Pais

PRO · BONO · HUMANI · GENERIS
The Rockefeller University 1901

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE · NEW YORK, NEW YORK 10021

11/23/84

Dear Harry

You are kind and thoughtful.
Thank you.

As ever
Bram

THE INSTITUTE FOR ADVANCED STUDY

HARRY WOOLF
Director

November 15, 1984

Professor Abraham Pais
Rockefeller University
1230 York Ave
New York, NY 10021

Dear Bram:

Out of one of my multifarious duties, this particular find occurred. The copies are inadequate, but I thought you would like to see them before I send them on to John Stachel and the Einstein Papers collection. Please return them to me.

With warm regards, I am,

Cordially yours,

Harry

Harry Woolf

to PHL for Info. before filing

THE INSTITUTE FOR ADVANCED STUDY

HARRY WOOLF
Director

June 19, 1984

Professor Abraham Pais
Princeton University
Department of Physics: Joseph Henry Laboratories
Jadwin Hall
P.O. Box 708
Princeton, NJ 08540

Dear Bram:

I hope that you will understand that your request puts me in something of a quandary. It has been my practice, here, to observe the archival rule-of-thumb of keeping the files of living people unaccessible, unless authorized by all involved. I could answer questions for you if you were to direct them to me, but I cannot permit a free perusal of the Institute's files. However, I shall try to be helpful to you in any other way that I can.

With much affection, I am,

Cordially yours,

Harry

Harry Woolf

Princeton University DEPARTMENT OF PHYSICS: JOSEPH HENRY LABORATORIES
JADWIN HALL
POST OFFICE BOX 708
PRINCETON, NEW JERSEY 08540

June 15, '84

Dear Harry

I have a request. For the purpose of my present writing I would like to ask permission to have a look at the correspondence files between the Institute Director and:

Dyson, Lee, Pauli, Yang.

The purpose of all this is to get straight some dates etc. having to do with arrival and departure; with more precision than is found in the "community of scholars" book.

I shall not take any of this from the premises, if I am permitted to have a look; nor shall I Xerox anything.

Thanks for your consideration – and love

Bram

File

Notes on Bram Pais (from PHL · April 1984)

Born in Amsterday on May 19, 1918. He obtained his Doctor's degree at the University of Utrecht in 1941.

During the years of occupation he continued to work under conditions of great difficulty.

Entered USA Sept. 16, 1946, on S. S. Edam....to come to the Institute

In a letter of Nov. 12, 1946, to Mrs. Aydelotte, he apologized for "keeping your main guest thus much from the company of your family. My interview with Professor Bohr was of great personal importance to me and I am grateful to you for allowing me to stay 'til a somewhat uncommonly late hour."

A professor in the SNS from 1950-63

Has always felt concerned for the Institute, that it should "continue to realize its unique potential in a changing world."

A scientist and historian of science who has always dealt with difficult questions (see Oppenheimer's description attached here)

A man of whom Einstein once asked if he really believed the moon only existed if he looked at it.

The title of his most recent book "Subtle is the Lord"...The Science and the Life of Albert Einstein shows a man committed to life as well as science

What he said of Einstein, that he was "an artist in exposition...a master of nuance," might well be said of him.

Now Detlov W. Bronk Professor of Physics at The Rockefeller University

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

February 7, 1951

Dr. Abraham Pais

Dr. Pais was born in Amsterdam on May 19, 1918. He obtained his Doctor's Degree at the University of Utrecht in 1941. During the years of the occupation, he continued to work under conditions of great difficulty, and the year after the war he was an assistant at the Institute of Theoretical Physics in Copenhagen. In the fall of 1946, Dr. Pais came to the Institute for Advanced Study.

The record of Dr. Pais' work in the last decade is almost a history of the efforts to clarify our understanding of basic atomic theory and of the nature of elementary particles. Pais first proposed the compensation theories of elementary particles, and much of his work has been devoted to exploring the success and limitations of these theories, and indicating the radical character of the revisions which will be needed before they can successfully describe the sub-atomic world. Pais has made important contributions to nuclear theory and to electrodynamics. He is one of the few young theoretical physicists who within the last decade have enriched our understanding of physics.

Statement prepared by J. R. Oppenheimer
Enclosure: Bibliography of papers by Dr. Pais

PRO·BONO·HUMANI·GENERIS The Rockefeller University 1901

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE • NEW YORK, NEW YORK 10021-6399

July 6, 1983

Dear Harry

Thanks so much for your good letter of June 28. I accept with pleasure your invitation to deliver the Joseph Hasen - Albert Einstein lecture on 27 April 1984 at 4pm, at the Institute.

In accordance with your request, the lecture will deal with the career and accomplishments of Niels Bohr, a topic quite fitting in a lecture series associated with the name of Einstein. I shall certainly make a point of mentioning the intellectual struggles in which these two engaged. In fact, I shall endeavor to add Princeton (specifically Institute) touches.

Sara and I also accept with pleasure your invitation to join you and the Board for dinner that evening.

Hope to see a good deal of you before that!

Warm regards
Bram Pais

THE INSTITUTE FOR ADVANCED STUDY

HARRY WOOLF
Director

May 2, 1984

to Former Fac. file or HW pers?

Professor Abraham Pais
Rockefeller University
1230 York Avenue
New York, NY 10021

Dear Bram:

It was a wonderful occasion having you back within the boundaries of the Institute and, of course, it was all epitomized in the superb lecture you gave. You not only informed everyone beautifully about some of the characteristics of Niels Bohr's science, but you warmed everyone's heart with the sense of his endearing human qualities. For all that and more, we are most grateful and the honorarium enclosed is only the smallest expression of that gratitude.

With thanks and warm good wishes, I am,

Cordially yours,

Harry

Harry Woolf

bcc: PHL

File: Pais

NEWS FROM THE ROCKEFELLER UNIVERSITY

1230 YORK AVENUE • NEW YORK, NEW YORK 10021-3699

CONTACT

Fulvio Bardossi,
Eugene H. Kone, or
Judith N. Schwartz
Public Information
(212) 570-8967

Biography of Professor Abraham Pais

Abraham Pais, Detlev W. Bronk Professor of The Rockefeller University, is one of the world's leading theoretical physicists and one of the founding fathers of particle physics. He and his colleagues investigate fundamental particle processes at high energies, symmetries of strong and weak interactions, and quantum field theory.

Dr. Pais has played a leading role in several developments which aim to provide an explanation for the behavior of the interactions in particle physics. For example, he stated the principle of associated production which was found to govern the behavior of "strange" particles.

A number of his contributions deal with the symmetry principles of physics, such as the SU(6) theory developed around 1965. He is a co-discoverer of the idea of "particle-mixing," which is necessary for the understanding of the so-called neutral K-particle complex.

Dr. Pais was born in Amsterdam, The Netherlands, on May 19, 1918. He received his B.S. degree from the University of Amsterdam in 1938 and the Ph.D. degree from the University of Utrecht in 1942, where he remained, working with Professor Leon Rosenfeld until he was forced into hiding to escape the Gestapo, in 1943, during the Nazi occupation of Holland. After the liberation of Holland in 1945, he went to the Institute of Theoretical Physics in Copenhagen, Denmark as a research fellow with Niels Bohr.

Dr. Pais came to the United States in 1946 to the Institute for Advanced Study in Princeton, New Jersey, as a temporary member. He became a permanent member in 1948 and professor in 1950. He joined The Rockefeller University in 1963 and was named Detlev W. Bronk Professor in 1981. He was the 1977 James Arthur Balfour Professor at the Weizmann Institute in Israel and has also served as a visiting professor at CERN, the European atomic energy center.

Among his numerous publications is the book, Subtle is the Lord...The Science and the Life of Albert Einstein (Oxford University Press, 1982), for which he won a 1983 American Book Award for Science and the 1983 American Institute of Physics-United States Steel Foundation Science-Writing Award in Physics and Astronomy.

In 1979, he received The 11th Annual J. Robert Oppenheimer Memorial Prize, awarded by the Center for Theoretical Studies of the University of Miami.

He is a member of the National Academy of Sciences, the Royal Academy of Sciences, Holland, the American Academy of Arts and Sciences, and the American Philosophical Society, and a fellow of the American Physical Society.

A resident of New York City and Princeton, New Jersey, Dr. Pais is married to the former Sara Ector Via. He has a son, Joshua, by a previous marriage.

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September 1983

THE INSTITUTE FOR ADVANCED STUDY
Princeton, New Jersey 08540

HARRY WOOLF
Director

September 8, 1983

Professor A. Pais
188 Jefferson Road
Princeton, New Jersey 08540

Dear Professor Pais:

Enclosed is your new Institute charge card which will enable you to continue to use the Institute's dining hall, libraries, and tennis courts. Please sign the accompanying statement and return it to the Office of Personnel, at your convenience.

You can renew it each year by simply requesting a new card from Personnel.

Sincerely yours,

Aida L. La Brutte
Secretary to the Director

Enclosure

PAIS

November 15, 1982

Professor A. Pais
Rockefeller University
1230 York Avenue
New York, New York 10021

Dear Bram:

As you will see from the attached letter from Mr. McCutchen, I am forwarding the material to you should you wish to correspond with him.

I hope all goes well.

Cordially yours,

Harry Woolf

Enclosures

cc. A. Pais

November 15, 1982

Mr. C. W. McCutchen
5213 Acacia Avenue
Bethesda, Maryland 20814

Dear Mr. McCutchen:

Thank you very much for your most recent communication about the Einstein problem with which you have been so long concerned. Unless I hear from you otherwise, I am taking the liberty of forwarding your comments and the document to which they are attached to Professor Abraham Pais, whom I consider one of the best informed Einstein scholars of our time.

Sincerely yours,

Harry Woolf

erne

The Einstein Decade (1905–1915)

1974

CORNELIUS LANCZOS

Professor Emeritus, Dublin Institute for Advanced Studies

Dear Dr Woolf Nov 2, 1982

The marked passage would be funny if the deception it perpetrates had failed. The facts suggest that Einstein and de Haas cooked their result. If some one else had got a result within 2% of a theoretical prediction that was wrong by a factor 2 this thought would ~~have~~ crossed many minds. Einstein biographers find the subject so embarrassing that they do not mention it, an otherwise remarkable way to treat an experiment named for the biographee. Lanczos could not leave it out of his exhaustive list, and the result was the "brilliant proof" of a wrong theory.

