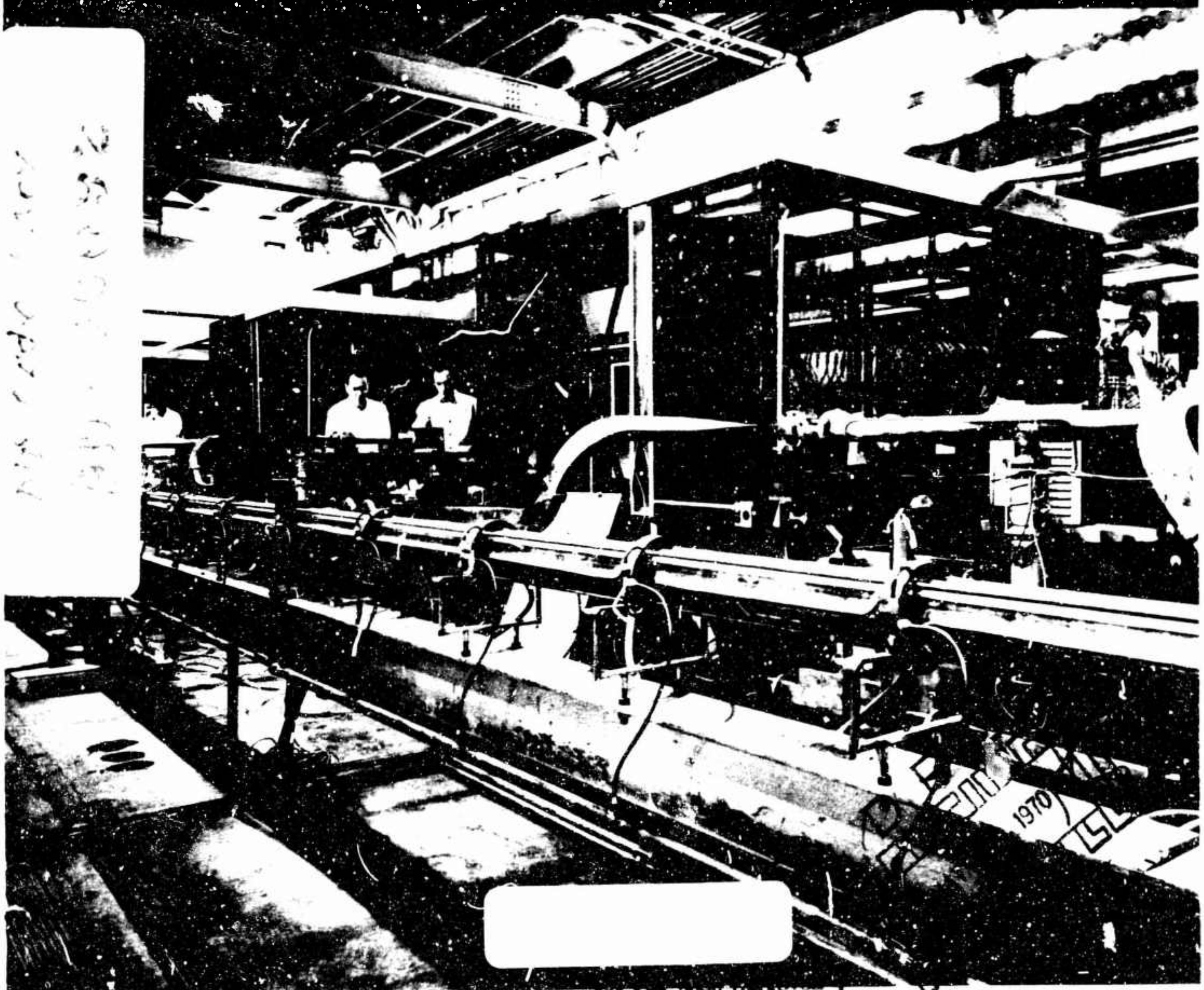
The AIR FORCE and NUCLEAR PHYSICS



A HISTORY of The AIR FORCE OFFICE of SCIENTIFIC RESEARCH NUCLEAR PHYSICS PROGRAM

HISTORY

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Office of Aerospace Research ★ United States Air Force

THE AIR FORCE AND NUCLEAR PHYSICS:

A History of the
Air Force Office of Scientific Research
Nuclear Physics Program

By

Nick A. Komons

and

David Bushnell

Historical Division
Office of Information
Office of Aerospace Research
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PREFACE

"The whole world knows," said a student of the contemporary American scene, "that we Americans have the highest standard of living, and that we owe it to our superlative capacity to extract tangible benefits from what were once only the unrealized dreams of Europe's pure science . . . . While others thought out the theories of nuclear physics, we built the first atomic bomb."*

No one would really quarrel with this statement as an accurate representation of the American's traditional approach to science. Americans are painfully practical. And they do have a "superlative capacity" to put science to work. Nor can it be disputed that they contributed little or nothing to the flowering of twentieth century science. But surely, in a contemporary context, the statement needs revision. Americans may still be practical, but they no longer rely upon the rest of the world for the foundations upon which to build their machines. Today they are spending more time, energy, and money in the pursuit of pure science than any other people on earth. And in the attainment of scientific excellence, they are second to none. This little volume on the nuclear physics program of the Air Force Office of Scientific Research serves, if nothing else, as a reminder of the extent to which the pursuit of science has permeated the American scene, including, what must seem to some people as the unlikeliest of places, the military establishment.

Of course, the Air Force is not seeking scientific knowledge merely for the sake of knowledge. It is seeking, rather, knowledge which will one day extend the range of military technology. No field of knowledge has contributed more in the recent past to extending this range than nuclear physics.

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*Gerald Sykes, The Hidden Remnant (New York: Harper & Brothers, 1962), 2.

In any work of this kind, communication with the reader who lacks a scientific background always presents a problem. It was the intention of the authors to make most of the material intelligible to the non-specialist, while at the same time doing no material violence to the subject matter. The authors freely admit, however, that they fell a little short of striking such a balance at all times. Most of Chapter VI will not be intelligible to the non-specialist. The same holds true for a few other scattered passages.

The following members and former members of the Nuclear Physics Division, AFOSR, read several versions of the manuscript in whole or in part and made numerous valuable suggestions: Colonel Joseph E. Duval, Chief, Nuclear Physics Division (1961-1963), Lt. Col. Charles K. Reed, Chief, Nuclear Physics Division (1957-1961), Mr. Ray R. Heer, Jr., Major Albert W. Harrison, Jr., and Mr. Doran W. Padgett. These individuals also gave freely of their time in supplying documentation and answering questions posed to them by the authors. But, of course, it goes without saying that, despite this assistance, any errors of fact and interpretation are the responsibility of the authors.

Nick A. Komons
David Bushnell
Historical Division
Office of Aerospace Research
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Chapter I

INTRODUCTION

Among the myriad scientific areas and sub-areas supported by the Air Force Office of Scientific Research (AFOSR), the field of nuclear physics is surely one of the most exciting. Its range is enormous, extending into the vast reaches of space and into the remotest depths of elementary matter. As age goes, it is a relatively new field of inquiry, born during the early decades of this century; yet it is wrestling with some of the oldest problems and concepts known to science -- elementarity, symmetry, force. While it is one of the purest of the basic sciences, it has in our time made the most dramati contributions to technological progress. In consequence, it is that area of science most often in the public's eye. But because of its uncommon approach to the nature of things, it is perhaps that area of science whose real character is most often misunderstood by the public at large. A fit subject for the purest academic, it has become the concern of government. Lauded for its contributions to the enrichment of life, it has provided the means by which mankind can obliterate itself. It is, in sum, a subject of many parts. The experience of AFOSR in supporting research in this area of modern science will be the concern of this narrative.

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"The world is now without mysteries," proclaimed the French chemist, Marcelin Berthelot, in 1885, reflecting the self-assurance of nineteenth century science. "We supposed that nearly everything of importance about physics was known," said Alfred North Whitehead, as he recalled the confident days of his youth. At a time when other branches of knowledge were reeling before the onslaughts of Darwin, Marx, and Freud, the physical sciences complacently took cover under the classical wings of Euclidean geometry and Newtonian mechanics. True, a few details here and there required pursuit; but the essentials would never suffer modification. They were immutable. Little did Whitehead, Berthelot, and company suspect what manner of vicissitude and what magnitude of change lay in store with the dawn of the twentieth century.[1]

Even before the nineteenth century was up, the classical picture of the atom as an indivisible particle was coming under revision as a result of J. J. Thomson's discovery of the electron in 1897. Then, swiftly and with suddenness, came Planck's announcement of the quantum theory in 1900, Einstein's first paper on relativity in 1905, Rutherford's discovery of the atomic nucleus in 1911, and, in the next decade, the development of wave mechanics. Hereafter there would follow a concerted and rapid penetration into nature's depths. A new, exciting, and revolutionary scientific era had been ushered in.[2]

Despite a startling series of successes, the new physics, unlike the classical, never managed to reach a plateau of self-contentment. There was confidence enough, to be sure, that answers could be had; but there was little faith that a given answer was the ultimate one. In 1924, Max Born, the German theoretical physicist, wrote a book on atomic theory. All he knew on the subject was contained within its covers. Yet Born designated the book, Volume I -- so certain was he that he would ultimately devote another volume to a second and as yet unborn atomic theory that would overthrow the first. Immutability had been abandoned. So, too, had the hope for completeness. Indeed, while man's probes into nature yielded many answers, they seemed to raise even more questions. The result, by mid-century, was that physics was a growing, fertile field of study, ever conquering new frontiers, yet ever on the edge of

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[1] Milič Čapek, The Philosophical Impact of Contemporary Physics (Princeton: D. Van Nostrand, 1961), xiii-xiv; Dialogues of Alfred North Whitehead (New York: New American Library, 1960), 12; Cecil J. Schneer, The Search for Order (New York: Harper & Brothers, 1960), 288-98; Harry Massey, The New Age in Physics (New York: Harper & Brothers, 1960), 17.

[2] A. d'Abro, The Rise of the New Physics (2 vols.; New York: Dover Publications, 1951), II, passim.

even newer ones. Moreover, while there appeared to be no end to the flow of new data, there seemed to be a growing difficulty in assimilation. Conceptually, the new physics was half-starved. Not since the 1920s, when de Broglie reconciled the existence of both particle and wave and Schrödinger and Heisenberg introduced quantum mechanics, had a significant conceptual scheme, explaining a great many events, been formulated. There seemed but one thing to do. To push ever deeper, to gather ever more data, and to hope for the simple, elegant concept to come.[3]

Meanwhile, the increase in scientific knowledge was generating a tremendous impact on a social, political, and technological scale. Rocketry, automation, nuclear power, space exploration -- all spoke of a coming new world order. And organized society, hungry for the fruits and power that science might give it, had a comparable impact upon the scientific community -- nourishing it, clothing it, giving it whatever it needed to carry on its work. Naturally, government was the one organ rich enough and interested enough to make the greatest monetary contribution toward this work. And the Atomic Energy Commission, the National Science Foundation, and the National Aeronautics and Space Administration were specifically established to make such contributions. But, of course, with the growing military significance of science -- more evident in so many ways in World War II -- the military would not be uninterested. It was not by accident that in the immediate postwar period the Office of Naval Research manifested a greater interest in the frontiers of physics than any other government agency. The Air Force's interest in science was to come shortly thereafter. Thus it is that the present generation, far from feeling that "the world is now without mys-

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[3]Banesh Hoffmann, The Strange Story of the Quantum (New York: Dover Publications, 1959), 72; Shneer, op. cit., pp. 288-98; Edward U. Condon, "Sixty Years of Quantum Physics," Bulletin of the Philosophical Society of Washington, Vol. 16 (1962), 83-102.

teries," actually spends millions in unraveling a seemingly endless series of mysteries.[4]

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[4]See for example, National Science Foundation, Federal Organization for Scientific Activities, 1962 (Washington: Government Printing Office, 1963).

Chapter II

THE AIR FORCE AND NUCLEAR PHYSICS

The Air Force's interest in basic research in nuclear physics dates back to even before the creation of a separate agency within the Air Force devoted entirely to basic research -- components of the now defunct Air Materiel Command having let contracts on the subject during the late 1940s. When AFOSR was founded in 1951, a large portion of these contracts were ultimately transferred to that organization.[1] With this transfer virtually all of the Air Force's basic research in nuclear physics was sponsored by AFOSR. This would remain the case to the present day.

That the Air Force should have engaged in the support of basic research in nuclear physics, which had no apparent or immediate application to Air Force problems, may seem strange indeed. Actually, at the time a full-scale program was launched, it even appeared strange to a good many people in the Air Force.[2] But to others, as the following statement from a passage of a 1956 project documentation reveals, the need for such endeavors was real:

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[1]Ltr., Robert M. Linsmayer to W. F. Libby, 11 March 1949, MSS; ltr., W. F. Libby to Col. Oliver Haywood, 13 August 1952, MSS; ltr., Capt. Seymour Shwiller, Physics Division, OSR, to W. F. Libby, 25 September 1952, MSS; R & D Project Card (RDB Form 1A) Project No. R-357-20-3, 27 June 1952. Strictly speaking, AFOSR was established only in 1955, but much the same organization had previously existed (1951-1955) as the Office of Scientific Research (OSR), a staff section in Headquarters Air Research and Development Command. To avoid the inconvenience of repeatedly shifting back and forth between two similar but not identical designations, the compound abbreviation, AFOSR, will be used throughout this study -- even when it would be technically more accurate to use OSR.

[2]For a brief discussion of the genesis and the motives behind the creation of AFOSR see chapter 2 of another OAR study, Nick A. Komons, A Decade of Chemical Research (OAR-7, May 1962). See also, in general, Office of Aerospace Research Chronology (OAR-8, August 1962).

> In designing vehicles for operation through and beyond the Earth's atmosphere, a whole new area of problems arises. Among these are aerodynamic heating on re-entry into the Earth's atmosphere, meteoric penetration of the missile skin, behavior of electronic equipment in high radiation densities, provision of auxiliary and propulsive power sources for operation over extended periods of time. To propel such vehicles greatly improved and radically advanced power plants employing new types of high energy fuels, including solar energy and nuclear energy, must be investigated. This project will be devoted to various researches aimed at obtaining the needed <u>fundamental</u> information.[3]

A somewhat more emphatic, but less specific, exposition was given by a member of AFOSR's present Nuclear Physics Division:

> ". is taken as axiomatic that development of the Air Force capability requires close association with advances in fundamental research within the U. S. and abroad. The required degree of association cannot be achieved if the [Department of Defense] isolates itself from active support of nuclear physics research and attempts to merely draw upon the available published literature. Understanding of what is going on requires far more than access to published literature.[4]

In other words, science will have a definite bearing on the Air Force's future capabilities, and the only way that the Air Force can be fully aware of significant developments in nuclear physics is to have a nuclear physics program of its own. So went the argument.

The program's beginnings were modest enough, the same being the case for AFOSR's entire basic research program. AFOSR's budget in the early 1950s stayed around the six million dollar mark, and nuclear physics took from $250 to $400 thousand each year. Under the organizational set-up at the time, nuclear physics was graced with no separate division or office of its own, being simply part of the Physics Division, which served as a sort of catch-all for most of the sub-areas in physics. In 1957, however, a separate Nuclear Physics Division was established,

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[3] RDB Project Card (DD Form 613) Project No. 3750, 22 April 1956, p. 1.

[4] Ray Heer, Jr., "Justification of the AFOSR Nuclear Physics Program," 9 November 1960, MSS; but see also RDT&E Project Card (DD Form 613) Project No. 9750, 16 January 1961, pp. 1-3.

falling under an expanded Directorate of Physical Sciences. Heading the new division was Major (now Lt. Col.) Charles K. Reed, who had come upon the scene that same year. Dr. William J. Otting headed the Physics Directorate.[5]

It was at about the time that the Division was created that the program itself began to pick up steam. From a total of 25 contracts in 1956, the Division had 53 active projects three years later. Moreover, its actual monetary resources began to climb even more sharply. By fiscal year 1957, the original budget of $250 thousand for nuclear physics research had quintupled; and the following fiscal year -- the year of Sputnik I -- the budget took an increase of over 100 percent to a total of $2.8 million. The Division's budget reached its all-time high of $3.4 million during fiscal year 1959, but only to drop to $2.5 million the next time around. From here on out, however, its budget would remain around the three million dollar mark. Since its organization, the Division has spent in the neighborhood of $20 million on basic research in nuclear physics.[6]

As for the nuclear physics program itself, its broad outlines have remained rather constant over the years. In 1952, the principal areas of interest were set down as elementary particles, nuclear structure, and cosmic radiation. At first, the program was mainly experimental; before long, however, it encompassed theoretical studies as well. Because of overlapping concepts, the Division made occasional excursions into relativity and gravity research. But, for the most part, elementary particles, nuclear structure, and cosmic radiation remained the hard core of AFOSR's nuclear physics program.[7]

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[5] R & D Project Card (RDB Form 1A) Project No. R-357-20, 5 May 1952, p. 1; Physics Division Research Program as of 1 January 1954; Physics Division Research Program as of 15 October 1955; Trends in the Nuclear Physics Program, Future Plans and Recent Accomplishments, 1959, MSS; Ray R. Heer, Jr., personal interview with N. A. Komons, 6 March 1963.

[6] Budget figures for fiscal years 1957 through 1963 were supplied by the Assistant for Plans & Programs Division, AFOSR.

[7] R & D Project Card (RDB Form 1A) Project No. R-357-20, 5 May

II

The ushering in of the new physics, while in great measure due to the endowments of fresh minds and fresh approaches, was essentially a collaborative effort between the physicist on the one side and the tools of an advancing technology on the other. In no field is this more evident than in the field of experimental high energy physics.[8]

To understand the critical role played by technology, one need only be reminded that the atomic nucleus, the largest entity that the high energy physicist deals with, is but a speck of matter with a diameter no more than a few ten-trillionths of a centimeter. Elementary matter is, therefore, utterly and hopelessly invisible, both to the naked and to the aided eye. "That it is possible to peer within this speck of matter," exclaimed one experimental physicist, "is one of the most impressive feats of modern physics."[9]

Of course, physicists do not actually "peer" within elementary matter. What they do is devise indirect approaches which can compensate for their inability to see. Ernest Rutherford, for example, who is chiefly responsible for the modern atomic model, was merely able to imagine what an atom looked like by constructing a model that accounted

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1952, pp. 1-2; ltr., Maj. Michael Zubon, Acting Assistant for Research, OSR, to Lt. Col. Lowell B. Smith, Chief, Projects Division, Deputy for Development, ARDC, 30 July 1952, MSS; Heer, personal interview with N. A. Komons, 6 March 1963; Trends in the Nuclear Physics Program, 1959, MSS; AFOSR, First Annual Report: 1956, pp. 67-71; Ray R. Heer, Jr., "Justification of the AFOSR Nuclear Science Research Program," 9 November 1960, MSS; AFOSR, Scientific Mission and Operational Management of the Air Force Office of Scientific Research . . . Fiscal Year 1959, p. 45; Lt. Col. Charles K. Reed, Presentation to the Physics Advisory Committee, ca. 1961, MSS.

[8] Alfred North Whitehead, Science in the Modern World (New York: Mentor Books, 1956),116; Paul Roman, Theory of Elementary Particles (Amsterdam: North-Holland Publishing Co., 1961), 2.

[9] Robert R. Wilson and Raphael Littauer, Accelerators: Machines of Nuclear Physics (Garden City, N. Y.: Doubleday Anchor, 1960), 22-23; Robert Hofstadter, "The Atomic Nucleus," Scientific American, Vol. 195 (July 1956), [illegible].

for his experimental results. His approach was to bombard a nuclear target, say, a thin sheet of gold foil, with a stream of alpha particles. Behind the foil he placed a photographic plate. After colliding with the foil, the particles hit the plate and were recorded there as dark spots. From the pattern created by the scattered particles as they bounced off the target and onto the plate, Rutherford could infer the structure of the target and the kind of events that took place as a result of the collisions. Essentially the same approach is used by today's physicists in order to compensate for their inability to see.[10] But before particle scattering experiments reached their present stage of development, more than a few technical problems had to be dealt with.

According to quantum theory, energy exists in discrete units (quanta), and physical systems can absorb energy only in such units. The energy levels of these quanta vary all along the spectrum. In other words, a quantum of light emitted from one part of the spectrum has a different energy level than a quantum emitted from another part. Quantum theory further states that a specific unit of energy is required to excite a particular physical system. For example, if one thousand electron volts (Kev) are required to excite a given system, this energy must come in one discrete package, not one electron volt at a time. (Physical systems the size of an atom will not accept energy in indefinitely small portions.) In the Rutherford study cited above, alpha particles of one to ten Kev were required to demonstrate that the atom was composed of a dense nucleus and a cloud of planetary electrons. When we move into the realm of the nuclear physicist, even higher energies are required. In order to excite the atomic nucleus and show that it is made up of protons and neutrons, particle beams of one million to 160 million electron volts (Mev) are needed. And even higher energies, sometimes more than one billion electron volts (Bev), are needed to examine the structure of

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[10] Isaac Asimov, Inside the Atom (New York: Abelard-Schuman, 1958), 27; George Gamow, The Atom and Its Nucleus (Englewood Cliffs, N. J.: Prentice-Hall, 1961), 25-29; Wilson and Littauer, op. cit., pp. 50-52.

the individual nucleons and other elementary particles. Thus, as the physicist goes down to smaller and smaller particles, he is forced to go to higher and higher energies.[11] And herein lies the chief problem: how does one generate ever increasing energies?

If physicists had been satisfied with the energies that natural radioactive sources such as radium and cobalt are capable of imparting, the field of nuclear physics would scarcely exist. These natural sources simply do not possess sufficient energy to yield the kind of detailed data sought by modern nuclear structure and elementary particle physicists. Of course, cosmic rays, another natural source of radiation, possess as much energy as the nuclear physicist will probably ever need; however, the cosmic ray flux is low at any energy and decreases as energy increases. Thus, neither will this source of natural radiation furnish the nuclear physicist with all the detailed data he is after. Since nature was not obliging enough to furnish man with a versatile and readily available high energy source, man was forced to devise artificial sources of his own.[12] This he did between 1926 and 1933, developing machines that accelerated beams of high energy protons and electrons and directed them at targets of atomic nuclei. By 1939, the cyclotron at the University of California was accelerating particles to an energy of 25 Mev -- twice the energy of the most energetic naturally emitted alpha particle. Shortly after World War II, as a result of the discovery of the principle of phase stability, man took his last major hurdle in the technology of particle accelerator development, and from here on out the sky seemed to be the limit in high energy particle production.[13]

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[11] V. F. Weisskopf, "Elementary Particles," in Recent Advances in Science (New York: New York University Press, 1956), 115-36.

[12] Wilson and Littauer, op. cit., p. 52; Atomic Energy Commission, A Ten Year Preview of High Energy Physics in the United States: Detailed Backup for Report of Ad Hoc Panel of the President's Science Advisory Committee to the Atomic Energy Commission, December 1960, p. III-1, hereinafter cited as Ten Year Preview.

[13] Edwin M. McMillan, "Particle Accelerators," in Experimental Nu-

Needless to say, beginning with this discovery, activity in the high energy field increased by leaps and bounds.

The trouble now became not one of technology -- although with each substantial jump in energy there were still technological hurdles to overcome -- but one of economics. High energy accelerators cost millions to construct. To this high initial expenditure must be added the continuing costs of operation and research -- costs that duplicate the initial investment every two years or so. Even the richest of universities could afford neither to build nor operate some of the proposed atom smashers. Under the circumstances, it was inevitable that high energy physics, like so many other segments of society, should turn to government for assistance. Government seemed to be the one remaining entity with the necessary financial resources to do the job.[14]

And, in the immediate postwar era, government did do the job. Money disbursed by federal agencies was responsible for the construction of most of the cyclotrons, synchrotrons, and bevatrons across the nation. Moreover, federal agencies paid for much of the bill for the actual experiments performed with these machines. In roughly one decade,

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clear Physics (3 vols.; New York: John Wiley & Sons, 1959), III, 639-774; Isaac Asimov, Intelligent Man's Guide to Science (2 vols.; New York: Basic Books, 1961), I, 245-46; Wilson and Littauer, op. cit., pp. 52-67, 117-27; Edward L. Ginzton and William Kirk, "The Two-Mile Electron Accelerator," Scientific American, Vol. 205 (November 1961), 49. A word about the principle of phase stability. At 25 Mev protons contained so much energy that their mass actually increased, causing them to lag and fall out of phase. (The effect had been predicted by Albert Einstein as early as 1905.) As a result, it appeared that man had reached the upper limit to which he was capable of accelerating particles. But in 1945, Edwin M. McMillan of the University of California and Vladimir I. Veksler, a Russian physicist, independently hit upon the idea of synchronizing the increase in mass of the particles with the frequency of the alternations of the electric field. It proved to be the way out. For this work McMillan and Veksler were awarded the Atoms for Peace Award in 1963. The New York Times, 31 July 1963.

[14] Ten Year Preview, p. III-1; National Science Foundation, Report of the Advisory Panel on High Energy Accelerators to the National Science Foundation, 25 October 1956, p. 2; AFOSR, Projections, 1962, pp. 13, 19.

1946 through 1956, federal agencies poured in excess of $100 million into high energy physics. More -- a great deal more -- was to come. Not unexpectedly, in good part through the efforts of these agencies, the United States assumed a commanding lead in the field of high energy physics in the immediate postwar period.[15]

III

While the Atomic Energy Commission (AEC) was far and away the greatest contributor to high energy physics, it was by no means the pioneer in the field. Perhaps not too surprisingly, a military agency, the Office of Naval Research (ONR), was the first federal agency to tap the high energy field. In 1946, two years before the AEC came forth with a high energy program, ONR had a host of high energy projects under contract.[16] From that day forward, the military services have to one degree or another been in the high energy business.

AFOSR was somewhat late in getting a high energy program on the road. Indeed, it was two years after the founding of AFOSR that the Air Force entered into a specific contract in this field. The reasons for this delay were quite obvious. With only $250 thousand allotted yearly to all of nuclear physics, a project or two in high energy would have taken nearly all of the nuclear physics budget. But when the purse strings were finally loosened, AFOSR took advantage of its opportunities. In May 1953, it extended a contract to Stanford University in support of Robert Hofstadter's electron scattering studies. In doing so, AFOSR was following the lead of the Office of Naval Research, which was already giving partial support to Hofstadter and to the operation of

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[15]Atomic Energy Commission, A Review of the Status of the High Energy Accelerator Field: Report of the Interagency Scientific Staff to the National Policy Council on High Energy Physics, 15 September 1958, pp. 2-3, table 1, hereinafter cited as AEC, Status of High Energy.

[16]Ibid., p. 3.

Stanford's linear accelerator, the machine which Hofstadter would use for his studies.[17] The Hofstadter contract, which began at an annual level of $42 thousand and rose to $200 thousand in fiscal year 1956, constituted AFOSR's sole high energy study for a period of three years.[18] Moreover, it was the only "on-site" project in this field ever sponsored by the organization. The reasons were once again economic. AFOSR could not afford to pay for expensive accelerator time.

Beginning with fiscal year 1957, AFOSR adopted an expedient which partially solved the problem. This was to support the user of an accelerator site rather than the site itself. This so-called users program ("the poor man's Bev program," is how one member of the Nuclear Physics Division described it) obviated any necessity of paying for the use and maintenance of an accelerator. What it paid for, other than the salaries of the investigators, was the cost of reducing and evaluating data. And in this respect it fulfilled a very real need, for it permitted investigators from institutions without accelerators to take advantage of the nation's far-flung, but highly concentrated, accelerator resources.[19]

The first users group to be supported was that of Aihud Pevsner of The Johns Hopkins University. The contract went into effect in July 1956 and was concerned with the investigation of new particles and their interactions with protons and neutrons. Later that year, the first European contract in high energy physics was awarded, to Professor Kai Siegbahn's experimental group at the University of Uppsala, Sweden. This was followed, in March 1957, by a contract with M.I.T. The program was rounded out the following fall when the Nuclear Emulsion Group of

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[17] Robert Hofstadter, "Proposal for Continuation of Research on Electron Scattering and Nuclear Structure," August 1963, p. 1, MSS.

[18] Nuclear Physics Division, "Nuclear Physics Division Research Program as of 1 July 1960," MSS.

[19] Heer, personal interview with N. A. Komons, 6 March 1963; Lt. Col. Charles K. Reed, "Briefing on the Nuclear Sciences Program," n.d., ca. 1960, MSS.

the University of Chicago received AFOSR's support. A few other projects, less costly and less significant than those mentioned, were picked up before fiscal year 1958 was out. By this time, AFOSR was spending at an annual rate of $626 thousand on high energy physics. All told, it was a program which concentrated on a half-dozen or so spectacular studies and made no pretense to covering the field.[20]

While it was no doubt true that AFOSR was making good use of its somewhat meager high energy funds, AFOSR's program was not above criticism. Its main drawback was its incompleteness -- incompleteness from the standpoint of contact with the techniques, the people, and the data of high energy physics. There was, in particular, a very limited association with the challenging field beyond the 1 Bev energy range. And from the standpoint of facilities, the Air Force had contributed not one cent toward the construction of high energy accelerators.[21]

Merely because the Air Force was not doing all that it might have been doing in an area of basic research was not in itself sufficient reason for alarm among the scientific community, especially since the area of research in question was one in which the United States enjoyed a clear-cut lead in the mid-1950s. But AFOSR's high energy program was not the only federal high energy program that left something to be desired. Indeed, most of the criticism that AFOSR was subject to applied equally well to the entire Defense Department and the National Science Foundation. And this fact, when coupled with increasing competition from abroad, did cause considerable concern among the members of the American scientific community.

