA jA + tZm]B, and *hB* = *nm /A* + ?ZgB]B, acting on the A and B atoms respectively.

I developed this way of looking at the problem for the first time[2] in 1932, and showed that the susceptibility χ of an alloy containing proportions P and Q of A and B atoms, with Curie constants C_A and C_B, was expressed by:

$$\chi = \frac{T(PC_A + QC_B) - PQC_AC_B(n_{AA} + n_{BB} - 2\, n_{AB})}{T^2 - T(P\, n_{AA}\, C_A + Q\, n_{BB}\, C_B) + PQC_AC_B(n_{AA}\, n_{BB} - n_{AB}^2)} \tag{9}$$

Instead of being represented by a straight line, the temperature dependence of the reciprocal susceptibility $1/\chi$ was now represented by a *hyperbola.*

I applied this theory to the interpretation of the properties of platinum-cobalt alloys and, a little later[3], to iron-cobalt, iron-nickel, and cobalt-nickel alloys.

At the time, this interpretation was not received with much favour. The existence of the straight Curie-Weiss lines in the $(1/\chi, T)$ representation was so well-established that when an experiment gave a curve it was rather preferred to break it down into a series of straight lines, each corresponding to a different magnetic state obeying the Curie-Weiss law.

3. *The fluctuations of the Weiss molecular field*

In Weiss' original theory the molecular field coefficient n and the Curie point θ both have positive values, and it is perfectly well understood that the molecular field, having a finite and positive value when the atomic moments are all parallel and in the same direction, can cause this ordered state. Weiss and his associates soon established that the paramagnetic properties of a number of salts were conveniently interpreted by a formula of type (3), but with a negative constant θ, *i.e.* with a negative molecular field. It could not be imagined how such a field could create an order at low temperatures.

On the other hand, Weiss' theory was incapable of explaining the properties of paramagnetic metals like manganese and chromium, which have a susceptibility almost independent of the temperature and too great to be attributable to a Pauli paramagnetism, *i.e.* due to electrons in an energy band.

Such was virtually the state of the problem in 1930, at which time I was interested in the difference between the two Curie points, *i.e.* in the fact that the Curie point θ_p deduced from the Curie–Weiss law, or paramagnetic Curie point, differed from the ferromagnetic Curie point θ_f, the temperature at which the spontaneous magnetization disappeared, whilst Weiss' theory implies the equality $\theta_f = \theta_p$. To explain this difference I invoked the thermal fluctuations of the molecular field, the existence of which seemed certain, since this field arises from the action of the neighbour atoms. These thermal fluctuations are *time-varying fluctuations*, but in this way I also came very naturally to interest myself *in spatially varying fluctuations* and to analyse more closely the consequences of the elementary law of magnetic interaction, namely the existence of a coupling energy between two close-neighbour atoms, equal to $w \cos \alpha$, where α denotes the angle between their magnetic moments.

4. Constant paramagnetism

The constant w may be either negative or positive: negative in the case of ferromagnetism and positive in the case of a negative molecular field. In the latter case recourse to the molecular field, permissible at high temperatures when the locations of all atoms are on an average identical, is no longer so at low temperature, since the atomic magnets must then tend to group themselves by pairs of atoms with antiparallel moments.

In this way I noted (ref. 5, p. 64) that in a *bcc* lattice, composed of two interlaced simple cubic sub-lattices, stable equilibrium at low temperature corresponded to an orientation of the atomic moments in one of the sub-lattices in a certain direction and an orientation in the other direction of the atomic moments in the other sub-lattice, as shown in Fig. 1 for the case of a plane lattice. This assembly deforms (Fig. 2) under the action of a magnetic field, and assumes an average induced magnetization, given for one atom by

$$\bar{\mu} = \frac{\mu^2 H}{6pw} \qquad (10)$$

where μ denotes the atomic magnetic moment and $2p$ is the number of neighbours. *In* this way a *constant paramagnetism* is obtained, *i.e.* a susceptibility independent of the field and of the temperature.

However, at high temperatures, we should observe a Curie–Weiss law of type (3).

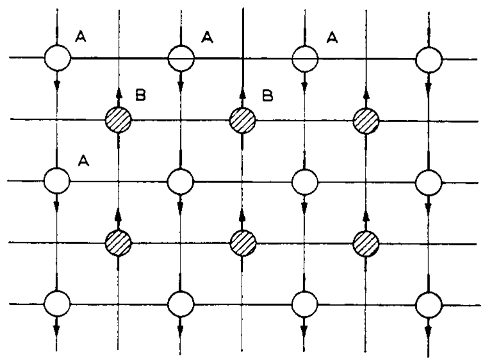

Fig. 1. Resolution of a plane lattice into two sub-lattices magnetized in antiparallel.

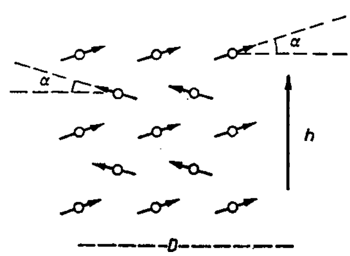

Fig. 2. Deformation of the antiparallel arrangement of atomic magnetic moments under the action of a field h.

In order to study the transition from constant paramagnetism to Curie–Weiss paramagnetism, I assumed for the sake of simplicity that the atoms could be grouped by pairs independent of one another, the internal energy of a pair being taken as $w \cos \alpha$. This led to the variation

$$\bar{\mu} = \frac{\mu^2 H}{3w}\left[1 - \frac{2w}{kT}\,\frac{1}{e^{2w/kT}-1}\right] \tag{11}$$

The temperature dependence of the reciprocal susceptibility is shown by the curve in Fig. 3. As T is raised from 0 to θ, the susceptibility varies by only 1.4%.