A. B. Pippard told me that de Haas was an awful experimenter, which might explain the bogus agreement. Maybe he did experiments that gave more or less random results and stopped when he got one or more that agreed with the prediction. But this does not explain why the deception was maintained. It is more than the consequence

ACADEMIC PRESS New York and London
A Subsidiary of Harcourt Brace Jovanovich, Publishers

of sycophantic biographers. Einstein lived in Princeton for years along with J. Q. Stewart, who did the experiment right and deserved credit for it, but apparently he never set the record straight.

Sincerely yours

Charles McCutchen

Oxford Univ. Catalogue

Pais, A.

"Subtle Is the Lord..."

The Science and Life of Albert Einstein

ABRAHAM PAIS

Writing about Albert Einstein without discussing his science is like writing about Joyce without discussing his literature. Here at last is the first comprehensive scientific biography of Einstein.

Only an author who is himself an eminent scientist could have produced this book. Abraham Pais has had not only that advantage but also the privilege of knowing Einstein personally during the last nine years of his life. In addition, he has had substantial access to the Einstein archives in Princeton—some 50,000 pages of letters and scientific papers. His guide through that thicket was none other than Helen Dukas, Einstein's personal secretary and a friend of Pais for over thirty years. The result is an unparalleled view of how Einstein worked and thought.

The book fills in many of the gaps about what we know of Einstein's life. It deals with his interests in philosophy, his concern with the Jewish destiny, and his opinions on great figures from Newton to Freud. But the core of the book depicts the state of physics at the turn of the century and shows how Einstein transformed the field. Pais explains how General Relativity made Einstein a charismatic figure at age 40, but he discusses as well the relatively unproductive second half of Einstein's life. A unique analysis of Einstein's unsuccessful attempt at a Unified Field Theory is presented for the first time. The book also discusses Einstein's contributions to and critique of another basic area of modern physics—Quantum Theory. New material provided by the Nobel Foundation tells how Einstein was awarded the Nobel Prize.

Abraham Pais, an award-winning physicist, is Detlev W. Bronk Professor at the Rockefeller University.

$22.50t, 853907-X, 456 pp., 14 illus., 6 x 9, LC 82-2273

Early Comments on "SUBTLE IS THE LORD..."

"Pais has written a superb history of Einstein's work. It is authoritative, excitingly written, and, above all, suffused with an understanding of the processes and content of twentieth century research in physics. Anyone interested in the history of physics will have to read this book." —Steven Weinberg

"Here is Einstein, the man and above all his science, in a masterwork that ranges over the central themes and struggles of twentieth-century physics. A major and splendid biography, written with authority, insight, and wit." —Sam Treiman

"I regard this book as the transcendent revelation of the man and his scientific work." —Frederick Seitz

"A remarkable book... One learns about Einstein through his work and in the context of the development of physics." —George Uhlenbeck

we're almost there. Best - Bram

Who's Who in America

Author (with J. D. Swift) Elements of Linear Algebra, 1962; also articles. Home: 1515 3d St Sacramento CA 95814

PAIGE, SATCHEL (LEROY ROBERT), former baseball player; b. Mobile, Ala., July 7, 1906; m. La Homa Brown, Aug. 18, 1942; 3 children. Played in Negro leagues, 1925-47; played with maj. league teams: Cleve. Indians, 1948-49, St. Louis Browns, 1951-53, Kansas City Athletics, 1965; coach Atlanta Braves, 1968; now v.p., goodwill ambassador Springfield (Ill.) Redbirds, Am. Assn. Named to Am. League All-Star Team, 1952, 53; elected to Baseball Hall of Fame, 1971. Author: (with David Lipman) Maybe I'll Pitch Forever, 1962. Address: care Springfield Cardinals PO Box 638 Springfield IL 62705*

PAIK, KWANIK KENNETH, photojournalist; b. Seoul, Korea, Sept. 20, 1940; s. L. George and E. K. (Choi) P.; came to U.S., 1963, naturalized citizen; B.S. in Polit. Sci., Yonsei U., Seoul; postgrad. in journalism U. Mo., 1968; m. Joan K. Rewerts, Aug. 2, 1968; children—Randy, Angie. Photographer, Coffeyville (Kans.) Jour., 1968, Kansas City (Kans.) Kansan, 1969; photographer Kansas City (Mo.) Times, 1969-72, photo editor, 1972-78; dir. news illustrations Star and Times, Kansas City, 1978, Times-Union Jour., Fla. Pub. Co., Jacksonville, 1979—; mem. staff Mo. Photo Workshop; lectr. on photojournalism to colls.; judge photo competitions. Served with Korean Marine Corps. Recipient Contbn. to Humanity award Overseas Press Club, 1974, Best Photog. Reporting from Abroad award, 1975. Presbyterian. Home: 3244 Hidden Lake Dr E Jacksonville FL 32216 Office: PO Box 1949-F Jacksonville FL 32231. *I witness and record a slice of our time; while sometimes satisfying and sometimes frustrating, I'm fulfilling my curiosities of this world through my chosen profession. Someday, my children will know what I did and I hope they will learn to contribute more to their world than I.*

PAIK, NAM JUNE, artist; b. Seoul, Korea, 1932; grad. U. Tokyo, 1956. Mem. staff Studio Electronic Music, Radio Cologne, 1958-61; with Fluxus Group, N.Y.C., early 60s; artist in residence WGBH-TV, Boston, 1969, WNET-TV, N.Y.C., 1971; one person shows Mus. Modern Art, N.Y.C., 1972, Kitchen, N.Y.C., 1973; exhbns. in Mus. Wiesbaden, W. Ger., 1962, Inst. Contemporary Arts, London, 1968, Mus. Modern Art, N.Y.C., 1968, 74, Brandeis U., Waltham, Mass., 1969, Everson Mus. Art, Syracuse, 1973. Office: care Galerie Bonino Ltd 48 Great Jones St New York NY 10012*

PAIN, CHARLES LESLIE, lawyer; b. Austin, Tex., Apr. 26, 1913; s. William Francis and Ruby (Gates) P.; B.A., LL.B., U. Okla., 1935; m. Roberta Wilmoth, Mar. 27, 1942; children—Charles Laurence, William Francis, Glenn David. Admitted to Okla. bar, 1935, asst. atty. Southwestern Light & Power Co., 1935-40; practice in Anadarko, 1956—, partner firm Pain & Garland, 1956—. Exec. sec. to Congressman Toby Morris, 1951-53. Mem. Okla. Bd. Bar Examiners, 1969-78. Pres., Black Beaver council Boy Scouts Am., 1971—. Served with AUS, 1940-46; col. Res. (Ret.). Recipient Silver Beaver award Boy Scouts Am. Mem. Am., Okla., Caddo County bar assns., Am. Legion, Res. Officers Assn., Order of Coif, Phi Beta Kappa, Phi Eta Sigma, Phi Alpha Delta, Sigma Chi. Democrat. Baptist. Lion (past dist. gov.). Home: 808 W Colorado Ave Anadarko OK 73005 Office: 111 SW 2d St Anadarko OK 73005

PAINE, ANDREW J., JR., banker; b. Chgo., Oct. 18, 1937; s. Andrew J. and Louise (Kelly) P.; B.S., DePauw U., 1959; M.B.A., Ind. U., 1962; grad. Stonier Grad. Sch. Banking, 1969; m. Jane Medaris, June 1960; children—Linda, Stephanie, Andrew. Asst. cashier [illegible] Indpls., 1965-66, asst. v.p., 1966-68, v.p., 1968-72, sr. v.p., 1972-76, exec. v.p. corp. devel., 1976-77, exec. v.p. corp. banking, 1977-79, pres., 1979—; vice chmn. Ind. Nat. Corp., dir. Waldemar Industries, Indpls. Mem. exec. council Indpls. Mus. Art; past pres.'s council Jr. Achievement Central Ind.; chmn. policy bd. Cities in Schs., 1979; trustee Children's Mus., Nat. Jewish Hosp., DePauw U., bd. dirs. Community Service Council Met. Indpls.; bd. govs. United Way; co-chmn. major gifts div. Meth. Hosp. Recipient Key Man award Indpls. Jaycees, 1972; Alumni citation DePauw U., 1978. Mem. Am. Bankers Assn. (vice chmn. corp. planning exec. com. 1981), Young Presidents Orgn., Ind. U. Sch. Bus. Alumni Assn. (past pres.). Methodist. Clubs: Columbia, Meridian Hills Country. Home: 6534 Hythe Rd Indianapolis IN 46220 Office: One Indiana Sq 501 Indianapolis IN 46266

PAINE, CHARLES WILLIAM ELIOT, horticulturist; b. Boston, May 25, 1936; s. Richard Cushing and Ellen Peabody (Eliot) P.; B.S., Cornell U., 1958; M.S., U. Calif., Davis, 1960; m. Linda Williams Gibson, May 29, 1971; children—Emily Campbell, Phebe Eliot, Charles Eliot Timothy. Asst. horticulturist Holden Arboretum, 1964-70, dir. Garden Center of Greater Cleve., 1970—. Trustee, Coll. of the Atlantic; bd. trustees, v.p. Student Conservation Assn. Served to lt. USN, 1960-63. Mem. Am. Assn. Botanical Gardens and Arboreta, Am. Hort. Soc., Royal Hort. Soc. Clubs: Country, Chagrin Valley Hunt. Office: 11030 East Blvd Cleveland OH 44106

PAINE, FREDERICK V., chem. co. exec.; b. Stockdale, Pa., Dec. 7, 1925; s. Frederick and Anne (Bellows) P.; B.S. in Chemistry, Westminster Coll., 1948; m. Barbara A. Johnson, May 13, 1950; children—Frederick C., Timothy J., Scott S. With Nalco Chem. Co., Oak Brook, Ill., 1955—, div. mgr., 1966-68, v.p., 1968-69, pres. indsl. div., 1969-72, pres. petroleum and process chem. div., 1972-73, group [illegible]

Gideons Internat., Phi Beta Kappa, Wheaton Coll. Honor Soc., Pi Kappa Delta. Author: The Significant Name in Greek, Toward the Mark-Studies in Philippians; Studies in the Book of James; The Christian and the Movies; Beginning Greek, 1961. Mem. transl. com. New Internat. Version Bible. Contbr. Wesleyan Advocate, United Evangel. Action. Address: 7 Circle Dr Houghton NY 14744. *In filling out some kind of personnel form during my freshman year in Wheaton (Illinois) College I encountered the item "What is your life objective?" Not yet having a definite career in view, I wrote, "To do the will of God." How little I realized the implications-that God exists, that He knows who I am, that He cares what I do or don't do-in short, that God has any will for me and that this can be known and followed in the full assurance of divine favor and help. The intervening years have brought the thrilling and ever-unfolding discovery of the good affirmative answers to these questions.*