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[20]Directorate of Physical Sciences, "Nuclear Physics Division Research Program as of 30 June 1957," MSS; Nuclear Physics Division, "Nuclear Physics Division Research Program as of 1 July 1960," MSS; R & D Project Card (DD Form 613-1) Project No. 3750, 1 April 1958, pp. 10-32.

[21]AFOSR, Scientific Mission and Operational Management of the Air Force Office of Scientific Research . . . Fiscal Year 1959, pp. 45-50; Minutes of Meeting of the AFOSR Physical Sciences Advisory Committee, 2 December 1960; Department of Defense, Status Report and Summary of Present Funding Procedures of the DOD Contract Research Program in

Although, in 1958, the United States possessed 60 percent of the world's supply of high energy accelerators, there were never enough accelerators to go around. Even with accelerators at American sites running around the clock, a rapidly accumulating backlog of important experiments awaited its turn on the machines. The situation was particularly acute in the multi-Bev range. In the Middle West, for example, there was not a single multi-Bev accelerator to take advantage of the rich human resources of that region. Moreover, as late as 1958, no accelerator in the United States was capable of producing all the known particle-antiparticle pairs.[22]

Meanwhile, the early and rather substantial lead that the United States had taken in this field was becoming more difficult to maintain. The most disturbing signs were coming out of the Soviet Union, which was apparently making a serious effort for leadership in the field. In 1957, at Dubna, the Russians had in fact unveiled a 10 Bev synchrotron, the most powerful accelerator up to that time. Although the AEC had even larger accelerators under construction during this period, American physicists were unable to explore energy regions comparable to those explored by their Russian counterparts for a span of about two years. Fortunately, in 1959, the European Committee for Nuclear Research (C.E.R.N.) completed the construction of its $30 million, 24 Bev alternating-gradient synchrotron, and the western world was not without access to a machine in this critical high energy range.[23]

Of course, with two keenly competitive societies contending, it was to be expected that leadership in a variety of endeavors would seesaw from one to another with considerable frequency. Given all this, how-

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Basic Nuclear Physics, 3 January 1956, passim, hereinafter cited as DOD, Nuclear Physics.

[22] National Science Foundation, Report of the Advisory Panel on High Energy Accelerators, 25 October 1956, pp. 1, 4; AEC, Status of High Energy, pp. 2, 12, 14; DOD, Nuclear Physics, passim.

[23] DOD, Nuclear Physics, passim; NSF, High Energy Accelerators, p. 1; Asimov, Intelligent Man's Guide, I, 247-48.

ever, it was not a time to stand pat. The Soviets, it was known, were not beyond concentrating a great deal of their resources in endeavors which they considered particularly important; and it was feared that they were doing this very thing in high energy physics. Indeed, the Russians were at that moment planning a 50 Bev synchrotron. What particularly piqued some officials, especially in the Department of Defense, was that the Soviet Union's temporary possession of the most powerful accelerator need not have been. As one Defense Department report put it, "The Russian lead clearly demonstrates the danger of a policy which contemplates our doing substantially less than ideas and people make us capable of doing."[24]

A good deal of the blame for this state of affairs was rightly or wrongly heaped upon the AEC. One charge was that the agency was taking an inflexible approach to the construction of new sites. For example, the AEC had set a figure of five million dollars as the upper limit for the cost of each new accelerator. This policy, finally abandoned in the late fifties, meant that the size and intensity of accelerators was being limited out of economic rather than scientific considerations. There was also a feeling in some quarters that the AEC was taking excessively long to approve proposals. It required the agency three years to approve the construction of the Cambridge electron accelerator. Other proposals waited equally long for approval.[25]

But the main line of fire was directed not at the AEC -- after all, between 1956 and 1958, the agency spent on an average $22 million annually on high energy physics -- but at the National Science Foundation and the Department of Defense. Actually, even before matters had come to a head, the National Science Foundation's Advisory Panel on High Energy

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[24] NSF, High Energy Accelerators, p. 1; DOD, Nuclear Physics, passim. The Russians ultimately broke ground for a 70 Bev synchroton in 1962. Translation of article, "Into the Depths of Matter, into the Secrets of Nature," Pravda, 27 April 1962.

[25] DOD, Nuclear Physics, passim.

Physics, in three successive reports, issued between 1954 and 1958, had warned of things to come and recommended increased support for high energy physics by both the Foundation and the military services. Despite these recommendations, sustained support from the NSF was the exception rather than the rule. The Navy, moreover, after getting into the field with both feet, began to tail off in its support, dropping from $4 million yearly in 1946 to $1.6 million a decade later. The Army's Office of Ordnance Research stayed completely out of the field. AFOSR's program, on the other hand, while it had the virtue of stability and sustained growth, was modest to a fault.[26]

One of the first groups to pass judgment on the AFOSR program was a committee of scientists brought together by Theodore von Kármán under the auspices of the National Academy of Sciences (NAS) at the behest of the Air Research and Development Command (ARDC) for the specific purpose of assessing the Air Force's research and development programs. The committee, known as the NAS-ARDC Study Group, released its report on the general sciences in 1958; one of its more emphatic recommendations was for increased AFOSR support of high energy physics. The recommended support, moreover, was in both research and construction. ("It would be highly appropriate," said the report, "to have the financing of one of the large accelerators now being planned in the country be entirely a responsibility of the Air Force.")[27]

Then the group went on to touch on a point which, for the next five years, was to be the main theme of the dialogue on high energy physics: "It is the opinion of the committee that it would be a serious mistake if the field associated with particle energies above one billion electron volts were delegated entirely to the Atomic Energy Commission as is

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[26]AEC, Status of High Energy, p. 4, table 1. The NSF reports were dated 2 May 1954; 10-12 September 1956; 7-8 August 1958.

[27]NAS-ARDC Study Group, A Report by the Committee on General Sciences (National Academy of Sciences/National Research Council, 1958), v, 2, 20.

the trend at present."[28] In other words, this was not merely a question of more support, but also of more diversified sources of support. It was a matter which would cause more than passing concern among government and university scientists. What disturbed the scientific community was the possibility that the AEC might become "the sole authority on what is and isn't a good idea." Patently, no intellectual endeavor could afford to rely on a single arbiter. Nor could the military. As a member of AFOSR's Nuclear Physics Division summed it up, "the AEC . . . is in no better position to anticipate and satisfy military requirements in the Nuclear Sciences than is NASA to satisfy Air Force requirements in aerospace."[29]

This matter of diversified sources of support was important in one other respect. Quite understandably, the AEC had fallen into the habit of concentrating the great bulk of its support on AEC-owned installations such as Brookhaven and Argonne. This meant that a large part of the research support going to universities -- if in fact there was to be serious support of high energy physics at these institutions -- had to come from other sources. In the past, the Navy had done much to correct the imbalance by throwing the weight of its support to universities. But nothing resembling balanced support had as yet been struck. With no high energy installations of its own to worry about, AFOSR readily perceived that it was at universities that its support was most needed and where it could do the most good -- provided, of course, that the funds were forthcoming.[30]

In August 1958, the dialogue shifted from the sidelines to the center of the stage. That month, the President's Science Advisory Committee

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[28] Ibid., p. 20.

[29] William E. Wright, "A Memorandum Describing the Adverse Effects of the Present FY 1961 Budget Figure," 21 September 1959, MSS; Ray R. Heer, Jr., "Justification of the AFOSR Nuclear Science Research Program," 9 November 1960, MSS.

[30] Reed, "Briefing on Nuclear Sciences Program," ca. 1960, MSS.

and the AEC's General Advisory Committee jointly recommended increased Defense Department support. In addition, they came forth with a specific recommendation that the construction of a proposed two-mile-long, 45 Bev linear accelerator at Stanford University become the responsibility of the Department of Defense -- mea... ONR and AFOSR. Eight months later, in April 1959, James R. Killian, Jr., President Eisenhower's Special Assistant for Science and Technology, echoed the same recommendations.[31]

Then, in June, the Executive Branch made its decision -- but it was not in favor of the Defense Department. At a basic research symposium, held in New York City, the President himself announced that the Stanford proposal would be the responsibility of the AEC.[32] Despite this setback, the advocates of a Defense Department construction program, primarily the President's Science Advisory Committee, the AEC's General Advisory Committee, and the Defense Science Board, kept plugging away. It was all in vain, and in December 1960, Charles K. Reed, the head of the Nuclear Physics Division, could announce to a meeting of AFOSR's Physical Sciences Advisory Committee that "the only possible Air Force participation [in high energy physics] is through users' programs."[33] As events soon proved, even this participation would be strictly limited.

By the end of 1960, AFOSR's high energy budget had undergone a steady, if painfully slow, rise, but nothing on a scale to permit the

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[31]Heer, "Justification of the AFOSR Nuclear Science Research Program," 9 November 1960, MSS; AEC, Status of High Energy, p. 10; Science, Vol. 129 (12 June 1959), 1583. The SAC/GAC report was to the Killian Committee and was entitled, "U. S. Policy and Actions in High Energy Physics." Charles K. Reed, Presentation to Physics Advisory Committee, 23 January 1959.

[32]Dwight D. Eisenhower, "Science: Handmaiden of Freedom," in Dael Wolfle (ed.), Symposium on Basic Research (Washington: American Association for the Advancement of Science, 1959), 133-42; Science, Vol. 129 (12 June 1959), 1583 & Vol. 130 (21 August 1959), 416; Washington Post, 27 September 1961.

[33]Minutes of the Meeting of the AFOSR Physical Sciences Advisory Committee, 2 December 1960, MSS.

organization to undertake the support of significant new projects. That year, at its annual meeting, AFOSR's Physical Sciences Advisory Committee deplored "the difficulty of justifying nuclear physics research within the Air Force."[34] Two weeks after this lament, a joint panel of the President's Science Advisory Committee and the AEC's General Advisory Committee, which had been preparing for the Congress a detailed study on the status of high energy physics in the United States, joined the issue once again. In the strongest possible terms, the panel urged AFOSR, ONR, and the NSF to increase their research participation.[35] The report said in part:

> High energy physics constitutes a national program which does not fail logically into the mission of a single agency. We believe that diverse support of this field through the Atomic Energy Commission, the Department of Defense (the Office of Naval Research and the [Air Force] Office of Scientific Research), and the National Science Foundation is especially useful both to high energy physics and to the agencies concerned . . . . For diverse support to be successful, it is necessary that the fraction contributed by the agencies [ONR, AFOSR, and NSF] which now carry a smaller part of the program should be increased or at least maintained as the program expands . . . .[36]

In the spring of 1961, the Defense Science Board, headed by Dr. Robert W. Cairns, endorsed the substance of these recommendations and asked that, as a minimum requirement, every effort be made "to prevent the present DOD program from deteriorating in quality or shrinking in scope."[37] Just as things seemed to be picking up, the Basic Science

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[34]Nuclear Physics Division, "Research Program as of 22 November 1961," MSS; Minutes of the Meeting of the AFOSR Physical Sciences Advisory Committee, 2 December 1960, MSS.

[35]Agenda of the 33rd Meeting of the Coordinating Committee on Science of the Office of the Director of Defense Research and Engineering, 28 February 1962, MSS.

[36]Ibid.

[37]Ibid.; "Extract from the Defense Science Board Meeting of 28 April 1961," enclosure to ltr., William E. Wright, "Discussion of Some Present

Panel of the Air Force's Scientific Advisory Board dashed cold water on the hoped for expansion. Meeting in June 1961, the panel recommended that the Air Force decrease its relative emphasis on high energy and increase instead its support of solid state physics. "If one [report] should cancel the effect of the other, as sometimes happens in Washington," wrote Dr. Lloyd Wood, Otting's successor at the helm of the Physics Directorate, to Professor Leonard I. Schiff, the Chairman of AFOSR's Physical Sciences Advisory Committee, "we then need not be concerned!"[38]

But there was concern, for, even though the prospect that high energy physics would be sacrificed in favor of solid state did not materialize, neither was AFOSR's high energy budget expanded. A particularly acute situation arose at the end of fiscal year 1961, when the AEC parted company with ONR and AFOSR in the joint support of the Stanford Mark III accelerator, leaving the military agencies to take up the slack. AFOSR, however, had to beg off sharing the burden on a fifty-fifty basis, and the Navy was forced to fill the vacuum, including a request for emergency funds. Leonard Schiff went so far as to take up AFOSR's plight with Joseph V. Charyk, Under Secretary of the Air Force, and Dr. Knox Millsaps, AFOSR's Executive Director, took it up with Dr. Brockway McMillan, Assistant Secretary of the Air Force for Research and Development. In the end, these efforts bore little, if any, fruit for AFOSR.[39]

Problems of the DOD Basic Research Program in Elementary Particle Physics," to Lt. Col. C. K. Reed, 24 March 1961, MSS; memo, Lt. Col. Charles K. Reed, "Discussion of Tentative FY 62 Budget Allocation," to Dr. Lloyd Wood, 12 May 1961, MSS.

38 Ltr., Lloyd A. Wood to Professor L. I. Schiff, 7 July 1961, MSS.

39 "Discussion of Some Present Problems of the DOD Basic Research Program in Elementary Particle Physics," enclosure to ltr., William E. Wright to C. K. Reed, 24 March 1961, MSS; Minutes of Meeting of the AFOSR Physics Advisory Committee, 14 & 15 September 1961, MSS; draft of ltr., Randal M. Robertson, Chairman, Technical Committee on High Energy Physics, to Dr. George B. Kistiakowsky, Chairman, Federal Council for Science and Technology, 11 February 1960, MSS; ltr., William S. Rodney to Dr. Knox Millsaps, 18 October 1961, MSS; ltr., L. I. Schiff to Joseph V. Charyk, 29 September 1961, MSS; ltr., Knox Millsaps to Brockway McMillan,

Meanwhile, however, the over-all effort to beef up high energy physics across the nation was bearing fruit, and the United States was attaining a position in the field that was in no danger of being challenged from any quarter. As far as research dollars were concerned, the AEC made the most spectacular gains. During fiscal year 1961, this agency had a high energy budget of no less than $87 million. Moreover, the Navy had succeeded in surpassing its all-time high of four million dollars set in the late 1940s. AFOSR, too, reached an all-time high, with a budget around the one million-dollar mark. By fiscal year 1963, the federal high energy accelerator program was running at an annual level of $175 million.[40]

But, as these figures clearly indicate, the trend was not toward more diversity of support; indeed, if anything, the new budgets aggravated, rather than alleviated, the imbalance. During fiscal year 1956, the AEC had 84 percent of the total federal budget for high energy; three years later it had 90 percent; the following year its share surpassed even this total. The difficulties attendant on expanded participation by the Defense Department in general and the Air Force in particular in a field widely believed to be the special province of the AEC were not easily, if at all, overcome.

IV

Strictly speaking, cosmic radiation studies are just as much a form of high-energy physics as is the work conducted with multi-Bev particle accelerators. Indeed, as mentioned previously, the energies present in the cosmic-ray flux far exceed any that are yet attainable with terres-

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15 November 1961, MSS; ltr., Maj. Gen. Daniel E. Hooks to L. I. Schiff, 14 March 1962, MSS.

[40] Chart, "High Energy Accelerator Physics: Financial History and Current Support by Agencies," enclosure to "A Summary of the 14 December 1960 Report of the PSAC-GAC Panel on High Energy Accelerator Physics"; Scientific American, Vol. 209 (July 1963), 64-65.

trial accelerators, and data on the reactions between cosmic radiation and the earth's atmosphere thus provide a wealth of basic knowledge on the nature of matter and energy. However, cosmic rays are also studied as one of the environmental factors influencing the success of manned and unmanned flight operations at high altitude and in space.

When AFOSR drew up its project R-357-20, Nuclear Physics, back in the first half of 1952, the inclusion of cosmic-ray studies was justified in the following terms in the project documentation:

> Cosmic radiation is a source of extremely high energy particles which are almost impossible to duplicate in the laboratory. A study of these ultra-high energy particles will provide clues to the nature of nuclear forces and will improve our knowledge of the radiation present in the upper atmosphere, which is essential for the successful performance of personnel and equipment at high altitudes.[41]

Cosmic radiation research, along with the rest of the AFOSR nuclear physics program, subsequently became part of the catch-all Project 3750, Propulsion Sciences -- which was indicative of the prevailing official emphasis on energy questions as distinct from space environment.[42] Within a few months after the launching of the first man-made satellite, however, all this changed. Cosmic radiation was transferred to a new Project 9774, Research in Space Environment (later renamed simply Environmental Research), of which it became one distinct research task. The purpose of cosmic-ray studies was now specifically related to astronautics: "to increase our understanding of the nature of matter and to ultimately put this understanding to work to help man attain efficient and safe space flight."[43] The new project was funded in the geophysics

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[41] R & D Project Card (RDB Form 1A), Project R-357-20, 5 May 1952, p. 1. Some of the task documentation for specific contractual efforts conducted under this project did make specific reference to the "possibility of travel outside of the earth's atmosphere" (R & D Project Card, Project 357-20-2, 20 May 1952, p. 2).

[42] R & D Project Card (DD Form 613), Task 37506, Physical Research in Propulsion Sciences, 4 April 1956, pp. 22-24.

[43] R & D Project Card, Project 9774, 1 April 1958, p. 6.

(804A) program area. On the other hand, the cosmic-ray task continued to be administered by the AFOSR Nuclear Physics Division, and in particular by Mr. Ray R. Heer, Jr., who was placed in charge of this effort when he came to work for AFOSR in 1955 and nursed it from a level of four contracts totalling $135,000 in August of that year to 13 contracts and grants totalling $568,000 at the end of June 1962.[44]

The post-Sputnik environmental emphasis did not, of course, mean that the old concern with the nature of matter and energy was now wholly forgotten. It did mean that cosmic-ray research was taking on a slightly more "applied" flavor, at least from the Air Force standpoint, regardless of whether the potential applications were or were not of interest to the investigators themselves. As stated in a January 1961 presentation:

> The motivation of the majority of the groups engaged in cosmic ray research has not changed. They are still performing their studies in a quest for basic knowledge; it is simply fortuitous for the Air Force that the knowledge which has become vital to the space program was already being sought. This is not unusual, however, and, in fact, is probably the best reason for support of basic research by the Air Force.[45]

The most obvious applications of cosmic-ray data were to be found in two areas: in the analysis of radiation hazards to men and equipment in space flight, and in devising ways to cope with radiation-induced or -related disturbances of long-range communications. However, this certainly did not exhaust the list. And, despite all changes in project names and official emphasis, there has been no change in the essential approach of AFOSR-sponsored cosmic radiation research, which includes

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[44] Interview with Mr. Ray R. Heer, Jr., Nuclear Physics Division, AFOSR, by Dr. David Bushnell, OAR Historian, 25 September 1962; "AFOSR Cosmic Ray Program, Administrative Information 1955 Thru 1962," chart prepared by Mr. Heer.

[45] Ray R. Heer, Jr., "804A Research on Geophysics, 9774 Environment Research, 37665 Cosmic Ray Research," 31 January 1961 presentation to AFRD, p. 1.

recording of cosmic-ray events both at ground stations and at altitude, detailed analysis of the observational data, and related theoretical studies. It has not included experimental exposures of biological specimens to cosmic radiation, such as some other Air Force agencies have conducted.

V

With the problems of high energy physics getting the undivided attention of so many people in government during the late 1950s, it was inevitable that someone would finally conclude that other areas of nuclear physics were not getting their just due. Actually, there was little to complain about when it came to AFOSR's cosmic ray program, which was perhaps the finest in the Department of Defense. But, by the summer of 1961, some voices were uttering a few minor plaints on behalf of low energy (or more appropriately nuclear structure) physics.

Perhaps it is overdrawing it a bit, but it might well be said that the misfortunes of low energy physics stemmed in part from the good fortunes of high energy physics. Because it worked on the frontiers of science and because to many it appeared to hold the ultimate answers, high energy physics attracted wide attention and consequently the lion's share of government support. On the other hand, nuclear structure physics appeared to be involved in necessary but tedious detail. It lacked, in other words, the glamour and allure of high energy; and this fact was definitely reflected on the account sheet.[46] Accordingly, in August 1961, the staff of the National Science Foundation issued a report delineating these and other problems and warned that "if this country is to reassert its leadership and restore to low energy nuclear physics some of its original spirit of adventure, it will be necessary for the Federal government to initiate and maintain more active programs of support in this area."[47]

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[46] National Science Foundation, "The Problem of Support Emphasis for Low Energy Nuclear Physics," August 1961, MSS.

[47] Ibid.

That experimental nuclear structure physics had decreased in importance relative to the rest of AFOSR's program in nuclear physics was undeniable. For example, during fiscal year 1954, nuclear structure represented, in terms of active projects, 66 percent of the nuclear physics program. Its dominance in terms of annual rate of expenditure was comparable. Eight years later, at the time that the National Science Foundation issued its report, nuclear structure represented 23 percent of the program in terms of active contracts and 24 percent in terms of annual rate of expenditure. In seeking an explanation for this, it should be remembered that nuclear structure's decline was relative and not absolute. Moreover, the 24 percent of annual rate of expenditure figure was indicative of the fact that nuclear structure, being as it was one of four areas (high energy, structure, cosmic radiation, theory) in the program, was drawing very close to one-quarter, or its fair share, of the Division's funds. It might also be added that until fiscal year 1961, experimental nuclear structure physics maintained a consistent lead in the number of active contracts.[48]

On the entire federal level, during fiscal year 1961, the government had contributed something on the order of $30 million to the low energy field. Of this, $12.6 million had gone to universities; the rest had been expended mostly by the AEC at its own installations.[49] In comparison, high energy physics was attracting four times as much support. Moreover, between 1957 and 1961, the percentage of the total federal basic research budget devoted to nuclear structure dropped from six percent to three percent. Certainly, when one takes into account the over-

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[48] Physics Division, "Physics Division Research Program as of 1 January 1954," MSS; Nuclear Physics Division, "Nuclear Physics Research Program as of 1 July 1960," MSS; Minutes of the Meeting of the AFOSR Physics Advisory Committee, 24 April 1960, MSS.

[49] "Approximate Federal Support in Nuclear Structure Physics," incl. to ltr., C. Eugene Hunting, NSF, to Lt. Col. Joseph Duval, 12 October 1961, MSS.

all federal record, no one could accuse AFOSR of not sufficiently appreciating the importance of nuclear structure physics.

Of course, there was no thought by anyone to bring the low energy total in line with that of high energy -- low energy being a less costly affair. What the National Science Foundation was really warning against was that with most of the excitement in physics centering around high energy there was some danger that an important matter such as nuclear structure physics might be lost in the shuffle. Some of the things that the NSF feared most, mainly that nuclear structure was losing its professional allure, were in fact in the process of being remedied through a natural process. The creation of more sophisticated low energy accelerators, such as the Tandem Van de Graaff, which permitted the taking of nuclear measurements with an incredible degree of accuracy, did much to stimulate the field, as did such significant discoveries as the Mössbauer effect. In any event, the NSF felt it best to establish a panel of experts to look into the field; and, in the summer of 1961, the Foundation did just that, setting up its Advisory Panel on Nuclear Structure Physics.[50]

The panel was composed of working nuclear structure physicists from the nation's universities, but it was assisted to a certain degree by government liaison representatives, who were invited to attend one of the panel's meetings. Colonel Joseph E. Duval, Colonel Reed's successor as the Nuclear Physics Division's chief, was picked as AFOSR's liaison representative to the panel. By March 1960, the panel's work was done and its report issued.[51]

The chief note struck by the panel, besides the inevitable recommendation for more federal funds, was for diversified sources of support; and in this respect the problems facing nuclear structure were a repetition of the problems that faced high energy. It is too early as

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[50]NSF, "The Problem of Support Emphasis of LENP," August 1961.

[51]Ltrs., J. Howard McMillen, Program Director for Physics, NSF, to Dr. Knox Millsaps, 19 July 1961; Dr. Lloyd Wood to J. Howard McMillen, 1 August 1961, MSS.

yet to appraise the effect of the panel's recommendations on the federal program in general and AFOSR's program in particular, but if the experience of the various high energy panels is to serve as a guide, they will probably have very little effect on the latter.[52] Indeed, the future of low energy physics in the Air Force appears to be no brighter than that of high energy. The trouble again is rising costs.[53] During the late 1950s, AFOSR was able to take on two large on-site research programs in this area -- one at the Washington University of St. Louis, another at Florida State University. At present, the cost of these two projects is running over half a million dollars a year. An additional project or two such as these to go along with the expensive projects in high energy and cosmic radiation, and AFOSR's budget for nuclear physics would be well taken care of. It had always been AFOSR's intention to sponsor a qualitative rather than a quantitative program; but it had never been its intention to become quite so narrowly selective.

VI

Even as early as 1958 the Nuclear Physics Division was coming to the conclusion that it was being priced out of the experimental field. As Colonel Reed, along with Dr. Otting, began to give the problem more thought, it became evident that the program would have to undergo some realignment.[54] The most obvious solution, and the one which Reed favored, was to place increasing emphasis on theoretical studies. Supporting such studies had certain definite advantages. For one thing, they were cheap. With a sum of money that would amount to only a fraction of the cost of one on-site high energy study, AFOSR could diversify its program

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[52] National Science Foundation, Research Trends, 1962-1967: Nuclear Structure Physics (National Science Foundation, 1962), passim.

[53] Lt. Col. Charles K. Reed, personal interview with N. A. Komons, 15 March 1963.

[54] Ibid.

considerably and at the same time increase its contact with the important problems in nuclear physics. There were other reasons dictating such a course, and at this early date they were even more compelling than the financial. Neglected by other government agencies, theoretical nuclear physics was in need of help. This meant, in turn, that some of the best theoretical physicists in the world would be available to AFOSR.[55]

It did not take much of an effort to double, triple, or even quadruple the theoretical program. In 1954, the program had begun rather inauspiciously with one contract totaling a mere four thousand dollars. By the time Reed came on the scene, in 1957, there were six contracts financed at an annual rate of $120 thousand. In three years time a member of the Nuclear Physics Division could inform a prospective contractor that "our theoretical program is the fastest growing program we have . . . ." And well he might, for in that year, 1960, the program had 21 active projects and was costing the Division in the neighborhood of $820 thousand annually. It had by this time outstripped the other three program areas in the number of active projects and stood second only to experimental high energy in its annual rate of expenditures.[56]

The growth of the theoretical program did not necessarily progress at random or without design. The new emphasis on theory, as mentioned previously, was in a sense AFOSR's way of maintaining broad contact with nuclear physics in the face of restricted budgets and rising costs. This could not be done, however, by supporting theorists who were largely isolated from the activities of the more important accelerator sites. If AFOSR was unable to participate very actively in supporting the actual experiments at these sites, then it would sponsor theoretical phys-

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[55] Major Charles K. Reed, "Theoretical Nuclear Physics," presentation to AFOSR Physical Sciences Advisory Committee, ca. 1958, MSS.

[56] Physics Division, "Physics Division Research Program as of 1 January 1954," MSS; Reed, "Presentation, 1961," MSS; ltr., Ray R. Heer, Jr., to Emmett L. Hudspeth, 25 March 1960, MSS.

icists who had firsthand access to the data gathered at these sites.[57] The first project of this sort sponsored by AFOSR was actually entered into in 1952 with Stanford University, where a group of theoretical physicists, headed by Leonard I. Schiff, played a prominent role in interpreting the data coming from the Mark III linac and suggested experiments to Hofstadter and others. So well was AFOSR pleased with the results of this project that it played a key role, in 1958, in the founding of the Stanford Institute of Theoretical Physics -- an outgrowth of the original Schiff group. Today, AFOSR sponsors the Institute's entire effort in theoretical physics, whether it be in elementary particle theory, relativity, or gravity research. When the two-mile linear accelerator ultimately comes into operation, the Institute is expected to perform the same tasks for this machine as it now does for the Mark III.[58]

In addition to Stanford, AFOSR reached into Harvard for another theoretical group in order to take advantage of the 6 Bev synchrotron on that campus. At Princeton, Professor Marvin Goldberger, who had access to the data of the 3 Bev Penn-Princeton proton accelerator, was brought under AFOSR support in 1958. In 1960, the Division extended a grant to the Institute for Advanced Study, Princeton, headed by J. Robert Oppenheimer, where year in year out the world's most renowned theoreticians come to do independent work. A no less impressive list was garnered by AFOSR in Europe: Abdus Salam, the Imperial College of Science and Technology, London; John Hamilton, University College, London; Walter Thirring, the University of Vienna; Maurice Levy, the University of Paris; H. J. Lipkin, Weizmann Institute of Science, Rehovoth, Israel.

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[57] Reed, "Briefing on Nuclear Sciences Program," ca. 1960, MSS; Heer, personal interview with N. A. Komons, 6 March 1963; Reed, personal interview with N. A. Komons, 15 March 1963.

[58] L. I. Schiff, "Quarterly Progress Report No. 1, Contract AF 18 (600)-545," 3 March 1953, MSS; Reed, "Briefing on Nuclear Sciences Program," ca. 1960, MSS; Heer, personal interview with N. A. Komons, 6 March 1963; ltr., Mel White, Chief, Information Services, AFOSR, to Capt. Carol Williams, ARDC, 18 November 1958, MSS; AFOSR, Projections, 1962, p. 16.