In another paper, which appeared almost at the same time[4], I proposed interpreting the constant paramagnetism of manganese and chromium by the previous mechanism, with values of θ respectively equal to 1720 °K and 4150 °K. To support this interpretation, I showed that on diluting manganese and chromium by copper, silver or gold, a progressive change from a constant paramagnetism to a Curie–Weiss paramagnetism was observed, as

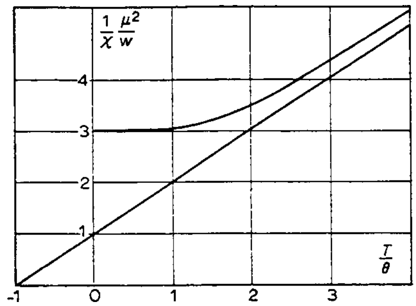

Fig. 3. Temperature dependence of the reciprocal susceptibility of au assembly of randomly-oriented pairs of atoms with anti-parallel moments.

might have been expected from the previous theory when the interactions, *i.e.* θ, are decreased.

In 1936, I again took up the theory of negative interactions[5], in the low-temperature region, allowing for the *coupling of the atomic moments with the crystal lattice.* The coupling energy was taken as $w'' \cos 2\theta$, where θ denotes the angle between the atomic magnetic moment and a privileged direction D of the lattice. It was then shown that the magnetic susceptibility is a complex function of the magnetic field H and the angle β that H makes with D, shown in Fig. 4 for various values of β at 10-degree intervals.

In particular, if the field is parallel to the privileged direction, the susceptibility, initially zero in weak fields, suffers a discontinuity for

$$H = H_0 = (8pww''/\mu^2)^{\frac{1}{2}}$$

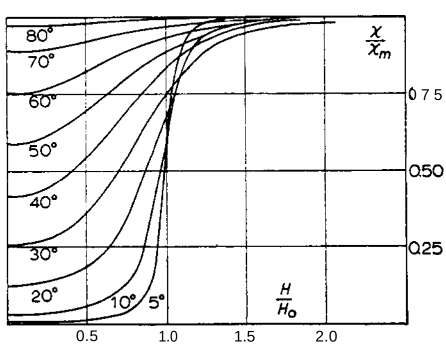

Fig. 4. Variation of the susceptibility of an assembly of atoms with antiparallel moments with the magnetic field and the direction of alignment of the magnetic moments with respect to the crystal lattice.

and takes a value $\chi_m = (\mu^2/4pw)$ independent of H: when H reaches the value H_0, the moments which were originally parallel to D and to H orient themselves abruptly in a perpendicular direction. Fifteen years later, in 1951, C. J. Gorter and his associates[5] observed this phenomenon for the first time in copper chloride, $CuCl_2 \cdot 2H_2O$, at 4.1 °K.

It was also deduced from this theory that in a polycrystalline sample, in which the directions D are randomly distributed, the susceptibility is weaker in weak fields than in strong fields, and varies schematically with temperature as shown in Fig. 5. It will be noted that there is no transition temperature: the change from constant paramagnetism to Curie-Weiss paramagnetism takes place perfectly smoothly.

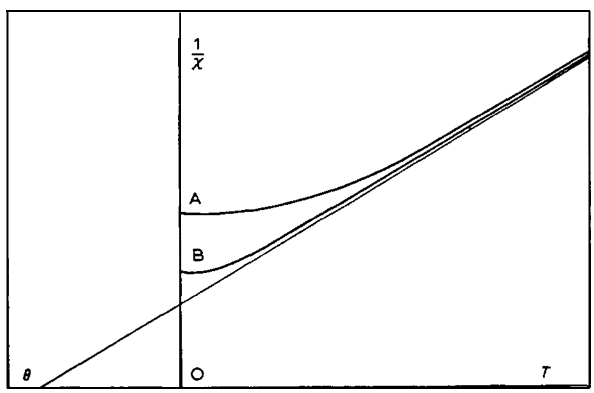

Fig. 5. Influence of the magnitude of the magnetic field on the susceptibility of an assembly of atoms with anti-parallel moments.

5. *Discovery of the antiferromagnetic transition point*

Still in 1936, I had the idea[5] of applying the theory of the local molecular field to the two sub-lattices A and B with magnetizations J_A and J_B used in the previous papers, and of representing the interactions by imaginary fields H_A and H_B with, at low temperature and for $H=0$, the fundamental relationship $J_B = -J_A$.

The result was that the two sub-lattices had to acquire spontaneous magnetizations in opposite directions, disappearing at a certain transition temperature θ_N, known nowadays as the Néel temperature, following a proposal made by C. J. Gorter. We were hence faced with a new kind of magnetic material, composed of the sum of two interlaced identical ferromagnetics spontaneously magnetized in opposite directions. Effects depending on the square of the spontaneous magnetization, such as the specific heat anomaly, should thus show the same variation as in ferromagnetic substances.

In the absence of coupling with the crystal lattice, the susceptibility remains constant in the interval $0 < T < \theta_N$ and then obeys a Curie–Weiss law for $T > \theta_N$, without suffering a discontinuity at the Curie point.

Two years later, Squire, Bizette and Tsaï[8] discovered that MnO possessed the predicted properties, in particular a transition temperature at $\theta_N = 116$ °K. Much later, in 1949, C. G. Shull and S. J. Smart[9] using a neutron diffraction, confirmed that the atomic moments effectively possessed the antiparallel orientations predicted by the sub-lattice theory.

F. Bitter then completed the theory[10] by calculating as far as the transition point θ_N on the susceptibility in a magnetic field parallel to the direction Δ of alignment of the antiparallel moments, and gave the name of antiferromagnetics to this new category of magnetic substances; in 1941 J. H. van Vleck[11] collected and reviewed the results obtained.

