Nobel Prizes: 1970 Awards Honor Three in Physics and Chemistry

The Nobel prizes in physics and chemistry were announced last week. Following are appreciations by researchers familiar with the recipients and their work.

Physics

1. Swedish Iconoclast Recognized after Many Years of Rejection and Obscurity

It is fitting that the massive contributions of Hannes Alfvén to the fields of plasma physics, space physics, and astrophysics be recognized with a Nobel prize. For much of Alfvén's career, his ideas were dismissed or treated with condescension; he was often forced to publish his papers in obscure journals; and he was continually disputed by the most reknowned senior scientist working in the field of space physics. Even today there is a rather pervasive unawareness of Alfvén's multifaceted contributions to fields of physics where his ideas are used with apparently little appreciation of who originated them.

Alfvén's approach to physics is based on deep physical insight and an extraordinary physical intuition. He is quick to place new observations into a framework larger than that required to explain the observations themselves. This is perhaps best illustrated by his discovery of hydromagnetic waves (which, incidentally, is perhaps the only work closely identified with his name in that these waves are often called Alfvén waves). Alfvén came to the discovery of hydromagnetic waves through thinking about the nature of sunspots. He reasoned that the sunspots were magnetic fields imbedded in the highly conducting body of the sun. These magnetic fields must be caused by currents, and the currents must be carried by the particles of ionized gas that constitute the sun. Yet, he reasoned, the current flowing through a magnetic field must experience a force causing both the currents and field to move. Alfvén thought through the entire phenomenon on purely physical grounds, and he arrived at the conclusion that a form of wave (an electromagnetic wave) could propagate through a highly conducting medium such as the ionized gas of the sun or through ionized gas anywhere. He then worked out the mathematical theory, which was published in 1942 in the open, free-wheeling letters section of *Nature*. This work was, for several years, condescendingly dismissed.

At the time Alfvén published his paper, Maxwell's theory of electromagnetism was a well-established, well-oiled edifice. Electromagnetic theory was regarded as a subject for textbook pedagogy and engineering applications. It was "well known" that electromagnetic waves could penetrate only a very short distance into a conductor. The theory was developed and experimentally verified that, as the conductor got better and better, the depth of penetration would go to zero. Thus, with a perfect electrical conductor, there could be no penetration of electromagnetic radiation. But, Alfvén was proposing a form of electromagnetic wave that could propagate in a perfect conductor with no attenuation or reflection. Alfvén's discovery was generally dismissed with such remarks as "If such waves were possible, Maxwell himself would have discovered them." It was not until 6 years later, during his first visit to the United States, when he gave several lectures on hydromagnetic waves, that his work was recognized as both correct and significant.

An oversimplified statement of what had occurred during this visit was that Fermi heard his lecture at the University of Chicago, nodded his head and said, "Of course." The next day the entire world of physics said, "Oh, of course."

Aside from the discovery of hydromagnetic waves, Alfvén's name has not been clearly associated with many of his important contributions in astrophysics and space physics. For example, in the early 1930's, cosmic rays were commonly thought to be gamma rays filling the entire universe. When they were discovered to be charged particles, Alfvén, in 1937, made the novel suggestion that the galaxy contained a large-scale magnetic field and that the cosmic rays moved in spiral orbits within the galaxy, owing to the forces exerted by the magnetic field. He argued that there could be a weak magnetic field pervading the entire galaxy if there was a very small amount of ionized gas spread throughout the galaxy.

This ionized gas could carry the electrical currents that could then create the galactic magnetic field. Alfvén's work was again dismissed, this time on the grounds that it was well known that interstellar space was a vacuum and certainly could not support the electrical currents he was proposing. But he had started the scientific community thinking about an idea that was later to become very fashionable. When the idea of a large-scale galactic magnetic field was recovered, it was done without directing attention to the man who made it easy to think and talk of such ideas.