PAINE, THOMAS OTTEN, aerospace exec.; b. Berkeley, Calif., Nov. 9, 1921; s. George Thomas and Ada Louise (Otten) P.; A.B. in Engring., Brown U., 1942; M.S. in Phys. Metallurgy, Stanford, 1947, Ph.D., 1949; m. Barbara Helen Taunton Pearse, Oct. 1, 1946; children—Marguerite Ada, George Thomas, Judith Janet, Frank Taunton. Research asso. Stanford, 1947-49; with Gen. Electric Co., 1949-68, 70-76, GE Research Lab., Schenectady, mgr. center advanced studies, Santa Barbara, Calif., 1963-68, v.p., group exec. power generation, 1970-73, sr. v.p. tech. planning and devel., 1973-76; pres. Northrop Corp., Los Angeles, 1976—; dep. adminstr., then adminstr. NASA, 1968-70; dir. Eastern Air Lines. Trustee Occidental Coll., Brown U., Asian Inst. Tech., Bangkok. Served to lt. USNR, World War II. Decorated Submarine Combat insignia with stars, USN Commendation medal; grand ufficiale della Ordine al Merito (Italy); recipient Distinguished Service medal NASA, 1970, Washington award Western Soc. Engrs., 1972; John Fritz medal United Engring. Socs., 1976, Faraday medal Inst. Elec. Engrs. (London), 1976. Fellow Am. Inst. Aeros. and Astronautics; mem. Nat. Acad. Engring., Nat. Acad. Scis., Am. Phys. Soc., IEEE, U.S. Naval Inst., Sigma Xi. Clubs: Explorers, Lotos, Sky (N.Y.C.); Cosmos, Army and Navy (Washington); Calif. (Los Angeles). Contbr. articles to tech. publs. Co-inventor lodex R magnets. Home: 765 Bonhill Rd Los Angeles CA 90049 Office: 1800 Century Park E Los Angeles CA 90067

PAINTER, JACK TIMBERLAKE, civil engr.; b. Kincaid, W. Va., July 23, 1930; s. Troy Earl and Nannie Bell (Proffit) P.; B.S. in Civil Engring., W.Va. U., 1950, M.S.C.E., 1955. Instr. civil engring. W. Va. U., 1950-51, 53-55, mem. faculty La. Tech. U., Ruston, 1955—, prof. civil engring., 1962—, Alumni Found. prof., 1977-78; vis. prof. Manhattan Coll., Coll. Forestry, SUNY, Syracuse, Cornell U., U. Wis., summers 1954-60. Nat. pres. Circus Fans Assn. Am., 1969; layreader Episcopal Ch. Served with USNR, 1951-52. Faculty fellow NSF, 1958-59; named Man of Year, Omicron Delta Kappa, 1970. Mem. ASCE, Soc. Automotive Engrs., Am. Soc. Engring. Edn., Soc. Steel Constrn., La. Engring. Soc., Tau Beta Pi (Outstanding award 1963, 68, 74, 78). Address: Box 6155 Tech Station Ruston LA 71272

PAINTER, JOHN HOYT, elec. engr.; b. Winfield, Kans., Mar. [illegible] 1934; s. John Paul and Marjorie Marietta (Slack) P.; B.S., U. Ill., Urbana, 1961 (Gen. Electric Found. fellow), M.S., 1962; Ph.D., So. Meth. U., 1972; postgrad. Coll. William and Mary, 1967-69; m. Lou Vaughan, June 7, 1955; children—John Mark, Paul [illegible] William Vaughan, Joy Lynn. Communications engr. NASA Manned Spacecraft Center, Houston, 1962-65; sr. engr. Motorola Govt. Electronics div., Scottsdale, Ariz., 1965-67; research engr. NASA Langley Research Center, Hampton, Va., 1967-74; asso. prof. elec. engring. Tex. A&M U., College Station, 1974-79, prof., 1979—; pres. ALTAIR Corp. cons., College Station, 1980—. Served with USAF, 1953-58. Recipient Recognition cert. NASA, 1975. Mem. IEEE (sr.). Club: Masons. Patentee digital communications processing. Home: 1119 Merry Oaks St College Station TX 77840 Office: Dept. Elec. Engring Tex A and M University College Station TX 77843

PAINTER, JOHN WILLIAM, diversified co. exec.; b. Herrin, Ill., July 24, 1929; s. Charles F. and Helen A. (Anderson) P.; B.S. in Bus. Adminstrn., U. Ill., 1950; m. Dorothy E. Woodward, Feb. 1, 1953; children—John W., Thomas A., Andrew W. Gen. sales mgr. [illegible] Aviation Co., Chgo., 1950-60; marketing mgr. Lord Mfg. Co., Erie, Pa., 1960-64; pres. Ohio Rubber Co. div. Eagle-Picher Industries, Willoughby, Ohio, 1964-73, group v.p. parent co., Cin., 1974-7[illegible] v.p., 1977, pres. Eagle-Picher Industries, 1977—, exec. v.p. [illegible] also dir.; dir. Central Trust Bank, Cin. Bd. dirs. Boys' Clubs of Cin. Mem. Alpha Chi Rho, Sigma Iota Epsilon. Clubs: Kenwood Country, Commonwealth, Bankers, Queen City (Cin.). Home: [illegible] Graydon Meadow Ln Cincinnati OH 45243 Office: 580 Bldg. P[illegible] 779 Cincinnati OH 45201

PAINTER, MARY ELLA, editor; b. Tulsa, July 15, 1920; d. F[illegible] Balf Parker and Maggie Mae (Renaud) P.; B.A., Oklahoma City U., 1943; postgrad. Columbia U., 1944; m. Charles J. Yarbrough, Aug. 7, 1946; children—Kirby John, Kevin Lee. Editorial asst., feature writer OWI, 1943-46, feature writer, news editor Dept. State, 1946-53, [illegible] USIA, Washington, 1953-78, editor USIA World, 1967-78, with ICA, 1978-80, editor US ICA World, 1978-80; mng. editor Foreign Policy Center News/Views, Washington, 1981; contbg. editor [illegible] Monitor, World Hunger Year, N.Y.C., 1981. Recipient Meritorious Service award USIA, 1964, Spl. Commendation, 1974; US ICA [illegible]

1967, pres., 1967—, pres., dir. Am. Nat. Growth, Income and Bond Funds. Served to capt., Signal Corps, AUS, 1940-46, 50-52; Korea. Decorated Bronze Star. C.L.U. Mem. Am. Assn. C.L.U.'s. Club: Rotary. Home: 7718 Beaudelaire Circle Galveston TX 77550 Office: Securities Management & Research Inc Moody Ave at Market St Galveston TX 77550. *I believe real success, like beauty, is in the mind of the beholder. And as long as we persist in trying there can be no final failure, since success is usually a series of failures that turn out all right.*

PAIS, ABRAHAM, educator, physicist; b. Amsterdam, Holland, May 19, 1918; s. Jesaja and Kaatje (van Kleeff) P.; B.Sc., U. Amsterdam, 1938; M.Sc., U. Utrecht, 1940, Ph.D., 1941; m. Lila Atwill, Dec. 15, 1956 (div. 1962); 1 son, Joshua; m. Sara Ector Via, May 29, 1976; 1 stepson, Daniel Via. Research fellow Inst. Theoretical Physics, Copenhagen, Denmark, 1946; prof. Inst. Advanced Study, Princeton, N.J., 1950-63; prof. physics Rockefeller U., N.Y.C., 1963—; Balfour prof. Weizmann Inst., Israel, 1977. Recipient J.R. Oppenheimer Meml. prize, 1979; Guggenheim fellow, 1960. Fellow Am. Phys. Soc.; mem. Royal Acad. Scis. Holland (corr.), Am. Acad. Arts and Scis., Nat. Acad. Scis., Council on Fgn. Relations. Club: Knickerbocker. Home: 450 E 63d St New York NY 10021 Office: Rockefeller Univ New York NY 10021

PAISNER, BRUCE LAWRENCE, TV and films co. exec., lawyer; b. Providence, July 4, 1942; s. Isadore and Reva Josephine (Novogroski) P.; A.B. cum laude, Harvard, 1964, J.D. cum laude, 1968; m. Nicole Pilotaz, Sept. 12, 1971; children—Jennifer Ann, Michael Sylvain. Corr., Life mag., 1964-65; asst. to chmn. bd. Time Inc., N.Y.C., 1968-70, v.p., 1975-80; gen. mgr. Time-Life Video, N.Y.C., 1970-73; pres. Time-Life Films Inc., N.Y.C., 1973-80; pres. Novacom, Inc., N.Y.C., 1980-81; pres. King Features Entertainment, Inc. subs. Hearst Corp., N.Y.C., 1981—. Clubs: University, Friars. Home: 1021 Park Ave New York NY 10028 Office: 235 E 45th St New York NY 10017

PAIVIO, ALLAN URHO, educator; b. Thunder Bay, Ont., Can., Mar. 29, 1925; s. Aku and Ida Julia (Hanninen) P.; B.Sc., McGill U., 1949, M.Sc., 1957, Ph.D., 1959; m. Kathleen Laura Blanche Austin, Jan. 9, 1946; children—Sandra, Annalee, Heather, Eric, Karina. Lectr., Sir George Williams U., Montreal, 1957; research psychologist Cornell U., Ithaca, N.Y., 1958-59; asst. prof. psychology U. New Brunswick, Can., 1959-62; asst. prof. U. Western Ont., London, Can., 1962-63, asso. prof., 1963-67, prof., 1967—. Served with Royal Can. Navy, 1944-45. Fellow Am. Psychol. Assn., Can. Psychol. Assn., Royal Soc. Can. Recipient Queen's Silver Jubilee medal Can., 1977. Mem. Psychonomic Soc., AAAS. Author: Imagery and Verbal Processes, 1971. Home: 919 Waterloo St London ON N6A 3X2 Canada Office: Dept Psychology U Western Ont London ON Canada