Finally, the situation may be summed up as follows, according to the orientation of the magnetic field H with respect to the antiferromagnetism direction Δ, *i.e.* the privileged direction of the crystal lattice to which the atomic mag-

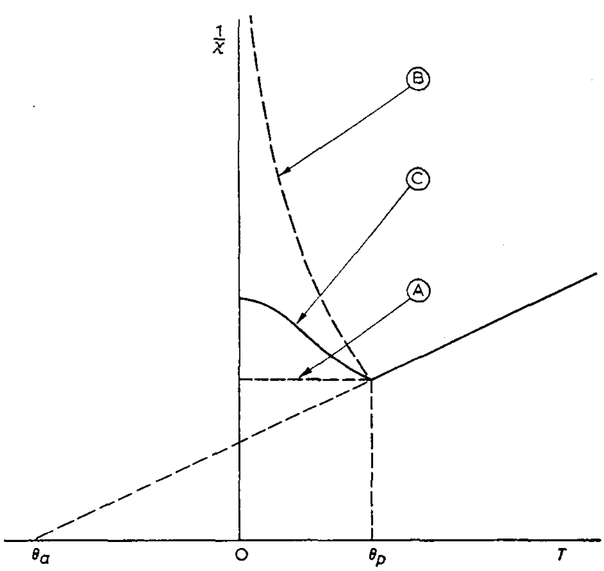

Fig. 6. Temperature dependence of the reciprocal susceptibility of an antiferromagnetic material.

netic moments are parallel, in one direction or the other, in the absence of an external applied field. When H is perpendicular to Δ, one obtains[7] curve A in Fig. 6. When H is parallel to Δ, curve B is obtained[10]. Finally, for a polycrystalline substance, the intermediate curve C is obtained, demonstrating at the Néel point θ_p the existence of a sharp maximum in the susceptibility, marked also by a sharp maximum in the specific heat. At temperatures below this maximum an antiferromagnetic order exists, which may be detected by neutron diffraction.

A large number of antiferromagnetic materials is now known: these are generally compounds of the transition metals containing oxygen or sulphur. They are extremely interesting from the theoretical viewpoint, but do not seem to have any applications.

6. *Other investigations in the field of magnetism*

It should be pointed out that the theoretical studies which I had developed from 1930 to 1938 had been carried out with the aim of determining the elementary interaction energies w between two close-neighbour magnetic atoms, whether similar or not. I expected to find a universal law giving w as a function of the distance between the interacting atoms alone, but this was obviously a rather oversimplified view and my expectations came to nothing. Nevertheless, positive results had been obtained and had made it possible to find interesting correlations between the anomalies of expansion, the effect of pressure, and the variation of the molecular field with temperature. Even now these questions are still on the agenda.

At all events, in 1938 I abandoned the problem of interactions and the local molecular field for about eight years to devote myself to other subjects such as the Rayleigh laws, fine particles, and the role of internal dispersion fields, to quote only those which led to the most important results.

The protection of ships against magnetic mines by a method I proposed, consisting of giving them a permanent magnetization equal and opposite to the magnetization induced by the terrestrial magnetic field, drew my attention to the laws of magnetization of ferromagnetics in weak fields, known as the Rayleigh laws. I was able to give an interpretation of these[12] based on the propagation of the Bloch walls in a randomly perturbed medium.

I also systematically studied the part played by the internal demagnetizing or dispersion fields, up to that time almost neglected. It was then possible to deduce a more correct theory of the laws of approach of technical magnetization to saturation taking into account the random orientation of crystallites[13] and the presence of cavities or non-magnetic inclusions[14], and to give a more general theory of the coercive force taking into account the irregularities of internal mechanical tensile stresses as well as the presence of cavities or non-magnetic inclusions[15]. On the same general lines, I developed a theory of magnetization for ferromagnetic single crystals[16] along successive modes, each specified by the number of *phases*, *i.e.* by the number of different categories of elementary domains with parallel spontaneous magnetizations. This theory, in complete agreement with the experimental facts, put the final full-stop to a subject which up to then had been very poorly understood.

Finally, I showed as far back as 1942 that sufficiently small ferromagnetic particles must enclose only a single elementary domain and, depending in their dimensions, must behave either as a superparamagnetic substance or as a set of small permanent magnets and show macroscopically a hysteresis loop with a high coercive force. In this way good permanent magnets can be made from particles of soft iron having only a shape anisotropy. These properties and their applications were developed by L. Weil. The publication of all these results was delayed until 1947[17] because of the German occupation and for reasons of patent rights.

7. *Magnetic properties of the spinel ferrites*

It was in 1947, after reading a paper by Verwey and Heilmann[18] on the structure of ferrites, that I returned to the study of these substances. These ferrites Fe_2O_3MO, with a spinel structure, where M is a divalent metal, are divided into two categories according to their magnetic properties: *paramagnetic ferrites,* in which M represents zinc or cadmium, and *ferromagnetic ferrites* in which M represents manganese, cobalt, nickel, etc. The latter have a considerable technical interest, for these ferromagnetics are electrical insulators, but their theoretical interest is also high in view of their curious properties, very different from those of the classical ferromagnetics: their saturation molecular moment of 1 to 5 μ_B (μ_B is the Bohr magneton), is much weaker than the total magnetic moment of the ions contained in the molecule, which varies from 10 to 15 μ_B. In addition, above the Curie point, the temperature dependence of the reciprocal susceptibility has a quite extraordinary hyperbolic shape, concave downwards, towards the temperature axis, and with a high temperature asymptote extrapolating back towards a negative absolute temperature.

Mlle Serres, to whom we owe some fine experimental work on ferrites, interpreted[19] the shape of the $(1/\chi, T)$ curve by the superposition of a temperature-independent paramagnetism due to the ferric ions, equal to that of α–Fe_2O_3. But as this paramagnetism is not an atomic property, we cannot see why the ferric ions should retain in the ferrites the same constant paramagnetism as in α-Fe_2O_3.

With regard to the crystalline structure, the metal ions occupy, in the interstices of a close-packed cubic lattice of oxygen ions, sites A surrounded by four O^{2-} ions and sites B surrounded by six O^{2-} ions. There are two categories of ferrites: *normal ferrites*, in which the two Fe^{3+} ions of the molecule occupy the two sites B and the M^{2+} ion site A, and *inverse ferrites*, where one of the Fe^{3+} ions occupies site A and the other one of the two sites B. From an X-ray examination, Verwey and Heilmann concluded that the ferromagnetic ferrites are inverse and paramagnetic ferrites are normal.