Perhaps the most interesting examples of the difficulties Alfvén faced in his scientific career occur in the field of interplanetary and magnetospheric physics. Interplanetary space was commonly considered to be a good vacuum, disturbed only by occasional comets and streams of plasma passing from the sun out into interplanetary space. Occasionally these solar-plasma streams would strike the earth's dipole field and perturb it somehow, causing interesting geomagnetic and auroral phenomena.

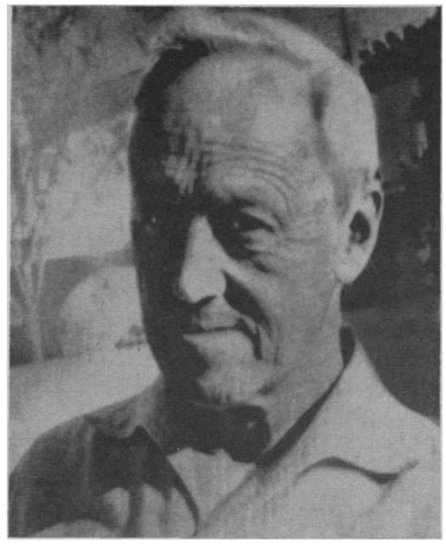
Alfvén produced some remarkably important and prophetic work in space physics starting in the late 1930's. For example, he discovered such concepts as the theorem of frozen-in flux wherein a plasma is regarded as frozen onto or tied onto the magnetic lines of flux that pass through the plasma. Thus when the plasma moves, the magnetic field moves with it. This powerful, simplifying tool has been used to great advantage in most of the recent developments in space physics. Yet, many who use this theorem do not know that it was one of Alfvén's discoveries.

The theorem of frozen-in flux is valid for only a specified set of physical conditions. The theorem is sometimes used in places where it either is not, or might not be, valid. It is typical of Alfvén, and at the same time somewhat amusing, to hear him now going from institution to institution trying to "unsell" his theorem of frozen-in flux on the grounds that the conditions for its validity are highly specialized and may not occur in some of the cases in which the theorem is being applied. These latest pleas have been disregarded, much as was his original work.

Alfvén also invented the guiding center approximation wherein the complex motion of a charged particle in a dipole magnetic field (such as the earth's) could be calculated in a very simple way. Before he developed this approximation, the actual particle trajectory had to be laboriously calculated point by point, using Störmer orbit theory, throughout its complex spiral motion. Finally, the entire concept of electric fields in space and their effect on the motion of charged particles was developed by Alfvén more than 30 years ago. Again, many users of these ideas do not recognize that they are Alfvén's.

It is usual in science that one or two major discoveries place their author in the ranks of leading authorities with great influence and power within their field of expertise. This has certainly not been the case with Alfvén. It is almost (but not quite) incredible to realize that at no time during these fruitful years had Alfvén been recognized as a leading innovator by the large majority of those in the scientific community who use his work.

For example, in 1939 he wrote a remarkable theory of magnetic storms and the aurora. This work contains ideas that have influenced much of the contemporary magnetospheric theory. (The term magnetosphere is of relatively recent origin, and refers to the region of space that contains the earth's magnetic field.) This paper is even more remarkable when compared with work published during this same time period by others. In this 1939 paper Alfvén used his guiding-center approximation and introduced the concept of electric field drift for plasma within the geomagnetic field. He also introduced the concept of a ring current formed from quasi-trapped radiation. He submitted the paper to the leading American journal, *Terrestrial Magnetism and Atmospheric Electricity,* only to have it rejected on the grounds that it did not agree with the theoretical calculations of the late Sydney Chapman and his colleagues. Alfvén was forced to publish this particular paper, along with much other important work, in journals that must, in candor, be called obscure. The particular paper mentioned above was published in *Kungliga Svenska Vetenskapsakademiens Handlingar.* Other important work was published in journals such as the Swedish *Arkiv för Matematik, Astronomi och Fysik.* In fact, as far as I am aware, Alfvén has seldom been afforded the luxury of the nearly automatic acceptance of papers that recognized authorities usually enjoy.