In the case of the European theoreticians, all of them worked intimately with experimentalists at the 28 Bev accelerator at C.E.R.N.[59]

When AFOSR first began considering an increased emphasis on theoretical studies, there was no thought given to a major recasting of the Division's program areas. The failure of the effort to increase the Defense Department's participation in high energy physics and the increasing costs of low energy physics, however, prompted AFOSR to think anew about its role in the nuclear physics field. No definite course of action has been decided upon as yet, but the indications are that unless there is a substantial, but entirely unexpected, rise in the Nuclear Physics Division budget a good many of the more expensive experimental projects in high energy and nuclear structure will be dropped. The program will then be recast, with major reliance placed upon theoretical efforts.[60]

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[59]RDT & E Project Card Continuation (DD Form 613c) Project No. 9750, 23 January 1961, pp. 32-53.

[60]Charles K. Reed, personal interview with N. A. Komons, 15 March 1963.

Chapter III

EXPERIMENTAL HIGH ENERGY PHYSICS

The dominant task of science throughout its history has been a search for unifying concepts. Implicit in this search is the unswerving faith of the individual scientist that nature, beneath a deceptive facade of chaos, is orderly, beautiful, and harmonious. It is a faith neither original nor exclusive with science. Being essentially an expression of aesthetic and cultural values, it has been propounded in one form or another in art, literature, and philosophy, and received what was perhaps its noblest artistic expression in the simple, symmetrical temples of the ancient Greeks. Yet, as unscientific as this faith may seem, it has formed the touchstone of science from the very beginning. And it will probably continue to do so for as long as science exists, for, like all matters devolving from within, it will not be easily overthrown by the mere appearance of things. "If nature were not beautiful," said Henri Poincaré, striking a chord usually reserved for the poet, "it would not be worth knowing, and if nature were not worth knowing, life would not be worth living." "[It] is more important to have beauty in one's equations," P. A. M. Dirac advised his fellow physicists, "than to have them fit experiment." Thus, the search for order, beauty, and harmony goes on.[1]

Perhaps the strangest and most chaotic area of modern science -- one which has repeatedly eluded the application of unifying concepts -- is

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[1]Abdus Salam, "Elementary Particles," in Arthur Garratt (ed.), Penguin Science Survey 1961 (2 vols.; Baltimore: Penguin Books, 1961), I, 31; Murray Gell-Mann and E. P. Rosenbaum, "Elementary Particles," Scientific American, Vol. 197 (July 1957), 72; Schneer, op. cit., pp. 13-20; Philip P. Wiener and Aaron Noland (eds.), Roots of Scientific Thought (New York: Basic Books, 1957), v; Hoffmann, op. cit., p. 268; Roman, op. cit., p. 6; Poincaré quoted in Verne H. Booth, Physical Science (New York: MacMillan Company, 1962), 151-52; P. A. M. Dirac, "The Evolution of the Physicist's Picture of Nature," Scientific American, Vol. 208 (May, 1963), 47.

that of elementary particle physics. Some thirty years ago, the world of elementary particles, inhabited only by photons, electrons, protons and neutrons, was orderly, harmonious, and, as many believed, essentially complete. What was essentially complete, however, was only the theory of the atom, with nearly all its properties capable of being deduced by mathematics in terms of the motions of negatively charged electrons around positively charged nuclei. But as the new high energy machines permitted physicists to probe deeper into the atom's core, events were found which could not be explained by the familiar electromagnetic theorems: the forces between nucleons seemed to lie out of the realm of Coulomb's Law. To make matters worse, a bewildering host of new particles began to appear, turning the initial excitement of discovery into a kind of anguish. Pions, muons, kaons, lambdas, sigmas, rhos -- a veritible nuclear zoo with anywhere from sixteen to thirty-six members, depending upon one's point of view. It was difficult enough to keep up with this confusing jumble, but even more difficult now was determining what constituted "elementarity." That the new particles differed markedly from the old was plain enough. Created in particle collisions during scattering experiments, many of them lived but a fraction of a billionth of a second. While the existence of some could be accounted for, others fitted nowhere in the scheme of things, and the word "strangeness" passed into the vocabulary of physics. Needless to say, the simple and orderly relationship that existed between the old particles was nowhere to be found in the new.[2]

Thus, the questions that the new discoveries posed were as numerous as they were immense, and high energy physicists seemed to have their work cut out for them for a long time to come. What, after all, were the truly elementary constituents of matter? What simple and orderly relationship existed between these particles? Moreover, were there more elementary particles to be found?

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[2]Gell-Mann and Rosenbaum, op. cit., passim; R. D. Hill, "Resonance Particles," Scientific American, Vol. 208 (January 1963), 39; Hoffmann, op. cit., p. 268; Roman, op. cit., p. 2.

II

These and like questions were in the air in 1951, when Stanford University unveiled its new linear accelerator, the so-called Mark III. Three years in the building, the machine was capable of accelerating electrons at an energy level of hundreds of millions of electron volts. What made the Mark III particularly distinctive was that it accelerated electrons rather than protons. Protons had indeed been accelerated to such energies, and while they proved very useful in a great many ways, they did not tell very much about the structure of nucleons. The force between nucleons, besides being the strongest force in nature, is one which man knew very little about. But electromagnetism -- the force between electrons and nucleons -- is something which science had understood for many years. Thus, the Stanford accelerator held out the very distinct possibility that the nucleons themselves would soon be probed.

To Robert Hofstadter, a young experimental physicist who had but recently arrived on the Stanford campus, this was truly an exciting prospect. Armed with a small grant from the Research Corporation, Hofstadter selected a few associates and began thinking about ways to attack the nucleus and its constituents.[3] His efforts would land him a Nobel Prize ten years later.

The experimental procedure finally worked out by Hofstadter was about as simple in principle as that employed by Rutherford half a century before. The basic idea was to bombard nuclei with electrons and observe how the electrons scattered. The Mark III accelerator, however, required much more sophisticated experimental apparatus than Rutherford's simple alpha ray emitter. In the first stage, so to speak, the

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[3] Wolfgang Panofsky, "The Linear Accelerator," Scientific American, Vol. 191 (October 1954), 40-44; L. I. Schiff, transcript of statement submitted to the Research Subcommittee of the Joint Committee on Atomic Energy, 13 February 1958, MSS; Robert Hofstadter, "Proposal for Research on Electron Scattering and Nuclear Structure," September 1952, MSS; Robert Hofstadter, "The Atomic Nucleus," Scientific American, Vol. 195 (July 1956), 58, 61.

high-energy electrons accelerated by the Mark III were passed through a magnetic field. Here they were sorted according to their momentum, and those electrons possessing the required energy (135 Mev) were passed through a narrow slit, directed against the target material, and there scattered in all directions. Finally, another magnet, equipped to handle energies up to 135 Mev, sorted out the scattered electrons once again, accepting those close to 130 Mev, which had been deflected as a result of elastic scattering, and rejecting all others. Ultimately, the accepted electrons were fed into a Cerenkov counter.[4]

The heart of this scattering method was that electrons would be deflected at particular angles, depending upon the structure of the nucleus. If a nucleus was tightly packed, there would be a considerable amount of scattering at large angles; if the nucleus was diffused or smeared out, backward deflection would be reduced in favor of forward scattering. In this way, Hofstadter expected to get some idea of the interior structure of nuclei and nucleons.[5]

By the time Hofstadter had his apparatus set up and in working order -- a job that took approximately two years -- more monetary aid was coming his way. The Office of Naval Research had paid for his apparatus, as it had paid for the linear accelerator itself, and was about to defray part of the costs for running his experiments. Then, in 1953, AFOSR agreed to finance a portion of the experimental costs under contract. The Atomic Energy Commission also entered the picture, and for a time these three agencies shared the costs of the project equally -- and well they might, for they ran to approximately one million dollars annually. The AEC finally dropped out of the picture in 1961. AFOSR has retained its connection with the project to this day.[6]

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[4] Robert Hofstadter, "Electron Scattering and Nuclear Structure" (AFOSR TR 57-34a), pp. 40-53; Hofstadter, "Atomic Nucleus," pp. 60-61.

[5] Ibid.

[6] Ltr., William E. Wright, Nuclear Physics Branch, ONR, to William J. Otting, 18 January 1957, MSS; R & D Project Card (DD Form 613) Project No. 3760, 4 April 1956, p. 12.

By early 1953, the scattering program got under way. For the first two years or so the main concentration was on heavy nuclei. Gold, tantalum, lead, and the nuclei of other heavy elements were fired upon with electrons. From this the group graduated to lighter nuclei, hydrogen, deuterium, and helium, but with an eye not just on the nucleus but also on the individual nucleons themselves: the proton and the neutron. Eventually a larger spectrometer, with an enormous, D-shaped, 45-ton magnet capable of analyzing electrons accelerated to 550 Mev, was built, and even deeper probes were made. Periodically, while Hofstadter and his associates were busy making their measurements, a group of theoretical physicists from Stanford's Institute of Theoretical Physics were invited to construct theoretical curves for various nuclear models. "The agreement between the experimental and theoretical curves," Hofstadter said later, "[was] nothing short of astonishing." But even more astonishing was the fact that such results were arrived at experimentally.[7]

The nuclear model which Hofstadter ultimately constructed was different in many important ways from earlier models, especially those constructed from data gathered from nucleon-nucleon interactions. Hofstadter's nucleus was blurred or smeared out at the edges. It had a "skin" of considerable thickness, one which constituted a region of decreasing density towards the outer edge. This thickness was found to be constant for nuclei between calcium and uranium. Moreover, the nucleus possessed a relatively uniform charge distribution, which tapered off gradually from the center to the outer edge.[8]

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[7]Hofstadter, "Proposal for Continuation of Research on Electron Scattering and Nuclear Structure," August 1953, MSS; Hofstadter, "Atomic Nucleus," pp. 63-64; R. Hofstadter, H. R. Fechter, and J. A. McIntyre, "Scattering of High Energy Electrons and the Method of Nuclear Recoil," Physical Review, Vol. 91 (16 July 1953), 422-23.

[8]Hofstadter, "Proposal for Continuation of Research on Electron Scattering and Nuclear Structure," September 1955, MSS; R. Hofstadter, H. R. Fechter, and J. A. McIntyre, "High-Energy Electron Scattering and Nuclear Structure Determinations," Physical Review, Vol. 95 (15 November 1953), 978-89.

All along, physicists had been observing the scattering patterns of high energy protons and had gotten the impression that the nucleus had a rather sharp boundary. What was really happening here, as Hofstadter explained, was that the proton, since it was capable of interacting effectively only with the outer layers of the nucleus, was sending back nuclear diffraction data that did not represent an interaction with the entire nuclear volume. The electron, however, free of the muddling effects of nucleon-nucleon reactions, peered into the entire nucleus, as it were. As Hofstadter had expected, the electron was proving to be an effective probe.[9]

The proton and the neutron proved to be surprisingly similar structures; indeed, as some theoretical physicists had suspected for some years, the proton and the neutron were really two different aspects of the same entity -- the nucleon. Both were equal in magnetic size. Each was made up of a fog-like cloud of mesons. Each increased in density towards an apparent hard-core center. And each had a diameter of approximately one forty-thousands of a billionth of an inch. The only difference between them -- a difference known for many years -- was their charge. In the case of the proton, the charged mesonic clouds added together; in the case of the neutron, they cancelled out.[10]

"The history of physics shows," said Robert Hofstadter, striking the theme of his Nobel lecture, "that whenever experimental techniques advance to the extent that matter, as then known, can be analyzed by reliable . . . methods into its 'elemental' parts, newer and more powerful studies subsequently show that the 'elementary particles' have a structure themselves. Indeed this structure may be quite complex, so that the elegant idea of elementarity must be abandoned."[11] This was the

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[9]Hofstadter, Fechter, and McIntyre, "High-Energy Electron Scattering," 986-87.

[10]Robert Hofstadter, "Structure of Nuclei and Nucleons," Science, Vol. 136 (22 June 1962), 1013-22; Hofstadter, "Quarterly Progress Report on Research on Electron Scattering," January 1956, MSS; Hofstadter, "Atomic Nucleus," passim.

[11]Hofstadter, "Structure of Nuclei and Nucleons," p. 1013.

case with the atom at the turn of the century, when Thomson and Rutherford found it to be discontinuous. And it now appears to be the case with the nucleon. Once believed to be a simple, indivisible particle, it is now but another complex physical body. Elementarity is an elusive, if persistent, idea.

III

J. Robert Oppenheimer once noted that if a particular scientific inquiry is well conceived it will not merely come up with a new answer, but with something far more valuable: a new question. "Out of such questions, and their progeny," he continued, "the growth of science and the growth of practice both arise."[12] Robert Hofstadter's investigation of the nucleon was just such an inquiry, raising questions as it answered them and sending physicists on the road to new discoveries. One experimental physicist to travel that road was Aihud Pevsner of The Johns Hopkins University.

The high energy group at Hopkins had been gathered together in mid-1956. Its beginnings, in contrast to the Stanford group, were modest. With no accelerator site of their own and with no other equipment to speak of, they were forced to journey from one end of the country to the other in order to run off any sort of experiment. But they did have the financial support of AFOSR -- $100 thousand annually -- and this saw them through. Pevsner's primary interest at the start was investigating the properties of the many newly discovered particles. After working for a year or so on emulsion stacks that had been exposed to high energy kaons at the Berkeley bevatron, Pevsner's group turned its attention in 1957 to the helium bubble chamber -- a device constructed by a group headed by Professor Martin Block of Duke University. The bubble chamber studies, conducted both at the Brookhaven National Laboratory and the University of California, launched the group in many directions: parity of the K-meson; decay asymmetries of the lambda; parity violation in

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[12] J. Robert Oppenheimer, "The Need for New Knowledge," in Wolfle (ed.), op. cit., p. 7.

strong interactions; hyperfragment rates, bindings, decays, and lifetimes. By late 1960 and early 1961, however, Pevsner's interest was turning increasingly to the discovery of new particles.[13]

There was really nothing new in the notion that still more elementary particles existed; theoretical physicists, particularly Heisenberg in Germany and Gell-Mann and Sakurai in the United States, had in fact been predicting the existence of such particles all through the 1950s. Intensifying this kind of speculation, and actually sending Pevsner on the trail of a new particle, were Hofstadter's electron scattering studies.[14] These experiments, it will be recalled, indicated that the magnetic properties of nucleons were not concentrated at a point, but distributed over a finite space. This discovery did not fit very satisfactorily with the then prevailing theories of the proton and neutron. These theories were based on the concept first expounded in 1935 by a Japanese theoretical physicist, Hideki Yukawa, that protons and neutrons bound themselves together in the nucleus by continually emitting and reabsorbing virtual pions. It was originally believed that the pions in this emission-absorption process were non-interacting. But in order to account for the unexpected charge distribution of the nucleon, physicists were now forced to conclude that the emitted pions did indeed interact. It was postulated that a pair of pions, while out on the nucleon cloud, were strongly attracted to each other, and formed what is known as a resonance. This pion interaction or resonance did in fact explain the charge distribution of the proton and neutron, but it raised as many questions as it answered. For example, were the two interacting pions merely two pions or were they really an elementary particle that

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[13]Aihud Pevsner, "Final Report on Air Force Contract 18 (603)-143," 7 June 1963, MSS; ltrs., Pevsner to Ray R. Heer, Jr., 27 July 1959, MSS; Pevsner to William Rodney, Nuclear Physics Division, AFOSR, 13 January 1961, MSS.

[14]Ltr., Pevsner to Ray Heer, 28 March 1962, MSS: R. D. Hill, "Resonance Particles," Scientific American, Vol. 208 (January 1963), 44.

broke down into two pions? It was this kind of questioning that spurred on the quest.[15]

How does one go about looking for a new particle? How indeed do physicists differentiate one particle from another? Difference in mass is, of course, an obvious way, as is difference in lifetime. But it would be two other characteristics of particles -- angular momentum and isotopic spin -- that would play a particularly key role in the identification of pion-pion resonances. Angular momentum is a basic quantity associated with the spin and orbital motion of a particle or a group of particles. Discrete values of angular momentum are designated by quantum numbers such as +1, +½, o, -½, -1. Plus refers to spin in one direction, minus to another direction. Isotopic spin, despite its name, has nothing to do with momentum. It is based, rather, upon the concept that the neutron and the proton are different charge states of the same particle, the nucleon. From the nucleon, the concept of isotopic spin is expanded to other particles; and quantum numbers are assigned which categorize these particles' charge state. To the physicist, these quantum numbers for angular momentum and isotopic spin have a deep significance, for the probability of various interactions between particles is very much dependent on them. And it was with the use of these numbers that physicists began calculating the possible interactions pions could take part in that would account for the nucleon's charge distribution.[16]

In June 1957, Yoichiro Nambu of the Enrico Fermi Institute for Nuclear Studies took a stab at the problem. He not only suggested what kind of resonance physicists should look for, but also predicted that the resonance was likely to be a genuine elementary particle. The particular particle that Nambu had in mind would decay into two pions, would

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[15]Abdus Salam, "Elementary Particles," in Arthur Garratt (ed.), Penguin Science Survey 1961 (2 vols.; Baltimore: Penguin Books, 1961), I, 43-45; Hill, op. cit., p. 44; ltr., Pevsner to Heer, 28 March 1962, MSS; Murray Gell-Mann and E. P. Rosenbaum, "Elementary Particles," Scientific American, Vol. 197 (July 1957), 77-79.

[16]Hill, op. cit., pp. 41-42; ltr., Pevsner to Heer, 28 March 1962, MSS.

have an isotopic spin of zero, and a mass of two to three times that of an ordinary pion.[17] Two years later, William R. Frazer and José R. Fulco, both of the Lawrence Radiation Laboratory at Berkeley, suggested a slightly different particle. A bit heavier than Nambu's particle, it had a rest mass equivalent to an energy of approximately 600 Mev, an isotopic spin of 1, and an angular momentum of 1. Like Nambu's particle, however, it was to decay into two pions. These suggestions and others gave experimental physicists a good basis to go on.[18]

In June 1961, exactly four years after Nambu's original suggestion, the first pion-pion resonance was tracked down -- the rho. But it was closer to the particle predicted by Frazer and Fulco than that by Nambu. Indeed, its isotopic spin, its angular momentum, and decay mode were in perfect agreement with their prediction. But at 760 Mev, the particle appeared to be a bit overweight. This, at least, was Pevsner's feeling. Thus, it was still uncertain that the rho was the particle responsible for Hofstadter's experimental results.[19]

Meanwhile, other physicists, particularly a group at the University of California, were taking another tack. In February 1960, Geoffrey F. Chew, a theoretical physicist at Berkeley, declared that the charge distribution of the nucleon could perhaps best be explained by an interaction involving three, rather than two, pions. Before too long, in September 1961, physicists at Berkeley found a three-pion resonance, the omega.[20]

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[17] Yoichiro Nambu, "Possible Existence of a Heavy Neutral Meson," Physical Review, Vol. 106 (15 June 1957), 1366-67.

[18] William R. Frazer and José R. Fulco, "Effect of a Pion-Pion Scattering Resonance on Nucleon Structure," Physical Review Letters, Vol. 2 (15 April 1959), 356-58; Hill, op. cit., p. 44.

[19] A. P. Erwin, R. March, W. D. Walker, and E. West, "Evidence for a π-π Resonance in the $I = 1$, $J = 1$ State," Physical Review Letters, Vol. 6 (1 June 1961), 628-30; ltr., Pevsner to Heer, 28 March 1962, MSS.

[20] Geoffrey F. Chew, "Three-Pion Resonance or Bound State," Physical Review Letters, Vol. 4 (1 February 1960), 142-43; B. C. Maglić, L. W.

Pevsner confirmed the Berkeley group's findings almost immediately; he had in fact been on the trail of the same particle. And although his data was not as complete as that of the Berkeley group, he merely had to look at the curve he had been plotting to see its long fingers graphically protruding at just below the 800 Mev range. It was definitely the omega; but was the omega the particle Chew predicted? Pevsner thought not, even though the data on the particle's spin and isotopic spin were not in yet. At 790 Mev, the omega had too much mass.[21]

On the same curve on which Pevsner had plotted and confirmed the discovery of the Berkeley group, another bump was showing its head -- this one at about 540 Mev. Just yet his data was not good enough for a public announcement, but Pevsner strongly suspected that he was on the trail of the predicted particle. "If additional events do not cause the bump to go away . . .," he wrote in September 1961, "this might cause even more excitement than the [omega] particle."[22]

For several months, Pevsner's group, in collaboration with a high energy group from Northwestern University, had been exposing the Alvarez 72-inch deuterium bubble chamber at the Lawrence Radiation Laboratory to a beam of 1.23 Bev pions. In all, they made some 35 thousand exposures. It was data from the partial analysis of these exposures that confirmed the existence of the omega. By December the group had analyzed the rest of the film, and to the surprise of no one, the bump on the curve was still very much there. The particle weighed in a 550 Mev (the mass Pevsner was hoping for), was neutral in charge (again what Pevsner had hoped for), and was given the name, eta. It decayed into three pions, so it was very definitely a first cousin of the omega and a somewhat more distant cousin of the rho. But it, too, like the others, did not correspond in all particulars to the predictions of the theoretical physicists. The

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Alvarez, A. H. Rosenfeld, and M. L. Stevenson, "Evidence for a T = 0 Three-Pion Resonance," Physical Review Letters, Vol. 7 (1 September 1961), 178-82; Hill, op. cit., pp. 44-45.

[21] Ltr., Pevsner to William Rodney, 6 September 1961, MSS; Science News Letter (12 May 1962), 292; ltr., Pevsner to Heer, 28 March 1962, MSS.

[22] Ltr., Pevsner to Rodney, 6 September 1961, MSS.

particle's angular momentum, instead of being 1, turned out to be zero.[23] In any event, whether agreeing perfectly with prediction or not, something new had been discovered.

What indeed had been discovered? The men who made the actual discoveries were rather hesitant in saying unequivocally that they had discovered new elementary particles. Pevsner, like the others, guardedly referred to his discovery as a "resonance," or a "resonance particle." Whenever the term particle was used alone, it was used in such a context that it would be understood that one meant a "resonance." All this really meant was that Pevsner would only admit to finding three pions that were very strongly attracted to each other. But consider the difficulties Pevsner was up against. The old particles such as the electron and the proton were stable entities that could easily be tracked in emulsion. The neutron's lifetime could be measured in terms of minutes. Even the strange particles that came later, while decaying in about a ten-billionth of a second, could still be tracked. But the so-called "resonances," like the rho, omega, and eta, flew apart so fast (a hundred-thousandth of a billion-billionth of a second) that they left no visible tracks. Their existence was merely inferred from the tracks left by their decay products -- in this case the pions.[24] Under the circumstances, it is still just about anyone's guess whether the rho, omega, and eta are merely pions moving together for a short time or true elementary particles.

IV

The discovery of the eta, omega, rho, and other like particles has added to the already considerable mental anguish of the elementary particle physicist. Each new discovery, if it merely tends to muddy an

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[23] Ltrs., Pevsner to Rodney, 13 January 1961, MSS; Pevsner to Heer, 28 March 1962, MSS; Pevsner, "Final Report," pp. 7-8; Science News Letter (12 May 1962), 292, Aihud Pevsner, et al., "Evidence of a Three-Pion Resonance Near 550 Mev," Physical Review Letters, Vol. 7 (1 December 1961), 412-13; Hill, op. cit., p. 42.

[24] Hill, op. cit., passim.

already confusing picture, is in a sense a step backward -- a forced retreat from simplicity. Such anguish has not always followed the discovery of new particles. The discovery of antiparticles is a clear case in point. The existence of both matter and antimatter was, as one scientist put it, "a pleasing confirmation of the symmetry of the universe."[25]

It was over a rather circuitous route that man first conceived of the idea of antimatter. In 1928, P. A. M. Dirac, the English theoretical physicist, sat down to construct an equation that described the motion of a free electron or proton. Dirac set for himself one guiding criterion: the equation should be invarient -- that is, the particle should appear the same both to a moving and to a stationary observer. Dirac succeeded, but he was due for a surprise. The equation did indeed describe an electron, but it also described another particle, a particle identical to the electron except in one important respect -- it carried negative energy. Such a particle, if accelerated by an electric field, would actually decelerate; if pushed up, it would travel down; if shoved to the right, it would go to the left. If it met up with its opposite number, it would make for it, causing the annihilation of both particles and leaving only energy in the form of gamma rays. The idea simply outran common sense, and a conspiracy of silence hung over Dirac's equation for a number of years. Then, in 1932, came unexpected experimental proof. Carl David Anderson, an American physicist, accidentally stumbled upon an anti-electron, or as he called it, a positron, in a Wilson cloud chamber. Once again, Dirac swung into action, this time carrying his theory to its logical conclusion. If electron-positron pairs exist, Dirac reasoned, then all particles must have their corresponding antiparticles. Dirac was proposing that all the solutions of his relativistic equation represented reality. But until experimental proof was found, the extension of Dirac's theory to protons and other particles was open to question.[26]

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[25]Asimov, Intelligent Man's Guide, I, 252.

[26]Salam, op. cit., pp. 36-38; Wilson and Littauer, op. cit., p. 138; Asimov, Intelligent Man's Guide, I, 238-39; Massey, op. cit., pp. 126-30; Gamow, Biography of Physics, pp. 263-67.

With the proton almost two thousand times as massive as the electron, experimental proof was slow in coming. Cosmic ray events had failed to yield conclusive evidence, and the accelerators of the day were not up to the job of producing antimatter. Physicists would have to await the development of multi-Bev accelerators. In 1955, a year after the giant bevatron at the University of California was constructed, Owen Chamberlain and Emilio Segrè trained a beam of 6.2 Bev protons on copper nuclei and trapped 60 antiprotons.[27] From now on there was no questioning the Dirac equation.

Just as everyone had suspected, the antiproton was an extremely short-lived particle in the presence of ordinary matter. Because of their opposite characters, protons and antiprotons have a tremendous attraction for each other. The end result is a violent collision in which both particles are annihilated -- leaving their equivalent in energy and minor particles. Physicists were extremely interested in this annihilation process, and the next step was to set up more experiments that would throw light on it and other questions. Fittingly, a group of physicists at Berkeley were the first to launch such experiments.[28] One of the men in this group was A. G. Ekspong, on leave from the Institute of Physics, Uppsala University. When he returned to Uppsala, as a member of Professor Kai Siegbahn's experimental group -- an AFOSR European contractor -- he brought with him one stack of nuclear emulsions that had been exposed to an antiparticle beam of 700 Mev/c. Along with an associate, B. E. Ronne, Ekspong got busy scanning and analyzing the stack.[29]

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[27] Owen Chamberlain, Emilio Segrè, Clyde Wiegand, and Thomas Ypsilantis, "Observation of Antiprotons," Physical Review, Vol. 100 (1 November 1955), 947-50.

[28] W. H. Barkus, R. W. Birge, W. W. Chupp, A. G. Ekspong, et al., "Antiproton-Nucleon Annihilation Process," Physical Review, Vol. 105 (1 February 1957), 1037-58.

[29] A. G. Ekspong, S. Johansson, and B. E. Ronne, "Antiprotons in Nuclear Emulsions" (AFOSR TN 58-82), 1.

The stack consisted of 120 pellicles, and after twice scanning, Ekspong identified 10 events as due to antiprotons. But for every antiproton produced, Ekspong found that there had been 550 thousand pions produced. This gives an indication of the enormity of the task of producing an antiproton and then finding it. From this very small sample of events, Ekspong and Ronne compared their data with the data of the Berkeley group. Where their data differed from the earlier analysis was in the number of pions interacting per star. The earlier data fixed this number at 1.3 pions; Ekspong revised it upward at 2.0 pions. In general, however, the data was in good agreement, especially in the important matter of the annihilation process. This process, Ekspong suggested, as did his earlier collaborators, took place in a region outside the nuclear surface. Ekspong based this conclusion on the fact that the number of pions that were absorbed or inelastically scattered was rather small.[30]

This small run of 120 pellicles was, of course, only the beginning, and Ekspong, in the company of Ronne and others, was back at Berkeley with another emulsion stack. This one yielded 200 antiprotons, and Ekspong made measurements of the elastic, inelastic, and annihilation cross sections, as well as working out the end products and secondary products of the annihilation process. Another run followed closely on the heels of this one.[31]

After analyzing over 350 events, Ekspong and Ronne came up with these results. When a proton and an antiproton collided at rest, the average pions emitted per annihilation were 4.68, give or take 0.12; when they collided in flight, the average pions emitted per annihilation were 5.11, give or take 0.12. The total energy of the primary process

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[30] Ibid., passim.