8. Foundation of the theory of ferrimagnetism

To interpret the magnetic properties, I assumed[20] that the predominant magnetic interactions were exerted between the ions placed at sites A and ions placed at sites B, and that they were *essentially negative.* At absolute zero, these strong negative interactions make the magnetic moments of the A ions align themselves parallel to one another to give a resultant moment $\mathbf{M}_{as}$ pointing in the opposite direction to the resultant $\mathbf{M}_{bs}$ of the magnetic moments of the B ions, these also being all parallel. The observable spontaneous magnetization is equal to the difference $|\mathbf{M}_{as} - \mathbf{M}_{bs}|$.

To study the consequences of these hypotheses, I applied to the two sub-lattices A and B the concept of the local molecular field developed earlier[2]. Letting λ and μ stand for the proportions of magnetic ions, all assumed to be identical ($\lambda + \mu = 1$), located in the A and B sub-lattices respectively, and designating by $-n$, $n\alpha$ and $n\beta$ the molecular field coefficients corresponding to the interactions AB, AA, and BB respectively, the discussion showed that the variation of the spontaneous magnetization with temperature would have to assume the rather extraordinary forms shown diagrammatically in Fig. 7, in which the capital letters correspond to the regions of the (α, β) plane shown in Fig. 8: region G at the bottom left corresponds to paramagnetism. All

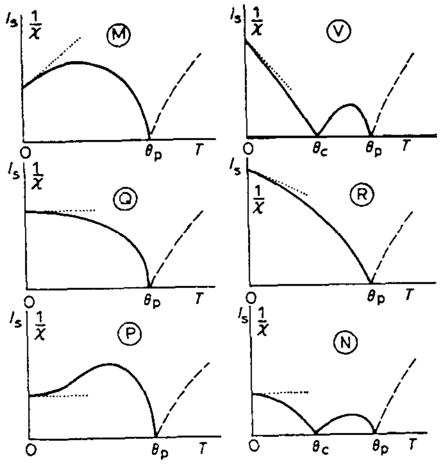

Fig.7. Temperature dependence of the spontaneous magnetization and the reciprocal susceptibility for a ferrimagnetic material: the various types possible.

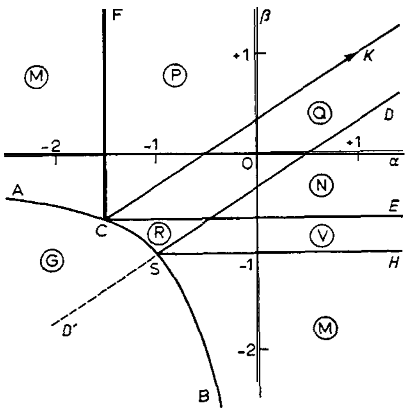

Fig. 8. Possible curve types in Fig.7, according to the values of α and β

the curve types illustrated in Fig. 7 were subsequently found experimentally.

Above the Curie point θ_p the ferrite becomes paramagnetic, with a susceptibility χ such that:

$$\frac{1}{\chi} = \frac{T}{C} + \frac{1}{\chi_0} - \frac{\sigma}{T-\theta} \tag{12}$$

with the notations :

$$\frac{1}{\chi_0} = n\,(2\,\lambda\mu - \lambda^2\alpha - \mu^2\beta)$$

$$\sigma = n\,C^2\,\lambda\,\mu[\lambda(1+\alpha) - \mu(1+\beta)]^2$$

$$\theta = n\,C\,\lambda\,\mu(2+\alpha+\beta)$$

We are hence confronted by substances whose ferromagnetism is due to *negative interactions,* which is quite a remarkable fact, and whose properties, appreciably different from those of classical ferromagnetics, justify a special name : I proposed calling them *ferrimagnetics.*

9. Comparison of the theoretical and experimental results

This theory immediately makes it possible to interpret[20] the properties of magnesium, lead, and calcium ferrites, those of magnetite Fe_3O_4 and of manganese antimonide Mn_2Sb. As examples, Figs. 9-12 show a comparison of the experimental and the calculated values in the case of Fe_3O_4 and of Mn_2Sb for the temperature dependence of the spontaneous magnetization and of the reciprocal susceptibility. Naturally, the same set of coefficients n, α and β is used in both cases. The result was encouraging, so a considerable experimental effort was launched immediately to test the theory, since the existing experimental material was fairly meagre.

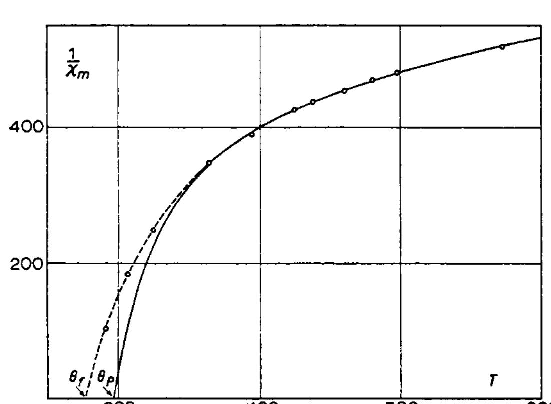

Fig. 9. Temperature dependence of the reciprocal susceptibility of magnetite (fitted curve and experimental points). Note the difference between the ferromagnetic and paramagnetic Curie points.

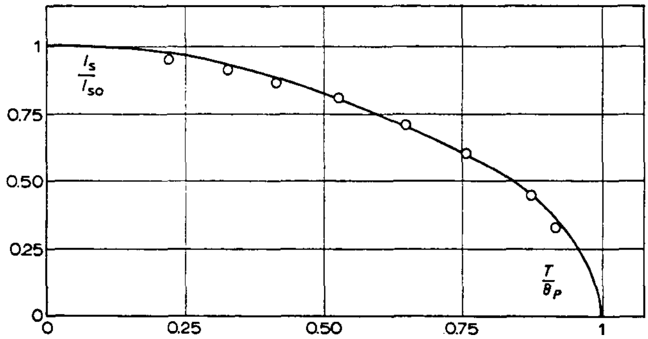

Fig.10. Temperature dependence of the spontaneous magnetization of magnetite(experimental points and curve calculated with the molecular-field coefficients deduced from the study of susceptibility).