Hannes Alfvén

I can offer here only a hypothesis as to why this situation developed. Sydney Chapman, some 20 years Alfvén's senior, was already well established as the leading scientist in what later would be called space physics. Chapman was an extremely prolific author. His list of publications, starting in 1910, only 2 years after Alfvén's birth, show an average output of nearly one paper per month during the years that Alfvén started his work in space physics. Alfvén published three or four papers per year. Chapman, with incredible vigor, attended nearly every major scientific meeting; Alfvén was much more restricted in his travels. Chapman formulated problems in space physics that he could handle with complete mathematical rigor. Since the problems were enormously complex, he would simplify the physical situation until he had a model that he could dispatch with mathematical exactness. This was Chapman's style. He had little use for Alfvén's intuitive approach that would sacrifice mathematical rigor in order to retain a physically real situation.

The authority of Chapman in this field was such that his opinions carried. Alfvén's work was drowned by Chapman's output. How could this have happened? After all, do not we all believe in the objectivity of science? Yet, when I entered the still small field of space physics in 1956, I remember clearly (and now with some embarrassment) that I fell in with the crowd in believing, for example, that electric fields could not exist in the highly conducting plasma of space. It was 3 years later that I was shamed by S. Chandrasekhar into investigating Alfvén's work objectively. My degree of shock and surprise in finding that Alfvén was right and his critics wrong can hardly be described. I went through a period of what might be termed rebirth in learning that most of my tools of research and many of my concepts regarding the behavior of plasmas on a cosmic scale were given to me by this extraordinary personality. I learned that a cosmic ray acceleration mechanism basically identical to the famous mechanism suggested by Fermi in 1949 had been earlier put forth by Alfvén. However, Alfvén explained his mechanism in terms of the concept of large-scale electric fields existing within a highly conducting plasma in space. At that time, common wisdom prohibited the acceptance of this concept and hence of Alfvén's acceleration mechanism.

Alfvén collected much of his early work into a book *Cosmical Electrodynamics* published by Oxford University Press in 1950. This book, being more generally available than the relatively obscure journals that carried his original work, had great impact on much work in space physics and plasma physics. For example, Lyman Spitzer, Jr., in his monograph *Physics of Fully Ionized Gases,* states that the introductory chapter closely follows the presentation of Alfvén.

Even before the discovery of the Van Allen radiation belt, S. F. Singer suggested that the decrease in the earth's magnetic field that occurs during geomagnetic storms was caused by particles moving in trapped orbits in the geomagnetic field as described by Alfvén. We now believe that this model is essentially correct and, furthermore, that the particles get into the inner part of the magnetosphere through a form of electric field drift motion that was pointed out by Alfvén in his 1939 paper. After the discovery of the Van Allen radiation belt, the use of the guiding center approximation became so common that the *Journal of Geophysical Research* actually published a paper that amounted to a rediscovery of the Störmer orbit theory that was originally published in 1907. Such was the power of the tools provided by Alfvén.

Alfvén writes and speaks with a style that makes his ideas clearly understood. But new ideas are sometimes slow to be accepted and sometimes are even unwelcome. Thus, he is often placed in the role of an iconoclast. His presence in the United States follows from some arguments he has had with the Swedish government on topics ranging from education to the construction of a nuclear power plant. (For example, he advised the Swedish government that the nuclear reactor they were building would never work. The turbine for that nuclear reactor is now being driven by a conventionally fueled boiler.)

Some of his feelings regarding relations with government are revealed in a science fiction work *The Tale of the Big Computer* published under the pen name of Olaf Johannesson (Coward-McCann, Inc., New York, 1968). In this book, he tells the story of how computers are steadily improved until they take over control of the government and then of the entire earth. As one finishes this book, he is left with a perhaps chilling thought, "How will it be possible to prevent this from happening?"