[31] A. G. Ekspong and B. E. Ronne, "Antiproton Interactions," (AFOSR TN 58-878), passim; R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1960, p. 5.

from a collision at rest was 391 Mev, give or take 10 Mev; in flight, 390 Mev, give or take 9 Mev. The differences between the values at rest and in flight were well accounted for. For one thing, the pions were emitted isotropically. For another, the antiproton at rest was annihilated on the nuclear surface, while the antiproton in flight travelled a mean free path in the nucleus before it was annihilated. The next step was to compare these results with existing statistical theories. What proved of interest was that the predictions of all the three leading statistical theories were compatible with these experimental results; thus no decision was made in favor of any particular theory. Upon the expiration of the Siegbahn contract, in late 1960, Ekspong was awarded a grant in his own right by AFOSR to continue the antiproton studies -- this time with particular attention on new forms of antimatter.[32]

V

In 1952, two Polish cosmic ray physicists, Marian Danysz and Jerzy Pniewski, examining a solid block of nuclear photographic emulsion after a high altitude balloon flight, stumbled upon an unusual and somewhat perplexing event. They found that a primary cosmic ray had struck a nucleus of either silver or bromine and had caused its disintegration, as evidenced by the star-like effect (numerous tracks shooting out in all directions from a common center) on their emulsion. This was all rather ordinary: a collision between a cosmic ray and an atomic nucleus had produced a star. What puzzled them, however, was that at the end of one of the tracks there was another star, this one quite smaller than the first. This could only have meant there had been two nuclear disintegrations. How was this possible? Normal nuclear matter could not have

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[32] A. G. Ekspong, A. Frisk, S. Nilsson, and B. E. Ronne, "Antiproton Annihilations in Complex Nuclei" (AFOSR TN 60-937), passim; RDT & E Project Card (DD Form 613c) Project No. 9750, 16 January 1961, pp. 5-6; RDT & E Project Card (DD Form 613c) Project No. 9750, 1 February 1962, p. 25, atch. 1.

carried away enough energy from the first star to produce a second star. Since normal matter could not carry such energy (estimated to have been between 140 and 180 Mev), then it must have been carried by some larger, highly unstable entity. Reconstructing the event, Danysz and Pniewski concluded that during the disintegration of the first star a heavy strange particle had somehow dislodged and taken the place of one of the protons or neutrons in the nucleus -- thus forming a heavy, or hyper-, nucleus. Time proved Danysz and Pniewski right, and a brand new field of study, the field of hypernuclei, was ushered in.[33]

One of the reasons physicists were attracted to the study of hypernuclei was that it afforded them the chance to observe strange particles and nuclei at the same time. Moreover, since a hypernucleus was really an analogue of an ordinary nucleus, it presented the nuclear physicist with a rare opportunity to exploit a powerful conceptual tool, analogy. For a field like elementary particle physics, which is still largely phenomenological, this was a particularly welcome event.[34]

Among the first groups to be attracted to the field of hypernuclei was the Nuclear Emulsion Group of the Enrico Fermi Institute for Nuclear Studies at the University of Chicago. The group came into existence in the early 1950s when the late Enrico Fermi, finding that counters were incapable of producing good results for his pion scattering experiments, turned to nuclear emulsions. Fermi originally assigned a coterie of graduate students to the emulsion work, but with the passage of time and with support thrown its way by the Navy and others, the group grew into a fullfledged member of the Institute. When Fermi died in 1954, Valentine L. Telegdi was put in charge of the group and encouraged to pursue a program of ultra-high energy studies. In November 1957, after the

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[33] M. Danysz and Jerzy Pniewski, Philosophical Magazine, Vol. 44 (1953), 348; V. L. Telegdi, "Hypernuclei," Scientific American, Vol. 206 (January 1962), 50-53; OAR Research Review (28 January 1963), 1-2.

[34] Telegdi, op. cit., p. 50; G. A. Snow and M. M. Shapiro, "Mesons and Hyperons," Review of Modern Physics, Vol. 33 (April 1961), 231-38; Riccardo Levi Setti, "Present Status of Experimental Hypernuclear Physics," presentation before the American Physical Society, November 1959, MSS.

group had established a name for itself, AFOSR lent it its support.[35]

For about three years after Danysz and Pniewski's discovery, the study of hypernuclei had been almost entirely confined to the field of cosmic radiation. Then, between September 1955 and March 1956, W. F. Frey and his associates at the University of Wisconsin reported fourteen cases of hypernuclei produced by accelerator beams. The Chicago group sensed the implications of these reports and began directing its efforts almost entirely to the new field. The first order of business was finding and identifying a large number of these strange nuclear species. After hurriedly preparing their theoretical calculations, Telegdi and company were off for Berkeley and the 4.5 Bev pion beam of the Lawrence Radiation Laboratory. This was the beginning of the group's extended travels in pursuit of hypernuclei, which started with periodic visits to Berkeley and Brookhaven and ultimately took them as far afield as Geneva, Switzerland.[36]

By the end of the year, the group had exposed its emulsions to both pion and proton beams in the multi-Bev range and had found 120 hyperfragment events in their stacks. This constituted approximately one-half of all the hypernuclei known up to that time. And in 1957, Telegdi and two other members of the group, Riccardo Levi Setti and William E. Slater, prepared a new world survey covering these events. Meanwhile, it became evident that a negative kaon beam was more effective than a pion beam at producing hyperfragments; and from 1957 on, when such a beam became available at Berkeley, the group relied on kaons most heav-

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[35] V. L. Telegdi, et. al., "Nuclear Emulsion Research with High Energy Accelerators: A Proposal for a Research Grant to AFOSR," December 1956, pp. 1-2, MSS; R & D Project Card (DD Form 613-1) Project No. 3750, 1 April 1958, p. 22.

[36] W. F. Frey, J. Schneps, and M. S. Swani, "Disintegration of Hyperfragments," Physical Review, Vol. 99 (1 September 1955), 1561-72; Frey, Schneps, and Swani, "Disintegration of Hyperfragments. II," Physical Review, Vol. 101 (1 March 1956), 1526-35; Telegdi, "A Proposal," pp. 9-10; ltrs., Charles K. Reed to F. F. Voss, 21 March 1960; Reed to V. L. Telegdi, 21 March 1960, MSS.

ily for hyperfragment production. But despite the introduction of new spectroscopic techniques such as bubble chambers, the nuclear emulsion remained the best way to trap these short-lived nuclear species.[37]

When Danysz and Pniewski stumbled upon the first hypernucleus, they had concluded that the heavy particle or hyperon that had bound itself with the protons and neutrons in the hypernucleus was the lambda -- a strange particle with a mass a little more than twice that of an ordinary nucleon. To this day, no one has definitely observed another strange particle to form a hypernucleus. Of all the other strange particles, the sigma appeared to be the most likely to duplicate the binding behavior of the lambda. R. G. Ammar, a member of Telegdi's group, did in fact make a systematic search for sigma hyperfragments, and none turned up. There were several events, however, that could be interpreted -- although not conclusively -- as hyperfragments of sigmas and nucleons; and the possibility of yet another form of hypernuclei was not ruled out.[38]

Except for this short excursion with the sigma, the group concentrated upon lambda-nucleon interactions. Included in these efforts was the systematic identification of different combinations of these hyperfragments (thirteen identified so far, ranging from hyperhydrogen-3 -- the lightest -- to hypercarbon-13 -- the heaviest) and the determination of their decay modes, binding energies, and charge. Richard H. Dalitz, a theoretical physicist at the Fermi Institute, whose interests paralleled those of the group, was invited periodically to lend a hand with the theoretical interpretations.[39]

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[37] Telegdi, "A Proposal." pp. 3-6; Telegdi, "Hypernuclei," p. 53.

[38] R. G. Ammar, et al., "Search for Hyperfragments," Physical Review, Vol. 120 (1 December 1960), 1914-16; Telegdi, "Hypernuclei," p. 56; Riccardo Levi Setti, "Present Status of Experimental Hypernuclear Physics," a presentation before the American Physical Society, November 1958, MSS.

[39] E. M. Silverstein, "A Systematic Study of Hyperfragments Produced

Perhaps the most significant aspect of this work was that dealing with the force between the lambda and the various nucleons. In an ordinary nucleus, the force between any two nucleons, be it between proton and proton or proton and neutron, is the same. The interaction between nucleons is therefore charge independent or charge symmetric. This fact was found to hold true for lambda-nucleon interactions, too. In other words, the lambda bound with equal force to a proton and to a neutron. It was also found, with a few exceptions, that the binding energy increased with the number of nucleons in the central core. Thus, hypercarbon-13 had the strongest binding energy, while hyperhydrogen-2 -- a hyperfragment consisting of a lambda and a single proton -- was not found to exist, no doubt because the binding energy between these two entities was too weak.

There were, however, significant differences between ordinary nuclei and hypernuclei. The force between nucleons is dependent in part upon the mutual orientation of their spins. The lambda-nucleon interaction was found to be spin dependent, too -- but, with this difference. In ordinary nuclei, the interaction between nucleons is stronger when their spins are parallel -- that is to say, pointed in the same direction. In hypernuclei, the lambda-nucleon interaction was found to be stronger in the antiparallel orientation, i. e., when their spins were pointed in opposite directions. There was one other important difference. Ordinary nucleons obey the exclusion principle, which says that no more than two protons and two neutrons may sit on the same energy level. The principle, however, applies only to two identical particles;

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by 4.5 GeV π^- in Nuclear Emulsion," Supplemento del Nuovo Cimento, Vol. 10 (1958), 41-67; R. Levi Setti and W. E. Slater, "Observation of a Mesonic Decay of a Helium Hypernucleus," Physical Review, Vol. 111 (1 September 1958), 1395-97; W. E. Slater, "A Systematic Study of Hyperfragments Produced by 4.5 GeV π^- in Nuclear Emulsion . . .," Supplemento del Nuovo Cimento, Vol. 10 (1958), 1-40; R. G. Ammar, et al., "Mesic Decays of Hypernuclei from K^--Capture. II," Il Nuovo Cimento, Vol 19 (1 January 1961), 20-35; P. E. Schlein and W. E. Slater, "Identification of Heavy Hypernuclei from K^--Capture by Primary Star Analysis," Il Nuovo Cimento, Vol. 21 (16 July 1961), 213-34.

thus, in hypernuclei the lambda particle can occupy the same energy state already occupied by two like nucleons.[40]

VI

In the fall of 1956, there occurred one of those rare, cataclysmic events which have punctuated science from time to time since the days of Copernicus -- and it nearly stood physics on its ear. Much has already been said about the scientist's firm belief in the existence of order and symmetry in nature. (It will be recalled that physicists were pleased with the discovery of antimatter because it bore more proof of a deep symmetry in nature.) This belief has, in turn, been translated and preserved in the form of a variety of symmetry laws. One such law, the concept of intrinsic equivalence of right and left (or mirror symmetry), is one of the oldest and one of the most firmly implanted in the scientific mind. Put very simply, mirror symmetry means that along with any physical process there is another physical process that looks exactly the same as the mirror image of the first. True, one can think of numerous examples where there is no symmetry between right and left. For instance, man has his heart on his left side and his stomach on the right, and there is no proof that the mirror image of man, with his heart on the right and his stomach on the left, exists anywhere in the universe. But asymmetries such as these were attributed to the original environmental conditions of organic life, and physicists held on to the belief that the laws of nature showed complete symmetry between right and left.[41] As one physicist put it, "[For nature] to give preference to a 'right-handed' world, say, would seem most unaesthetic."[42]

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[40]See footnote 39 and V. L. Telegdi, "Final Report on the Scientific Activity Under Contract AF 49 (638)-209," passim, MSS.

[41]C. N. Yang, "Law of Parity Conservation and Other Symmetry Laws," Science, Vol. 127 (14 March 1958), 565.

[42]Roman, op. cit., p. 346.

The various symmetry laws led quite naturally to a variety of conservation laws. One such conservation law, a direct result of right-left symmetry, was the law of the conservation of parity. Parity is not a physical process but a mathematical property and is therefore difficult to describe in concrete terms. The law was constructed in 1924 when physicists began assigning mathematical values to the energy levels in complex nuclei. According to this law, particles were said to have either odd parity or even parity, and this value would always be conserved. For example, in a weak interaction, an odd parity particle had to decay into two or more particles whose combined parity added up to an odd value (1—> 1, 1, 1 or 2, 1); an even parity particle had to decay into two or more particles whose combined parity added up to an even value (2—> 1, 1 or 2, 1, 1). From the day this principle was propounded to the fall of 1956 (roughly 32 years) no one seriously questioned its validity. It was, after all, another grand extension of the existence of symmetry in nature. Moreover, it had held true in all phenomenological tests, particularly in strong interactions.[43]

Then, in 1953, some trouble began to brew. The trouble lay in what physicists came to call the theta-tau puzzle. In working on the parity of the kaon, physicists found that the particle decayed at times into two pions and at other times into three. Since pions possessed odd parity, the two-pion decay added up to even parity; the three-pion decay, to odd parity. Physicists immediately jumped to the conclusion that what they were observing not one particle, the kaon, but two different kinds of mesons, which they christened, theta (even parity) and tau (odd parity). By 1956, just about everyone was satisfied that the theta and tau were two different particles.[44]

There were, however, a couple of skeptics in the crowd -- mainly two Chinese-born theoretical physicists, Chen Ning Yang of The Institute

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[43]Asimov, Intelligent Man's Guide, I, 262; Yang, op. cit., p. 566; Gamow, Biography of Physics, p. 322; Hoffmann, op. cit., p. 271.

[44]Yang, op. cit., p. 566; B. Bleaney, "Non-Conservation of Parity," Nature, Vol. 179 (1 June 1957), 1101-02.

for Advanced Study, Princeton, and Tsung Dao Lee of Columbia University. What puzzled these two men (as it did indeed puzzle others) was that, except for parity, the theta and tau had identical properties. Indeed, were it not for their disagreement in parity, it would have easily been concluded that the theta and tau were one and the same particle, the kaon. Thus the theta-tau puzzle. Brooding over the problem for most of the spring and summer of 1956 and failing to find any evidence in the literature that conclusively proved the conservation of parity in weak interactions, Lee and Yang took the big step in October of that year. Pointing to their failure to uncover experimental evidence in the literature, they held in an article in The Physical Review that the belief that parity was conserved in weak interactions was merely "an extrapolated hypothesis." "One might even say," they went on, "that the present theta-tau puzzle may be taken as an indication that parity conservation is violated in weak interactions." This was treading on hallowed ground, and, knowing full well that their careers might be hanging in the balance, they retreated somewhat: "This argument is, however, not to be taken seriously because of the paucity of our present knowledge concerning the nature of the strange particles." They concluded by suggesting several possible experiments for determining parity conservation.[45]

Despite Lee and Yang's guarded tones, the suggestion was clear enough, and for a period of several months physics was rent with strife. In the professional in-fighting that followed, no less a personage than Wolfgang Pauli dismissed the suggestion out of hand.[46] Meanwhile, the experimental wheels began to turn, and Columbia University and the National Bureau of Standards became the sites of two elegant experiments,

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[45]T. D. Lee and C. N. Yang, "Question of Parity Conservation in Weak Interactions," Physical Review, Vol. 104 (1 October 1956), 254-58.

[46]M. Fierz and V. F. Weisskopf (eds.), Theoretical Physics in the Twentieth Centruy: A Memorial Volume to Wolfgang Pauli (New York: Interscience Publishers, 1960), passim; Hoffmann, op. cit., p. 273.

performed in close consultation with Lee and Yang. One of them, conducted in Washington, D. C., by Chien-Shiung Wu, a colleague of Lee's at Columbia, was a very involved affair, requiring the freezing of neutrons at a temperature near absolute zero. Neutrons, like other elementary particles, have an intrinsic spin, and can be considered to be rotating either clockwise or counterclockwise, depending upon which of the poles they are facing. What Wu did, essentially, was to set up two experiments, each the mirror image of the other. In other words, one set of neutrons were, let us say, facing the north pole and spinning clockwise; the other set faced the south pole and spun counterclockwise. In neutron decay, electrons are emitted. If parity were to be conserved the emitted electrons had to fly with equal probability in both directions -- those from one set of neutrons in a parallel direction to the axis of spin, those from the other, in an antiparallel direction. Thus, when viewed in a mirror, this emission process would not display a "handedness," or a preference for any direction. But the electrons stuck to one direction, and the conservation of parity in weak interactions tumbled into the hoary past.[47]

The news of Wu's success set off a series of similar experiments, with experimental physicists eager to see for themselves the violation of a law that had stood on a par with the conservation of energy in its supposed immutability. Moreover, even though there was much evidence that parity was conserved in strong and electromagnetic interactions, there was still a strong urge to put parity to the test in these interactions, too.[48]

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[47] C. S. Wu, et al., "Experimental Test of Parity Conservation in Beta Decay," Physical Review, Vol. 105 (15 February 1957), 1413-15; Yang, op. cit., p. 567; Gamow, Biography of Physics, pp. 322-23; Asimov, Intelligent Man's Guide, I, 263; Hoffmann, op. cit., pp. 274-75, P. M. S. Blackett, letter to the editor, Physics Today, Vol. 14 (February 1961), 86-88.

[48] Ltr., Pevsner to Heer, 27 July 1959, MSS; Hoffmann, op. cit., p. 275.

AFOSR contractors and grantees were not immune to the excitement caused by Lee and Yang. One AFOSR scientist -- a contractor of the General Physics Division -- wrote an incisive essay on the subject for the British periodical, Nature.[49] Aihud Pevsner and the group at Johns Hopkins, recognizing that the violation of parity meant a re-examination of the fundamental assumptions behind physical measurements, put aside what they were doing to study decay asymmetry in the muon. Following in the footsteps of a group at Columbia (Garwin, Lederman, and Weinrich), they studied this asymmetry in a magnetic field of 700 gauss, and presented their findings at the International High Energy Physics Conference at Rochester in the spring of 1957.[50]

The Columbia group, just mentioned, had been the second group to confirm experimentally the violation of parity. Unlike Wu, however, it had examined the decay asymmetry of the muon, rather than the asymmetry in neutron decay. Another near miss for first place had been the effort of V. L. Telegdi and Jerome I. Freidman of the Emulsion Group of the University of Chicago. Telegdi and Freidman, like Garwin, Lederman, and Weinrich, had decided to examine the asymmetry in the decay chain, pion-muon-positron. In October 1956, just after Yang and Lee's paper appeared in print, Freidman and Telegdi exposed a stack of nuclear emulsion pellicles (1 mm thick) to a pion beam produced by the University of Chicago's synchrocyclotron. After recording over 1300 pion-muon-positron decays, they had the evidence for the decay asymmetry -- but a bit behind the pace set by Garwin and company. In the same paper in which they announced these findings, Freidman and Telegdi made the suggestion that the formation of a muon-electron compound, known as muonium, may have had something to do with the depolarization of the muon.[51] This suggestion

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[49] Bleaney, op. cit., passim.

[50] Ltr., Pevsner to Heer, 27 July 1959, MSS; Richard L. Garwin, Leon M. Lederman, and Marcel Weinrich, "Observations of the Failure of Conservation of Parity . . .," Physical Review, Vol. 105 (15 February 1957), 1415-17.

[51] Garwin, et al., op. cit.; Jerome I. Freidman and V. L. Telegdi,

sparked a set of experiments at Yale, where two AFOSR contractors, Gregory Breit and V. W. Hughes, had been experimenting with muonium for quite some time. Muonium is in many ways analogous to the atom. Instead of a nucleus composed of protons and neutrons, however, it has a central core composed only of a single muon. Around this central core is an orbiting electron. In the decay of a muon, a positron and two photons are emitted. As Garwin and Lederman showed, when the muon was strongly polarized along its line of motion, a large asymmetry, with respect to the spin direction of the muon, appeared in the angular distribution of the positron. With Telegdi and Freidman furnishing the spark, the question occurred to Breit and Hughes whether or not the degree of polarization of the muon could be determined by observing the asymmetry of the positron emission from muonium. This task proved fruitful, for it provided Breit and Hughes with an excellent means of testing numerous assumptions regarding the formation of muonium.[52]

Meanwhile, after definite proof of the violation of parity in neutron and muon decay was in, activity shifted to parity conservation in other particles. Among the first to investigate the possible asymmetry in the decay of strange particles were four physicists (Elihu Boldt, Herbert S. Bridge, David O. Caldwell, and Yash Pal) from M.I.T.'s high energy group, whose efforts were jointly supported by ONR, AEC, and later, AFOSR. The particular strange particle that this group decided to observe was the lambda; but before these four had gotten their experiment underway, the fact that parity was violated by the decay of a lambda had already been reported in the literature. This had been done by observing the asymmetry of the emitted pion. But the unsymmetrical emission of pions was only one way in which the violation of parity

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"Nuclear Emulsion Evidence for Parity Nonconservation in the Decay Chain $\pi^+ - \mu^+ - e^+$," Physical Review, Vol. 105 (1 March 1957), 1681-82.

[52]G. Breit and V. W. Hughes, "Information Obtainable on Polarization of the μ^+ and Asymmetry of e^+ in Muonium Experiments," Physical Review, Vol. 106 (15 June 1957), 1293-95.

manifested itself in lambda decay, as Boldt and his associates soon proved. What they did was to measure the appearance of a longitudinal polarization in a decay proton, which was also emitted by a decaying lambda. This was the first example of the violation of parity in a weak decay process that did not involve neutrinos.[53]

It had, then, been proven beyond a doubt that parity was violated in weak interactions. It had also been shown conclusively that parity was conserved in electromagnetic interactions and in strong interaction involving the ordinary nucleons. The strange particles taking part in strong interactions were a tougher nut to crack. In fact, as late as October 1960, only in the case of direct lambda production (pion + proton ⟶ lambda + kaon) was it certain that parity was conserved in strong, strange-particle producing interactions. About this time, Aihud Pevsner began giving some more thought to the problem of parity, and in association with a group from Duke University, decided to take a closer look at the lambda. They used a helium bubble chamber which they bombarded with K-mesons, and the lambda which they would be examining was that created by a two-step process, the so-called sigma conversion (a sigma is created in a primary interaction and subsequently interacts with a nucleon inside the helium nucleus to produce a lambda). Actually, up to this time, a great deal of evidence produced by similar cloud chamber studies pointed to a strong forward-backward asymmetry in the angular distribution of the lambda. The same results, however, had not been obtained when a lambda was created in hydrogen. There was, to say the least, some confusion to reckon with. Pevsner and his associates helped to clear some of it up. After searching through more than 500 kaon-helium interactions, they found not one shred of evidence that parity was violated in strong interactions. After this it could be said with some measure of assurance that parity was conserved both in the

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[53]Elihu Boldt, Herbert S. Bridge, David O. Caldwell, and Yash Pal, "Helicity of the Proton From Λ^0 Decay," Physical Review Letters, Vol. 1 (1 October 1958), 256-58.

case of direct lambda production and in the case of sigma conversion.[54]

Meanwhile, the violation of parity in weak interactions did not cause as much anguish as one would originally have thought. It is not that physicists accepted as a fact that nature was in some cases unaesthetic -- that she would actually show a preference for the right over the left, or vice versa. Nothing of the sort. In fact, they found in the violation of parity further evidence of the symmetry in nature. Lee and Yang themselves pointed the way. "There is actually no a priori reason," they wrote, "why [parity] violation is undesirable. . . . the question could still be raised whether there could not exist corresponding elementary particles exhibiting opposite asymmetry such that in the broader sense there will still be over-all right-left symmetry." In other words, the violation of parity here on earth could be due to a local preponderance of, say, right-handed electrons over left-handed electrons, just as there is a local preponderance of matter over antimatter.[55]

[54]Block, <u>et al.</u>, "Search for Evidence of Parity Non-Conservation in K-He Interaction," <u>Physical Review</u>, Vol. 120 (15 October 1960), 570.

[55]Lee and Yang, <u>op. cit.</u>, 258.

Chapter IV

EXPERIMENTAL NUCLEAR STRUCTURE PHYSICS

In contrast to elementary particle physics, the world of nuclear structure physics is comparatively simple and orderly. (Of course, when compared to molecular and atomic physics it is in relative disarray.) Instead of dealing with dozens of particles and antiparticles, the nuclear structure physicist deals consistently with a small number of the elementary constituents of matter -- photons, electrons, protons, and neutrons. Moreover, he usually deals with them in combinations -- that is, in the manner in which they appear in the atom. His concern, then, is not so much with particles as individual entities as it is with particles as they appear in the atomic nucleus. And the list of particles that he consistently deals with is not likely to proliferate. The reason is quite simple. Out of the thirty or so particles so far discovered, only those mentioned above have been found to play any kind of role in nuclear structure. No nuclei composed entirely of antimatter have been found to exist. Thus, antiparticles automatically fall out of the low energy realm. (The positron is an exception.) The same holds true for strange particles; they, too, play no discernible role in nuclear structure. It is thus with ordinary or normal matter that nuclear structure physics concerns itself.[1] The strange new world on the frontiers of science is left up to more pioneering souls.

The comparative simplicity of the low energy field is not, however, an indication of the absence of diverse quantities. It is, rather, an indication of the presence of discernible relationships among various entities -- something with which elementary particle physics is not blessed. Indeed, nuclear structure physics deals with more forms, with

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[1] H. A. Bethe, "Nuclear Structure and Transmutations," in Morris H. Shamos and George M. Murphy (eds.), Recent Advances in Science (New York: New University Press, 1956), 67-113.

more quantities and qualities than high energy. Its world ranges from the simplest nucleus, that of the hydrogen atom (i.e., the proton), to the massive nucleus of the nobelium atom, made up of hundreds of protons and neutrons. Nuclear masses range from one to about 250, and for every mass number within this range, at least one nuclear species has been found. There are, therefore, a great many species to deal with.[2]

Of course, since these species are considerably larger than the individual particles that the high energy physicist attacks, the low energy physicist has to arrange his materials accordingly. It will be recalled that it takes less energy to excite an atom than a nucleus, and less to excite a nucleus than its individual constituents. Therefore, since he deals with larger species, the nuclear structure physicist needs less energy. (He usually works with energies just below the meson threshold, about 160 Mev.) To be more specific, the nuclear structure physicist uses smaller accelerators (sometimes he uses natural radioactive materials), smaller generators, smaller spectrometers than his counterpart in high energy physics. His purpose, then, is not to probe farther and farther into the depths of matter. His exploration is more on a lateral plane. Perhaps an analogy is in order. The high energy physicist is in spirit and in purpose somewhat akin to the trailblazer who opened up America's landed frontier. His interest is to penetrate the unknown, to push back the frontiers of knowledge. The nuclear structure physicist has come to reside upon one of the regions opened up by the high energy physicist. He is, in other words, the settler, the man who clears the forest, seeds the earth, taps for water and ore, and ultimately builds the city. The analogy aside, the nuclear structure physicist seeks to determine the masses, charges, energy levels, lifetimes, binding energies, and shapes of the various known nuclei. With this data he hopes to be able to construct a conceptual scheme which will do for the nucleus what, say, the periodic table has done for the

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[2]Ibid.; NSF, "The Problem of Support Emphasis for LENP," August 1961.

atom -- provide an easily discernible relationship among all nuclei.[3]

II

The first fifty years of the twentieth century saw many remarkable developments in man's knowledge of physical phenomena. One of the most remarkable was the formulation of quantum electrodynamics, a mathematical theory which can, within certain limits, correctly predict a great many physical events. Indeed, the success of this theory has been such that physicists have been hard put to verify a great many of its predictions experimentally. It is simply a case of theory outrunning experiment.[4]

Many of AFOSR's efforts in nuclear structure physics during the 1950s were devoted to restoring the balance between theory and experiment. One such effort -- although not conspicuously successful -- was the work of a Dartmouth College physicist, Frank Titus, on the photoelectric effect. Postulated by Albert Einstein in the first decade of this century -- an effort for which Einstein received the Nobel Prize -- the photoelectric effect is one of three competing mechanisms by which photons remove electrons from matter, the other two mechanisms being pair-production and the Compton effect. At energies of about one Mev the cross sections for these last two competing processes were well-known by 1958, the year Titus submitted his first proposal to AFOSR. This was not true, however, of the cross sections for the photoelectric effect. What data there was of these cross sections was none too reliable. This was due to the fact that it had been gathered by indirect means: by measuring the total absorption of photons and then subtracting the cross sections of the competing processes from the total. Titus hoped to obviate the errors inherent in such methods by measuring the photoelectric cross sections directly. Titus also hoped that his inves-

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[3]Bethe, op. cit., passim; NSF, "The Problem of Support Emphasis for LENP," August 1961; AFOSR, Projections, 1962, p. 18.

[4]Alfred K. Mann, "A Proposal for Research," May 1953, MSS.

tigations would demonstrate which of the several conflicting theoretical calculations of photoelectric cross sections at these energies was most nearly correct.[5]

Working with two or three graduate students at the most, Titus began measuring the energy of photoelectrons that had been ejected from their foils by monoenergetic photons. By the end of 1961, he successfully measured the cross sections of photoelectrons ejected by 1.12 Mev gamma rays from targets of tantalum, gold, tin, and molybdenum. He was also able to do this for tantalum and gold at 2.62 Mev. He found, however, that for targets of a low charge, such as aluminum, he could only obtain results for Compton electrons, the photoelectric cross section for such low charge targets being negligible.[6]

The primary motive for conducting research of this kind is to learn more about the electronic and nuclear structure of matter. This certainly was Titus' motive, as it was the motive of two University of Pennsylvania physicists, Jules Halpern and Alfred K. Mann, who worked under two separate AFOSR research contracts for most of the decade of the fifties. While Titus' study involved the bombardment of electrons with photons, both Halpern and Mann directed their efforts at the bombardment of various nuclei with photons. Halpern, however, concentrated on the various reactions nuclei went through when struck by gamma rays; Mann concentrated on the scattered rays themselves.[7]

For the sake of simplicity, a nuclear reaction of the type that Halpern dealt with can be divided into two stages. In the first stage the accelerated particle is captured by the bombarded or target nucleus,

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[5]R & D Project Card (DD Form 613-1) Project No. 9774, 1 April 1959, p. 34; Frank Titus, "Technical Report on Contract AF 49 (638)-634 for Period Ending 31 August 1961," MSS.

[6]Titus, "Technical Report for Period Ending 31 August 1961," MSS.