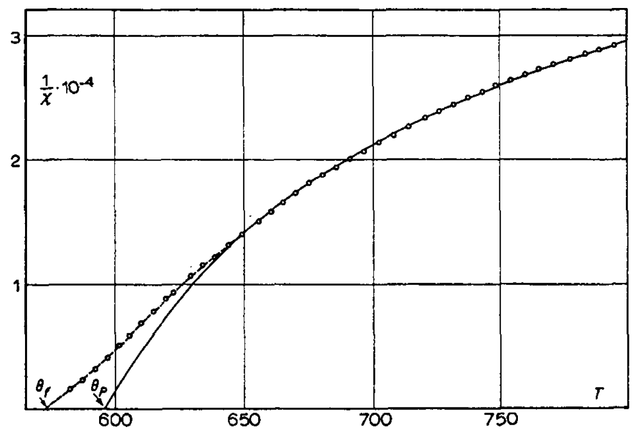

Fig. 11. Temperature dependence of the susceptibility of manganese antimonide (experi - mental points and fitted curve).

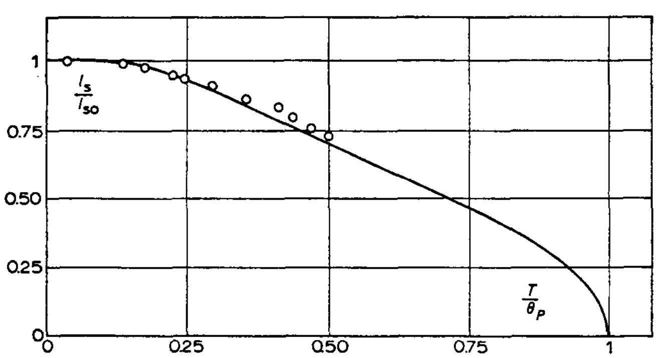

Fig.12. Temperature dependence of the spontaneous magnetization of manganese antimonide Mn_2Sb (experimental points and curve calculated with the molecular field coefficients deduced from the study of susceptibility).

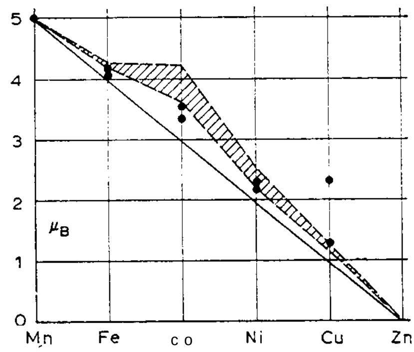

Fig.13. Saturation magnetization of certain ferrites at absolute zero, for various **M** ions.

In *pure inverse ferrites* Fe_2O_3MO, with M= Mn, Fe, Co, Ni, the saturation magnetic moment at absolute zero must simply be that of the M ion, since the magnetic moments of the two Fe^{3+} ions exactly cancel each other, located as they are on different kinds of sites. Fig. 13 enables the theory to be compared with experiment. The points correspond to the experimental results[21]. The straight line corresponds to the theoretical predictions with the « spin only » values of the magnetic moments, and the shaded region to the theoretical predictions corrected by taking into account the incompletely quenched orbital angular moment, deduced from the determination of the effective moment of the corresponding paramagnetic salts. The agreement is very satisfactory. With regard to the copper ferrite, experiment shows that the saturation magnetic moment depends on its heat treatment. Cooled slowly, this ferrite assumes the inverse structure whilst at high temperatures the Fe^{3+} ions are distributed at random over the sites A and B, for the difference in energy between the normal structure and the inverse structure is slight, of the order of magnitude of kT. A detailed analysis of the phenomenon shows that this interpretation is correct and hence lends support to the theory of ferrimagnetism[22].

For the same ferrites, it has also been possible to interpret appropriately the experimental results on the temperature dependence of the spontaneous magnetization[23] and of the susceptibility above the Curie point[24]. Figs. 14 and 15, relating to the three ferrites of iron, cobalt, and nickel, show the agreement between the theory and experiment.

Over and above this, on studying magnetite by neutron diffraction, Shull *et al.*[25] confirmed that the magnetic moments of the atoms placed at sites A were effectively pointed in the direction opposite to the moments of the atoms at sites B.

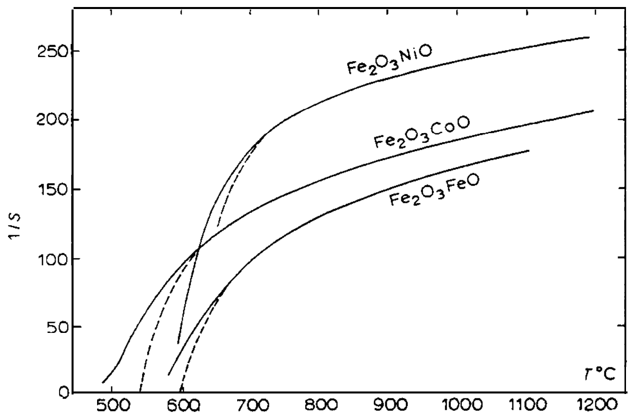

Fig.14. Temperature dependence of the inverse susceptibility ofdifferent ferrites (experimental curves shown in full lines, calculated curves dotted).