Alfvén has most recently been working on the problem of the origin of the solar system. With his characteristic ability to generalize from a specific problem to a universal truth, he has started with the observation that planets are commonly formed with satellite systems. He thus works from the basic premise that a theory that explains the origin of the solar system must also explain, by the same mechanism, the origin of the satellite systems. He has pointed out the vital role that a voyage to the asteroids could play in unraveling the early history of the solar system, and he has tied it all together by introducing new concepts regarding the role of plasma physics in the early stages of formation of our solar system.

The research described here typifies the variety and the ingenuity of Alfvén's contributions to our understanding of physics. It seems both proper and just that the first Nobel prize awarded to a space physicist should go to Hannes Alfvén who has contributed so much to this field.

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2. French Research Leader Receives Award for Accomplishments in Solid State Field

One of the recipients of the 1970 Nobel prize in physics is Louis Néel of the University of Grenoble, the ninth Frenchman to be so honored. His award, "for fundamental work and discoveries concerning antiferromagnetism and ferrimagnetism which have led to important applications in solid state physics," is the first in the area of solid state physics since Shockley, Brattain, and Bardeen shared the prize in 1956. The fundamental work referred to in the citation for the prize was carried out relatively early in Néel's career. For example, the characteristic properties of an antiferromagnetic medium were first put forward by him in work appearing from 1932 to 1936. (In these early papers he referred to constant paramagnetism rather than antiferromagnetism because of the nature of the magnetic susceptibility of such a medium at low temperature.) A definitive theoretical treatment of the magnetic properties of ferrites (and of ferrimagnetism) was published in 1948. These papers laid the theoretical foundations for a fundamental understanding of the microscopic magnetic properties of solids upon which Néel himself in later investigations, and J. H. Van Vleck, P. Anderson, and T. Nagamiya, among others, have subsequently built.

It was early recognized that two conditions are necessary for the existence of ferromagnetism: (i) the atoms must have a net magnetic moment due to an unfilled electron shell and (ii) the exchange integral J between near neighbor atoms must be positive. Néel argued that for many of the nonferromagnetic substances containing magnetic atoms the exchange integral is negative and that the lowest energy state should correspond to a maximum number of antiparallel moment pairs. He treated as a special case, now considered the classical antiferromagnet, a structure divided into two interpenetrating sublattices, such that atoms on one sublattice have nearest neighbors which are only on the other sublattice. With a negative exchange interaction, and with near neighbor interactions alone, Néel predicted that the structure should undergo an ordering temperature below which the spontaneous magnetization of either sublattice has the same temperature dependence as that for an ordinary ferromagnetic substance. However, the magnetization directions for the two sublattices are antiparallel so that no net spontaneous magnetization exists. At very low temperatures all of the atoms on one sublattice have their moments pointing in one direction, and those of atoms on the other sublattice are antiparallel to the first. Néel showed that the magnetic susceptibility of such a system is nearly constant at low temperatures.

Materials which exhibit these properties have been designated as antiferromagnetic, and the critical ordering temperature for antiferromagnets is universally called the Néel temperature, T_N, in analogy with the corresponding Curie point T_C for ferromagnets. Above T_N, the susceptibility of an antiferromagnet, as predicted by Néel, follows the Curie-Weiss law

$$X = c/(T + \theta)$$

with a negative paramagnetic Curie temperature θ, where X is the susceptibility and c is the Curie constant.

At the time of Néel's researches the methods available for detecting or demonstrating antiferromagnetism experimentally—namely, the measurement of magnetic susceptibility or of specific heat—were relatively indirect. Beginning in 1949 with experiments at the Oak Ridge National Laboratory by C. Shull, E. O. Wollan, and W. Strauser, antiferromagnetic spin configurations were determined directly by neutron diffraction techniques in complete agreement with the theory, the bases of which were given by Professor Néel.