PAK, HYUNG WOONG, publisher; b. Ham-Hoong, Korea, Nov. 6, 1932; s. Kyung-Koo and Myung-Sook (Lee) P.; came to U.S., 1955, naturalized, 1968; A.B., U. Chgo., 1958; m. Alexandra Theresa Badarak, 1960; m. 2d, Diana Lee Stenen Woodruff, 1975; children—Jonathan Tong-Hee, Michelle Hyun-Mi Lee. Instr., Japanese, U. Chgo., 1962; editor and publisher Chgo. Rev., 1958-63, cons., 1963-65; asso. editor Ency. Britannica Press, Chgo., 1963-64, sr. editor social scis. and humanities, 1964-66; dir. instrnl. materials div. and sales mgr. sch. and coll. div. Bantam Books, Inc., N.Y.C., 1966-69, v.p., editorial dir. Instrnl. Media Am., Inc., N.Y.C., 1969-70; gen. mgr. sch. dept. Appleton-Century-Crofts/New Century, N.Y.C., 1970-72; v.p., editorial dir. D. Van Nostrand Co., N.Y.C., 1972-74, pres., 1974-76; pres. Chatham Sq. Press, N.Y.C., 1976—; pub. Urizen Books, Inc., N.Y.C., 1978-81. Cons. Modern Age: Quar. Rev., 1962-63. Served with Republic Korea Army, 1950-54. Mem. Mus. Modern Art, Friends City Center, Met. Mus. Art, Friends Am. Ballet Theatre, Met. Opera Guild. Club: Arts (Chgo.). Home: 1015 Sharpless Rd Melrose Park PA 19126 Office: 401 Broadway New York NY 10013

PAKE, GEORGE EDWARD, research exec., physicist; b. Jeffersonville, Ohio, Apr. 1, 1924; s. Edward Howe and Mary Mabel (Fry) P.; B.S., M.S., Carnegie Inst. Tech., 1945; Ph.D., Harvard U., 1948; m. Marjorie Elizabeth Semon, May 31, 1947; children—Warren E., Catherine E., Stephen G., Bruce E. Physicist, Westinghouse Research Labs., 1945-46; mem. faculty Washington U., St. Louis, 1948-56, 62-70, prof. physics, provost, 1962-69, exec. vice chancellor, 1965-69, Edward Mallinckrodt prof. physics, 1969-70; v.p. Xerox Corp., mgr. Xerox Palo Alto (Calif.) Research Center, 1970-78, v.p. research, 1978—; prof. physics Stanford U., 1956-62. Mem. gov. bd. Am. Inst. Physics, 1957-59; bd. dirs. St. Louis Research Council, 1964-70, mem. physics adv. panel NSF, 1958-60, 63-66; chmn. physics survey com. Nat. Acad. Sci.-NRC, 1964-66. Mem. St. Louis [illegible] Bus. and Indl. Devel. Commn., 1963-66; chmn. bd. Regional Indsl. Devel. Corp., St. Louis, 1966-67, St. Louis Research Council, 1967-70, mem. President's Sci. Adv. Com., 1965-69. Bd. dirs. St. Louis Country Day Sch., 1964-70, Central Inst. for Deaf, 1965-70; trustee Washington U., 1970—, Danforth Found., 1971—. Fellow Am. Phys. Soc. (pres. 1977); mem. Am. Assn. Physics Tchrs., AAUP, AAAS, Am. Acad. Arts and Scis., Nat. Acad. Sci., Sigma Xi, Tau Beta Pi. Club: University (Palo Alto). Author: (with E. Feenberg) Quantum Theory of Angular Momentum, 1953; Paramagnetic Resonance, 1962, (with T. Estle) The Physical Principles of Electron Paramagnetic Resonance, 1973. Home: 10 Arastradero Rd Portola [illegible]

com. of center, 1967-68, chief Office Earthquake Research and Crustal Studies, 1967-70, research geophysicist, 1970—, chief seismicity and earth structure and nat. earthquake info. service, 1975-77, ad hoc panel on earthquake prediction OST, 1964-[illegible]; distinguished lectr. Soc. Exploration Geophysicists, 1964; chmn. interagy. staff group on minority participation in sci. and engring. U.S. Dept. Interior, 1972-74. Chmn. land use adv. com. Douglas County (Colo.), 1975-76; bd. dirs. Nat. Consortium for Black Profl. Devel., 1976-79. Adv. council Am. Vets. Com., 1960-61; steering com. N[illegible] Civil Liberties Clearing House, 1950-52. Served as pvt. AUS, 1943-45. Recipient Distinguished Service award U.S. Dept. Interior, 1970. Mem. N.A.A.C.P. (exec. com. Tulsa br. 1949, co-chmn. membership com. 1949), Soc. Exploration Geophysicists, Am. Geophys. Union, Seismol. Soc. Am., Am. Geol. Inst. (chmn. ad hoc com. to minority participation program 1973-75), Geol. Soc. Am. (chmn. com. on minority participation in geol. scis., 1975-80), AAAS. Mem. United Ch. Christ (state social action bd. 1964-66). Home: 27[illegible] S Sandy Ridge Rd Sedalia CO 80135 Office: US Geol Survey F[illegible] Center Denver CO 80225

PAKULA, ALAN J., producer, dir.; b. N.Y.C., Apr. 7, 1928; ed. Yale. Producer's apprentice Metro Goldwyn-Mayer, 1950; producer's asst. Paramount Pictures, 1951, producer, 1955; stage prodns. include Comes A Day; Laurette; There Must Be a Pony; films include Fear Strikes Out, 1957, To Kill a Mockingbird, 1963, Love With the Proper Stranger, 1964, Baby the Rain Must Fall, 1965, Inside Daisy Clover, 1966, Up the Down Staircase, 1967, The Stalking Moon, 1969, Sterile Cuckoo, 1969, Klute, 1971, Love and Pain, 1973, The Parallax View, 1974, All The President's Men, 1976, Comes A Horseman, 1978, Starting Over, 1979. Address: care Stan Kamen William Morris Agency 151 El Camino Blvd Beverly Hills CA 90212*

PAL, PRATAPADITYA, museum curator; b. Bangladesh, Sept. [illegible] 1935; came to U.S., 1967; s. Gopesh Chandra and Bidyut Kana (Da[illegible]) P.; M.A., U. Calcutta, 1958, D.Phil., 1962; Ph.D. (U.K. Commonwealth Scholar), U. Cambridge (Eng.), 1965; m. Chitralekha Bose, Apr. 20, 1968; children—Shalmali, Lopamudra. Research asso. Am. Acad. of Benares (India), 1966-67, keeper Indian collection Mus. Fine Arts, Boston, 1967-69; curator Indian and Islamic art Los Angeles County Mus. Art, Los Angeles, 1970—, acting dir. in 1979; adj. prof. fine arts U. So. Calif., 1971—, vis. prof. U. Calif., Santa Barbara, 1980. Bd. dirs. Internat. Documentation Center, San Bernardino, Calif., Tibetan Found., N.Y.C., trustee Pacific Asia Mus., Pasadena, 1981—. JDR 3d Fund fellow, 1964, 69, NEA fellow, 1974. Mem. Asiatic Soc. (Calcutta), Asia Soc. Author: The Arts of Nepal, vol. 1, 1974, vol. 2, 1979; The Sensuous Immortals, 1977, The Ideal Image: Gupta Sculptures and its Influence, 1978; The Classical Tradition in Rajput Painting, 1978. Office: Los Angeles County Museum Art 5905 Wilshire Blvd Los Angeles CA 90036

PALADE, GEORGE EMIL, educator, scientist; b. Jassy, Romania, Nov. 19, 1912; s. Emil and Constanta (Cantemir) P.; Bachelor Hasdeu Lyceum, Buzau, Romania; M.D., U. Bucharest (Romania), [illegible] Irina Malaxa, June 12, 1941 (dec. 1969); children—Georgia Teodora, Philip Theodore; m. 2d, Marilyn G. Farquhar, 1970. Came to U.S., 1946, naturalized, 1952. Instr., asst. prof., then asso. prof. anatomy Sch. Medicine, U. Bucharest, 1935-45; vis. investigator, asst. [illegible] prof. cell biology Rockefeller U., 1946-73; now prof. cell biology Yale; correlated biochem. and morphological analysis cell structures. Recipient Albert Lasker Basic Research award, 1966, Horwitz prize, 1970, Nobel prize, 1974. Fellow Am. Acad. Arts and Scis., mem. Nat. Acad. Sci. Author sci. papers. Office: Cell Biology Sect Yale U New Haven CT 06510

PALAMARA, FRANCIS JOSEPH, corp. exec.; b. Bklyn., July [illegible] 1925; s. Joseph B. and Marie Elizabeth (Spina) P.; B.B.A., St. John's U., 1950; m. Dorothy A. White, Aug. 21, 1948; children—Mary A[illegible], J. Francis, Diane M., Thomas J., Ann M., Louise, Marie, Robert. Accountant, Coopers & Lybrand, N.Y.C., 1949-55; v.p. finance Ja[illegible] Talcott, Inc., N.Y.C., 1955-66; v.p. adminstrn. CIT Financial Corp., N.Y.C., 1966-67, pres. subsidiary Meinhard Comml. Corp., 1968-[illegible] exec. v.p. Shelter Resources Corp., N.Y.C., 1969-71, Pittston Co., N.Y.C., 1971-72, 78-81; exec. v.p., chief fin. officer ARA Services, Inc., 1981—; exec. v.p., chief operating officer N.Y. Stock Exchange, Inc., N.Y.C., 1972-78. Served as ensign USNR, 1943-46. C.P.A., [illegible] Mem. Am. Inst. C.P.A.'s, N.Y. Soc. C.P.A.'s, Financial Execs. Inst. Club: N.Y. Athletic (N.Y.C.). Office: Independence Sq. Philadelphia PA 19106

PALAMOUNTAIN, JOSEPH CORNWALL, JR., coll. pres.; b. West Newton, Mass., Nov. 26, 1920; s. Joseph Cornwall and [illegible] (Coles) P.; B.A. summa cum laude, Dartmouth, 1942, LL.D. (hon.), 1976; M.A., Harvard, 1948, Ph.D., 1951; M.A., Wesleyan U., Middletown, Conn., 1959; L.H.D. (hon.), 1968; m. Anne Tonnesen, Aug. 8, 1946; children—Brantley Coles, Bruce [illegible] Teaching fellow, tutor in govt. Harvard, 1947-51, instr., 1951-5[illegible], prof., 1952-55, also sr. tutor Adams House, 1951-55; asso. prof. Wesleyan U., 1955-59, prof., 1959-65, provost, 1961-65; pres. Skidmore Coll., Saratoga Springs, N.Y., 1965—, cons. Brookings Instn., 1959-60; vis. lectr. Yale, 1964-65. Asso. N.Y. U. Med. Ctr. Bd., 1965. Dir. Adirondack Trust Co. Chmn. Gov.'s Com. on Establishment Br. of U. Conn. in Middlesex County, 1963-65; [illegible] Nat. Citizens Com. for a Cabinet Dept. Edn., 1977-79, mem. council State N.Y. Joint Legis. Com. on Higher Edn., 1970-74; [illegible]

HW

PRO·BONO·HUMANI·GENERIS
The Rockefeller University
1901

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE • NEW YORK, NEW YORK 10021-6399

8/30/82

Dear Harry

Your note makes me feel very good. My acknowledgments to the Institute and to you – you will be the last to take exception to this order – came from the heart and from the head.