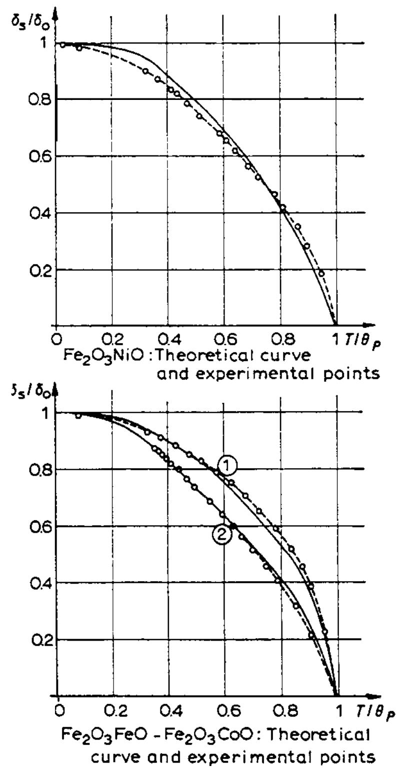

Fig. 15. Temperature dependence of the spontaneous magnetization of various ferrites (experimental points and curves calculated with the molecular-field coefficients deduced from the study of the susceptibility).

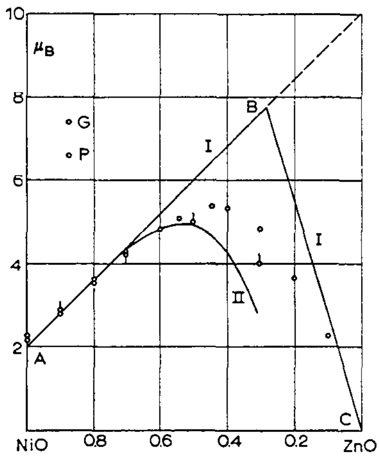

Fig. 16. Variations of the saturation magnetization of composite nickel and zinc ferrites with the zinc content (experimental points and calculated curves).

The theory of ferrimagnetism also makes it possible to explain the behaviour of composite ferrites, like the nickel-zinc ferrites with the formula $Fe_2Ni_{1-x}Zn_xO_4$. An atom of zinc which replaces an atom of nickel will locate itself on an A site, since that is the place preferred by zinc, simultaneously, the Fe^{3+} ion located on this A site will occupy at B the place left vacant by the departure of the Ni ion at the same time as its magnetic moment of 5 μ_B reverses direction. The net variation of the saturation magnetic moment is therefore equal to 8 μ_B, the difference between 10 μ_B resulting from the reversal and 2 μ_B resulting from the departure of the Ni^{2+} ion. The slope of the initial tangent to the curve giving the saturation magnetization as a function of x is therefore equal to 8 μ_B. The experimental results confirm this prediction[26] (Fig. 16).

10. The case of pyrrhotite

Shortly afterwards, the same theory made it possible to solve the riddle posed by pyrrhotite, Fe_7S_8, a ferromagnetic compound studied a long time ago by P. Weiss[27], which has a small saturation magnetic moment, of the order of 3 μ_B, whilst from its formula a value of about 30 μ_B might be expected. Pyrrhotite has the same crystal structure, Ewald's type B8, as the sulphide FeS, which is a typical antiferromagnetic: in the latter the successive planes of iron atoms, perpendicular to the ternary axis, are magnetized in a certain direction in one sense and the other in alternation. F. Bertaut has shown[28] that actually pyrrhotite is a compound containing vacancies whose formula should rather be written Fe_7S_8T, where T denotes vacancy, *i.e.* a site that would be occupied by an Fe^{2+} ion in the compound FeS, but that is vacant in Fe_7S_8. At low temperature the vacancies assume an ordered distribution and group themselves, as shown in Fig. 17, on the even-order iron planes: in short, the vacancies tend

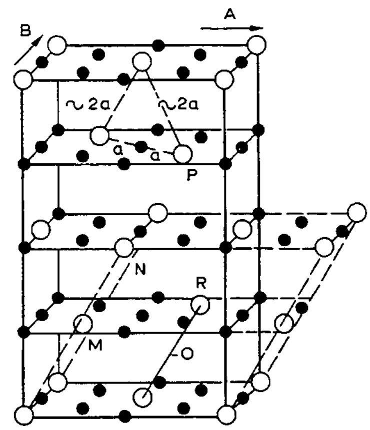

Fig. 17. Crystal structure of pyrrhotite at low temperature. The black circles represent the iron atoms and the white circles the vacancies. The odd-order iron planes and the oxygen atoms are not shown.

to space themselves as far as possible from one another. On the odd-order iron planes all the sites are occupied. The two sub-lattices equivalent in the case of FeS become different, and we are then dealing in the case of Fe_7S_8 with true ferrimagnetism. When the temperature rises above 900 °K, the ordering of the vacancies disappears and the two sub-lattices become statistically equivalent : correlatively, the paramagnetic susceptibility of pyrrhotite assumes values close to those of FeS.

11. Discovery of the garnet ferrites

There is no doubt that the garnet ferrites offer the finest illustration of ferrimagnetism. These substances, with the general formula $Fe_5M_3O_{12}$, where M is a trivalent rare-earth metal ion, form an important class of magnetic compound whose properties are explained simply and accurately by ferrimagnetism with three sub-lattices. Besides, since the metal ions entering into their composition can be replaced by a very wide variety of other ions, these compounds are of great interest in the theoretical study of interactions. From the point of view of applications, they are excellent insulators, can be prepared in large crystals, and have very sharp resonance lines; they can be used at very high frequencies in a large number of devices.

The history of their discovery begins in Strasbourg in 1950, where Forestier and Guiot-Guillain[29] heated an equimolecular mixture of Fe_2O_3 and M_2O_3 (M = rare earth metal) and obtained strongly ferromagnetic products whose Curie points ranged from 520 to 740 °K and to which they attributed a perovskite-type structure[30]. They then demonstrated the curious fact[31] that with M = Yb, Tm, Y, Gd, or Sm, these products had two Curie points spaced about a hundred degrees apart and varying regularly as a function of the atomic radius of M.