The word ferrimagnet was proposed by Néel to describe compounds such as the ferrites. Ferrites are a class of compounds with the general formula $MOFe_2O_3$ where M is a divalent metal such as Cu, Mg, Ni, or Fe (in which case the compound is magnetite

Fe_3O_4). At room temperature these substances are generally ferromagnetic, in the sense that they exhibit a spontaneous magnetization, but the magnitude of the saturation magnetization is generally low, of the order of one or two Bohr magnetons per iron atom. Above the Curie point the magnetic susceptibility X does not obey a simple Curie-Weiss law, but instead the curve of $1/X$ plotted against T is strongly concave with respect to the temperature axis. These observations are difficult to understand since Fe in the ferrites exists in the form of trivalent ions (except for magnetite) with a magnetic moment of five Bohr magnetons at saturation. In addition, one would expect that the ferrites would follow a Curie-Weiss law in the paramagnetic region with a Curie constant equal to that of the ionic salts of Fe^{+3}, but this is not at all the case.

Néel was able to resolve these difficulties by postulating an unbalanced antiferromagnetic structure to which he gave the name ferrimagnet. The nature of the unbalance is determined by the crystal structure of the material, which for many ferrites is the spinel structure. This structure can be envisaged as a cubic close packing of oxygen atoms in whose interstices are located the smaller metal ions. These ions occupy tetrahedral sites, A, surrounded by four oxygen atoms and octahedral sites B, with six oxygen atoms as immediate neighbors. In the spinel structure twice as many octahedral sites as tetrahedral sites are occupied. The spinel structure is further characterized as *normal* or *inverted*. In the normal spinel the B sites are occupied by the trivalent metal ions and the A sites by the divalent metal ions. In the inverse spinel the A sites are occupied by trivalent ions, while on the B sites divalent and trivalent ions are distributed at random in equal proportions. These may exist as well-mixed spinels in which the inversion is incomplete.

Néel studied the properties of these various spinels, in the molecular field approximation, as a function of the several exchange parameters: interactions between ions on the same set of sites J_{AA}, J_{BB}, and on different sites J_{AB}, and was able to account for a large number of the observations. In particular for Fe_3O_4, an inverted spinel, the magnetic moments of the tetrahedral A site ions are supposed to be coupled antiferromagnetically to those of the octahedral B site ions. Since the latter are in the majority,

Louis Néel

Fe_3O_4 is resultantly ferromagnetic, and Néel was able to explain quantitatively the observed magnetic moment per Fe atom obtained from magnetization data. This ferrimagnetic model for Fe_3O_4 was strongly supported by polarized and unpolarized neutron diffraction experiments performed at the Oak Ridge National Laboratory by C. Shull, E. O. Wollan, and W. C. Koehler in 1951. Subsequently neutron experiments by many workers on a wide variety of spinel structures have confirmed the theory of Néel.

The ferrites are generally nonconducting ferrimagnets of great technological significance particularly in high-frequency engineering.

It is now possible to "tailor-make" spinels and other ferromagnetic oxides for a wide variety of technical applications as a result of the development of theoretical descriptions of the magnetic properties of these materials. This progress is due in no small part to the work of Professor Néel.

It was my privilege to spend the academic year 1958–1959 at the Institute Fourier. This was a time of intense activity in extending and understanding the elegant magnetization studies of R. Pauthenet on the rare earth iron garnets, and one of my experiments was the study, with A. Herpin and P. Mériel, of the magnetic structure of holmium iron garnet by neutron diffraction. The high temperature form predicted by Néel was exactly confirmed; at low temperatures strong crystal field interactions on the holmium ion moment produces departures from colinearity, resulting in an "umbrella-like" configuration of Ho^{+3} moments, but otherwise the arrangement is that anticipated by Néel. These substances have been used in a wide variety of devices; most recently they have become important and exciting new candidates for bubble domain materials with computer application.

Professor Néel has also concerned himself with such diverse questions as the "law of approach to saturation" of a ferromagnet, as magnetic coupling phenomena in thin films, and as the fundamental antiferromagnetic nature of metamagnetic materials.