[7]Jules Halpern, "Proposal for Studies in Photonuclear Reactions from the Department of Physics, University of Pennsylvania," 15 May 1952, MSS; Mann, "A Proposal for Research," May 1953, MSS.

forming a compound nucleus. In the second stage, the now overly energetic nucleus falls apart by several competing modes of decay. There are differences, however, depending upon what particle one uses. A photon, for example, has no rest mass and will react differently with a nucleus than will a particle, say, with an incident charge and a considerable rest mass such as a neutron. Thus, in the case of the photons that Halpern would use, during the first stage of the reaction there would be an interaction between an electromagnetic field and the charges of the particles in the target nucleus. This kind of reaction was rather amenable to a direct calculation -- much more so than a proton-nucleon or neutron-nucleon reaction. When Halpern first began work under an AFOSR contract, in the fall of 1952, some of the things about photonuclear reactions that needed attending to were the energy distributions of the emitted particles, the ratios of neutrons to protons, neutrons to deuterons, and neutrons to alpha particles emitted from the nucleus, and the angular distribution of the emitted particles. In these cases and others only the general features had been examined experimentally. Measurements of greater precision were in order.[8]

Halpern's earliest endeavors were directed at measuring the angular distributions of photoneutrons -- that is, neutrons which had been ejected out of the nucleus when struck by gamma rays. The first three elements bombarded were copper, silver, and palladium. In all three cases, Halpern found that neutrons were emitted with spherical symmetry. When he extended his work to beryllium and bismuth, however, he found that in the case of beryllium there was a 15 percent asymmetric component.[9]

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[8]Halpern, "Proposal for Studies," 15 May 1952, MSS; ltr., Seymour Shwiller to Jules Halpern, 5 May 1952, MSS; R & D Disposition Form (DD Form 96), 27 October 1952, MSS.

[9]Jules Halpern, "Quarterly Progress Report on Contract AF 18 (600) -472," 15 January 1954, MSS; G. A. Ferguson, J. Halpern, R. Nathans, and P. F. Yergin, "Photoneutron Cross-Section in He, N, O, F, Ne, and A," Physical Review, Vol. 95 (1 August 1954), 776-80; B. P. Fabricand, B. A. Allison, and J. Halpern, "Angular Distribution of Photoneutrons from

From photoneutrons, Halpern went to photoprotons. During the course of these investigations Halpern's laboratory was the first to make a comprehensive survey of giant resonance systematics and discovered the correlation between resonance width and the shell structure of the nucleus. It also made an exhaustive study of the photodisintegration of the deuteron at energies from 5 to 22 Mev.[10]

Alfred Mann's contract with AFOSR began in October 1953, approximately one year after Halpern's efforts. At that time the University of Pennsylvania was already deeply involved in the study of elastic scattering of gamma rays. The calculations, however, were still rather crude. For example, the experimental cross section for elastic scattering of 2.62 Mev gamma rays by lead was no less than three times larger than that calculated by theory. Mann intended to perform his initial experiments by using natural radioactive materials such as cobalt-60 and gold-198 as his gamma ray sources.[11]

During the first year of the contract Mann was concerned primarily with the elastic scattering of gamma rays at energies ranging from 0.411 to 1.33 Mev. Nuclei of copper, tin, and lead were the principal targets. The experiments involved the measurement of the absolute differential cross sections for elastic scattering at six angles between 15 and 90 degrees. The types of elastic scattering that Mann concerned himself with during this period were those resulting from the deflection of photons by nuclei (Thomson scattering) and the deflection of photons by tightly bound electrons (Rayleigh scattering). It was found that at 0.411 and 0.662 Mev the experiments involved Rayleigh scattering almost

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Carbon and Beryllium," Physical Review, Vol. 103 (15 September 1956), 1755-57.

[10]Halpern, "Quarterly Report for Contract AF 18 (600)-472," 15 April 1957, MSS; Halpern, "Proposal for Continuation of Contract AF 49 (638)-454," 15 November 1959; Halpern, "Resumé of Research Under Contract AF 49 (638)-454," 19 December 1960, MSS.

[11]Ltr., William J. Otting, Jr., to A. K. Mann, 15 June 1953, MSS; Mann, "A Proposal for Research," May 1953, MSS.

entirely. By the time he was through, Mann felt that his data would provide a reasonably stringent test of the theory of that scattering process.[12]

Another scattering effect predicted by quantum electrodynamics, but which was forbidden by classical physi 'as the scattering of light (that is, photons) by light. Closely related to this process is the scattering of light by a static electric field. The effect was first discussed qualitatively by Delbrück in 1933 (and thus named, Delbrück scattering), but twenty years later the effect had yet to receive the kind of experimental attention it deserved. For example, there were still no calculations for the arbitrary angles of this scattering process. So Mann turned his attention to Delbrück scattering during the second stage of the contract.[13]

In 1957 and 1958, Mann directed his gamma rays, ranging from energies of 1.33 Mev to 2.62 Mev, upon targets of tin, uranium, and lead. He measured the scattering at angles between 15 and 105 degrees -- all of which was the coherent sum of Delbrück, Rayleigh, and Thomson scattering. Now the amplitude of the Thomson process was well known. The data which Mann had but recently obtained for Rayleigh scattering provided exact values for that process at 1.33 Mev and approximate values at 2.62 Mev. Mann could take it from there, using the data for these two processes along with the sum for all scattering, and calculate the value for Delbrück scattering. He found, however, that at 1.33 Mev the difference between all scattering and that due to Thomson and Rayleigh scattering was not sufficiently large to permit a definite identification of the Delbrück process. At 2.62 Mev he had slightly better success. While failing to definitely identify Delbrück scattering at this

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[12] A. K. Mann, "Proposal for the Extension of Contract AF 18 (600)-894," 11 May 1954, MSS; A. K. Mann, "Quarterly Report on Contract AF 18 (600)-894," September 1955, MSS.

[13] A. M. Bernstein and A. K. Mann, "Scattering of Gamma Rays by a Static Electric Field" (AFOSR TN 58-83), p. 3.

energy, Mann felt that the process had taken place at intermediate angles (30 to 75 degrees). Unfortunately, there existed no theoretical prediction of the angular distribution for such scattering, and Mann could not calculate the amplitudes of the process from the experimental results alone.[14]

III

Perhaps the most ambitious task that Mann and his associates undertook to accomplish was measuring the anomalous magnetic moment (the so-called "g-factor") of a free electron. Indeed, this is one of the most difficult and intricate experiments that a physicist can undertake. P. A. M. Dirac, who had worked out the theory of the electron, predicted that that particle's g-factor was equal to the value, 2. This value had pretty much universal acceptance until shortly after World War II. At that time some rather precise experiments showed that Dirac's prediction was off by about one part in one thousand. This discrepancy might seem ever so slight, but to the physicist it made a great deal of difference; in fact, it ultimately proved crucial, for it arose out of the fact that the electron is constantly emitting and reabsorbing virtual photons -- a process giving rise to measureable effects. Thus, the name, "anomalous" magnetic moment, stuck as a reminder that the electron's g-factor did not correspond exactly with Dirac's theory.[15]

While post-World War II measurements were precise enough to detect the anomaly in the electron's magnetic moment, they were not really precise enough to satisfy physicists. It was recognized that these measurements, mostly conducted by indirect means, such as observing the particle's precession angle, were only very close approximations of the elec-

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[14] Ibid., passim.

[15] Memo for files, Ray R. Heer, Jr., 11 October 1957, MSS: Sheldon Penman, "The Muon," Scientific American, Vol. 205 (July 1961), 51; OAR Weekly Activity Report, 14 April 1961.

tron's true magnetic value. Thus, in October 1957, Mann and his associates began thinking of a new way to attack the problem.[16]

The manner of attack that this group chose was not an improvement or extension of past experimental methods. What the group decided was to devise a new and direct approach. Up to then, the best indirect measurement possessed an inaccuracy of about half a percent. Direct measurements had yielded better results, but Mann believed that he could improve upon them. He planned to do this by measuring the frequency at which an electron goes through a "spin-flip" during a resonance experiment. This was much easier said than done. The spin-flip experiment required producing a beam of electrons that traveled at the slow rate of about 100 thousand electron volts (Kev). No such beam had ever been produced. Moreover, neither polarizers nor detectors of such slow moving electrons had ever been successfully made. The long and short of it was that the Pennsylvania group had assigned itself quite a task -- one which to this day (August 1963) they have yet to master.[17]

Meanwhile, other physicists began thinking along parallel lines. If the direct measurement approach led to better results in the measurement of the anomalous magnetic moment of the electron, perhaps it would lead to comparable results in the measurement of the anomalous magnetic moment of the muon. What made this experiment so important was that it would shed a great deal of light not only on the muon, but on electromagnetism, too. The muon, as far as anyone could tell, was merely an overweight electron (about 207 times as heavy as an electron), which took part only in electromagnetic and weak interactions. Like the electron, it was believed to be indifferent to nuclear forces. All of this puzzled physicists, for they could not explain the considerable differ-

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[16] A. K. Mann, et al., "Proposal for Extension of Contract AF 18 (600)-894," 31 December 1959.

[17] Ibid.; A. K. Mann, et al., "Proposal for Extension of Contract AF 49 (638)-537," 31 December 1961; A. K. Mann, et al., "Final Report on Contract AF 49 (638)-537," July 1962.

ence in the mass of the two particles. Why should nature need two particles identical in all respects except mass? To put it another way, why should the electron exist in two sizes? The suspicion was that the electron and the muon were not so identical as they seemed to be. Somewhere there had to be some hitherto undetected breakdown or departure in the muon's structure which would explain its mass. And here is where a precise measurement of the muon's magnetic moment was of such crucial importance. It would prove rather conclusively whether or not the muon was governed by electromagnetic theory in the same manner as the electron. If some deviation from electromagnetic theory was found, it would provide the clue to the origin of the particle's mass.[18]

Actually, by 1961, more than one group measured the anomalous magnetic moment of the muon, but the direct method used by a group of physicists at C.E.R.N. is the one we are concerned with here. The experiment itself was a most remarkable affair, requiring no less than three years to conduct. It also required an imposing team of scientists from five nations: G. Charpak and T. Muller (France), J. C. Sens (Netherlands), F. J. M. Farley (Great Britain), A. Zichichi (Italy), and Richard L. Garwin and V. L. Telegdi (United States). AFOSR's connection with the project came rather indirectly and unexpectedly. Telegdi had taken a year's leave of absence from his duties at the Enrico Fermi Institute in order to participate in the experiment. His year at C.E.R.N. was taken care of by a National Science Foundation fellowship. But the year went by, the fellowship ran out, and the experiment was still not completed. So Teledgi appealed to AFOSR for financial assistance -- assistance which AFOSR gladly gave.[19]

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[18]Penman, op. cit., p. 46; O. R. Frisch, "Fundamental Particles," in Science Survey (New York: MacMillan Co., 1960), 19; OAR Weekly Activity Report, 14 April 1961.

[19]G. Charpak, et al., "Measurement of the Anomalous Magnetic Moment of the Muon," Physical Review Letters, Vol. 6 (1 February 1961), 128-32; ltr., V. L. Telegdi to C. K. Reed, 27 July 1960, MSS; OAR Weekly Activity Report, 14 April 1961.

At one time, it was widely felt that the g-factor of the muon would never be measured. Indeed, compared to the muon, the electron was rather simple to measure. For one thing, the electron was stable, while the muon scarcely had a lifetime to speak of. Furthermore, with a mass 200 times that of the electron, the muon would have to be polarized in a much stronger magnetic field. But the C.E.R.N. team managed. Using a magnet weighing 85 tons and exerting a field of 16,000 gauss, this team was able to polarize the muons emitted by decaying pions, which, in turn, had been created by the C.E.R.N. cyclotron. Two things were measured that contributed to the value of the g-factor of the muon: the length of time the muon spent in the magnetic field and the angle at which it emitted an electron. The calculations came out to 2.001145 $\pm$ .000022. According to Dirac's prediction and quantum theory the figure is 2.001165. Thus, to an accuracy of one percent in the anomalous part of the g-factor, the experiment confirmed that the muon behaves exactly like a heavy electron.[20] As the C.E.R.N. group explained, "the muon appears to be a heavy electron with no interactions except the electromagnetic and the weak. This concept gives no explanation for the muon-electron mass difference, but allows the muon magnetic moment to be calculated from the Dirac equation and quantum electrodynamics . . . ."[21] With this went the hope of explaining the mass of the muon by some hitherto undetected interaction.

IV

Not all the work in nuclear structure physics is as spectacular or as exciting as that embarked upon by the C.E.R.N. team. In fact, much of it is tedious, unexciting, and mundane -- but necessary. Such is certainly the case with the accurate measurement of atomic masses, a field in which Professor H. E. Duckworth of Hamilton College, McMaster University, Hamilton, Ontario, an AFOSR contractor since 1953, has been

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[20] Penman, op. cit., pp. 53-54; Charpak, et al., op. cit., pp. 128-32.

[21] Charpak, et al., op. cit., p. 128.

laboring in for over a decade. The purpose of Duckworth's research remained the same through one contract and/or grant renewal after another: to study nuclear stability and nuclear energetics through the determination of atomic masses.[22]

It might seem odd that the masses of nuclei have to be elaborately computed, since the masses of their constituent parts, protons and neutrons, are well known. The fact of the matter is, however, that the combined masses of a nucleus' constituent nucleons are greater than the mass of a nucleus. Indeed, if this were not so, a nucleus -- for that matter any particle -- would fly apart, just as in the case of lambda hypernuclei. It will be recalled that the protons and neutrons in a nucleus keep from flying apart by constantly exchanging virtual mesons. The difference in energy between the mass of a nucleus and the combined mass of its constituents is accounted for by the mass of these mesons. This difference is indeed the nucleus' binding energy, which accounts for an atom's stability. It is therefore of extreme importance to physicists to know the difference between the deduced and observed masses of an atomic nucleus.[23]

In 1953, Duckworth and his group at Hamilton College began planning and constructing a large double-focusing mass spectrometer. When it was completed, four years later, the machine consisted of a semi-circular magnetic analyzer and an electrostatic analyzer. Twenty-eight separate magnets sitting side by side established the magnetic field and, in this way, simulated a continuous semi-circular field. Using the magnetic analyzer only, plus a special beam-modulation technique, Duckworth's group was able, in 1958, to determine the atomic masses of krypton-84 and -86,

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[22] R & D Project Card (RDB Form 1A) Project No. R-357-10-5, 5 September 1952, p. 1; R & D Project Card (DD Form 613-1) Project No. 3750, 4 April 1957, p. 91; RDT & E Project Documentation (DD Form 613c) Project No. 9750, 1 February 1962, p. 13, atch. 1.

[23] See for example the discussion in Henry E. Duckworth, "Masses of Atoms of A > 40," Progress in Nuclear Physics (London: Pergamon Press, 1957), V, 138.

xenon-129, and mercury-200, -201, and -204 with a precision of approximately one part in two million.[24]

One of the first problems that Duckworth gave thought to was establishing secondary standards of atomic mass, particularly for atomic values greater than 30. Oxygen-16 had for many years been for the physicist the standard of atomic mass. There was a growing need, however, to designate other nuclei as secondary standards, which would serve as accurate milestones along the mass table. Accordingly, such nuclides as hydrogen-1, hydrogen-2, and carbon-12 were suggested as secondary standards among the lighter elements. Duckworth, in turn, suggested a few secondary standards along the mass table for atomic masses greater than thirty, ranging from sulfur-32 to thorium-232.[25]

As Duckworth saw it, his suggested standards would be useful in the computation of mass values on the basis of transmutation data. The trouble with the present standard was that all computation data were linked by a chain of reaction data to that of oxygen-16, with no single link extending over more than three mass units. "For long chains," Duckworth pointed out, "this method leads to a serious accumulation of errors which would be largely avoided if secondary standards could provide absolute values of mass at appropriately spaced intervals."[26]

Duckworth felt that the heavier standards should possess certain definite attributes. As a group, they should be suitably spaced throughout the mass table, preferably from 10 to 20 mass units apart. Moreover, they should be atoms whose masses could be conveniently determined by

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[24]H. E. Duckworth, J. T. Kerr and G. R. Bainbridge, "A Large Semi-Circular Mass Spectrometer for Atomic Mass Determinations" (AFOSR TN 58-67), passim; John T. Kerr and Henry E. Duckworth, "Atomic Masses of Kr^{84}, Kr^{86}, Xe^{129}, Hg^{200}, Hg^{201}, and Hg^{204}" (AFOSR TN 58-505), passim or in Canadian Journal of Physics, Vol. 36 (July 1958), 986-88.

[25]Henry E. Duckworth, "Secondary Standards of Atomic Mass for A > 30" (AFOSR TN 58-68), 245.

[26]Ibid., 245-52.

mass spectroscopic methods. As Duckworth summed it up, "the nuclide in question should be (1) an abundant isotope, (2) belong to an element from which ions are easily obtained, (3) form doublets with precisely known comparison masses, . . . (4) possess, if possible, a mass number which is divisible by 2 and 3." The nuclides ultimately suggested by Duckworth were sulfur-32, titanium-48, zinc-64, krypton-84, palladium -104, tin-120, xenon-132, neodymium-144, dysprosium-164, tungsten-186, lead-208, and thorium-232.[27]

V

While Duckworth was collecting and analyzing data on atomic masses, physicists from the Bartol Research Foundation of the Franklin Institute were spending the better part of four years, under an AFOSR contract, collecting and analyzing data on another characteristic of atomic nuclei: their disintegration energies or "Q-values." The project began in the fall of 1954 and marked for the Bartol Foundation the pursuit of one aspect of nuclear physics which it had not previously engaged in. The principal investigator on the project was C. E. Mandeville, but the man who would devote most of his time and energy to the investigation was D. M. Van Patter, who had only recently arrived at Bartol by way of M.I.T. and the University of Minnesota.[28]

The general procedure in determining Q-values is to bombard a particular nucleus with a stream of low energy protons (between 1.8 and 4.0 Mev) and observe the energies of the emitted alpha particles, deuterons,

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[27] Ibid. But see also Duckworth, "Masses of Atoms of A > 40," pp. 138-61; J. T. Kerr, G. R. Bainbridge, J. W. Dewdney and H. E. Duckworth, "Some New Atomic Mass Determinations Made with a Large Single-Focusing Mass Spectrometer" (AFOSR TN 58-618), passim; N. R. Isenor, R. C. Barber and H. E. Duckworth, "Some Recent Determinations of Atomic Masses in the Strontium-Zicronium Region," Canadian Journal of Physics, Vol. 38 (June 1960), 819-23.

[28] Ltr., W. F. G. Swann, Director, Bartol Research Foundation, to Commanding General, Air Research and Development Command, 28 June 1954, plus enclosure, "Research Proposal," MSS.

and other charged particles, including elastically and inelastically scattered protons. As it happened, Bartol was well equipped for such a task. The Office of Naval Research had helped provide the Foundation with a large Van de Graaff accelerator, and the institution already had at hand a 16-inch, double-focusing magnetic spectrometer, which had been built a couple of years before. Nevertheless, the equipment was not in shape for immediate use, especially the spectrometer, and the first nine months of the contract were spent in getting the equipment in order. Finally, in mid-July 1955, the magnetic spectrometer was connected to the Van de Graaff, and Mandeville and Van Patter were ready for business.[29]

The first three nuclei tackled were phosphorous-31, chlorine-35, and chlorine-37, and their Q-values were determined within an average accuracy of plus or minus 0.008 Mev. This was followed by observations of magnesium-24 and aluminum-27. Measuring the ground-state disintegration energies of these nuclei was of general interest to all physicists since such data would provide them with a means of determining the difference between the masses of all nuclei involved in each nuclear reaction. In fact, before the scientists at Bartol were through, their work and the work of others on disintegration energies made it possible for physicists to obtain the mass values of nuclei from hydrogen-1 to sulfur-32 by using only nuclear reaction data -- no resort to mass spectroscopic data being necessary.[30]

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[29] C. E. Mandeville, D. M. Van Patter, et al., "Determination of Nuclear Disintegration Energies" (AFOSR TR 59-28), p. 1; C. E. Mandeville, et al., "Quarterly Report of the Work of the Bartol Research Foundation," 1 November 1954-31 January 1955, pp. 1-3; 1 May - 31 July 1955, pp. 1-2.

[30] D. M. Van Patter, C. P. Swann, W. C. Porter, and C. E. Mandeville, "Q-Value Measurements for Phosphorus and Chlorine," Physical Review, Vol. 103 (1 August 1956), 656-61; Mandeville, Van Patter, et al., "Determination," pp. 2-5; D. M. Van Patter, W. C. Porter, and M. A. Rothman, "Q-Value Measurements for Aluminum and Chlorine," Physical Review, Vol. 106 (1 June 1957), 1016-19.

One of the requirements in this kind of endeavor is to get the data out to other physicists in an easily accessible form. But with Q-values of nuclei scattered throughout the literature and with determinations made at various levels of accuracy, comprehensive data was not easily run down. The obvious solution was for someone interested enough to undertake to compile and analyze the available, but widely scattered, data. This Van Patter undertook to do, in 1957, in collaboration with Professor Ward Whaling of the California Institute of Technology. The compilation covered all the ground-state nuclear disintegration energies measured at all laboratories between May 1954 and February 1957.[31]

Soon after this compilation was published, Van Patter turned to the examination of heavier nuclei, in particular the heavier even-even nuclei, so called because they are made up of an even number of protons and an even number of neutrons. When Mandeville's contract ran out in 1958, Van Patter secured a renewal from AFOSR, this time with himself as principal investigator, and was thus enabled to continue his work. By 1960, it was time for another survey, this one on the low-lying states of even-even nuclei. He compiled data on fifty-five nuclei in all, some of which, particularly that gathered at Oak Ridge, had never been published before. The most interesting aspect of this compilation was Van Patter's comparison of the experimental results with theoretical predictions. Not long before the compilation was undertaken, two Russian theoreticians, Davydov and Filippov, had arrived at the unusual assumption that nuclei had permanent deformations which were axially unsymmetric. This was in direct contradiction to the universally held assumption that these deformations were axially symmetric. In any event, it was an attack on a symmetry principle, and a vigorous controversy ensued. While falling short of settling the controversy conclusively, Van Patter's data did, however, lend credence to the Russians' claim.[32]

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[31] D. M. Van Patter and Ward Whaling, "Nuclear Disintegration Energies II," Review of Modern Physics, Vol. 29 (October 1957), 757-66.

[32] Mandeville, Van Patter, et al., "Determination," pp. 6-12; AFRD Weekly Activity Report, 15 January 1960.

VI

Perhaps the most impressive research projects ever supported by AFOSR in experimental nuclear structure physics were those at the Washington University of St. Louis and at Florida State University, being in a sense the low energy counterparts of AFOSR's highly prized high energy studies at Stanford, Johns Hopkins, and Chicago. Failing this, they are easily the most costly projects AFOSR has ever undertaken in this area of study. The Florida State project originally cost about $200 thousand annually and grew to about $400 thousand annually by fiscal year 1963. The Washington University project presently runs about $220 thousand annually.[33]

The Washington University contract is much the older of the two, first going into effect in July 1953. Its main direction has been the study of nuclear forces and the energy of nuclear levels. Just before the project got under way, Franklin B. Shull, the principal investigator, was engaged in measuring the excitation energies of medium-mass nuclei taking part in the so-called stripping reactions. Then interest shifted. Theoretical physicists had pointed out that the angular distribution of the protons taking part in such reactions was strongly dependent upon the changes of nuclear spin and parity taking place during the reaction. Thus, a member of the group, Manuel Bretscher, undertook to measure the angular distributions of protons in proton-neutron reactions involving the isotopes sodium-23, titanium-47 and titanium-48. Others in the group examined carbon-12, potassium-39 and potassium-41. All of this work was done by using a nuclear emulsion technique. Concurrent with the emulsion, the group devised and used an electronic technique, which would do everything the nuclear emulsion did, but faster and better. When the new scattering chamber was constructed and instrumented, angular distribution studies were undertaken with iron-56 and -57, chromium-52 and -53, nickel-60, -61, and -62, and zinc-64, -66, -67, and -68. Finally a new

[33] Ltr., Knox Millsaps to Brockway McMillan, 15 November 1961, MSS.

fast-counting technique was developed and was used in 1957 to study the reactions (1) deuteron + beryllium-9 ⟶ beryllium-10 + proton, (2) deuteron + beryllium-10 ⟶ beryllium-11 + proton, (3) deuteron + beryllium -11 ⟶ carbon-12 + neutron.[34]

In the Florida State project, work on which began in March 1960, virtually the entire experimental and theoretical effort is built around a unique research facility, a 13 Mev Tandem Van de Graaff positive ion accelerator. When this facility was completed, in the spring of 1960, there were only two others like it in the entire world -- one at the University of Wisconsin, the other in Canada. That Florida should possess such a machine was due in great measure to the enterprise of its former governor, Leroy Collins, who steered a $5.2 million appropriations bill for nuclear research through the Florida legislature. Florida State got $2.3 million of this appropriation, most of it going for the accelerator. Building this accelerator was significant not only because the Tandem design doubled the useful range of energies, but also because it was more than a mere extension to higher energies. Experiments could be conducted with this accelerator which would have been impossible with other accelerators of comparable or even higher energy. AFOSR readily appreciated this fact and had the principal investigator, Georges M. Temmer and Norman P. Heydenburg, who had come to Florida State via the Carnegie Institution of Washington, under contract even before the first beam was produced by the Van de Graaff.[35]

Since 1960, the main direction of the research at Florida State has been the understanding of reaction mechanisms. But on the whole the investigations have been quite varied, winding in and out of a great many problems in nuclear structure physics. For example, in 1960, three in-

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[34]Franklin B. Shull, "Final Status Report for Contract No. AF 18 (600)-777," 19 November 1957, MSS.

[35]Memo, Charles K. Reed to General H. F. Gregory, 20 May 1958, MSS; Florida State University, "A Proposal for the Support of a Nuclear Physics Research Program with the Tandem Van de Graaff Accelerator at the Florida State University," 1958; ltr., Harvey Hall to Brig. Gen. B. G. Holzman, 19 January 1960, MSS; AFRD Weekly Activity Report, 15 April 1960.

vestigators on the project, L. D. Pearlstein, Y. C. Tang, and K. Wildermuth, measured the energy levels of helium-5 and lithium-5, finding that the wave functions of these two isotopes deviated appreciably from the standard shell model of the nucleus. Meanwhile, Heydenburg and Temmer, along with two assistants, were busy obtaining the angular distribution of 4.43 Mev gamma rays from the reaction, C^{12} $(p,p'\gamma)$ C^{12}. By December 1961, they had measured and analyzed 32 angular distributions, and found that these distributions changed sensitively with changes in the bombarding energy, especially in the vicinity of the two resonance peaks, which had been set at 5.36 Mev and 5.89 Mev in the excitation curve.[36]

One particular angle that Temmer investigated virtually by himself was the possible presence of a resonant transfer process in nuclear reactions. The problem is of special interest since it points up once again the important uses analogy can be put to. In 1959, two atomic physicists, F. P. Ziemba and E. Everhart, while measuring the variation of the charge states of helium ions, stumbled upon a significant discovery. They observed that a fraction of neutral helium atoms was emerging at a 5 degree and 10 degree angle to an incident positive helium beam of variable energy. When they plotted this neutral fraction, it exhibited a series of regularly spaced peaks and valleys with values of about 70 to 20 percent at the extremes. The wavelength increased slightly and gradually toward the very low energies. The phenomenon was striking, and Ziemba and Everhart advanced the explanation that at about 250 Kev an electron from the target atoms was jumping to the positive helium ion and neutralizing it; at 80 Kev the electron had enough time to jump to the positive helium ion and back again to its original atom; at lower

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[36] G. M. Temmer, "Annual Report for Contract AF 49 (638)-427," 31 December 1960, MSS; L. D. Pearlstein, Y. C. Tang, and K. Wildermuth, "Energy Levels of the He 5 and Li 5" (AFOSR TN 60-1155), passim; H. S. Adams, J. D. Fox, N. P. Heydenburg, and G. M. Temmer, "Angular Distribution of 4.43-Mev Gamma Radiation from C^{12} $(p, p'\gamma)$ C^{12}," Physical Review, Vol. 124 (15 December 1961), 1899-1903.

energies it was able to make several of these transfers.[37]

What Temmer was wondering was whether or not a nuclear analogue of this atomic process might not exist. With the availability of controlled beams of alpha particles and heavy ions such an investigation was now possible. Temmer calculated the possibility theoretically and felt that such a process should be observable. He suggested a typical example: the ground state of lithium-6, which possessed less binding energy in the ground state than any other stable nucleus and was, therefore, an excellent test body. He also suggested eight other processes involving neutron transfer, proton transfer, triton transfer, and alpha particle transfer to test the idea. Up to now, however, the suggestion remains only an idea, having yet to be proven experimentally.[38]

Of the group's more concrete accomplishments, the feat of producing the first usable alpha particle beam and the first usable helium-3 beam certainly ranks close to the top.[39] Indeed, the production of these beams opened new avenues of investigation into nuclear reactions and structure. In 1962, using the alpha particle beam, the group investigated a variety of reactions -- alpha particle + carbon -12 —> oxygen-15 + neutron; alpha particle + silicon-28 —> silicon -31 + neutron; alpha particle + sulfur-32 —> argon-35 + neutron; alpha particle + sulfur-34 —> argon-37 + neutron -- in order to determine the threshold energies, to both the ground and excited states, of the products. The results yielded values which were accurate to

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[37] F. P. Ziemba and E. Everhart, "Resonance Phenomena in Large-Angle Helium Ion-Helium Atom Collisiion," Physical Review Letters, Vol. 2 (1 April 1959), 299-300; G. M. Temmer, "On the Possibility of Resonant Transfer Processes in Nuclear Reactions," Physics Letters Vol. 1 (1 April 1962), 10-12.

[38] Temmer, "Resonant Transfer Processes in Nuclear Reactions," pp. 10-12.