These results attracted the attention of research workers at Grenoble: Pauthenet and Blum[32] prepared a gadolinium ferrite and showed that besides the two Curie points, $\theta_1 = 570$ °K and $\theta_2 = 678$ °K, this compound had a third transition temperature $\theta_3 = 306$ °K, identifiable as a *compensation temperature* in the sense of the ferrimagnetism theory, *i.e.* as a temperature corresponding to a change of sign of the spontaneous magnetization (types V and N in Fig. 7).

To explain these facts, I suggested[33] that the Fe^{3+} ions had to form a ferrimagnetic arrangement A, with a structure independent of M, whose resultant spontaneous magnetization magnetized the B sub-lattice of the M^{3+} ions in the opposite direction. The molecular field h_A representing the action of A on B had to be sufficiently weak for the magnetization of B to be effectively equal to $C_B h_A / T$, at least above 100 °K, the temperature θ_3 being one at which the magnetization of B is equal and opposite to that of A. In particular, it followed from this that the temperature θ_3 had to be the lower the smaller is the Curie constant C_B of the M^{3+} ions. In agreement with these predictions, it was shown a few days later[34] that the ferrites of dysprosium and erbium also had compensation temperatures, located at 246 and 70 °K respectively. Despite these successes, this assumed structure was incompatible with that of a perovskite with formula $FeMO_3$.

Starting from the assumption of another compound mixed with the perovskite, Bertaut and Forrat[35] showed in January 1956 that it was a question of a cubic compound, $Fe_5M_3O_{12}$, of space group O_h^{10} Ia3d, with 8 molecules in the unit cell, of structure identical to that of the semi-precious stones known as *garnets* (Fig. 18). The primary ferrimagnetic arrangement comprises 24 Fe^{3+}

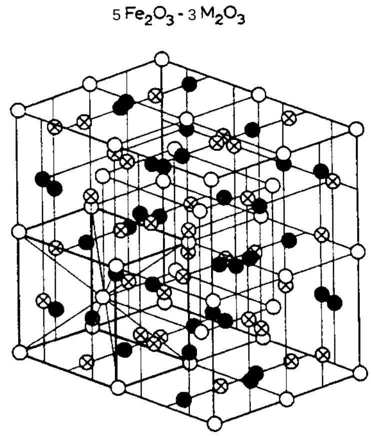

Fig. 18. Crystal structure of the garnet ferrites. o, Fe ion on site a; ⊗, Fe ion on site d; ●, M ion on site c. (after Strukturbericht and F.Forrat)

ions on sites d surrounded by 4 O^{2-} ions, and 16 Fe^{3+} ions on sites a, surrounded by 6 O^{2-} ions. This arrangement bound by strong interactions, has magnetic properties effectively independent of the nature of the ions M, to which it is coupled antiferromagnetically be weak interactions. It can be studied magnetically in the isolated state by taking a non-magnetic ion such as Y or Lu for M.

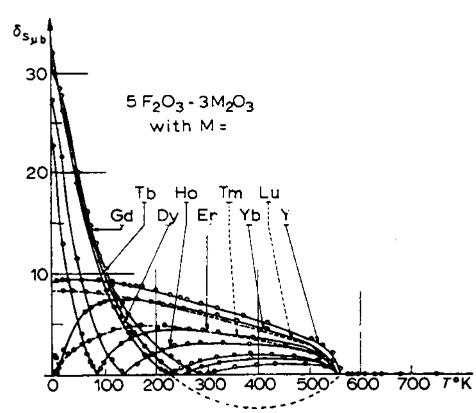

Fig.19. Temperature dependence of the spontaneous magnetization of a series of garnet ferrites. In particular, it will be noted how the compensation point varies with the atomic number of the rare earth.

12. Interpretation by ferrimagnetism with three sub-lattices

The generalization of the theory of ferrimagnetism to 3 sub-lattices and the representation of the interactions by means of 9 local molecular fields, 6 of them independent, raises no problem. The agreement of the calculated curves with the experimental results is very satisfactory, as shown in Fig. 19, taken from Pauthenet's fundamental paper[36] on the temperature dependence of the spontaneous magnetization of various garnets. The near identity of their Curie points well illustrates the fact that the nature of the M ions does not modify the ferrimagnetic arrangement of the Fe^{3+} ions. Above the Curie point, in the paramagnetic region, the agreement of the calculated curves with the experimental curves is also satisfactory (Fig. 20).

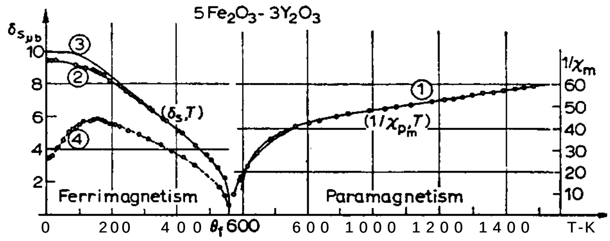

Fig. 20. Yttrium garnet. Temperature dependence of the spontaneous magnetization and of the inverse susceptibility; experimental points and curves calculated using the same molecular field coefficients. Curve (4) refers to an yttrium garnet containing a small amount of gadolinium.

13. Superexchange

Both in respect of antiferro- and ferrimagnetism, the theory makes it possible to deduce from the experimental curves the elementary interaction energy w between two close-neighbour magnetic atoms. This calculation undertaken for spinel ferrites and a few antiferromagnetics such as MnO or NiO brings out some new facts[20,37]. In these substances the interactions between neighbouring iron ions are negative, at exactly the mutual distances for which they are positive in the pure metal or in alloys. It is further observed that there are considerable interactions between magnetic atoms separated by oxygen atoms, which ought to act as screens. The answer is even more revolutionary: it is actually a case of *superexchange interactions,* in which the oxygen atom separating two iron atoms plays an essential part. A considerable time ago

Kramers had already predicted[38] the possibility of such interactions, but this was the first time that their objective existence had been clearly demonstrated.

Thus, in oxides and ferrites exchange interactions of the classical type M-M coexist with superexchange interactions of the type M-O-M; this, then, is first-class material for testing the theories of the various interactions.