The influence of Professor Néel's scientific accomplishments in magnetism and solid state science has indeed been profound; no less significant are the results of his efforts and vision and administrative genius in creating at Grenoble a superior center for scientific research. Since 1946 he has been professor in the faculty of science at Grenoble and the director of the Laboratory for Electrostatics and the Physics of Metals (The Institute Fourier) during which time the school of Néel has become internationally recognized for excellence in research in magnetism. (It is interesting to note that more than 20 U.S. scientists have chosen to spend periods of at least 1 year in Néel's laboratory since 1958.) Néel is currently director of the Center for Nuclear Studies at Grenoble where strong programs in neutron diffraction, crystal growth, and Mössbauer studies have been instituted. He has been largely responsible for the concept and realization at Grenoble of a laboratory for high magnetic fields, high pressures, and very low temperatures, and for the joint Franco-German high flux reactor laboratory. His interest in applied science and instruction remains strong: he is the director of the Polytechnic Institute of Grenoble, and a member of the Consulting Committee on Higher Education. In spite of many demands on his time and energy he rarely refuses to sit on the examining committee of a doctoral candidate. He is, as well, a director of the National Center for Scientific Research.

Professor Néel has been awarded many honors and decorations in recognition of his service to his science and his state. It is fitting, and well deserved, that he head his list of honors with the title Nobel laureate in physics.

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Chemistry

3. Research on Sugar Nucleotides Brings Honor to Argentinian Biochemist

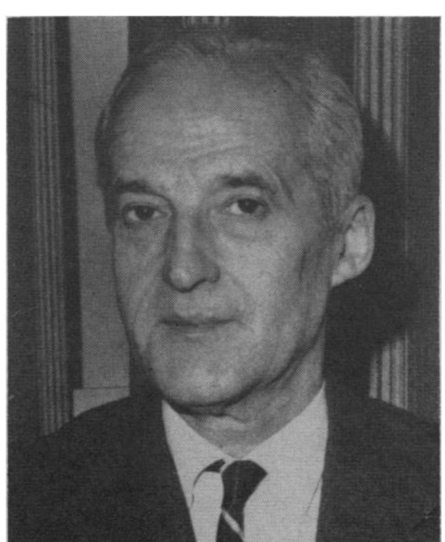
One day in Buenos Aires some 20 years ago, a young and inexperienced biochemist in despair asked Luis Leloir, "But how do you know, Dire,* what experiment to do next, where to direct your research?" Back came this answer: "Oh, you just take hold of one end of the line and keep pulling, little by little." Leloir has pulled quite a bit by now, bringing in one discovery after another. We will pull on another rope, that of memory, and see what comes to the surface. A vivid remembrance is that of the day when, attracted by a small newspaper ad offering a fellowship, I went to see the director of the Instituto de Investigaciones Bioquímicas, Fundación Campomar. The imposing name referred to a small and rather dilapidated one-story house, transformed into a laboratory. On a small patio, a slightly built man wearing a grey and rather shabby duster was gassing test tubes with hydrogen sulfide. He introduced himself as Leloir, and without interrupting his polluting task said amiably: "Go ahead and talk if you please. While I am working I will listen to you." This lack of ceremony was applied equally to the young applicant and to famous visitors.

Indeed, it is characteristic of Leloir that he talks to everybody in the same courteous, unassuming way—the mark of a gentleman—and that his experiment must go on at all times—the mark of a worker. His rare demonstrations of anger occur only when someone shows bad manners or wastes his time with useless talk. The serenity that Leloir displays at all times and his quiet sense of humor have been a powerful influence in maintaining a calm atmosphere and in soothing bad tempers in the laboratory. His Spartan use of words does not make it easy for a student or a fellow to work with him. Yet, in his brief encounters with the Dire, the disciple will be surprised again and again by the penetrating remark which abruptly opens unexpected vistas and makes him swear under his breath, "How the hell does he do it . . . ?"