[39] Captain Albert W. Harrison, Jr., personal interview with N. A. Komons, 10 April 1963.

less than one percent and, with one exception, agreed with previous data. The exception was the value for the products of the reaction, alpha particle + sulfur-32 —> argon-35 + neutron. The ground state threshold for the products of this reaction was 140 Kev higher than expected. The group was thus forced to reconfirm the value of the argon-35 mass. To do this, Temmer and his associates turned to the more conventional proton beam and trained it on chlorine-35 nuclei (proton + chlorine-35 —> argon-35 + neutron). This time the results were in agreement with previous data. But the discrepancy of 140 Kev has yet to be resolved.[40]

[40] RDT & E Project Documentation Continuation (DD Form 613a) Project No. 9750, 1 February 1963, p. 5.

Chapter V

COSMIC RADIATION

No artificial accelerator has yet produced energies equal to those obtained in the natural flux of cosmic rays, charged subatomic particles that constantly bombard the earth from all directions in space. Both the origin of the cosmic rays and the manner in which they acquire their great energy are still topics of scientific speculation. Cosmic rays have most often been discussed in recent years, at least on the popular level, as one of the various hazards facing human space travelers; but their existence was first discovered, in the early years of the present century, as a result of research conducted on the normal background radiation, to which they make a fairly significant contribution. Subsequently, cosmic-ray observations provided much basic knowledge concerning the nature of matter and energy, including the first experimental evidence of the existence of such elementary particles as muons and pions. The latter were not found in the primary cosmic radiation but in the secondary radiation created through interaction of the primary rays with the earth's atmosphere -- which becomes, for the cosmic-ray physicist, a kind of open-air atom smasher. Even today cosmic-ray studies continue to serve much the same purposes as other high energy physics research, alongside the parallel growth of interest in this radiation as an environmental factor affecting both manned and unmanned space flight.

II

There has been a certain amount of continuity in the list of institutions doing research in this area with AFOSR support. One original contractor that has remained with the program down to the present is the Enrico Fermi Institute for Nuclear Studies at the University of Chicago, where cosmic-ray research has been under the direction of Dr. John A. Simpson. In fact this Chicago project was one of those inherited by AFOSR, when the organization was created in 1951, from the Flight Research Laboratory at Wright-Patterson Air Force Base.[1] Simpson and his associates have been mainly interested since the beginning in the low-energy component of cosmic radiation (that is, below one bev per nucleon), which is the most abundant and, quite apart from its abundance, the most biologically significant.[2] It is also the part most easily subject to deflection by the terrestrial or other magnetic fields.

Initially the Chicago work involved collection of data on the neutron intensity in cosmic radiation by means of instrumentation carried aloft in high-flying aircraft or stationed at ground laboratories at various altitudes and latitudes.[3] Back in 1952, the ground stations ranged in location from Chicago, Illinois, to Huancayo, Peru, but the observational network was subsequently expanded until project documentation could quaintly observe that Dr. Simpson's studies would be "conducted on a worldwide basis in such widely separated locations as Sweden, Italy, Peru, New Mexico, and [sic] the United States."[4] Shipboard observations have been taken, in cooperation with the United States Navy; while for airborne measurements the use of aircraft has been increasingly

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[1] R & D Project Card, Project R-357-20-6, 27 June 1952.

[2] The reader should always bear in mind that the term "low energy," as applied to cosmic rays, does not mean the same as it does in references to "low energy physics" as distinct from "high energy physics." As mentioned in a previous chapter, any investigation of the primary cosmic radiation is a form of high energy physics. It is just that the energies present in the primary cosmic-ray flux as it approaches the earth are so much greater than any obtainable with terrestrial accelerators that a different frame of reference must be used.

The fact that low energy particles are potentially more hazardous than high energy particles may also appear odd at first glance. However, a high energy particle is more likely to pass straight through tissue, interacting with a relatively limited number of atoms in the biological material; a lower-energy particle, by contrast, may well be stopped altogether and proceed to deposit all its remaining energy in tissue.

[3] R & D Project Card, Project R-337-20-6, 27 June 1952.

[4] R & D Project Card, Task 37506, 4 April 1956, p. 23.

supplemented by balloon flights. The latter have often been launched from such sites as Prince Albert, Saskatchewan, where the efficacy of geomagnetic shielding against cosmic rays is lessened by nearness to the magnetic pole, and particles of relatively lower energy are thus able to approach the earth.[5] Starting in 1959, satellite experiments have been conducted, made possible largely by the National Aeronautics and Space Administration.[6] Likewise the original concentration upon neutron measurements was later modified to obtain data on a wider range of both primary and secondary cosmic-ray particles. From the standpoint of instrumentation, however, certainly the major contribution of the Chicago group has been the development of counters especially designed for neutrons and, in general, low-energy particles. In fact the basic neutron counter developed at the University of Chicago with AFOSR support was adopted as standard for worldwide measurements in the International Geophysical Year.[7]

The observational program of the Chicago group has led to a number of major accomplishments. One was the careful mapping, on the basis of observations carried out in the mid-1950s, of the earth's "outer" magnetic field -- that is, the field that is "required to account for the terrestrial distribution of cosmic-ray particles" as distinct from the "field distribution computed from surface magnetic measurements." The required observations were obtained with assistance from many sources, including both the United States Navy and the Air Force Cambridge Research Center. By establishing that large differences exist between

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[5] Cf. J. A. Simpson, et al., "Effective Geomagnetic Equators for Cosmic Radiation," Physical Review, Vol. 102, (15 June 1956), 1648 and Peter Meyer, "Primary Cosmic-Ray Proton and Alpha-Particle Intensities and Their Variation with Time," Physical Review, Vol. 115, (15 September 1959) 1734-41.

[6] See, e.g., AFRD, Weekly Activity Report, 29 April 1960.

[7] R & D Project Card, Project 9774, 1 April 1958, p. 19; AFOSR, Weekly Activity Report, 24 June 1959.

geomagnetic coordinates at ground level and what might be termed the cosmic-ray magnetic coordinates, this effort cleared up many seeming anomalies that had existed in the data on cosmic-ray intensity distribution.[8] Another notable accomplishment of the Chicago group was the gathering of the most definitive data known on the cosmic radiation associated with the giant solar flare of 23 February 1956. That was the fifth -- and largest -- major increase in cosmic radiation in conjunction with a solar flare since the first such observation made in 1942. Extensive data were obtained from all the ground measurement stations, and, thanks to an alarm system developed by the Chicago researchers which warned them when the event was beginning, they were able also to release a balloon that reached 90,000 feet over Chicago to gather cosmic-ray data during the time of intensification.[9]

The new insight gained by analysis of the February 1956 flare data is one of the developments in cosmic-ray research in recent years that have led to greater recognition than heretofore of the solar contributions to and solar influence upon the primary cosmic-ray flux. The Chicago group, for example, has obtained strong evidence that the so-called Forbush decreases (sharp intensity and spectral changes in cosmic radiation that occur during some magnetic storms) are of extraterrestrial, solar origin. It had been known previously that a Forbush decrease was associated with magnetic fields, but the source of the magnetic modulation was uncertain. To solve the problem, the Chicago scientists first compared measurements made during a Forbush decrease by means of neutron counters at Climax, Colorado, with measurements of the same event obtained from balloon-borne instrumentation at about 20 miles altitude. Then,

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[8] Simpson, et al., "Effective Geomagnetic Equators for Cosmic Radiation," Physical Review, Vol. 102 (15 June 1956), 1648-53; R & D Project Card, Project 3750, 22 April 1957, p. 116.

[9] P. Meyer, et al., "The Solar Cosmic Rays of February 1956 and Their Propagation Through Interplanetary Space," Physical Review, Vol. 104 (1 November 1956), 768-83; R & D Project Card, Project 3750, 22 April 1957, p. 116.

at the time of a later Forbush decrease, th y made a similar comparison between data from ground instrumentation and data obtained from a cosmic-ray detector contributed by the Chicago group to the payload of the satellite Explorer VI (which was launched on 7 August 1959 into an orbit of 4100 miles perigee and over 30,000 miles apogee). The relative change in cosmic-ray flux during a Forbush decrease, as measured at balloon altitude, was 1.8 times as great as that measured on the ground, the difference being attributed to atmospheric absorption. And when the satellite experiment was made, the decrease as measured by Explorer VI was 1.9 times that recorded at ground level. The relative change was thus virtually the same, even though the satellite instrumentation was far enough from the earth to be little affected by the earth's own magnetic field. It thus appeared highly probable that the mechanism producing Forbush decreases is seated in the sun and not the earth.[10]

As the foregoing example suggests, cosmic-ray research often produces important new knowledge about the magnetic fields of interplanetary space as well as about the radiation itself -- with cosmic rays serving, essentially, as a magnetic-field probe. Such magnetic-field data, moreover, have direct application not only to radio-communication studies but also to studies involving space-vehicle guidance and other comparable problems.[11]

The Chicago group has likewise obtained further understanding of the relationship between the sun and cosmic radiation by means of balloon flights that carried special instrumentation to compare time variations in the flux of protons and alpha particles in the primary cosmic rays. Careful analysis of the data indicated that the precise differences detected between alpha particles and protons -- for example, (relative) decrease in low-energy protons at the same time as the flux of low-energy alphas was (relatively) increasing -- could logically be

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[10] AFRD, Weekly Activity Report, 26 August 1960.

[11] Cf. AFOSR, Weekly Activity Report, 24 June 1959.

explained only on the basis that the sun is a producer of relatively low-energy alpha particles. This result was not particularly startling, as many people had already believed that the sun was contributing alpha particles to the cosmic-ray flux; but the Chicago scientists provided much more conclusive evidence than previously available.[12] In still other experiments, they found good evidence that there is a component of electrons in the primary cosmic rays -- a matter that had long been subject to controversy. Later balloon flights, in the summer of 1961, confirmed this finding and in addition produced strong indications that a part of the high-energy primary electrons come from the sun. This apparently constituted the first direct detection of such solar cosmic-ray electrons.[13]

III

The preceding highlights do not, of course, give a complete picture of the work conducted over the years by Prof. Simpson and his associates at the University of Chicago. Neither is it possible to do full justice, in a report of this scope, to the achievements of a research group headed by Dr. M. F. Kaplon at the University of Rochester, whose association with the AFOSR cosmic-ray program goes back to 1952. The Rochester scientists, like the Chicago group, have collected data with both ground-based and balloon-borne equipment, but they have been concerned to a greater extent with the high-energy part of the cosmic-ray spectrum. (This is the part that is of greatest interest for the study of nuclear interactions and elementary particles.) Thus they have, for example, conducted low-latitude balloon flights precisely to minimize the recording of low-energy particles.[14] In conjunction with the direct gathering of cosmic-

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[12] AFOSR, Weekly Activity Report, 23 October 1959, and AFRD, Weekly Activity Report, 16 December 1960.

[13] OAR, Weekly Activity Report, 28 July 1961; OAR Research Review, 3 December 1962.

[14] Heer, "804A Research on Geophysics," 31 January 1961 presentation, p. 3.

ray data, they have also conducted accelerator experiments, for purposes of calibration and comparison, at Brookhaven National Laboratory and the University of California.[15]

One topic of special interest to the Rochester group has been the so-called light elements (lithium, beryllium, and boron) in the cosmic-ray flux. These elements have not, in general, received as much attention as the protons and alpha particles, which are far more numerous, or the heavier particles, which are few in number but more spectacular in their effects. The determination of the relative abundance of the "lights" is a very difficult problem, but it is one to which the Rochester group has devoted a large and continuing effort. Kaplon and his associates made an important contribution toward establishing the fact -- long controversial but now generally accepted -- that the "lights" do occur in the primary cosmic-ray flux as it approaches the earth.[16] It still has not been established to what extent they are present in the "source regions" of galactic cosmic radiation and to what extent they originate by fragmentation en route (i.e., as by-products of the collision of heavier particles with interstellar particles of matter). However, the mere fact that fragmentation may play a major role in creating these particles makes their study of considerable interest for the question of the origins of cosmic radiation and also for possibly determining the amount of matter in interstellar space (i.e., deducing from the particles' abundance the quantity of matter needed to account for their existence by the fragmentation process). Since it is widely thought that the isotope helium-three (He^3) as found in the primary cosmic ray flux may also arise in whole or in part through fragmentation, studies of the isotopic abundance of helium are of cosmological and astrophysical interest for much the same reasons as the determination of the

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[15]Cf. R & D Project Card, Project 3750, 22 April 1957, p. 115, and R & D Project Card, Project 9774, 1 April 1958, p. 18.

[16]Air Force Scientific Research Bibliography 1950-1956 (AFOSR 700, 1961), Vol. I, pp. 796-803; OAR Research Review, 9 February 1962.

light-element component. And the Rochester group, working with AFOSR support, reported what was the first known study of the relative abundance of helium isotopes (He^3 and He^4) in the primary cosmic radiation.[17]

IV

Prof. S. Fred Singer of the University of Maryland is an investigator whose association with the AFOSR cosmic-ray program began in 1954 and continued until the end of 1962. He has not conducted such extensive data gathering and experimentation as the Chicago and Rochester groups, but he has still contributed some important new observational data, and he has served as a theorist and "idea man" of considerable originality.[18] Singer's calculations of the motions of charged particles trapped in the earth's magnetic field -- which he outlined to the American Physical Society, for example, in April 1956 -- directly forecast the Van Allen radiation belts that were later discovered through satellite experimentation. Even his conclusion that a part of the charged particle population would originate from cosmic-ray albedo (i.e., secondary particles bouncing back upward) appears to be confirmed by later satellite data.[19]

As a matter of fact, Singer himself was one of the earliest proponents of artificial satellite experimentation, so that several of the reports he prepared under AFOSR contract in the mid-1950s had to do with possible cosmic-ray or other experiments that might be carried out by means of satellite vehicles. He had his own proposal for a scientific satellite that he dubbed MOUSE (minimum orbital unmanned satellite of the earth); and even before Sputnik I he was busy theorizing about such

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[17] OAR, Weekly Activity Report, 5 May 1961

[18] R & D Project Card, Task 37506, 4 April 1956, p. 23; Heer, personal interview with Dr. Bushnell, 25 September 1962.

[19] Bulletin of the American Physical Society, Series II, Vol. I (26 April 1956), 229.

problems as the shielding of manned spacecraft against cosmic rays.[20] Since the start of the satellite era, moreover, Singer and associates at the University of Maryland have played a key role in the theoretical analysis and interpretation of the newly discovered radiation belts and related phenomena.[21]

It was also enti[illegible] appropriate that S. Fred Singer turned up as the leading scientific [illegible]laborator in AFOSR's celebrated Project Far Side, of [illegible]956-1957, whose objective was to develop a balloon-launched research rocket system that would efficiently and inexpensively convey instrumentation to extreme altitudes explicitly to search for trapped radiation. There were some who felt this was a strange business for a basic research contracting agency like AFOSR to become involved in; and the project was assigned to the AFOSR Directorate of Advanced Studies (since discontinued), not the Nuclear Physics Division. Nevertheless, Prof. Singer designed a cosmic-ray experiment to be taken aloft by the Far Side vehicle and also served as a scientific adviser for the entire undertaking. Because of operational failures in the October 1957 test series, Singer never retrieved any cosmic-ray data; and the project as a whole was somewhat inconclusive in its results. However, one or more of the Far Side vehicles launched did substantially exceed the altitude record of Sputnik I, so that the project received some short-lived publicity as a triumph of United States space science in the immediate aftermath of the first Soviet satellite experimentation.[22]

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[20] Air Force Scientific Research Bibliography, Vol. I, pp. 373-79. On MOUSE see, e.g., Sky and Telescope, November 1954, pp. 15, 17.

[21] See, e.g., S. Fred Singer, "'Radiation Belt' and Trapped Cosmic-Ray Albedo," Physical Review Letters, Vol. I (1 September 1958), 171-73; RDT&E Project Documentation (DD Form 613), Project 9774, 1 February 1962, atch. 2, p. 8.

[22] Aviation Week, 28 October 1957, p. 31; Management Report (ARDC Form 111), Project 4750, 31 October 1957; New York Times, 1 November 1957; Brig. Gen. Hollingsworth F. Gregory, Cmdr., AFOSR, address to Aircraft Manufacturers Representatives, Washington, D. C., 12 November 1957.

Even while he was still following the progress of Project Far Side, in the summer of 1957, Singer was working on another noteworthy experiment. Using special instrumentation designed to detect small, very short-lived increases in the intensity of the low-energy component of cosmic radiation, and with a B-47 aircraft provided by the Rome Air Development Center, Singer on 9 August 1957 detected low-energy cosmic rays originating in a relatively minor solar flare. This was believed to constitute an observational "first," or at least the first time that such an observation had been made with any assurance. Two separate intensity increases were detected, each lasting about two to three minutes, and they corresponded well with optical and magnetic observations from other sources of a small solar flare.[23] The experiment confirmed the previous speculations of Singer and various other scientists to the effect that many more solar events contribute to the cosmic-ray flux than had actually been observed:

> Thus was resolved a question which had plagued cosmic ray physicists: Why did some solar flares produce low energy cosmic rays while others did not? Dr. Singer reasoned that all flares did produce cosmic rays and that instrumentation to detect some of the more transient events had to be designed, built and tested. The correctness of the reasoning is evident since the events were detected. . . .[24]

Singer has continued his special interest in short-time cosmic-ray variations and their "correlation with small solar flares and/or geomagnetically disturbed conditions."[25] He has sought measurements, unsuccessfully, at ground stations, both at high altitude and at very high latitude (Thule, Greenland); and he has made some use of balloons; but his program

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[23] J. J. Corrigan, S. Fred Singer, and M. J. Swetnick, "Cosmic-Ray Increases Produced by Small Solar Flares," Physical Review Letters, Vol. I (1 August 1958), 104-05; AFOSR, Second Annual Report Air Force Office of Scientific Research 1957 (AFOSR TR 58-71), p. 107; AFOSR, "Weekly Activity Report to Hq ARDC," 2 October 1957.

[24] AFOSR, Scientific Mission and Operational Management of the Air Force Office of Scientific Research...Fiscal Year 1959, p. 49.

[25] RDT&E Project Card (DD Form 613), Project 9774, 31 January 1961, p. 13.

has been oriented principally toward the use of aircraft, as in the August 1957 experiment. Singer was particularly anxious to take measurements at high altitude and latitudes with a fast-moving vehicle so that cosmic-ray intensities could be studied along a single geomagnetic meridian in a short time span, thus minimizing the effect of time variations in background radiation. What he really needed was a U-2. However, to obtain test time on such an aircraft is no easy matter. Although instrumentation designed by Singer has been flown by U-2 on more than one occasion, his project suffered numerous delays due to the non-availability of test aircraft, and final analysis has not yet been made of data obtained on U-2 flights.[26]

V

The Chicago, Rochester, and Maryland efforts have been those of longest duration in the over-all AFOSR cosmic radiation research program. But numerous other scientists and institutions have participated at one time or another, both in the United States and abroad. Prof. John Green of the University of New Mexico received support for several years in his study of extensive air showers -- cascades from single primary particles -- for which he devised an unusual ground-level detector known as a "liquid scintillator telescope." Green was especially interested in developing a theory of shower development, for better extrapolation from secondary to primary cosmic-ray phenomena.[27] At New York University, Prof. Serge A. Korff received support for a number of studies concerned with cosmic radiation, including one that began in 1959 of cosmic-ray variations over a geologic time scale, as indicated by

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[26] Ibid., pp. 13, 37; Heer, personal interviews with Dr. Bushnell, 25 September 1962 and 28 February 1963.

[27] John R. Green, Preliminary Report on the Development of a Large Scintillator for Observation of Extensive Air Showers (AFOSR TN-57-432), 20 June 1957, and A Large Scintillator for Observation of Cosmic Rays (AFOSR TN-57-433), 22 July 1957; AFOSR, Weekly Activity Report, 31 July 1959; AFRD, Weekly Activity Report, 24 March 1961; R & D Project Card, Project 9774, 1 April 1960, p. 11.

clues contained in 3000-year-old Antarctic ice and in meteoric material.[28]

Two more AFOSR-supported studies, currently in progress, involve extensive use of high-altitude balloon flights in gathering cosmic-ray data. One of these, directed by Prof. Robert R. Brown of the University of California, Berkeley, is concerned with cosmic radiation in the polar regions, which for lack of geomagnetic shielding offer the greatest intensity of galactic cosmic rays. Brown's research team has correlated data from Alaskan flights with similar data obtained over northern Europe and also with measurements of ionospheric absorption, in the latter case to determine how the cosmic-ray flux and composition following solar events are associated with radio communications effects. In the other study, a team at Washington University, St. Louis, headed by Prof. Michael Friedlander, has had considerable success with balloon flights simultaneously conducted at two different locations -- e.g., Missouri and Saskatchewan, to observe the effect of different amounts of geomagnetic shielding. In August 1962, moreover, Friedlander's group obtained data from balloon flights that were conducted within nine minutes of each other, in the United States and the Union of South Africa. This notable experiment supplied information on the influx of low-energy primaries a half world apart, but at essentially the same instant in time.[29]

VI

The AFOSR cosmic-ray program has also made considerable use of overseas research capabilities. The first foreign contractor was Dr. C. B. A. McCusker of the Dublin Institute for Advanced Studies, who since the early 1950s had been studying the rate and direction of arrival of cosmic-ray showers using ground-level equipment at Dublin.[30] His project

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[28] R & D Project Card, Project 9774, 1 April 1960, p. 13, and 31 January 1961, p. 39.

[29] RDT&E Project Documentation, Project 9774, 1 February 1962, atch. 2, pp. 4, 14; Heer, "804A Research on Geophysics," 31 January 1961 presentation, p. 6; Heer, personal interview with Dr. Bushnell, 2 June 1963.

[30] R & D Project Card, Project 9774, 1 April 1958, p. 7.

involved a continuation of the work at Dublin plus activation of a similar (though smaller) data-gathering station at the University College of the West Indies, Jamaica. McCusker hoped to confirm the principal finding he had made previously at Dublin, which was the apparent existence of an anisotropic effect in the arrival of cosmic rays -- i.e., that they do not arrive evenly from all directions in space as had been generally assumed. The indications of anisotropy were interesting from a theoretical standpoint as well as suggesting that, to minimize radiobiological dose rates in travel through space, there might be "preferred 'lanes' which space ships would choose to traverse."[31] Still another ground station was added later, at Sydney, Australia, thus giving "continuous celestial sphere coverage with the exception of the polar regions." In due course McCusker himself decided to settle in Australia, where he joined the faculty of the University of Sydney -- without giving up his Dublin and Jamaica data-gathering posts. He has continued to do important research on cosmic rays. However, he has not yet convinced the majority of his fellow cosmic-ray physicists that the "anisotropy" is real. Or, to be more precise, there is still no conclusive evidence of anisotropy for the relatively low-energy radiation that McCusker is studying.[32]

AFOSR is still supporting McCusker's work, although it no longer specifically sponsors ground-level studies of a possible directional effect. Rather, it has recently given assistance to McCusker for high-energy cosmic-ray studies, involving the analysis of balloon-borne nuclear emulsions. For this purpose AFOSR has arranged various balloon flights on his behalf in the United States, from Holloman Air Force Base, New Mexico, and Goodfellow Air Force Base, Texas.[33]

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[31] History of Air Research and Development Command, July-December 1957, Vol. II, p. 335.

[32] R & D Project Card, Project 9774, 1 April 1959, pp. 7-8; Heer, personal interview with Dr. Bushnell, 4 October 1962.

[33] Management Report, Task 9774-02, 22 March 1962; interview with Mr. Heer, 4 October 1962.

McCusker was only the first of several European scientists who have received AFOSR support for cosmic radiation research. Beginning in 1958 assistance was given to a group at Lund University in Sweden, headed by Prof. Sten von Friesen. Von Friesen and his colleagues were interested in very high-energy interactions, and in connection with this research they were developing a new photoelectric technique for the identification of primary cosmic-ray tracks in nuclear emulsion by measuring small differences in track width. The method offered great accuracy in determining the composition of the cosmic radiation, and especially in identifying the so-called light elements. The Swedish group used this new method in analyzing emulsions which had been flown for them (by the Office of Naval Research) in the United States.[34] Another AFOSR-supported European effort, under Prof. F. G. Houtermans at the University of Bern, Switzerland, has been concerned with the thermoluminescence of meteorites, thereby seeking to determine the amount of radiation to which the meteorites have been exposed in past eons. The project is also of some interest for re-entry studies, since it involves the observation of effects that occur as meteors plunge down into the earth's atmosphere. Then, too, a team of scientists at Germany's Max Planck Institut für Aeronomie has obtained support for a study of time variations in low-energy ionizing radiation from the sun. Their principal contribution has been in developing detection equipment to be taken aloft on balloon flights from Kiruna, Sweden; and the resulting test data are correlated with similar data from high latitudes in the Western Hemisphere, notably including the Alaskan balloon-flight data obtained by Brown and associates of the University of California.[35]

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[34] R & D Project Card, Project 9774, 1 April 1960, pp. 12-13, and 31 January 1961, p. 36; AFRD, Weekly Activity Report, 1 April 1960; K. Kristiansson, et al., Photometric Charge Determinations of Heavy Primaries of Cosmic Radiation (AFOSR-TN-60-1469, June 1960).

[35] RDT&E Project Documentation, Project 9774, 1 February 1962, atch. 2, pp. 3, 10; interview with Maj. William C. Bryan, Physics Division, EOAR, 12 June 1962.

This does not exhaust the list of European and United States scientists performing research on cosmic rays with AFOSR support. However, the majority of the investigations in those two geographic areas have been mentioned, including all the most productive efforts as well as some that are interesting or indicative for other reasons. Just one topic remains to be considered: the support of cosmic-ray studies in South America, where some of the most important work in this field is currently in progress.