Till soon, I hope.

As ever
Bram

PRO·BONO·HUMANI·GENERIS The Rockefeller University 1901

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE • NEW YORK, NEW YORK 10021-6399

April 1, 1982

Dear Val

Here is a copy of my talk!

As you said over the phone, it would be allright to make copies at the Institute. I want to ask your help in sending one copy each to the following:

Val	Chemiss ✓	✓ David Boulware
✓ Winny	✓ Kennan	✓ Harry Woolf
✓ Franz } Cafeteria	✓ John Bahcall ✓	✓ Felix Gilbert
✓ Bea }	✓ Armand Borel	✓ Morton White
✓ Caroline Underwood	✓ Pat Sherr	
✓ Lily Agar	✓ Mary Wisnovsky	
✓ Sabina	Tullio Regge	
	✓ Otto Neugebauer ✓	

Please feel free to make as many copies as you want and give them to any one who might like one.

Freeman Dyson is not on my list, since I am sending him a copy myself.

In addition I would like to ask for ~~20~~ 25 copies for myself, to send to other people in Princeton.

/over

When you have those available, please put them all together in one interoffice envelope and send to Jadwin.

Many many thanks!

Enjoy your Easter —

As ever
Bram

I have no new jokes.

PS On page 1 line 3 from bottom
Could you change please
Mädchenschule
into
Töchterschule ?

HELEN DUKAS

(Remarks by Abraham Pais at the memorial ceremony, Institute for Advanced Study, March 15, 1982.)

Tov shem meshemen tov, wejom hamawet mejom hewaledot. A good name is worth more than precious oil, and the day of death more important than the day of birth.

We called her Helena. Einstein called her Dukas, and referred to her as "die Dukas". Her first name was Helene. She was born in Freiburg im Breisgau, in Germany, on October 17, 1896. Her father, Leopold Dukas, a native of Sulzburg, a small town in the Black Forest, was a wine merchant who also produced the famed Schwarzwalder Kirschwasser in a government controlled distillery behind the house in Freiburg. Her mother, Hannchen Dukas-Liebmann, hailed from Hechingen in Hohenzollern, the same town where Elsa Einstein, Albert Einstein's second wife, was born. From the Hechingen days Elsa knew Hannchen and also Helen's grandmother, who was known in the community as "Tante Lisette". These acquaintanceships of Elsa were to seal Helen's destiny.

The Dukases had seven children. The first two were girls, then there were two boys, then, after quite a few years, three more girls. Helen was the oldest of these three. She used to refer to herself as "the oldest of the youngest". From age six to fifteen she attended the Höhere Töchterschule in Freiburg. That was all the formal education she received. In 1909 her mother died of tuberculosis, "erschöpft von den sieben Kindern", exhausted from the seven children. Celine, Helen's oldest sister, took charge of the running of the household until she married. Thereafter it was Helen's turn, she had to leave the school on the Holzmarktplatz without having completed the curriculum.

Helen stayed in Freiburg until 1921, taking care of the children of her father (who died in 1919), and also starting her first job, as kindergarten teacher. Then she went for a year to Munich, as governess in the home of Raphael Straus, an uncle of one of Einstein's assistants in the 1940's, Ernst Gabor Straus. (When, in 1944, Ernst introduced himself to Helen, she told him at once that she had been at his brith, the festive day of circumcision). Thereafter she moved to Berlin where some of her family had settled meanwhile. For some years she worked as secretary for a small publishing house, until that company went out of business and she was without a job.

It was during that period, on April 11, 1928, to be precise, that Elsa Einstein asked Helen's older sister Rosa if she knew someone suitable to be secretary to her husband. The two women knew each other through "Jüdische Waisenhilfe", an organization for aid to Jewish orphans. Rosa was its executive secretary, Elsa its honorary president. The two women had become friendly especially because of the Hechingen connection. In response to Elsa's question, Rosa proposed Helen. Elsa thought this was a fine idea. She had seen Helen a few times, when the latter came to the Einstein apartment on the Haberlandstrasse to bring letters which she, Elsa, had to sign. She asked Rosa to have Helen visit her. That same day Rosa called Helen and told her the news. Helen's reaction was: "Du bist verrückt geworden, so was kann ich nie tun", you have gone mad, I can never do something like that. Nevertheless, the next day Helen visited Elsa who plied her with tea and cookies and urged her at least to try.

The day thereafter Helen came back. It was Friday the thirteenth. Einstein was bedridden at that time, recuperating from heart disease. Elsa brought Helen to the bedroom. Einstein greeted her with a friendly smile and an outstretched hand, saying: "Hier liegt eine alte Kindsleich", here lies an old child cadaver. He immediately asked her to call the Kultusministerium in connection with his duties as member of a committee of the League of Nations. Helen, brought up as "authoritatsgläubiges deutsches Schulmädchen" (German schoolgirl with religious belief in authority), did so with trepidation. All went smoothly, however, and "for the first time I experienced what magic effect the name 'Einstein' had". Next she typed some letters. Then Einstein told her he thought they would get along very well. "Then the remaining feelings of inferiority left me, even though in the next twenty-seven years I never lost one iota of respect nor a certain shyness".

In the course of time Einstein developed a growing appreciation and respect for Helen's capabilities, along with a strong affection which never went beyond the fatherly.

I turn to the next great event in Helen's life: her three months' trip to California, together with the Einsteins and Walther Mayer, Einstein's assistant (December 1930-March 1931). Helen kept a diary which she gave me to read. Its bulk is devoted to the California trip. Otherwise it is, alas, quite sketchy. She tells of the hotel in Antwerp where she had her first room with bath ever; of playing ping-pong with Einstein on board the Belgenland; of Jimmy Walker presenting the key to New York City to Einstein; of an old woman who breaks through a police cordon, presses Einstein's hands and says, "now I can die in peace"; of Helen Keller first addressing Einstein then touching his skull and his face, with Einstein in tears; of the relief of having to mend socks in a New York hotel; of a visit to Cuba; of attending the Tournament of the Roses in Pasedena; of the historic first meeting between Einstein and Michelson; of meeting Charlie Chaplin at a dinner in his house attended also by the Einsteins, Marion Davies and William Randolph Hearst; and of being taken to the world premiere of "City Lights" at the Los Angeles Theatre. All this, written in fine style and with great wit, represents an important witness account of uncommon events that occurred wherever Einstein went.

In December 1932 the Einsteins left for what was planned as another trip to California, without Helen this time. Hitler came to power the next month, and they never returned to Germany, but did go back for a last European encampment, in Belgium. Helen joined them there, and in October 1933 she, the Einsteins and Mayer sailed for America. They were taken off the ship as it entered New York harbor and brought by launch to the Battery in downtown Manhattan. There Edgar Bamberger and Herbert Maass, trustees of the Institute for Advanced Study, were waiting and took them by car to the Peacock Inn in Princeton. A few days later the Einsteins and Helen moved into a house on 2 Library Place. In the autumn of 1935 they moved to 112 Mercer Street. Soon thereafter Elsa's daughter Margot joined them there.

After the death of Elsa in 1936, Helen took full responsibility for managing the household, along with her continuing secretarial duties. Einstein's sister Maja, who had joined the household in 1939, was bedridden from 1945 until her death - in the Mercer Street home - in 1951. It was

Helen who arranged for the medical attention.

We have Helen's own account of her daily activities in Princeton from a letter she wrote to Carl Seelig in the early 1950's. After having described a typical day in the life of the Professor she turns to "meine Wenigkeit", my humble self:

"[To describe my own days] is much more difficult, but then that is less important. It is more complicated because I have so many duties, I mean all kinds of things. I also try, as much as possible, to stick to an organized routine, but that is not always easy. At about 8:30 I come downstairs, a bit later on the four days on which I have household help, something which is not so common in this country. Then I make breakfast or at least coffee and tea etc.. On the days on which our Irish help is here I go to town after breakfast, first to buy food, then to go to the bank and to the post-office or to do what else is necessary. Around eleven, sometimes later, I get back, feed the dog, open the mail, throw-Gottlob - most of it in the waste paper basket, go to the kitchen and prepare lunch. In between I quickly write letters if that is necessary, else I do something about the house. After lunch comes the sacred rest - but on three days per week the dishes get done first. From four o'clock on there is either dictation of letters or the writing of letters of the previous day or replies which I attend to myself. Sometimes there are visitors for tea. Six o'clock: back to the kitchen for the evening meal. In the evenings there is either dictation, or the typing of letters, or reading, or a movie if there is something of interest. Of course there are also visits from friends or to friends. Now and then there is a trip to New York, but not often, because it is strenuous and because I first must carefully prepare my departure. Usually something comes in between at the last moment. I have of course given only a crude outline. On top of all this comes my special bane, the telephone, the doorbell, the curious, the reporters, the crazies etc. Telegrams and express letters have long since failed to make any impression. What I hate most is the filing of letters, especially because I have so little space. I have filing cabinets even in the hallways and there are books everywhere, innumerable crates in the basement. I have often wished that Gutenberg had never lived! I do not know if you can make yourself a picture. I appear to myself as the farmer's wife who is asked by someone making a statistics how she and people like her spend the day. She enumerates her duties: to cook, to wash, to iron, to milk the cows, to feed the chicken, to clean, to sew, to work in the garden etc. Finally the man asks: 'What do you do with your free time'? To which she replies: 'Then I go to the John', dann gehe ich aufs 'Oertli'. With me it is not quite as bad, but still the comparison does hold a little bit."