Frugality does not stop with words. One rainy day water leaked into the library of the decaying quarters which housed the Institute. The roof was never fixed—it was too costly; but Leloir built a system of gutters of waterproof cardboard *inside* the library to catch the seeping water and lead it away from the books into strategically placed pans. Such ingenious and, above all, money-saving inventions were greatly encouraged in the laboratory, and several homemade apparatus and gadgets still in use bear the mark of Leloir's craftsmanship. This parsimony brought one important point home: One should not be discouraged by dearth of material means and unexpected difficulty should be a challenge to one's ingenuity and resourcefulness.

Luis Leloir comes from an old Argentine family, but he was born in 1906 in Paris when his family was on a brief visit to France. He grew up and was educated in Buenos Aires and studied medicine at Buenos Aires University. It is said that a man of genius usually shows a wide range of interests, and that he leaves his mark in more than one field. This is especially true of Leloir. After completing his internship in 1932, he briefly engaged in endocrinological research under the guidance of Bernardo A. Houssay, who, in later years, received a Nobel prize in medicine.

Attracted by the burgeoning science of biochemistry, in 1935 Leloir went to Cambridge, England, where Sir Frederick Gowland Hopkins directed the Institute of Biochemistry. After his return to Buenos Aires, he worked from 1937 to 1944 in the Institute of Physiology of the medical school, studying the oxidation of ethanol and, later, of fatty acids. It may come as a surprise to some readers that Leloir and Muñoz were the first to obtain reproducibly an enzymatic system capable of oxidizing fatty acids in vitro. This important discovery was not followed up, and Leloir joined a team that was studying renal hypertension under the leadership of Eduardo Braun-Menendez. This work culminated in the discovery of the hypertensive peptide angiotensin. Leloir had a principal role in establishing the mechanism by which angiotensinogen is transformed into angiotensin by the enzyme renin.



Luis Leloir

Perón's access to power in Argentina led to the disbanding of the group at the Institute of Physiology, and Leloir spent 1945 and 1946 in the United States.

Back in Buenos Aires, and with the help of his former teacher, Professor Houssay, he obtained the support of a businessman, Jaime Campomar, to establish a private institute for biochemical research. The Institute was dedicated in November 1947, and on that day began a most fruitful period for Leloir and his associates. The initial group included, in addition to Leloir, Ranwel Caputto, Carlos Cardini, Naum Mittelman, Alejandro Paladini, and Raul Trucco. The atmosphere of the laboratory was that of a closely knit family. The oppressive political environment contributed to the cohesiveness of the group and imparted a feeling of pioneering effort to what might otherwise have been an ivory-tower situation. Those days will not be easily forgotten by the participants.

The goal initially set by the group was to discover how milk sugar, or lactose, was synthesized. The problem was first attacked from the opposite direction, that is, by studying the degradation of lactose by yeast. This approach gave unexpected dividends. The first steps, a hydrolysis of lactose to glucose and galactose, followed by phosphorylation of galactose in the unusual 1-position, were quickly established. The further transformation of galactose 1-phosphate into glucose 6-phosphate required heat-labile enzymes and two heat-stable cofactors. One of the latter was identified as glucose 1,6-diphosphate and found to act as a coenzyme for phosphoglucomutase, the enzyme that catalyzes the conversion of glucose 1-phosphate into

* "Dire," short for director, is the affectionate appellation used by Leloir's co-workers.

glucose 6-phosphate, thus providing Leloir and his group with their first big achievement. After purification, the structure of the second heat-stable cofactor was established in a remarkable tour de force. The substance turned out to be uridine diphosphate glucose (UDPG), the first sugar nucleotide. The discovery of this new type of substance and of its function in sugar interconversion and in the biosynthesis of complex carbohydrates is the basis for the present Nobel award.

The mechanism by which UDPG participates in the conversion of galactose 1-phosphate to glucose 1-phosphate was established by Kalckar and his co-workers and by Leloir himself. The uridylyl group is enzymatically transferred from UDPG to galactose 1-phosphate, thus giving rise to UDP-galactose. An epimerase converts this nucleotide back into UDP-glucose, and the cycle is restarted. Thus, the role of sugar nucleotides as substrates for monosaccharide interconversion was established.