VII

South America played a part in the AFOSR cosmic-ray program almost from the start, even though the connection was at first indirect. As already mentioned, the University of Chicago group under Prof. Simpson had one of its ground observing stations in Peru. To be exact, this was located at Huancayo, seat of the Geophysical Institute of Peru (Instituto Geofísico del Perú), which had been established originally by the Carnegie Institution but became an autonomous Peruvian research institute in 1947. Later on, in 1959-1962, the Geophysical Institute became an AFOSR contractor in its own right for the purpose of carrying out spectrophotometric observations of solar flares. This was not a study of cosmic radiation *per se*, but the topic was closely related to solar production of and influence on cosmic radiation, and the study was monitored by the Nuclear Physics Division.[36]

Even before the last-mentioned study was initiated, AFOSR had begun support of other work at the Chacaltaya Laboratory of Cosmic Physics (Laboratorie de Física Cósmica de Chacaltaya) in Bolivia. This work has since developed into one of the most promising efforts in the AFOSR cosmic-ray program -- and in fact in the entire array of AFOSR-supported

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[36]Alberto Giesecke M., "El Instituto Geofísico del Perú," *Ciencia Interamericana*, May-June 1962, pp. 3-5, 8; R & D Project Card, Project 9774, 1 April 1959, p. 33; RDT&E Project Documentation, Project 9774, 1 February 1962, atch. 2, p. 6.

research in all fields of science. At first glance, of course, Bolivia would seem an unlikely place for a major scientific undertaking. Not only is it relatively isolated -- a landlocked and mountainous nation that is easily accessible only by air -- but Bolivia is poor economically even by Latin American standards. Both isolation and the lack of financial resources have created difficulties for its scientific development. However, the highly competent director of the Chacaltaya Laboratory, Dr. Ismael Escobar V., has been making good use of available funds, equipment, and personnel ever since the facility was established in 1949 under the aegis of the nearby University of San Andrés at La Paz. Escobar, who also has continued to serve as professor of physics at the University, has sought with some success to make Chacaltaya and La Paz a small international center of physical research, with a number of visiting professors and investigators from the United States, South America, Europe, and Asia on hand at any one time. Moreover, the Chacaltaya site itself has certain definite advantages for cosmic-ray work, in which the staff has specialized from the beginning. Its elevation, which is roughly 17,700 feet, significantly reduces the amount of atmospheric absorption that incoming particles undergo. It is also at low latitude: 16 degrees south geographic, and less than that geomagnetic. This, of course, makes the radiation flux more nearly monoenergetic, because of the geomagnetic shielding effect that keeps out low-energy particles while allowing the higher-energy particles to reach the earth's atmosphere. Location near the equator means further that there is a very good "view" of the plane of the galaxy, which is an important factor for some types of observation.[37]

United States Air Force support of Dr. Escobar's cosmic-ray research began in 1958. This research was concerned mainly with (a) time varia-

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[37]Ismael Escobar V., "El Laboratorio de Física Cósmica de Chacaltaya," Ciencia Interamericana, November-December 1961, pp. 3, 7; Heer, "Bolivian Trip Report," 24 August 1962; AFOSR, Weekly Activity Report, 8 April 1959; RDT&E Project Card, Project 9774, 31 January 1961, p. 32.

tions in cosmic radiation and their association with solar phenomena including magnetic storms and meteorological data, and (b) extensive air showers, with special (but not exclusive) attention to the possible existence of asymmetries in direction of arrival of the cosmic particles causing them. Principally for the sake of the time-variation studies, much effort had to be devoted to the improvement of local weather observations, so that corrections for the influence of pressure and temperature changes could be made on the basis of direct meteorological soundings rather than mere extrapolation from those taken elsewhere. This problem was largely overcome during 1961, when (among other things) the United States Air Force through its 4th Weather Group provided the necessary equipment for a rawinsonde station. Useful cosmic-ray data were collected, however, before as well as after 1961. These data supplied further evidence, for example, that the source of daily variations in cosmic-ray intensity is extraterrestrial, since there was a time lapse in recording the variations between cosmic-ray telescopes (directional counters) pointing at different parts of the sky -- whereas a local, terrestrial source would presumably have caused variations to be noted by all the instrumentation simultaneously. Also, data collected on Forbush decreases showed indications of anisotropy in the observed pattern, contrasting with the isotropy usually observed at middle-latitude stations. A possible explanation would be the existence of some anisotropy for high-energy but not for low-energy particles.[38]

For the research on extensive air showers, special equipment was brought to Bolivia that had formerly been used by the cosmic-ray group at the Massachusetts Institute of Technology (M.I.T.) under Prof. Bruno Rossi. (AFOSR had been supporting Rossi's work to a small degree,

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[38]Escobar, "El Laboratorio de Física Cósmica de Chacaltaya," Ciencia Interamericana, November-December 1961, p. 4; RDT&E Project Card, Project 9774, 31 January 1961, pp. 14-15, 32 (and also other editions of the same project documentation); Heer, "Bolivian Trip Report," 24 August 1962; OAR Research Review, 30 July 1962.

through its part in a joint contract also funded by the Atomic Energy Commission and the Office of Naval Research.) The equipment consisted of a "group of symmetrically arranged counters connected to a common recording station through accurate delay lines." Necessary on-site modifications were performed by Dr. Juan Hersil, an associate of Dr. Escobar who had formerly worked with the M.I.T. group. This instrumentation array was not taken directly to Chacaltaya but rather was located at another point on the outskirts of La Paz, at a mere 14,000 feet elevation. Even so, it constituted the highest array of air-shower counting equipment ever assembled. One thing the resulting data showed was that the structure of showers at 14,000 feet was markedly different from what one would extrapolate from sea-level data.[39]

However, the most important single fact about the extensive air showers study is that it served as a nucleus around which to organize the present Bolivian Air Shower Joint Experiment (BASJE), whose essential objective is to gather data on the high-energy gamma-ray component of primary cosmic radiation. Cosmic gamma rays are exceedingly hard to detect and study; but they are of unusual interest because they are uncharged (unlike the nucleonic component) and therefore are not subject to magnetic deflection as they travel from their point of origin. In effect, they have a "memory" of the direction from which they came. They are thus of considerable interest for all questions relating to the origins of cosmic radiation. Because the production of high-energy gamma rays is likely to occur only under very special conditions, they can also tell something about the nature and distribution of interstellar matter. One place where the necessary conditions may well exist is near the center of our galaxy, in association with great turbulence and star-forming activity; and precisely because dust interferes with optical observations in this region, gamma-ray astronomy appears to be the

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[39]AFOSR, Weekly Activity Report, 8 April 1959; RDT&E Project Card, Project 9774, 31 January 1961, p. 32; AFRD, Weekly Activity Report, 27 January 1961.

most promising technique available. Cosmic gamma rays offer possibly the only means of detecting the existence of molecular hydrogen in interstellar space; they can provide a fruitful basis for comparisons between our own and neighboring galaxies; and there are still other important possibilities, over and above whatever information gamma-ray studies may offer regarding the traditional concerns of cosmic-ray physics.[40] Hence recent years have seen a distinct growth of interest in gamma-ray observations, on the part of AFOSR investigators and others. NASA's Explorer XI satellite, for example, detected primary gamma rays from space in the energy range from 100 Mev to 10 Bev, but satellite-size instrumentation is not adequate to detect a significant sampling from the small flux at 10^4 Bev and above, which must be studied in order to deal with the key astrophysical problems just listed. On the other hand, primary gamma rays in this energy range may cause extensive air showers through interaction with the atmosphere. Such gamma-induced showers can be detected with ground-based instrumentation, preferably at very high altitude -- say, in the Bolivian Andes.[41]

The preliminary discussions and planning that went into the creation of BASJE took place mainly in Tokyo, Japan, and Cambridge, Mass., and involved close collaboration between Drs. Minoru Oda and Koichi Suga of the Tokyo Institute for Nuclear Studies and Drs. Bruno Rossi and George Clark and associates at M.I.T. But these scientists were naturally in contact with Dr. Escobar, whose Chacaltaya Laboratory was the ideal site for a cosmic gamma-ray experiment; and, by a contract with starting

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[40] Heer, transcript of presentation to AFOSR Physics Advisory Committee, 23 April 1961, pp. 7-8; RDT&E Project Card, Project 9774, 31 January 1961, p. 38.

[41] CAR Research Review, 11 March 1963. The desirability of a Bolivian location was due not only to the altitude factor but also to the fact that it was a good place from which to view the plane of the galaxy (and especially its center) where star formation is most likely (RDT&E Project Documentation, Project 9774, 1 February 1962, atch. 2, p. 9).

date of 1 October 1960, AFOSR began to contribute toward the financial support of the undertaking. Much of the necessary equipment was made at M.I.T. (for which purpose Dr. Suga moved temporarily to Cambridge) and was shipped to Chacaltaya in the spring of 1961. The air-shower array already in use by the Bolivian group was also earmarked for inclusion in the BASJE instrument complex and was similarly moved to Chacaltaya.[42]

The main difficulty that had to be overcome in designing the BASJE project was that of differentiating between air showers initiated by the charged nucleonic component of cosmic radiation and those initiated by high-energy cosmic gamma rays. The problem was not easy, but a solution did exist:

> It was calculated that approximately 1 in every 3,000 EAS [extensive air showers] should be due to a primary gamma ray, the rest due to the high-energy nucleonic component. But... it was predictable that a gamma-induced EAS should have very few, if any, mu mesons (the main penetrating component of EAS) whereas ordinary EAS do have mu's. The approach, then, was to combine, in anti-coincidence, an EAS detector with a penetrating particle detector. If both detectors record an event, it is highly probable that the event is due to a high-energy nucleon, and it is discounted. If, however, only the EAS detector records an event, it is possible that the event is due to a primary gamma and the event is worthy of further analysis....[43]

There are also other differences between the two types of air showers, but the presence of mu mesons is the indicator principally relied upon in BASJE. Thus the key element in the experiment is an array of mu meson (penetrating particle) detectors, consisting of scintillation counters carefully shielded to keep out the soft components of ordinary air showers. The shielding consists of over 100 tons of reinforced concrete, which in turn supports 160 tons of lead sulfide provided by the Bolivian government.[44]

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[42] OAR Research Review, 11 March 1963, RDT&E Project Card, Project 9774, 31 January 1961, p. 38.

[43] OAR Research Review, 11 March 1963.

[44] Ibid.

This installation became operational about the beginning of 1962.[45] As data were collected, they were put on punched cards in Bolivia and sent to M.I.T. for computer analysis and further study. In the first six months almost a million extensive air showers were detected, of which it was thought that around 260 might have been due to gamma rays. These preliminary results suggested that there was "no striking anisotropy" in gamma radiation in the energy range of 10^4 to 10^6 Bev: there was some increase in gamma radiation from near the galactic plane, "but not by significant amounts." The gammas appeared to be both galactic and extragalactic, as also suggested by certain experiments conducted elsewhere. All these and other conclusions were tentative, pending (among other things) a more positive identification of the events assumed to be gamma-induced. In the meantime, however, more information was also being obtained about ordinary air showers not considered in the gamma-ray study itself.[46]

At the close of the first six-month period, the international team of scientists associated with the project initiated a review of both the data gathered so far and the theoretical analysis on which experimental procedures have been based. It is possible that changes will be made in details of the experiment, for instance to increase the amount of shielding used.[47] In any event, even though BASJE is still in its early stages it represents "the biggest, most imaginative, and possibly most important thing AFOSR has ever attempted in cosmic ray physics."[48] It is

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[45] Science Week, 3 August 1962.

[46] OAR Research Review, 11 March 1963; RDT&E Project Documentation, Project 9774, 1 February 1962, atch. 2, p. 9; Heer, "Bolivian Trip Report," 24 August 1962.

The preliminary data discussed in the text had, of course, been supplemented by additional data as of the time of writing (spring 1963); but the picture that emerged from the data was still essentially the same as that indicated.

[47] OAR Research Review, 11 March 1962.

[48] Heer, presentation to Physics Advisory Committee, 23 April 1961, p. 7.

promising enough, in fact, for AFOSR to devote roughly one-fifth its cosmic-ray research budget to this one scientific effort.[49] What, precisely, will be discovered from the experiment is of course still a matter of speculation -- with the BASJE experimenters themselves taking great pains to avoid excessive or premature claims. Just as in most other scientific endeavors, if the results could be predicted with any clear assurance there would be little need to perform the experiment. Much of the fascination and promise of BASJE thus lies in the mere fact that the project is probing matters whose great potential significance is matched only by our equally great lack of present scientific knowledge concerning them.

[49] Science Week, 3 August 1962.

Chapter VI

THEORETICAL NUCLEAR PHYSICS

Of all the theoretical groups sponsored by the Nuclear Physics Division, that headed by Leonard I. Schiff at Stanford University has ranged over as varied a terrain as any other, although many of the problems it has tackled over the years were directly or indirectly connected to the experimental work performed with the Mark III electron linear accelerator. The problems occupying the members could be anything from interpreting data, suggesting experiments, or pursuing some pet project of their own. For example, during 1959, the group suggested four detailed experiments to test electrodynamics at distances less than 10^{-13} centimeters. At the same time, it went on with its work of interpreting the data coming from the Mark III and found time to do some original work on gravitation.[1]

Since the Stanford machine accelerated electrons, it was natural enough that a substantial portion of the work occupying Schiff's group concerned electron scattering and electromagnetic field theory. And as far as this phase of its work was concerned, it is well to keep in mind the intimacy between Schiff's group and Hofstadter's experimental group. In 1956, for instance, while Hofstadter and W. K. H. Panofsky, another AFOSR contractor, were bombarding heavy nuclei, Schiff's group worked out a formalism for the asymptotic evaluations of elastically scattered electrons.[2] The following year, in another theoretical study, the group demonstrated that in the case of electron scattering at about 200 Mev and higher it was possible to justify the use of high energy approximation for incident electrons. Then, by making a further approx-

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[1] R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1960, pp. 8-9.

[2] R & D Project Card (DD Form 613-1) Project No. 3750, 4 April 1957, p. 107.

imation on its final and very complex formula for scattering, the group illuminated the most characteristic effects of proton correlations. The effects of these correlations within nuclei, it was shown, arise in neither elastic nor inelastic scattering; rather, they appear when the scattering cross section is summed over the final nuclear states. In other words, the group was saying that in order to exhibit the effects of proton correlations the scattering experiment should be performed with poor energy resolution.[3]

As for work of more recent vintage, the group devised a new method of computing the energy level spectrum of nuclei. The method considers static potentials containing infinite repulsive cores. When applied to oxygen-16, the method was especially successful. Here the results showed a reasonable agreement with experiment both for the first five energy states and for the ground state binding energy. Strange particles received some attention, too. The decay mode of the charged kaon (kaon ⟶ pion + positron + electron) was calculated and was found to occur once in 10^5 events. This was a new and more accurate estimate, for previously it was believed to occur once in every 10^7 events.[4]

II

As pointed out in another chapter in connection with Hofstadter's experiments, the cumulative effect of a variety of high energy studies during the 1950s was to focus a great deal of theoretical attention upon the electromagnetic structure of the proton and neutron. This was certainly true at Stanford, where in 1956, D. R. Yennie, Maurice Lévy, and D. G. Ravenhall completed a study of the theoretical implications of these experiments.[5] It was also true at Princeton University, where an

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[3] R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1958, p. 3.

[4] RDT & E Project Documentation Continuation (DD Form 613a) Project No. 9750 (1 February 1963), pp. 5-6.

[5] D. R. Yennie, M. M. Lévy, and D. G. Ravenhall, "Electromagnetic Structure of Nucleons" (AFOSR TN 56-559) November 1956.

AFOSR grantee, Professor Marvin L. Goldberger, and his associates attacked the electromagnetic structure of the nucleon by making an extensive use of dispersion relation techniques. In doing so, the Princeton group was leaning heavily on the work of another AFOSR contractor, Stanley Mandelstam, who was a member of a theoretical group under Professor Robert Karplus at the University of California, Berkeley.

The field of dispersion relations represents what is perhaps one of the most significant theoretical advances in elementary particle physics in recent years; and the advance is almost wholly due to Stanley Mandelstam. The aim of the technique is to describe the interaction of physical systems. Consider, for example, a simple interaction in particle scattering where a scattered wave is linearly related to a primary wave. In this case, the scattered wave and the primary wave represent the wave character of a particle interacting with a scattering medium. The mathematical relation which connects the scattered and primary wave is called a dispersion relation. One of the first uses that dispersion relations were put to was in establishing a connection between light's refractive index and the linear absorption coefficient of the medium it traverses. In this case, since only a single relation at a given frequency connects the refractive index and the absorption coefficient, the relation is called a single dispersion relation. Until Mandelstam completed his work in 1959, only this kind of single dispersion relation was possible. What Mandelstam did that year was to extend the single dispersion relation to a double dispersion representation. He thus made it possible to treat reactions in systems where two particle transitions were involved. The technique makes use of integral representations in quantum field theory. By ignoring detailed models and by using approximation methods, it extracts from field theory vigorous and far-reaching information. Moreover, since it employs only the general structure and invariance properties of the field description, the results, irrespective of the specific form of the ultimate description, will be valid. The technique has found applicability not only in nuclear reactions, but also in numerous other areas: (1) the propagation of radiation in the atmosphere, (2) pion-nucleon scattering, (3) nuclear structure, (4) shielding problems, (5) general electrical systems, (6) field theory. Appropriately, the technique became known as the "Mandelstam Representation."[6]

Now back to Goldberger and his associates. Resorting to Mandelstam dispersion theory, they were able to relate scattering amplitudes and other physically meaningful data by employing the analytic qualities of these properties. Moreover, where no method existed before for the analysis of strong interaction dynamics and nuclear forces, such a method was now available to them. Accordingly, the electromagnetic structure of the nucleon was studied exhaustively. To its satisfaction, the group was able to point to the contributions of the two-pion intermediate state to the magnetic moments and mean square radii of nucleons. It was suspected that the electromagnetic structure of the meson itself may have played a significant role here. In any event, it was reasonable to assume that the two-pion state did partially account for these properties. Eventually, while dealing with potential scattering problems, Goldberger and company asked the question, "Can dispersion theory coupled with unitarity be a substitute for the Schrödinger equation in non-relativistic theory?" The answer was an emphatic yes.[7]

The techniques of dispersion relations -- this time in the hands of A. O. Barut, a theoretician from Syracuse University -- also proved helpful in bringing some order among the recently discovered resonance particles. The two-pion and three-pion resonance states have already been

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[6]RDT & E Project Card (DD Form 613c) Project No. 9750, 16 January 1961, p. 9; AFOSR Weekly Activity Report, 24 July 1959; Stanley Mandelstam, "Determination of the Pion-Nucleon Scattering Amplitude from Dispersion Relations and Unitarity. General Theory," Physical Review, Vol. 112 (15 November 1958), 1344-60; see also by the same author, "Analytic Properties of Transition Amplitudes in Perturbation Theory," Physical Review, Vol. 115 (15 September 1959), 1741-51.

[7]R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1959, pp. 5-6, 49, 1 April 1960, p. 9.

referred to. But in addition to these, a number of other resonances involving pions have cropped up: pion-N, pion-kaon, and pion-lambda. It was Barut's hope that some form of classification and systematization could be found that would fit these resonances, just as one was found in the late 1950s for all the unstable strange particles known up to that time. What Barut found was that the solution of the dispersion relations had a true and infinite number of extra solutions. The true solution corresponded to the perturbation theory solution and was generated uniquely. The extra solutions exhibited resonance behavior. They corresponded to the inclusion of unstable intermediate states into the theory.[8]

At Harvard University, Professor Julian Schwinger, a member of a theoretical group receiving AFOSR support, tackled an even broader problem. His objective was to provide a uniform theory of all particles, both stable and unstable. For this task he did not employ dispersion relations, but relied principally on the mathematical structure of relativistic quantum field theories. It is one of the good fortunes of physics that a mathematical method developed in one area is applicable to another. For example, field theory is common to both relativistic quantum mechanics and statistical physics, and can be approached through either.

Schwinger restricted his discussion to the realm of quantum statics. Since it lacks a specific reference to time, this realm is concerned only with measurements performed at a common time or with idealized systems whose properties are unchanged in time. What he did to achieve his objective was to employ the example of a spinless boson field. This permitted him to develop the structure of the simplest Green function, which, in turn, provided him with a uniform theory. Paying some attention to the time decay laws of unstable entities, Schwinger emphasized that a complete account of the relevant physical situation had to be con-

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[8] RDT & E Project Card (DD Form 613c) Project No. 9751, 16 January 1961, pp. 18-19.

tained in the situation's mathematical representations. This led him to conclude that "an essential failure of the exponential decay law marks the limit of applicability of the physical concept of unstable particles."[9]

III

During the course of 1957 and 1958, the Princeton group spent some time on the decay of the pion. As a consequence, Professor Goldberger came up with the first serious quantitative calculation of that particle's lifetime that had been obtained directly from quantum field theory. The theoretical lifetime agreed to within 10 percent with the experimental value.[10] Shortly thereafter, at the University of Paris, Professor Maurice Lévy, who was working with AFOSR's support in approximately the same area, attempted to derive Goldberger's formula for the rate of pion decay by considering the possibility that the divergence of the axial vector current in beta decay may be proportional to the pion field. Lévy was able to present three models of the pion-nucleon interaction that had the required property. The first model had the advantage of being easily generalized to strange particles. But it had one disadvantage: it was unrenormalizable. The second model was renormalizable; however, it involved postulating a new particle. Moreover, it was not easily extended to strange particles. The third model bore a strong resemblence to the second, with the exception that it was not necessary to postulate a new particle.[11]

In 1962, Marvin Goldberger and Maurice Lévy found themselves working in approximately the same area once again. This time, however, their

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[9] RDT & E Project Card (DD Form 613c) Project No. 9750, 23 January 1961, pp. 8, 53.

[10] R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1959, p. 6.

[11] RDT & E Project Card (DD Form 613c) Project No. 9750, 16 January 1961, p. 11.

views diverged.[12] In June of that year, Goldberger and two of his colleagues, Richard Blankenbecler and L. F. Cook, suggested that the photon might not be an elementary particle. They reached their conclusion in this manner. Beginning with the 1960s, a few theoretical physicists, Geoffrey Chew among them, began associating strongly interacting particles with trajectories of so-called "Regge poles." This meant that in a technical sense these particles were now to be regarded as nonelementary. Blankenbecler, Cook, and Goldberger took note of this -- indeed, they even did some work themselves on the problem -- and began wondering why the photon should be excluded from the list of nonelementary particles. They reasoned thus: "We would like to argue that since photons interact with all charged particles (including the so-called strongly interacting ones) if they [the strongly interacting particles] are nonelementary, then the photon must be nonelementary as well."[13] By September, however, Lévy had given thought to the problem himself and took exception to Goldberger's position. In strong interactions, Lévy argued, where a consistent field theory does not exist, it is purely a matter of taste -- "or, as has been said, a 'philosophical' question" -- whether one does or does not regard Regge poles as fundamental. But the question of the photon was altogether different. With the photon, one finds himself in the realm of quantum electrodynamics. Here a field theory does exist. "And since a Regge behavior of the cross sections can be obtained by a consistent high energy approximation to the field," Lévy concluded, "we do not see any reason at present to question the elementary nature of the photon."[14]

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[12] RDT & E Project Documentation Continuation (DD Form 613a) Project No. 9750, 1 February 1963, p. 6.

[13] R. Blankenbecler, L. F. Cook and M. L. Goldberger, "Is the Photon an Elementary Particle?", Physical Review Letters, Vol. 8 (1 June 1962), 463-65.

[14] Maurice Lévy, "Electromagnetic Radiative Corrections and the Elementary Nature of the Photon," Physical Review Letters, Vol. 9 (1 September 1962), 235-38.

Interactions in general were also occupying the attention of A. O. Barut. As is known, weak interactions occur in a variety of forms. They may involve leptons (electrons, muons, photons); or they may not involve leptons. While a description of each of these forms had been achieved, there was no unified and symmetric description of all the forms of these interactions. Barut achieved such a unified description. He did so by employing both Boson and Fermi currents, along with a fine structure in the coupling constant. The effect of the strong interactions, of course, could not be calculated; therefore, the coupling constants were the <u>effective</u> coupling constants. In a sense, Barut's theory assumed a phenomenological character.[15]

Another AFOSR contractor, Professor Walter Thirring, along with two of his associates at the University of Vienna, approached weak interactions from another angle. There was a time, Thirring pointed out, when all the elements were considered to be elementary. Today, by the same token, no one seriously believes that all the thirty or so elementary particles are genuinely elementary. Should they be eventually reduced in number, would there still be three basic interactions (strong, electromagnetic, weak) among particles or would these interactions merely represent special aspects of one universal interaction? Actually, Thirring was not attempting to answer this question for all time. He was merely hoping to shed a little more understanding on the three fundamental interactions.

Thirring approached his task by reducing strong interactions to a universal weak interaction. In other words, the former was considered as a high energy phenomenon of the energy-dependent weak interaction. To put the assumption to the test, Thirring chose the pion as an example, considering it as a nucleon-antinucleon bound state which was coupled by a weak interaction. Calculating the coupling constant between the

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[15]RDT & E Project Card (DD Form 613c) Project No. 9751, 16 January 1961, p. 19.

pion (i.e., the bound state) and a nucleon, he found it to be on the same order as strong interactions. From this he could conclude that some of the elementary particles that take part in strong interactions are merely composites of two or more particles whose interactions can be represented by a universal weak interaction. As for the pion, the fact that it interacted strongly with a nucleon was merely a physical consequence of the fact that it contained extreme relativistic nucleon-antinucleon pairs.[16]

IV

The principal atomic model, the so-called Bohr atom, was more or less complete thirty-five years ago. There is no comparable theoretical model for the nucleus, although there is no paucity in the number of proposed models. In a sense, all of the work in nuclear structure physics, whether it be in electrodynamics, nuclear energetics, or reaction mechanisms, is directed toward the task of furnishing the theoretical physicist with the data he needs to construct an appropriate model or else select one from those already proposed. Naturally, AFOSR-sponsored physicists are among those directly involved in constructing a satisfactory nuclear model.

Among them is David S. Saxon, a theoretical nuclear physicist at U.C.L.A. One of the models that Saxon gave thought to during 1961 is the so-called optical model. It had been shown that this model accounted quite successfully for the angular distributions and polarizations of protons scattered elastically by intermediate and heavy nuclei. But attempts to apply the model to light nuclei were not nearly so successful. What Saxon wanted to find out was whether this limitation was really inherent in the model. Actually, Saxon was of the opinion, even before he began to examine the model carefully, that it was not. He undertook his task by making a systematic analysis of a typical light

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[16]<u>AFRD Weekly Activity Report</u>, 5 August 1960.

nucleus. He chose carbon because of the availability of a great deal of accurate experiment^l data. After he was finished with his exhaustive analysis, which was carried over a wide range of parameters, Saxon and his co-workers, J. S. Nodvick, C. B. Duke, and M. A. Melkanoff, found that the optical model provided excellent fits for the experimental data on the elastic scattering of protons by carbon at intermediate energies.[17]

The most successful nuclear model proposed so far, however, and one which, with additional refinements, seems most likely to be adopted in the end is the so-called shell model. Actually, in constructing this model, nuclear physicists took a cue from atomic physicists and pictured the protons and neutrons in the nucleus as occupying shells and subshells, just as electrons do in an atom.[18] A recent test of the shell model was performed by two AFOSR grantees, H. L. Anderson and H. E. Tyren, at the University of Chicago. While their test was an experimental one rather than theoretical, it falls quite logically into this discussion.

Tyren conceived of the experiment at his home base at the University of Uppsala, Sweden. His approach was to observe the two emerging protons from (p, 2p) reactions in light nuclei and look for discrete energy groupings among these emerging protons. He found, however, that the 185 Mev protons he was using did not enable him to attain sufficient precision. Strange as it might seem, even though Tyren was working in the area of nuclear structure, he felt the need for energies normally employed by high energy physicists. He therefore sought out Anderson at Chicago, where a 450 Mev proton synchrocyclotron was available, and proposed a collaboration. Anderson was only too willing, as was AFOSR

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[17] Ltr., David S. Daxon to Charles K. Reed, 4 May 1959, MSS; J. S. Nodvich, C. B. Duke, and M. A. Melkanoff, "Optical Model Analysis of Elastic Proton Scattering on Carbon at Intermediate Energies" (AFOSR 1344), passim.

[18] Gamow, Biography of Physics, pp. 299-301.

to shift its funds from Uppsala to Chicago. The experiment was further improved by constructing two precision double-focusing magnetic spectrometers to detect and analyze the two emerging protons. After bombarding a variety of light nuclei, including lithium, beryllium, boron, carbon, and oxygen, Anderson and Tyren reached the conclusion that the shell model provides a most accurate description for these nuclei.[19] It could indeed be the model physicists are looking for.

A nuclear model somewhat more recent than either the optical or the shell model is the quasi-alpha model, which was given its form, under AFOSR sponsorship, in 1956, by R. W. King and his co-workers at the Purdue Research Foundation. One of the features of the model is that it accounts simultaneously for the quasi-alpha character of complex nuclei and for strong spin-orbit coupling; yet, the model imposes symmetries consistent with nucleon-nucleon interactions. Using this model, the workers at Purdue constructed wave functions for several states in the d5/2 and f7/2 region. Particularly encouraging were the results of detailed calculations concerning beta decay, which compared very favorably with direct experimental data. Moreover, once again concerning the beta decay formalism, the new model provides estimates of the ratio of Fermi to Gamow-Teller matrix elements. The new model also offered the opportunity of repeating a large number of the investigations of low energy phenomena that were based on other models.[20]

V

Studies in gravitational theory have not loomed too prominently in the AFOSR nuclear physics program, as indeed they have not in the world of physics in general. At present only about a half dozen AFOSR grantees are giving any sort of attention to gravity and related prob-

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[19] Heer, personal interview with author, 6 March 1963; Ray Heer, Jr., "Presentation to OAR," 4-5 March 1963.

[20] R & D Project Card (DD Form 613-1) Project No. 9750, 1 April 1958, p. 27.

lems; and of these only two are doing so on a full-time basis. Their contributions, however, have been both interesting and varied. Consider, for example, some of the work done during 1962. Professor Stanley Deser of Brandeis University contributed an essay entitled "Cannonical Analysis of General Relativity," to a book on relativity. At Syracuse University, Dr. A. Peres, a member of Professor Peter G. Bergmann's theoretical group, was able to represent the general theory of gravitational radiation recoil in complete analogy with electromagnetic radiation recoil. The significance of Peres' work is that it permits a physicist who is versed in electromagnetic theory to understand the problem without benefit of any previous knowledge of general relativity. At Maryland University, Professor Joseph Weber and his associates have been working in the area of detection and generation of gravity waves since 1960. Unfortunately, after setting up their equipment in the laboratory, noise emanating from heavily trafficked streets off the campus played havoc with their measurements, and Weber and his cohorts are now busily excavating a cave, where they eventually plan to move their experiment. Meanwhile, Weber co-authored a paper on "Instantaneous Interaction and the Transverse Modes of the Gravitational Field."[21]

Perhaps the most interesting group of studies on gravitation conducted by an AFOSR investigator have been those of Leonard Schiff. Two in particular stand out: one on the possibility that antimatter has a gravitational mass opposite to that of ordinary matter, the other on a new experimental test of general relativity.

Lying at the heart of Einstein's general theory of relativity is the principle that gravity and inertia are equivalent. Galileo was the first to demonstrate that gravitational and inertial forces are proportional, and Newton recorded this proportionality in his third law of motion, which states that to every action (e.g., gravitational force) there is always an equal and opposed reaction (inertial force). Ein-

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[21]RDT & E Project Documentation (DD Form 613c) Project No. 9750 1 February 1963, pp. 5-7, 13, 25, 27.

stein went even further than either Galileo or Newton by maintaining that gravity and inertia are not merely proportional, but actually the same thing.[22]

Since 1915, the year Einstein first proposed his general theory, there has been no absolute experimental test of the principle of equivalence. Between 1890 and 1922, a Hungarian physicist, Baron Roland von Eötvös, conducted a surprisingly precise series of experiments, which, while failing to give absolute confirmation of the equivalence of gravity to inertia, lent the principle a great deal of credence. An even more accurate series of experiments were conducted during the late 1950s at Princeton University by Professor R. H. Dicke and his associates; this study pointed even more forcefully to the equivalence of gravity and inertia. There remained, however, one complication. This was the notion, expressed by some cosmic-ray physicists, that antimatter had a gravitational mass opposite in sign to ordinary matter. Since ordinary particles and their corresponding antiparticles were known to have the same inertial mass, this notion, if proven true, would have dealt a death blow to the general theory. Into this situation came Leonard Schiff.[23]

In attacking the problem, Schiff relied heavily upon Eötvös' experiments. Eötvös' data were sufficiently accurate for Schiff to conclude that "the main factors that contribute to the inertial mass of a body also contribute equally or nearly equally to its gravitational mass." Working from this solid base constructed by Eötvös, Schiff pro-

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[22]Martin Gardner, Relativity for the Million (New York: MacMillan Company, 1962), 69-75; George Gamow, "Gravity," Scientific American, Vol. 204 (March 1961), 94.