Two stories I owe to Helen may serve to illustrate the bizarre happenings which would occasionally take place on Mercer Street. One day, around ten in the morning, the bell rang. As Helen opened the door she saw a man who said to her: "I am Judge T. and would like to see Professor Einstein." Helen called Einstein who came to the door. When the judge saw him he passed out. After he came to he had to be helped into a taxi. The next morning the judge returned, quite contrite. He explained that he had been quite intoxicated on his previous visit, in spite of the early hour, the reason being that he was in town for the University Reunion.

The second story concerns an event on a day in 1939. The bell rang. Helen went to the door. She saw a well-dressed man who said "Madam, I have come to the United States to see the World Fair, Professor Einstein, and the Grand Canyon".

For those left behind there are no better ways for coping with death than to have loved well and to have served well. Helen was no exception. Shortly after April 1955 she threw herself with renewed vigor into a major task, the creation of an Archive for Einstein's papers and letters. At the time of Helen's death these Archives consisted of roughly 5000 pages of published and unpublished manuscripts, 3000 pages of notebooks and travel diaries, and 52000 pages of correspondence, of which 29000 are of a non-scientific nature. The correspondence has been abstracted and catalogued both alphabetically and chronologically. Eminent men, especially Gerald Holton and Martin Klein, helped in scientific matters of which she would smilingly confess her complete incompetence. Otherwise nearly all the labor was hers. My book on Einstein bears ample testimony to her achievements and will help, I hope, to secure her a well-deserved place in the history of physics. Here I shall only add two comments.

Helen's most important single contribution to the Archives, in my judgment, was the result of a correspondence between her and Frau Janke, David Hilbert's "Dukas". After Hilbert's death in 1943 the Hilbert-Einstein correspondence had been given to the University Library in Goettingen, where it lay forgotten. Because of an initiative by Helen, the letters were recovered by Frau Janke and sent to Princeton. This correspondence contains a crucial exchange in November 1915 without the knowledge of which the history of general relativity cannot properly be written. Helen's talents for knowing where to look and whom to ask have enriched the Archives very substantially.

My second remark may serve as but one illustration of Helen's astounding familiarity with the contents of the Archives. At one point I was interested in the contacts between Einstein and Poincaré. So I went to Helen and asked her if there had ever been an exchange of letters between them. No, she said. That simple answer saved me considerable labor. Then she said that there was of course this exchange about Poincaré with that Swedish mathematician. Which mathematician, I asked. Mittag-Leffler, she said. Can I see those letters, I asked. Of course, she said. I read the letters and caught a new glimpse of Einstein's feelings about Poincaré. As I returned the letters to her, she commented that there was of course that lecture by Poincaré in Berlin in 1910. How do you know about that, I asked. Because it is in the first chapter of Moszkowski's Einstein biography, she said. I looked up that book. There it was, just as she had told me.

I believe that death was on Helen's mind in the last year of her life during which she said to me several times that only helping me with my book kept her alive. She certainly did prepare for death. Article thirteen of Einstein's last will contains the stipulation: "To deliver and turn over to Hebrew University any funds or specific property held in trust, at any time, upon the written direction of the said Helena Dukas during her life-time..." As of January 1, 1982, the Estate of Albert Einstein had transferred all papers, publications, manuscripts and all other property to the Hebrew University.

Helen's was a rich life. She did her work with competence and good cheer. She read voraciously. She was intensely Jewish, though not religious. Though not free of what I call Fallen Hero Syndrome, which causes confusion between greatness and immaculacy, she was fundamentally capable of that tough realism which so often marks women of quality and substance. She was blessed with the abilities to give and to receive affection. She was lucid until the end.

In the summer of 1981 I wrote the following lines in the Preface to my biography of Einstein: "No one helped me more than Helen Dukas, more familiar than anyone else at this time with Einstein's life, trusted guide through the Einstein Archives in Princeton. Dear Helen, thank you, it was wonderful". A few weeks ago I added the following sentence in proof: "I have left the text of this Preface as it was written before the death of Helen Dukas on February 10, 1982."

Rockefeller University
New York, New York, 10021

(Note. The details of how Helen started her work for Einstein are described in her letter to Carl Seelig, now in the Wissenschaftshistorische Sammlung, ETH Bibliothek, Zürich, Hs. 304:118. Her letter to Seelig describing a day in the life on Mercer Street is in that same collection, Hs. 304:133.)

June 16, 1981

MEMORANDUM

To: Office of Personnel

From: Aida L. La Brutte
Secretary to the Director

Subject: Professor A. Pais

Dr. Woolf has authorized the renewal of Professor Pais' Institute card for academic 1981-82. Therefore, although he will not be an official member of the Institute, his card should be renewed through August 1982.

Thank you.

cc. Allen Rowe

cc. Allen Rowe

June 15, 1981

Professor Abraham Pais
School of Natural Sciences

Dear Bram:

This is simply to put into writing so that it is of record that I have given you the authority to retain your key to the library for the coming academic year. Additionally, we shall renew your Institute card so as to retain all the rights and privileges that go with it (it begins to sound like the award of an honorary degree, though I am inclined to believe that the card is worth more).

Cordially yours,

Harry Woolf

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY 08540
Telephone 609-734-8000

SCHOOL OF NATURAL SCIENCES

June 9, 1981.

Dear Harry

The second term has ended – but I intend to stay on until early August. The work continues to go well. At the end of this week I shall have more than 300 pages ready for the Oxford University Press. One fun thing happened recently. I got hold of all documentation of proposals that Einstein get the Nobel prize. The Swedes have given me permission to use it. So now I can tell the story of why E. did not get the Nobel prize for relativity.

I write to put one request to you. I want to ask if I may retain the key to the humanities library building. As you know, I shall continue to spend a good fraction of my time in Princeton. Access to that library would be most helpful to me.

All warm wishes to you, Pat and the children

As ever

Bram

June 11, 1981

To Whom It May Concern

This is to certify that Dr. Abraham Pais has been and will be working at the Institute for Advanced Study in Princeton, New Jersey from August 15, 1980 to August 15, 1981. He is a member of the School of Natural Sciences at the Institute for Advanced Study during that period, residing in Princeton and on leave of absence from the Rockefeller University in New York City.

Harry Woolf
Director

Pais

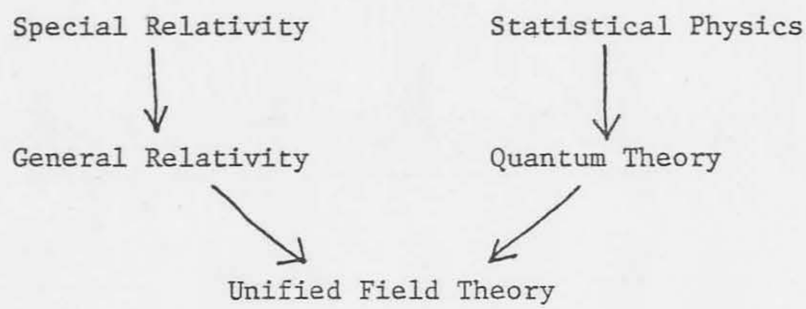
To the reader

Turn to the table of contents, follow the entries in italics and you will find an almost entirely non-scientific biography of Einstein. Turn to the first chapter and you will find a non-technical tour through the entire book, some personal reminiscences, and an attempt at a general assessment.

The principal aim of this work is to present a scientific biography of Albert Einstein. I shall attempt to sketch the concepts of the physical world when Einstein became a physicist, how he changed them, and what scientific inheritance he left. This book is an essay in open history, open because Einstein's oeuvre left us with unresolved questions of principle. The search for their answers is a central quest of physics today. Some issues cannot be discussed without entering into mathematical details, but I have tried to hold these to a minimum by directing the reader to standard texts wherever possible.

Science, more than anything else, was Einstein's life, his devotion, his refuge, and his source of detachment. In order to understand the man it is necessary to follow his scientific ways of thinking and doing. But that is not sufficient. He was also a highly gifted stylist of the German language, a lover of music, a student of philosophy. He had deep concerns about the human condition. (In his later years he used to refer to his daily reading of the New York Times as his adrenaline treatment.) He was a husband, a father, a stepfather. He was a Jew. And he is a legend. All these elements are touched on in this story; follow the entries in italics.

Were I asked for a one-sentence biography of Einstein, I would say: He was the freest man I have ever known. Had I to compose a one-sentence scientific biography of him, I would write: Better than anyone before or after him he knew how to invent invariance principles and make use of statistical fluctuations. Were I permitted to use one illustration, I would offer the following drawing



with the caption: The Science and the Life of Albert Einstein. This picture with its entries and its arrows represents my most concise summary of Einstein's greatness, his vision, and his frailty. The book is largely an attempt to explain this cryptic description of a skeletal figure. Toward the end of the book the picture will return.

The generosity, wisdom, knowledge, and criticism of many have been invaluable to me in preparing this work. To all of them I express my deep gratitude. No one helped me more than Helen Dukas, more familiar than anyone else at this time with Einstein's life, trusted guide through the Einstein Archives in Princeton. Dear Helen, thank you, it was wonderful. I have benefited importantly from discussions with Res Jost, Sam Treiman, and George Uhlenbeck, each of whom read nearly the whole manuscript, made many

suggestions for improvement, and gave me much encouragement. I also gratefully record discussions on particular subjects, with Valentin Bargmann, Banesh Hoffmann, and Ernst Straus on Einstein's life, on general relativity, and on unified field theory; with Robert Dicke, Peter Havas, Malcolm Perry, Dennis Sciama, and John Stachel on relativity; with Armand Borel on Poincaré; with Eddie Cohen, Mark Kac and Martin Klein on statistical physics, with Anne Kox on Lorentz; and with Harold Cherniss and Felix Gilbert on topics ranging from Greek philosophy to the Weimar Republic. Special thanks go to Beat Glaus from the ETH and George Rasche from the University for helping me to find my way in archives in Zürich. To all of them as well as to those numerous others who answered questions and inspired with comments: thank you again.

This book was completed at the Institute for Advanced Study in Princeton. I thank Harry Woolf for his hospitality and for support from the Director's Fund. I am greatly beholden to the Alfred P. Sloan Foundation for an important grant which helped me in many phases of preparation. For permission to quote from unpublished material I express my deep appreciation to the Einstein Estate, the Pauli Estate, the Rijksarchief in the Hague (Lorentz correspondence), the Boerhaave museum in Leiden (Ehrenfest correspondence). I also thank the K. Vetenskapsakademiens Nobel Kommittéer in Stockholm, and in particular Bengt Nagel, for making available to me the documentation regarding Einstein's Nobel Prize.

