

American Association for the Advancement of Science

BOARD OF DIRECTORS

Paul M. Gross, *Retiring President, Chairman*
 Alan T. Waterman, *President*
 Laurence M. Gould, *President Elect*
 Henry Eyring
 H. Bentley Glass
 Don K. Price
 Mina Rees
 Walter Orr Roberts
 Alfred S. Romer
 William W. Rubey
 Paul E. Klopsteg
Treasurer
 Dael Wolfe
Executive Officer

VICE PRESIDENTS AND SECRETARIES OF SECTIONS

MATHEMATICS (A)
 Magnus R. Hestenes
 Wallace Givens
 PHYSICS (B)
 Elmer Hutchisson
 Stanley S. Ballard
 CHEMISTRY (C)
 Milton Orchin
 S. L. Meisel
 ASTRONOMY (D)
 Paul Herget
 Frank Bradshaw Wood
 GEOLOGY AND GEOGRAPHY (E)
 John C. Reed
 Richard H. Mahard
 ZOOLOGICAL SCIENCES (F)
 Dietrich Bodenstein
 David W. Bishop
 BOTANICAL SCIENCES (G)
 Aaron J. Sharp
 Harriet B. Creighton
 ANTHROPOLOGY (H)
 David A. Baerreis
 Eleanor Leacock
 PSYCHOLOGY (I)
 Lloyd G. Humphreys
 Frank W. Finger
 SOCIAL AND ECONOMIC SCIENCES (K)
 Kingsley Davis
 Ithiel de Sola Pool
 HISTORY AND PHILOSOPHY OF SCIENCE (L)
 Adolph Grünbaum
 N. Russell Hanson
 ENGINEERING (M)
 Clarence E. Davies
 Leroy K. Wheelock
 MEDICAL SCIENCES (N)
 Francis D. Moore
 Oscar Touster
 DENTISTRY (Nd)
 Paul E. Boyle
 S. J. Kreshover
 PHARMACEUTICAL SCIENCES (Np)
 Don E. Francke
 Joseph P. Buckley
 AGRICULTURE (O)
 A. H. Moseman
 Howard B. Sprague
 INDUSTRIAL SCIENCE (P)
 Alfred T. Waidelich
 Allen T. Bonnell
 EDUCATION (Q)
 H. E. Wise
 Herbert A. Smith
 INFORMATION AND COMMUNICATION (T)
 Foster E. Mohrhardt
 Phyllis V. Parkins
 STATISTICS (U)
 Harold Hotelling
 Morris B. Ullman

PACIFIC DIVISION

John P. Tully
President
 Robert C. Miller
Secretary

SOUTHWESTERN AND ROCKY MOUNTAIN DIVISION

Anton H. Berkman
President
 Marlowe G. Anderson
Executive Secretary

ALASKA DIVISION

Allan H. Mick
President
 George Dahlgren
Executive Secretary

The American Association for the Advancement of Science was founded in 1848 and incorporated in 1874. Its objects are to further the work of scientists, to facilitate cooperation among them, to improve the effectiveness of science in the promotion of human welfare, and to increase public understanding and appreciation of the importance and promise of the methods of science in human progress.

Serendipity in Research

One of the popular misconceptions concerning research is the importance of serendipity. The public has come to think that the successful scientist is one who has "a gift for finding valuable or agreeable things not sought for." A few examples, such as Perkin's discovery of an artificial dye, have dramatic value and hence are overemphasized. Perkin's discovery, while important to the course of development of chemistry at the time, plays only a minor role in the structure of science today. Organic chemistry, one of man's greatest intellectual triumphs, was built as a cumulative result of answers to a series of closely directed questions. Occasionally a chance observation has led to unexpected enlightenment. In general, however, progress has come because experimenters were seeking it.

Consider advances in another field—nuclear physics during the 1930's. This was an area where, to the highest degree, a kind of serendipity entered in. The discoveries of the neutron, artificial radioactivity, and uranium fission were unexpected. Yet in each instance the experimenters involved were extraordinarily competent. They had posed clear-cut questions. Chadwick in 1932 was attempting to study the physics of interaction of alpha particles on beryllium when he noticed that a "hard" gamma ray accompanied the reaction. On exploring the matter further he found that he was dealing not with gamma rays but with neutrons. The Joliot-Curies were studying the reaction of aluminum with energetic alpha particles. They observed that when the source of particles was removed, the aluminum target continued to emit radiation.

The most unexpected and far-reaching discovery in nuclear physics was that of uranium fission, reported by Hahn and Strassmann in 1939. In this case the discovery was more a result of careful work than anything else. Earlier, Fermi and his group had irradiated uranium with neutrons, and they thought they had discovered transuranic elements. Hahn and Strassmann were following up this work and found what they believed might be radium, presumably arising from neutron-stimulated emission of alpha particles from uranium. A first step in the isolation of radium is coprecipitation of radium and a barium salt. Later the mixture is recrystallized and the two elements can be separated. But in the products from uranium the radioactivity precipitated with the barium could not later be separated from it. When this was confirmed, Hahn and Strassmann were forced to conclude that they had produced barium from uranium. In a sense the discovery involved luck, but only in part. The experimenters had posed an interesting, clear-cut question, "Is radium a product of irradiation of uranium?" They devised an appropriate set of experiments to answer the query. The result was certain to be important, whatever it was. If they had proved that radium was a product, the result would have been considered very important, though not so significant as what they actually found.

Other developments in nuclear physics, such as the discovery of carbon-14 and other radioactive tracers, were sought, as was the understanding of nuclear forces. Indeed, most of the structure of nuclear physics is a product of carefully planned research rather than a series of happy incidents. In general, the research worker gets no more from his experiments than he puts in by way of thought, preparation, performance, and analysis. Serendipity is a bonus to the perceptive, prepared scientist, not a substitute for hard work.—P.H.A.