[23]R. H. Dicke, "New Research on Old Gravitation," Science, Vol. 129 (6 March 1959), 621-24; R. H. Dicke, "The Eötvös Experiment," Scientific American, Vol. 205 (December 1961), 84-94; Gamow, "Gravity," p. 106; L. I. Schiff, "Gravitational Properties of Antimatter" (AFOSR TN 58-1062), passim.

ceeded to make a few preliminary calculations of his own and compared the results with Eötvös' data.[24]

Schiff selected the positron as a sort of guinea pig and considered three cases which corresponded to the various assumptions concerning the effect of gravity on a positron: (1) an electron and a positron behave in the same way, (2) the total gravitational rest mass of a positron is equal to and opposite in sign from that of an electron, (3) the gravitational rest mass of a positron is equal and opposite to that of an electron, but its kinetic energy is acted on normally by a gravitational field.[25]

After working out the solutions to these three assumptions mathematically, Schiff came to the conclusion that "positrons are very likely to have normal gravitational properties."[26] It should be noted, however, that Schiff did not feel he had constructed a completely iron-clad case, as the inclusion of the phrase, "are very likely to," makes evident. So he turned from mathematics to an interesting thought experiment.

If matter and antimatter have opposite signs of gravitational mass, he noted, they would be separated from each other on a cosmological scale. This separation would be contrary to everything experimentalists have observed up to now, matter and antimatter having hitherto shown a strong attraction for each other. But this, nevertheless, would be the consequence if matter and antimatter possessed opposite signs of gravitational mass. There would be other consequences. Imagine, Schiff asked, a laboratory in a region of the universe which is made up entirely of antimatter. Now, then, all the positrons, antiprotons, and antineutrons that make up this "anti-laboratory" would possess a gravitational mass opposite in sign to their corresponding normal particles. But would everything in this "anti-laboratory" have negative gravitational mass?

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[24]Schiff, "Gravitational Properties," p. 2.

[25]*Ibid.*, p. 7.

[26]*Ibid.*, pp. 6-14.

Decidedly not. Both the electromagnetic and nucleon binding energies, which would hold the anti-laboratory together, would have a positive mass. This is because photons, which are responsible for electromagnetic energy, and pions, which are responsible for nuclear binding energy, are their own antiparticles. It follows, therefore, that they would possess positive gravitational mass, just as they do on earth. Because of this it also follows that the results of any experiment performed in the anti-laboratory would differ from the results of the same experiment performed in a laboratory on earth or any other normal region of the universe. This would destroy the notion of symmetry in the universe.[27] With this, Schiff saw no need to develop his arguments any further. Instead, he proceeded to tackle general relativity from a new angle.

An experiment which could prove conclusively the equivalence of mass to inertia would only serve as partial confirmation of the general theory of relativity. It would only provide a test for the equivalence principle, which is really not broad enough to be accepted as proof for the whole of the general theory. The trouble with the experiments that Eötvös and Dicke performed, as far as confirming the general theory goes, is that they did not make use of particles that are in "free fall." Actually, the precession of the perihelion of the orbit of the planet Mercury provides to this day the only firm experimental test of the general theory. Thus, Leonard Schiff attempted to provide experimental physicists with a theoretical basis for a new experimental test of general relativity.[28]

After making the theoretical calculations, Schiff suggested an experiment which entailed observing the precession of the axis of a torque-free gyroscope. This gyroscope could either be fixed in an

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[27] Ibid., pp. 14-17.

[28] Dicke, "New Research," p. 621; L. I. Schiff, "Motion of a Gyroscope According to Einstein's Theory of Gravitation" (AFOSR TN 60-449), pp. 1-5; AFOSR Weekly Activity Report, 16 October 1959.

ordinary laboratory, and thus be rotating along with the earth, or it could be sent up in a satellite. Of the two alternatives, Schiff preferred the second, despite the difficulties it would present in monitoring. The earth-bound gyroscope, because the magnitude of its precession would be comparable to the precession of Mercury, would produce very small effects. On the other hand, the gyroscope's precession would be enhanced if the gyroscope were up in a satellite. The magnitude of the precession, both for the earth-bound and for the satellite gyroscope was calculated at about 6×10^{-9} radians for every orbital revolution. But for the earth-bound gyroscope, such a revolution would require 24 hours; for a gyroscope in space it would take about an hour and a half. Moreover, since the satellite gyroscope would not have to be supported against gravity, most of the experimental difficulties inherent in this kind of experiment would be greatly reduced. But, as mentioned previously, the task of monitoring such a gyroscope, even if the satellite attained only moderate altitudes, would be greater than for a gyroscope in a laboratory. In any event, NASA is today sufficiently interested in Schiff's suggestion that it is giving serious thought to performing the satellite experiment.[29] AFOSR, too, is maintaining an interest in the suggestion. Professor W. M. Fairbanks, a colleague of Schiff's at Stanford, is studying the satellite idea in more detail under an AFOSR grant.[30]

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[29] Schiff, "Motion of a Gyroscope," passim; AFOSR Weekly Activity Report, 16 October 1959; AFRD Weekly Activity Report, 9 December 1960; Heer, personal interview with author, 6 March 1963.

[30] RDT & E Project Documentation (DD Form 613c) Project No. 9750, 8 January 1963, p. 25.

Chapter VII

TRITIUM AND RADIOSTRONTIUM

Not all the studies the Nuclear Physics Division has sponsored fall neatly into a prescribed niche among its areas of concentration. Some of the work sponsored was not even basic research as such. For example, the Division sponsored symposiums and conferences. It also financed the development and construction of instrumentation. In addition, the Division sponsored a few research projects which, for all practical purposes, were outside the realm of nuclear physics. This chapter will deal with two such examples.

II

Not too long after scientists became aware of radioactivity they realized that man was being constantly exposed to natural radioactive matter, if only in the most minute quantities. Moreover, it was also determined that man himself was slightly radioactive. For example, his system contains, among other radioactive materials, quantities of carbon-14. This particular isotope is produced in the terrestrial atmosphere by cosmic ray bombardment, incorporated into atmospheric carbon dioxide, and later absorbed by plants. Man eats the plants and in turn incorporates the isotope in his system. This particular fact led to a rather interesting line of inquiry. One characteristic held in common by all radioactive elements is that they decay at a readily ascertainable rate. Some may decay completely in a fraction of a second, others may take billions of years; but in each case the rate is constant. Thus, the ages and the lifetimes of radioactive elements can be determined with extreme accuracy. Now, then, since living things contain radioactive matter, it occurred to some that the ages of plants and animals long since dead could be ascertained by determining the extent of the decay of the radioactive content in their systems. Willard F. Libby, an American chemist, began the studies in this direction when he initiated carbon-14 dating soon after World War II.[1]

Meanwhile, with the increasing pace of thermonuclear explosions, resulting as it did in the wholesale dispersion of radioactive debris, there followed a corresponding set of studies, conducted primarily by the Atomic Energy Commission, to determine the extent of man-produced radioactive contamination. Thus, man's interest in his radioactive environment took on two rather distinct aspects: the assay of natural radioactivity and the assay of man-produced radioactivity. The work of Professor Libby on the natural tritium content of the earth's waters and that of Dr. C. W. Thornthwaite on the climatic and hydrologic factors affecting the redistribution of strontium-90 in the soil are two typical examples dealing with each of these aspects.

III

Of these two efforts, Willard F. Libby's study on the distribution of tritium was more far-reaching, and it certainly created the greater interest among the scientific community. The study grew directly out of Libby's efforts to perfect ultrasensitive radiation detectors -- an outgrowth, in turn, of his monumental studies on the occurence of radiocarbon in living matter, which ultimately landed Libby among the celebrated ranks of Nobel laureates.[2]

The Air Force's connection with the tritium study dates back as far as June 1949, when the now defunct Office of Air Research, at Wright-Patterson Air Force Base, decided to support Libby's work on sensitive radiation detection techniques conducted at the Institute of Nuclear Studies (now the Enrico Fermi Institute). When, in Septem-

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[1] Asimov, Intelligent Man's Guide, I, 241; Gamow, The Atom, 85-89.

[2] Ltr., Willard F. Libby to Chief, Office of Air Research, 26 September 1950, MSS; Science, Vol. 120 (31 December 1954), 1087; Vol. 132 (11 November 1960), 1384; Scientific American, Vol. 203 (December 1960), 74.

ber 1950, Libby and his co-workers were ready to employ their newly devised techniques on tritium detection, the Office of Air Research decided to give its support to this phase of Libby's work, too. Before long, however, the Air Force's research and development structure underwent an extensive reorganization, which resulted in the dismembering of the Office of Air Research. Consequently, in July 1952, Libby's tritium study was transferred to the Physics Division of the newly created [Air Force] Office of Scientific Research; and there it remained, continuing to receive Air Force support until September 1958, when the fundamental aspects of the study finally ran their course.[3]

In 1954, Libby was appointed to the Atomic Energy Commission, and Professor E. A. Martell succeeded him as principal investigator in October of that year. Martell in turn left the Institute of Nuclear Physics in August 1956, whereupon Professor Anthony Turkevich took over the project. But Libby continued to have an abiding interest in the study during his service with the AEC and was often consulted on many aspects of the work.[4]

Tritium is one of three known isotopes of hydrogen -- the others being protium and deuterium. The essential feature that distinguishes these isotopes from each other is found in their respective nuclei. Protium (ordinary hydrogen) contains a single proton in its nucleus; deuterium, one proton and one neutron; tritium, one proton and two neutrons. Deuterium was first isolated in 1932 by the American chemist Harold Urey. Working on the theory that a certain amount of deuterium, too small and too diffused to be revealed by any detection device, existed in water, Urey slowly evaporated a sample of liquid hydrogen and found

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[3]Ltr., Robert M. Linsmayer to W. F. Libby, 11 March 1949, MSS; ltr., Libby to Col. Oliver Haywood, 13 August 1952, MSS; ltr., Capt. Seymour Shwiller, Physics Division, OSR, to Libby, 25 September 1952, MSS.

[4]Ltrs., G. G. Bruder, ARDC Procurement Office, to W. B. Harrell, Vice President, University of Chicago, 7 October 1954, MSS; ltr., W. B. Harrell to W. J. Otting, 6 April 1956, MSS.

his hunch to be correct: the remaining hydrogen was heavily concentrated with deuterium. Then, in 1934, two Englishmen, M. L. E. Oliphant and Paul Harteck, by bombarding deuterium with its own nucleus, produced tritium artificislly.[5]

Unlike deuterium, tritium proved to be radioactive, emitting beta rays. This isotope is, however, one of the weakest emitters of such rays known to man: its electrons are capable of penetrating only the thinnest of matter. It is fairly certain -- at least Libby came to this conclusion in 1946 -- that tritium is one of nature's by-products, being produced in the earth's atmosphere by bombarding cosmic rays. It is then brought down to earth in very minute quantities by precipitation. Having a half-life of 12.5 years, the isotope remains on earth for approximately 18 years, before it decays into helium-3, a nonradioactive substance. The earth's supply of the isotope, of course, is being constantly replenished by rainfall.[6]

One can see that, with its half-life known, the isotope would open up many possibilities to man once its natural rate of occurence on earth had been established. This is what Libby set out to do.

For the first year or so of the contract, Libby and his associates devoted most of their time to devising an accurate and tractable detection technique. Their problem was in many ways similar to that faced by Urey while the latter was in the process of discovering deuterium: tritium, like deuterium, occurred in ordinary water in undetectable quantities. Thus, the protium-tritium ratio in a water sample would

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[5]Asimov, Intelligent Man's Guide, I, 241-42.

[6]W. F. Libby, "Sensitive Radiation Detection Techniques for Tritium, Natural Radioactivities, and Gamma Radiation," 1 December 1951, final report on AF contract 33(038)-18013, p. 7; W. F. Libby, "Atmospheric Helium Three and Radiocarbon from Cosmic Radiation," Physical Review, Vol. 69 (1 & 15 June 1946), 671-72; W. F. Libby, "Natural Tritium Assay, Routine Method for Absolute Assay of Beta Radioactivity, and the Reactions of Negative Pi Mesons with Elementary Bromine," supplemental report to Air Force Contract No. 33(038)-18013, dated 1 December 1952, pp. 10, 12.

have to be radically reduced -- perhaps 4000 fold -- before tritium could appear in sufficient concentration to be detected. At the suggestion of Urey, who was at the University of Chicago at the time, Libby decided to concentrate tritium in water samples by electrolysis. A separating plant, a drafty shack in a moat adjoining Libby's basement laboratory, was constructed for the purpose and modelled after a similar plant originally designed by Urey in 1934.[7]

The plant consisted of a gas-fired still and 38 water-cooled steel cells of a capacity of three liters each. The water sample to be assayed for tritium was distilled in the still, mixed with sodium hydroxide, and then electrolyzed in the plant. After about 72 hours of electrolysis, the initial sample was reduced to about one-sixth of its original volume. Then the process began over again: distillation, mixing, electrolysis. After three of these runs, the sample was reduced to a final volume of one cubic centimeter or less, containing a heavy tritium concentration.[8]

While the water underwent electrolysis, it gave off hydrogen and oxygen gases. The plant was designed so that these gases were collected by iron header pipes and conducted out of doors. During the process, however, the gases would mix in the small confines of the pipes, and the possibility of an explosion, despite precautions, was always present. Two explosions did occur -- luckily, at times when the plant was unattended -- and gaskets and connections blew sky high. After this, the head[illegible] nifold was disconnected, and the gases were allowed to escape right into the drafty shed, where an explosion was less likely to occur.[9]

After a given sample had undergone electrolysis, the actual measurement for tritium began. The enriched water sample was distilled

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[7]Ltr., W. F. Libby to Chief, Office of Air Research, 26 September 1950, MSS; Libby, "Sensitive Radiation Detection Techniques," passim.

[8]Libby, "Natural Tritium Assay," pp. 2-3.

[9]Ibid., p. 4.

at room temperature with a mixture of freshly dehydrated calcium oxide and finely divided zinc dust. The mixture was then gently heated in a pyrex tube to about 500°C, giving off zinc oxide and hydrogen gas. The hydrogen gas was then passed through a vacuum line into a specially constructed Geiger counter, where the measurements were taken.[10]

Libby's ultimate objective was to measure the tritium content of rains and surface waters around the world. This required not only an extensive rain sampling program, but also the utilization of rivers, lakes, and oceans. Water could be ordered from most places around the globe, but when it came to acquiring samples from such remote areas as Wake Island, Libby obviously needed some help. This is where the Air Force, with its far-flung installations, became a useful partner. At the request of AFOSR, the Tropical Pacific Project at Wheeler Air Force Base, Wahiawa, Hawaii, collected rain and ocean water at such places as Honolulu, Oahu, Johnston Island, Wake, and the Marshalls and shipped it to Chicago via the Air Force Cambridge Research Center.[11]

After the first extensive world-wide sampling, it was evident that tritium was not being equally distributed around the globe. Ocean rain, for example, had an average content of one tritium atom to 10^{18} (quintillion) hydrogen atoms. Europe's western coast ran about 2.5 tritium atoms to 10^{18} hydrogen atoms, while the great Mississippi Valley yielded an average of six tritium atoms. From this, Libby could easily conclude that continental rains were richer in tritium than either ocean rains or coastal rains. Libby attributed this to two factors. First, it appeared that the moisture which traveled over great land masses suffered

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[10] Ibid., pp. 5-6

[11] Memo, Harry S. Baer, Jr., Research Information Branch, OSR, "Info for Weekly Activity Report," 12 February 1953, MSS; ltrs., Lt. Col. John W. Kodis, ARDC, to Commanding General, CRC, 2 April 1953; Libby to Otting, 21 May 1953; H. E. Landsberg, GRD, to Commander, ARDC, 16 July 1953; Milton Greenberg, GRD, to Commander, ARDC, 28 July 1953; Otting to Libby, 5 January 1954; Landsberg to Commander, ARDC, 5 January 1954; Libby to Otting, 14 January 1954, MSS.

longer exposure to cosmic rays than did the moisture over the oceans. Naturally, this cut down the production of tritium in that portion of the atmosphere that stretched over the Atlantic or Pacific. Second, there appeared to be less moisture in the air surrounding a great land mass than in that surrounding a great sea. Thus, for continental areas, this worked to raise the ratio between tritium and hydrogen atoms, while doing the reverse for ocean areas.[12]

Tritium concentrations not only fluctuated from one location to another, but they also underwent seemingly erratic fluctuations in a given location. In the Chicago area, for example, where Libby and his associates conducted their most exhaustive sampling merely by turning on the tap, dipping into Lake Michigan, or catching rain drops on a nearby roof, tritium concentrations during one six-month period ranged from a low of 3.60 tritium atoms per 10^{18} hydrogen atoms to a high of 34.5. Much of this fluctuation was due to variations in air masses, the origin of the precipitating clouds, and other meteorological factors. For example, the average water molecule which falls as rain has been out of the sea for approximately three months. Depending upon the extent of precipitation, however, some molecules will remain in the atmosphere for longer periods, some for shorter periods -- all of which means that some rain molecules will be exposed to cosmic rays for longer periods than others. It followed, therefore, that rainfall coming after an extended period of draught would be more radioactive than rainfall following on the heels of a long wet spell.[13] As for the total world assay of tritium, Libby calculated that about 200 grams of the isotope was present in the atmosphere at any given time. All told, both that in the atmosphere and that in water amounted to no more than 15 kilograms -- or approximately 33 pounds.[14]

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[12]Haro von Buttlar and W. F. Libby, "Natural Distribution of Cosmic Ray Produced Tritium" (AFOSR TN 54-338), pp. 1-3, 5, 8.

[13]Ibid., pp. 12-13.

[14]Libby, "Natural Tritium Assay," p. iii.

In the spring of 1954, immediately following the thermonuclear tests conducted in connection with Operation Castle, Libby's tritium assays shot up. Actually, Libby had expected as much and was prepared to make the most out of the test series. He already had excellent data on the world's concentration of natural tritium, along with specific information about hundreds of given areas. This permitted him to measure how much tritium was distributed around the world as a result of Operation Castle. On 19 March, four days after the first nuclear explosion, Chicago rain water yielded 385 tritium atoms per 10^{18} hydrogen atoms. The last Chicago rain previous to the test series, collected on 20 February, yielded only 4.2 tritium atoms. Before the spring was out, it was clear to Libby that Operation Castle was producing more tritium than the cosmic rays themselves.[15]

Libby found, however, that this great quantity of man-produced tritium failed to cross the equator -- all of it falling in the Northern Hemisphere. This was due to two factors: the points at which the bombs were detonated and the relatively short time that the bomb-produced tritium remained in the atmosphere. Libby estimated that this tritium remained airborne for about forty days -- not long enough to be carried across the equator. All in all, the Castle tests deposited about 200 x 10^7 tritium atoms on each square centimeter of the Northern Hemisphere. A series of Soviet tests, in late 1955, followed by another United States series, Operation Redwing, in 1956, bore out most of the conclusions drawn from Operation Castle.[16]

From the beginning of the research effort, Libby was fully cognizant of the potential usefulness of natural tritium. Since tritium was

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[15] Buttlar and Libby, "Cosmic Ray Produced Tritium," pp. 3, 20-23; trip report, W. J. Otting, 9 March 1955, MSS; Friedrich Begemann and W. F. Libby," . . . World-Wide Water Circulation Patterns from Cosmic Ray and Bomb Tritium" (AFOSR TN 56-561), pp. 1-2.

[16] Begemann and Libby, "Cosmic Ray and Bomb Tritium," pp. 2, 4, 10-11; Friedrich Begemann, "Tritium Assays of Natural Waters Measured in 1956-1957," pp. 1-2.

constantly decaying at a known rate and since the isotope could only be produced in the atmosphere, Libby's tritium assays provided the means for dating a variety of products -- bottled or long standing water being only the most obvious. To determine whether tritium content correlated with the expected exponential decay law, Libby purchased a variety of vintage wines from Spain and Southern France. The wine checked out perfectly. The same thing could be done with agricultural products -- measuring the time elapsed since their harvest.

Another potential use envisioned by Libby was the identification of the ultimate source of water supply. A tritium assay would reveal, for example, whether a well's water supply was dependent upon rainfall, and, if so, subject to seasonal fluctuations. Other possibilities were open, especially in the field of meteorology. Vertical mixing in air masses could be tested, as well as the source of moisture in these masses. Finally, since tritium decayed into helium-3, the minimum rate of helium-3 production on earth could be determined.[17]

IV

An exploding nuclear weapon can produce as many as 200 different kinds of radioactive isotopes. Not all of these substances are potential human hazards, but those that are could conceivably inflict more human casualties than the blast of the bomb itself. One such substance is the much talked about strontium-90.[18]

Existing on earth in no detectable degree prior to man-induced nuclear explosions, strontium-90 has been injected into the atmosphere by detonated nuclear test devices, dispersed throughout the globe by lati-

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[17] Memo to RDTRR, Arthur E. Roden, Physics Division, OSR, "Dr. W. F. Libby -- University of Chicago," 16 September 1954, MSS; trip report, W. J. Otting, 9 March 1955, MSS; W. F. Libby, "The Potential Usefulness of Natural Tritium," [copy of article submitted for publication], MSS; Gamow, "The Atom," 87-89.

[18] Arne Engstrom, et al., Bone and Radiostrontium (New York: John Wiley & Sons, 1957), 11; Asimov, Intelligent Man's Guide, I, 375-76.

tudinal and longitudinal winds, and brought to earth chiefly by rainfall. While previously unknown to man, the substance is really no more than a radioactive isotope of strontium, a long-time member of the periodic table first discovered by Martin Klaproth as far back as 1793 and isolated by Humphry Davy in 1808. It belongs to the alkaline earth metals, along with such elements as calcium and barium. These substances, known as "bone-seekers" to the biochemists, lodge themselves in the tissues of bones when taken in by the human body. It is this fact -- its chemical similarity to calcium -- that makes strontium-90 dangerous to human life. Taken up by vegetation from the soil and transmitted to humans feeding on this vegetation, or on the milk of animals feeding on it, strontium-90 is metabolized by the body just as if it were harmless calcium and eventually stored in the bones. Since the minerals in the bones are replaced very slowly, as compared with substances in soft tissues, strontium-90, once taken in, will remain in the human organism for years -- time enough, if absorbed in large enough doses, to irradiate the red bone-marrow with beta rays and induce leukaemia and other cancer-like diseases.[19]

Short of all-out nuclear war, the chances of an individual absorbing a lethal dose of strontium-90 depend upon a great many things, the most obvious being the extent of nuclear testing in the atmosphere. Other factors can be crucial, too. Climatic conditions, soil composition, even diet, can play key roles in determining the fate of the population of a given geographic area. But the exact role of these factors had not begun to be fully explored until June 1958, when AFOSR contracted the Laboratory of Climatology at Centerton, New Jersey, to examine some of these factors and determine their effect upon radioactive contamination.[20]

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[19] Engstrom, et al., op. cit., passim; Asimov, Intelligent Man's Guide, I, 376-79; H. V. Brondsted, The Atomic Age and Our Biological Future (New York: Philosophical Library, 1957), 73; George L. Bush and Anthony A. Silvidi, The Atom (New York: Barnes & Noble, 1961), 136-37; C. W. Thornthwaite, J. R. Mather, J. K. Nakamura, "Movement of Radiostrontium in Soils," Science, Vol. 131 (8 April 1960), 1015.

[20] R & D Project Card (DD Form 613) Project No. 9774, 1 April 1959,

Dr. C. W. Thornthwaite, the director of the laboratory, and two associates, J. R. Mather and J. K. Nakamura, all professional climatologists, knew from studies conducted by the U. S. Department of Agriculture that radiostrontium had a definite behavioral pattern in the soil. The isotope, according to these studies, moved downward in the soil as a wave of decreasing amplitude. It was obvious, therefore, that a dangerous concentration of the substance in the vital top-soil columns of the earth would eventually diminish. Hence, the problem of Thornthwaite, Mather, and Nakamura was clear-cut. At what rate did the isotope leave the vital top-soil area? What were the factors affecting the downward movement of the substance?[21]

As Thornthwaite saw his task, it boiled down pretty much to a problem in hydrology. Water played a dual role in determining the concentration of radiostrontium in the soil: first, it brought it down to earth; second, it acted as a leaching agent in carrying the radioactive substance from the top-soil to deeper soil regions. Thornthwaite's approach was to employ a highly sophisticated bookkeeping method he had devised some years before and keep scrupulous records of radiostrontium's gradual descent into deeper and deeper soil regions.[22]

Reducing his work by perhaps more than half was the fact that he was not obliged to gather soil samples of his own. The Health and Safety

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p. 29; contract between the Drexel Institute of Technology and the U. S. Air Force Office of Scientific Research, "Climatic and Hydrolic Factors Affecting the Redistribution of Strontium-90," 6 January 1958, MSS.

[21] John R. Mather, "Annotated Bibliography on Precipitation Chemistry" (AFOSR TN 60-876), passim; John R. Mather, "The Role of the Water Balance in the Redistribution of Strontium in the Soil" (AFOSR TN 60-97), p. 2.

[22] J. R. Mather and J. K. Nakamura, "The Climatic and Hydrologic Factors Affecting the Redistribution of Sr^{90}" (AFOSR TR 60-101), passim; J. R. Mather, J. K. Nakamura, and C. W. Thornthwaite, "The Climatic and Hydrologic Factors Affecting the Redistribution of Sr^{90}" (AFOSR 1623, November 1961), passim; Mather, "Role of the Water Balance," p. 1.

Laboratory of the Atomic Energy Commission had been making nationwide field measurements of radiostrontium concentration over a period of five years, and Thornthwaite and his associates saw that they could very conveniently use this data.[23]

To supplement his bookkeeping technique, and as a means of projecting into the future, Thornthwaite constructed a mathematical model of the downward movement of strontium-90 in the soil. What he did essentially was to divide a given sample of soil into one-half inch zones. Then, for convenience, he set up cycles of movement: whenever one-tenth of the radiostrontium in a given zone had moved down to the next zone, a full cycle of movement had been completed. The mathematical model would function within this fixed framework, calculating the concentration of radiostrontium in any one-half inch zone after a given number of cycles.[24]

After three years of taking this kind of data from a variety of soils from a variety of geographic areas, Thornthwaite, Mather, and Nakamura were able to divide the United States into 15 regions, according to the ability of the top-soil of each region to rid itself of radiostrontium. Two factors had an overwhelming effect upon radiostrontium's downward movement: the volume of precipitation (or leaching solution) and the ability of the soil to exchange cations.[25]

Considering only soil-type and allowing all other factors to be equal, radiostrontium moves downward toward the water table at a faster rate in sand than in any other type of soil. Sandy loam, silt loam, and clay follow in that order in their ability to remove the radioactive substance. From sand to clay there is approximately a ten-fold increase in removal time.[26]

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[23] Mather, Nakamura, and Thornthwaite, op. cit., p. 7; ltr., C. W. Thornthwaite to Major C. K. Reed, 14 July 1958, MSS.

[24] C. W. Thornthwaite and Sally Thornthwaite, "Equation and Table for Determination of the Wave of Leaching in the Soil" (AFOSR TN 60-875), passim; AFOSR Weekly Activity Report, 20 March 1959.

[25] Mather, Nakamura, and Thornthwaite, op. cit., pp. 64-67.

[26] Ibid.

Radiostrontium leaves the top-soil of the Eastern Seaboard, the South, and the Pacific Northwest -- areas with high precipitation -- faster than any other sections of the United States -- 99 percent being removed from the top six inches in three years. Along the plains and some sections of the Southwest, removal time ranges from five to ten years. In the great arid regions of New Mexico, Arizona, Nevada, Utah, and southeastern California, removal time runs as high as 24 years. Hence, in the great population areas, strontium-90 made a fairly rapid downward descent in terms of removal from vital regions of agricultural significance. However, the high removal time for the arid regions is somewhat misleading since Thornthwaite did not take into account the natural radioactive decay of the isotope. With a half-life of 28 years, the substance would leave the top-soil of arid regions considerably faster than Thornthwaite's figure would indicate. Furthermore, it should be remembered that the initial concentration of radiostrontium in arid areas would be less than in regions with high rainfall.[27]

In conclusion, Thornthwaite had definitely established that the movement of radiostrontium is influenced by the volume of water in the soil. Thus, this movement is subject to the control of man. The speed of the isotope's descent could be increased by the application of supplemental irrigation. "Even moderate amount of irrigation," Thornthwaite and his associates concluded, "would speed the process of strontium leaching by many years . . . ."[28]

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[27] Ibid., 68

[28] Ibid., 69

GLOSSARY

AEC	Atomic Energy Commission
AF	Air Force
AFB	Air Force Base
AFOSR	Air Force Office of Scientific Research
AFRD	Air Force Research Division
ARDC	Air Research and Development Command
Atch.	Attachment
BASJE	Bolivian Air Shower Joint Experiment
Bev	Billion electron volts
C.E.R.N.	European Committee for Nuclear Research
DD	Defense Department
DOD	Department of Defense
GAC	General Advisory Committee, AEC
GRD	Geophysics Research Directorate
Ibid.	in the same place
Incl.	Inclosure; including
Kev	Thousand electron volts
Ltr.	Letter
Mev	Million electron volts
MSS	Manuscript collection
NAS	National Academy of Sciences
NASA	National Aeronautics and Space Administration
n.d.	no date

NSF	National Science Foundation
OAR	Office of Aerospace Research
ONR	Office of Naval Research
Op. cit.	in the work cited
OSR	Office of Scientific Research
Passim	here and there
PSAC	President's Science Advisory Committee
R & D	Research and Development
RDT & E	Research Development Test and Evaluation
TN	Technical Note
TR	Technical Report

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