The initial successes of Leloir's group brought some fame and some welcome financial assistance. The Rockefeller Foundation was the first to help. Of great importance was a grant made by the National Institutes of Health in 1951 and continued until last year, when restrictions in foreign aid forced its cessation.

New sugar nucleotides, UDP-acetylglucosamine and GDP-mannose, were isolated from yeast in Buenos Aires, and soon many others were found in different laboratories. In 1961 40 sugar nucleotides were listed in a review article, and the number now is probably close to 100. The inability to find a function for the new compounds initially caused some despair in the Institute, but indications were already accumulating that these substances might participate in glycosyl transfer reactions. The first confirmation was provided in 1953 by the discovery of Dutton and Storey that UDP-glucuronic acid served as a precursor of glucuronic acid conjugates. During the same year in Buenos Aires the first disaccharide, trehalose phosphate, was synthesized enzymatically from UDP-glucose and glucose 6-phosphate with the use of a preparation from yeast. Shortly thereafter Leloir and his associates reported the enzymatic formation of sucrose and sucrose phosphate. The pathway was now clearly indicated, and soon the synthesis of the first polysaccharide, chitin, was announced by Glaser and Brown, thus providing a function for the elusive UDP-acetylglucosamine. By now the field was expanding in an exponential fashion, and in a few years there were many new examples of the sugar nucleotides' two main functions, namely (i) serving as substrates for enzymes which interconvert monosaccharides, and (ii) acting as donors in glycosyl transfer reactions, thus leading to the formation of glucosides, di- or oligosaccharides, and homo- or heteropolysaccharides. For Leloir this new phase culminated in the discovery of the mechanism of glycogen synthesis, a function previously assigned to phosphorylase. Again, UDP-glucose was the precursor. That was the last discovery to be made in the old house. Soon thereafter Perón's dictatorship was overthrown, and the new government, more benevolent toward science, provided the Institute with much larger premises. The day we left the old lab, Leloir said in a prophetic voice: "We will remember with regret the happy days we spent here!" He was right: in the new building there were no leaking roofs to fix.

The staff of the laboratory rapidly expanded, partly because of the larger space available and partly as a result of an agreement with the School of Science of the University, which increased the number of positions. Leloir himself was appointed Professor Extraordinarius and later was made chairman of the department of biochemistry, a position he recently relinquished to have more time for his research.

Leloir has received many prizes and other honors in recent years, and a continual stream of invitations has caused him to travel with relative frequency. Coming from a family of private wealth, Leloir has donated all of his prize money as well as his professor's salary and many collections of journals to the Institute. Indeed, he is as generous with his own money as he is parsimonious with that of others.

Despite his new activities and the increase in administrative problems resulting from the expansion of the Institute, Leloir has not for a single day interrupted his experiments. New projects dealing with different glycogen synthetases, then the discovery of ADP-glucose as the precursor of starch in plants, and more recently studies on the structure and formation of particulate glycogen have kept him busy. You will find him there, at his laboratory bench.

The world is made richer by the presence of persons like Leloir. With his example he taught many of us a style of life and, with his work, he has enlarged the horizon of human adventure.

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Science Funds: NSF Survey Probes Effects of Shifts in Federal Aid

A report released 15 October by the National Science Foundation provides the first broad scale picture of what has happened to the financing of academic research and science education in this period of decelerating federal support. Titled "Impact of Changes in Federal Science Funding Patterns on Academic Institutions,"* the report confirms the existence of a financial recession in university science activities. And it clearly points out that private institutions are the hardest hit. But it also provides ground for believing that, in general, the situation is appreciably short of the cataclysm routinely depicted by many leaders of the scientific community in recent years. In any case, though interpretations may widely differ, the report—actually a summary that will be expanded upon in a publication probably early next year—becomes at once a basic document for discussing the financial health of science in the universities. (Medical schools, perhaps the most financially

*NSF 70-39, 8 pages, available without charge from the National Science Foundation, Office of Economic and Manpower Studies, Washington, D.C.