14. *Conclusions*

Despite its rather naive simplicity, the local molecular-field method has had some undeniable successes in linking up, in an intellectually very satisfying way, a large number of already known facts and in leading to the discovery of new facts.

It should be noted that all the structures discussed in this paper are *collinear:* all the atomic magnets are on average (in time) parallel, in one sense or other, to the same direction. However, the method of the local molecular field may be extended also to non-collinear structures, such as that of *helimagnetism*, which Yoshimori and Villain discovered independently and absolutely unexpectedly: in this way the phenomena can be interpreted remarkably simply and specifically.

In spite of everything, it hardly seems recommendable to extend the method to more complex structures, such as the umbrella structure, requiring the main crystal lattice to be broken down into a large number of sub-lattices. Indeed, under these conditions, an atom belonging to a given sub-lattice has only a very small number of neighbours in each of the other sub-lattices, often it has only one or two. The molecular field method, which consists of replacing the instantaneous action of an atom by that of an *average atom*, has many more chances of leading to a correct result as it deals with a greater number of atoms. It is probably the more correct, the higher the atomic spin number. Independently of this problem, the method applied to a large number of sub-lattices loses its main advantage of simplicity.

The method also has more insidious traps. If the parameters are suitably chosen, it can lead to the calculation of curves showing the temperature dependence of the spontaneous magnetization, or of the paramagnetic susceptibility, which coincide remarkably with the experimental results, say to within a few parts in a thousand. Under these conditions it could be expected that the elementary interaction energies deduced from these parameters would correspond to the true values with the same accuracy. Nothing is farther from the truth: errors of 10-20% and even more are made. Some prudence is therefore indicated.

On the other hand, recourse to the local molecular field seems essential, for the most rigorous methods lead to inextricable complications. It should be remembered that we still do not have the rigorous solution to even the simplest case, this being the case of a single cubic lattice with identical atoms of spin 1/2 and interactions reduced to those exerted between close-neighbour atoms. What, then, are we to think of the case of garnets with 160 atoms in the unit cell, spins of up to 5/2, and at least six different coupling constants? The imperfections in molecular field methods must be treated with indulgence when we consider the simplicity with which the successes discussed in the first lines of these conclusions were obtained.

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Biography

Louis Néel was born in Lyons on 22 November 1904. In 1931 he married Hélène Hourticq; they have three children, Marie Françoise, Attachée d'Administration at the Conseil d'Etat, Marguerite, married to Guély, Professeur agrégée d'histoire, and Pierre, who is a television producer. Louis Néel studied at the Ecole Normal Supérieure in Paris from 1924–1928, where he was appointed lecturer in 1928. In 1932 he obtained the degree of Doctor of Science at the University of Strasbourg, where he was appointed Professor at the Faculty of Science (1937–1945). He was Professor in Grenoble since 1945. In 1946 he became Director of the laboratory for electrostatics and metal physics (Centre National de la Recherche Scientifique). From 1954 until 1970 he was Director of the Institut Polytechnique de Grenoble and of the Ecole Française de Papeterie; in 1970 he was appointed President of the Institut National Polytechnique in Grenoble. He served as director of the Centre d'Etudes nucléaires de Grenoble from 1956 to 1970. From 1949 to 1969 he was a member of the Board of Directors of the C. N. R. S.; scientific adviser to the French Navy since 1952; French representative at the Scientific Committee of the North Atlantic Treaty Organization.

Louis Néel began his first research work on magnetism between 1928 and 1939 in Professor Weiss' laboratory in Strasbourg. Called up for war service in 1939, he worked on the defence of ships of the French fleet against German magnetic mines and invented an effective new method of protection (neutralization). After the Armistice of 1940, he went to Grenoble and established the Laboratoire d'Electrostatique et de Physique du Métal, which in 1946 became one of the external laboratories of the Centre National de la Recherche Scientifique. This laboratory extended rapidly and gave rise to new laboratories; even so, it still has a staff of more than 250 at the present time.

In 1956 Louis Néel created and subsequently developed, as part of the French Atomic Energy Commission, the Centre d'Etudes Nucléaires de Grenoble. He also contributed to the decision to install the Franco-German high-flux reactor in Grenoble (1967).

Although he continued with research, sometimes critical and difficult, on the specific heat of nickel, Louis Néel has mainly concentrated on theoretical problems, which have formed the subject of more than 150 publications. Besides his discovery of the concepts of antiferromagnetism and ferrimagnetism and its consequences, for which he was awarded the Nobel Prize, Louis Néel tackled and solved a number of other problems and extended our knowledge of many aspects of magnetism. The most important of these are as follows: theory of Rayleigh's Laws; magnetic properties of fine grains; magnetic viscosity; internal dispersion fields; superantiferromagnetism; and hysteresis.

The following distinctions and honours have been awarded to Professor Néel: Chevalier de la Légion d'Honneur (military) in 1940, Officier in 1951, Commandeur in 1958, and Grand Officier in 1966; Croix de Guerre with palms (1940); Commandeur de l'Ordre des Palmes Académiques (1957); Chevalier du Mérite Social (1963); Holweck Prize (1952); Gold Medal of the Centre National de la Recherche Scientifique (1965). He is a member of the French Academy of Science (Paris, 1953); a foreign member of the Soviet Academy of Science (1959), the Royal Dutch Academy of Science (1959), the Deutsche Akademie der Naturforscher Leopoldina (1964), the Rumanian Academy (1965), the Royal Society (London) (1966), and the American Academy of Arts and Sciences (1966).

Prof. Néel is honorary doctor of the Universities of Graz (1948), Nottingham (1951), Oxford (1958), Louvain (1965), Newcastle (1965), Coimbra (1966), Sherbrooke (1967), and Iassy (1971). He holds an honorary degree from the Polytechnic Institute of Turin (1960). He is an honorary member and former president (1957) of the Société Française de Physique. From 1963 to 1966 he was President of the International Union of Pure and Applied Physics.

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