I had the great good fortune of my dear wife Sara's counsel and support.

File Pais, A. 1st draft

Chapter 6. Subtle is the Lord. . .

(a) The Michelson-Morley experiment

(b) The precursors.

1. What Einstein knew. 2. Voigt. 3. FitzGerald.
4. Lorentz. 5. Larmor. 6. Poincaré.

(c) Poincaré in 1905.

(d) Einstein before 1905.

1. The Pavia essay. 2. The Aarau question. 3. The ETH student. 4. The Winterthur letter. 5. The Bern lecture. 6. The Kyoto address. 7. Summary.

III. RELATIVITY, THE SPECIAL THEORY

6 Subtle is the Lord . . .

(a) The Michelson-Morley experiment

Maxwell's article "Ether", written for the ninth edition of the Encyclopedia Britannica [M1], begins with an enumeration of the "high metaphysical . . . [and] mundane uses to be fulfilled by aethers" and with the barely veiled criticism that, even for scientific purposes only, "all space had been filled three or four times over with aethers." This contribution by Maxwell is an important document for numerous reasons. To mention but three, it shows us that, like all his contemporaries, Maxwell was deeply convinced of the reality of some sort of aether: "There can be no doubt that the interplanetary and interstellar spaces are not empty but are occupied by a material substance or body, which is certainly the largest, and probably the most uniform body of which we have any knowledge"; it tells us of an unsuccessful attempt by Maxwell himself to perform a terrestrial optical experiment aimed at detecting the influence of an aether drag on the earth's motion; and it informs us of Maxwell's opinion that effects of the second order in v/c (v-velocity of the earth relative to the aether, c= light velocity) are too small to be detectable. This last comment was prompted by Maxwell's observation that "All methods ... by which it is practicable to determine the velocity of light from terrestrial experiments depend on the measurement of the time required for the double journey from one station to the other and back again," leading to an effect at most of $0((v/c)^2)=0(10^{-8})$.

However, Maxwell still hoped that first order effects might be astronomically observable. The example he gave was the determination of the light velocity from the eclipses of Jupiter's satellites when Jupiter is seen from the earth at nearly opposite points of the ecliptic. If one defines the aether* in the sense of Maxwell, (or, rather, in the sense of Augustin Jean Fresnel (1788-1827)), a medium in a state of absolute rest relative to the fixed stars, in which light is propaga and through which the earth moves as if it were transparent to it, then one readily sees that the Jupiter effect, if it were to exist at all, is of first order in the velocity of the solar system relative to the aether.

Maxwell requested and received data on the Jovial system from David Peck Todd, Director of the Nautical Almanac Office in Washington, D.C. On March 19, 1879 Maxwell sent a latter of thanks in which he referred Todd to his Encyclopedia article and, in particular, reiterated his remark on the second order nature of terrestrial experiments. This letter (not reproduced in his collected papers) was written when Maxwell had less than eight months to live and Einstein was five days old. After Maxwell's death, the letter was forwarded to the secretary of the Royal Society who saw to its publication in the January 29, 1880 issue of Nature [M2].

A year and a half later, in August 1881, there appeared an article in an issue of the American Journal of Science, authored by Albert A. (for Abraham) Michelson, Master, U.S. Navy [M3].Michelson (1852-1931), then on leave from the Navy and doing post-graduate work in Helmholtz's laboratory in Berlin, had read Maxwell's posthumous letter. Being already an acknowledged

*For a review of aether theories and aether models see especially [L1].

expert on measurements of the velocity of light (he had by then published three papers on the subject [L2]) he had concluded that Maxwell had underrated the accuracy with which terrestrial experiments could be performed. The instrument he designed in Berlin in order to measure Maxwell's second order effect is known as the Michelson interferometer. In order not to be bothered by urban vibrations, Michelson performed his experiments at the astrophysical observatory in nearby Potsdam. The method he employed was to compare the times it takes for light to travel the same distance either parallel or transversely to the earth's motion relative to the aether. A stationary aether would yield a time difference corresponding to an extra $\sim$ 1/25 of a wavelength of yellow light travelling in the parallel direction, and effect which can be detected by letting the transverse and parallel beams interfere. For easily accessible details of the experiment I refer to textbooks* and only state Michelson's conclusion: There was no evidence for an aether wind. "The result of the hypothesis of a stationary aether is thus shown to be incorrect, and the necessary conclusion follows that the hypothesis is erroneous," [M3].

Early in 1887 Michelson wrote to Rayleigh** that he was "discouraged at the slight attention the work received" [M4], a statement which perhaps was justified if one counts the number of those who took note, but not if one considers their eminence. Kelvin and Rayleigh (both of whom Michelson had met at Johns Hopkins University in Baltimore, in 1884 [S1]) certainly

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* See e.g. [P1].

** For details of the Michelson-Rayleigh correspondence see especially [S2] and [H1].

paid attention. So did Lorentz who found an error in Michelson's theory of the experiment [L3] and who was dubious about the interpretation of the results [L4]. Lorentz's misgivings and Rayleigh's urgings contributed to Michelson's decision - he was now at the Case school of applied science in Cleveland - to repeat his experiment, this time in collaboration with Edward Williams Morley (1838-1923), a chemist from next door Western Reserve University. Proceeding along the same general lines as the Potsdam experiments, they built a new interferometer. Great care was taken to minimize perturbative influences. In August 1887, Michelson wrote to Rayleigh that again a null effect had been found [M5]. The paper on the Michelson-Morley experiment came out the following November [M6]. Understandably, the negative outcome of this experiment was initially a disappointment, not only to its authors, but also to Kelvin, Rayleigh and Lorentz.

However, more importantly, the experimental result was accepted. There had to be a flaw in the theory. In 1892 Lorentz queried Rayleigh: "Can there be some point in the theory of Mr. Michelson's experiment which had as yet been overlooked?" [L5]. In a lecture before the Royal Institution on April 27, 1900, Kelvin referred to the experiment as "carried out with most searching care to secure a trustworthy result" and characterized its outcome as a nineteenth century cloud over the dynamical theory of light [K1]. In 1904 he wrote in the preface to his Baltimore Lectures: "Michelson and Morley have by their great experimental work on the motion of the ether relatively to the earth raised the one and only serious objection against our dynamical explanations ..." [K2].

In later years, Michelson repeated this experiment several times, up until 1929. Others did likewise, notably Dayton Clarence Miller (1886-

1941), at one time a junior colleague of Michelson at Case. In 1904, Morley and Miller were the first to do a hilltop experiment: "Some have thought that [the Michelson-Morley] experiment only proves that the ether in a certain basement room is carried along with it. We desire therefore to place the apparatus on a hill to see if an effect can there be detected," [M8]*. Articles in the Reviews of Modern Physics in 1933 [M9] and 1955 [S2] give many technical and historical details of these experiments. No one has done more to unearth their history than Robert S. Shankland (b. 1908), whose papers are quoted extensively in this section. For the present purposes there is no need to discuss these later developments, except for one interlude which directly involved Einstein.

On April 2, 1921, Einstein arrived for the first time in the United States, for a two month' visit. He was in the company of Chaim Weizmann. (Einstein once said to me: "Meine Beziehungen, zu dem Weizmann waren, wie der Freud sagt, ambivalent".) The purpose of the trip was to raise funds for the planned Hebrew University in Jerusalem.** Einstein's early American experiences included a parade in his honor down New York's Broadway and a reception at the White House by President Harding. In May he gave four lectures on relativity theory at Princeton University [E1]. While he was there, word reached Princeton that Miller had found a non-zero ether drift during preliminary experiments performed (from April 8-21 [S4]) at Mount Wilson observatory. Upon hearing this rumor Einstein made what may well be his most often quoted comment: "Raffiniert

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*Michelson was the first to point out that perhaps the ether might be trapped in the basements in which he had done his early experiments [M4].

**That University was formally opened on April 1, 1925.

ist der Herr Gott, aber boshaft ist er nicht", "Subtle is the Lord but he is not malicious." Nevertheless, on May 25, 1921, shortly before his departure from the States, Einstein paid a visit to Miller in Cleveland where they talked matters over. There are two postscripts to this story. One concerns transitory events. On April 28, 1925 Miller read a paper before the National Academy of Sciences in Washington, D. C. in which he reported that an aether drift had definitely been established [M10]. Later that year he made the same claim in his retiring address in Kansas City as president of the American Physical Society [M11]. The outcome of all this was that Miller received a thousand dollar prize for his Kansas City paper from the American Association for the Advancement of Science [L6] -- presumably in part an expression of the resistance to relativity which then could still be found in some quarters [B1] -- while Einstein got flooded with telegrams and letters asking him to comment. The latter's reactions to the commotion are best seen from a remark he made in passing in a letter to Besso at that time: "I have not for a moment taken [Miller's results] seriously," [E2]. As to present times, quantum field theory has drastically changed our perceptions of the vacuum. But that has nothing to do with the aether of the nineteenth century and earlier which has gone for good.*

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* In 1951 Dirac briefly considered a return to the aether, however [D1].

The second postscript to the Miller episode concerns a lasting event. In May 1921, Oswald Veblen (1880-1960), then a professor of mathematics at Princeton University, had overheard Einstein's comment about the Lord. In 1930 Veblen wrote to Einstein in Berlin, asking his permission to have this statement chiseled in the stone frame of the fireplace in the common room of Fine Hall, the newly constructed mathematics building at the university [V1]. Einstein consented.* The mathematics department his since moved to new quarters, but the inscription in stone has remained in its original place, Room 202 in what once was Fine Hall.

Let us now move back to the times when Einstein still was virtually unknown and ask how Michelson reacted to Einstein's special theory of relativity and what influence the Michelson-Morley experiment had on Einstein's formulation of that theory in 1905.

The answer to the first question is simple. Michelson, a genius in instrumentation and experimentation, never felt comfortable with the special theory. He was the first American scientist to receive a Nobel prize, in 1907. The absence of any mention in his citation**

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* In his reply to Veblen, Einstein gave the following interpretation of his statement: "Die Natur verbirgt ihr Geheimnis durch die Erhabenheit ihres Wesens, aber nicht durch List", "Nature hides its secret because of its essential loftiness, but not by menas of ruse" [E3]. In June 1966 Helen Dukas prepared a memorandum about this cause of events [D2].

** The citation reads: "For his optical precision instruments and the spectroscopical and metrological investigations carried out with their aid," [N1].

of the aether wind experiments is even less surprising than the absence of any mention of relativity in Einstein's citation fifteen years later. It is more interesting that Michelson himself did not mention these experiments in his acceptance speech [N1] -- not quite like Einstein, who responded to the award given him in 1922 for the photoelectric effect by delivering a lecture on relativity, in 1923 [E4]. Truly revealing, however, is Michelson's verdict on relativity given twenty years later in his book "Studies in Optics" [M12]. He noted that the theory of relativity "must be accorded a generous acceptance," and gave a clear exposé of Lorentz transformations and their consequences for the Michelson-Morley experiment and for the experiment of Armand Hippolyte Louis Fizeau (1819-1896) on the velocity of light in streaming water. Then follows his summation: "The existence of an ether appears to be inconsistent with the theory . . . But without a medium how can the propagation of light waves be explained? . . . How explain the constancy of propagation, the fundamental assumption (at least of the restricted theory) if there be no medium?"

This is not the lament of a single individual but of an era, though it was an era largely bygone in 1927, when Michelson's book came out. Michelson's words are the perfect illustration of the two main themes to be developed in this Chapter. The first one is that in the early days it was easier to understand the mathematics than the physics of special relativity. The second one is that it was not a simple matter to assimilate a new kinematics as a lasting substitute for the old dynamics.

Let us now turn to the second question, the influence of the Michelson-Morley experiment on Einstein's initial relativity paper [E5]. The importance of this question goes far beyond the minor issue of whether Einstein should have added a footnote at some place or other. Rather, its answer will help us to gain essential insights into Einstein's thinking and will prepare us for a subsequent discussion of the basic differences between the approaches of Einstein, of Lorentz and of Poincaré.

Michelson is neither mentioned in the first nor in any of Einstein's later research papers on special relativity. One also looks in vain for his name in Einstein's autobiographical sketch of 1949 [E6], in which the author describes his scientific evolution and mentions a number of scientists who did influence him. None of this should be construed to mean that Einstein at any time underrated the importance of the experiment. In 1907 Einstein was the first to write a review article on relativity [E7]. (This is also the first of his papers in which he went to the trouble of giving a number of detailed references.) Michelson and Morley are mentioned in that review; also in a semi-popular article which Einstein wrote in 1915 [E8]; again in the Princeton lectures of 1921 [E1]; and in the book "The meaning of relativity" [E9], (which grew out of the Princeton lectures), where Einstein called the Michelson-Morley experiment the most important one of all the null experiments on the aether drift.

However, neither in the research papers, nor in these four reviews does Einstein ever make clear whether he knew of the Michelson-Morley experiment before 1905. Correspondence is no help either. I have only come across one letter, written in 1923, by Michelson to Einstein [M13], (and none by Einstein to Michelson), in which Michelson, then head of the physics department at the University of Chicago, offers Einstein a professorship at his University. No scientific matters are mentioned. The two men finally met in Pasadena. There was great warmth and respect between them, as Helen Dukas (who was with the Einsteins in California) has told me. On January 15, 1931, at a dinner given in Einstein's honor at the Atheneum of Cal Tech, Einstein publicly addressed Michelson in person for the first and last time: "I have come among men who for many years have been true comrades with me in my labors. You, my honored Dr. Michelson, began with this work when I was only a little youngster, hardly three feet high. It was you who led the physicists into new paths, and through your marvelous experimental work paved the way for the development of the theory of relativity. You uncovered an insidious effect in the ether theory of light, as it then existed, and stimulated the ideas of H. A. Lorentz and FitzGerald out of which the special theory of relativity developed," [E10]. One would think that Einstein might have associated himself explicitly with Lorentz and Fitzgerald had he believed that the occasion warranted that. He was perceptive enough to know that this would haven been considered as an additional compliment to Michelson rather than as a lack of modesty.

Michelson was very ill at the time of that festive dinner. He died four months later. On July 17, 1931, Einstein, back in Berlin meanwhile, gave a speech in Michelson's memory before the Physikalische Gesellschaft of Berlin [E11]. The talk ended with a fine ancedote. In Pasadena Einstein had asked him whey he had spent so much effort on high precision measurements of the light velocity. Michelson had replied. "Weil es mir Spass macht," ("Because I think it is fun"). Einstein's main remark about the Michelson-Morley experiment was: "Its negative outcome has much increased the faith in the validity of the general theory of relativity." Even on this most natural occasion one does not find an acknowledgment of a direct influence of Michelson's work on his own development.

Nevertheless, the answers to both questions: Did Einstein know of Michelson's work before 1905? Did it influence his creation of the special theory of relativity? are: yes, unquestionably. We know this from discussions between Shankland and Einstein and from and address entitled "How I created the relativity theory" given by Einstein on December 14, 1922 at Kyoto University, (and referred to in what follows as the Kyoto address). Let us first note two oral statements made by Einstein to Shankland, written down by Shankland soon after they were made and published by him some time later [S6], as well as part of a letter which Einstein wrote to Shankland [S7]*.

a) Discussion on February 4, 1950. "When I asked him how he had learned of the Michelson-Morley experiment, he told me that he

* This letter, written at Shankland's request, was read before the Cleveland Physics Society on the occasion of the centenary of Michelson's birth.

had become aware of it through the writings of H. A. Lorentz, but only after 1905 [S.'s italics] had it come to his attention! 'Otherwise,' he said, 'I would have mentioned it in my paper.' He continued to say that experimental results which had influenced him most were the observations on stellar aberration and Fizeau's measurements on the speed of light in moving water. 'They were enough,' he said," [S6].

b) Discussion on October 24, 1952. "I asked Professor Einstein when he had first heard of Michelson and his experiment. He replied, 'This is not so easy, I am not sure when I first heard of the Michelson experiment. I was not conscious that it had influenced me directly during the seven years that relativity had been my life. I guess I just took it for granted that it was true.' However, Einstein said that in the years 1905-1909, he thought a great deal about Michelson's result, in his discussion with Lorentz and others in his thinking about general relativity. He then realized (so he told me) that he had also been conscious of Michelson's result before 1905 partly though his reading of the papers of Lorentz and more because he had simply assumed this result of Michelson to be true," [S6].

c) December 1952, letter by Einstein to Shankland. "The influence of the crucial Michelson-Morley experiment upon my own efforts has been rather indirect. I learned of it through H. A. Lorentz's decisive investigation of the electrodynamics of moving bodies (1895) with which I was acquainted before developing the special theory of relativity. Lorentz's basic assumption of an ether at rest seemed to me not convincing in itself and also for the reason that it was leading to an interpretation of the Michelson-Morley experiment which seemed to me artificial," [S7].

What do we learn from these three statements?

First, that memory is fallible. (Einstein was not well in the years 1950-1952 and knew already then that he did not have much longer to live.) There is an evident inconsistency between Einstein's quoted words of February 1950 and his two later statements. It seem sensible to attach more value to the later comments, made upon further reflection, and, therefore, to conclude that Einstein did know of Michelson and Morley before 1905. Also, one infers that oral history is a profession which should be pursued with care and caution.

Secondly, there is Einstein's opinion that aberration and the Fizeau experiment were enough for him. This is the most crucial statement Einstein ever made on the origins of the special theory of relativity. It shows that the principal argument which, ultimately, led him to the special theory was not so much the need to resolve the conflict between the Michelson-Morley result and the version of aether theory prevalent in the late nineteenth century but rather, independently of the Michelson-Morley experiment, the rejection of this nineteenth century edifice as inherently unconvincing and artificial.

In order to appreciate how radical Einstein's departure was from the ancestral views on these issues, it is necessary to compare his position with the "decisive investigation" published by Lorentz in 1895 [L4]. In Section 64 of that paper we find the following statement, italicized by its author: "According to our theory the motion of the earth will never have any first order [in v/c] influence whatever on experiments with terrestrial light sources." By Einstein's own account he knew this 1895 memoir in which Lorentz discussed, among other things, both the aberration of light and Fizeau experiment. Let us briefly recall what was at stake. Because of the velocity v of the earth, a star which would be at the zenith if the earth were at rest is actually seen under an angle α with the vertical, where

$$\mathrm{tg}\ \alpha = \frac{v}{c} \quad . \qquad (6.1)$$

The concept of an aether in absolute rest was actually introduced in 1818 by Fresnel in his celebrated letter to Dominique François Jean Arago (1786-1853) for the express purpose of explaining this aberration effect (which would be zero if the aether moved along with the earth). As to the Fizeau effect, Fresnel had predicted [F1] that if a liquid is moving through a tube with a velocity v relative to the aether and if a light beam traverses the tube in the same direction, then the net light velocity c' in the laboratory is given by

$$c' = \frac{c}{n} + v\,(1- \frac{1}{n^2}), \qquad (6.2)$$

where n is the refractive index of the liquid.* Fresnel derived this result from the assumption that light imparts elastic vibrations to the aether it traverses. According to him, the presence of the factor $(1- \frac{1}{n^2})$, (now known as Fresnel's drag coefficient) expresses the fact that light cannot acquire the full additional velocity v since it is partially held back by the aether in the tube. In 1851 Fizeau had found reasonable experimental agreement with Eq. (6.2) [F2].

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* Here it is assumed that the liquid acts as a non-dispersive medium.

Returning to Lorentz, he discussed both effects from the point of view of electromagnetic theory and gave a new derivation of the Fresnel drag in terms of the polarization induced in a medium by incident electromagnetic waves.* Throughout this paper of 1895 the Fresnel aether is postulated explicitly. In rejecting these explanations of aberration and the Fizeau experiment, Einstein therefore chose to take leave of a first order _terra firma_ which had been established by the practitioners, limited in number but highly eminent and influential, of electromagnetic theory. I shall leave for the next chapter a discussion of his reasons for doing so. Note, however, that it was easy to take the Michelson-Morley experiments for granted (as Einstein repeatedly said he did) once a new look at the first order effects had led to the new logic of the special theory of relativity. Note also that this experiment was discussed at length in Lorentz' paper of 1895!

Finally, there is the Kyoto address. It was given in German and translated into Japanese by Jun Ishiwara (1881-1947)** [I1]. Part

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* For a calculation along these lines see the book by Panofsky and Phillips [P2].

** From 1912-1914 Ishiwara had studied physics in Germany and in Switzerland. He knew Einstein personally from those days. He also translated a number of Einstein's papers into Japanese.

of the Japanese text was retranslated into English [O1]. I quote a few lines from this English rendering: "As a student I got acquainted with the unaccountable result of the Michelson experiment, and then realized intuitively that it might be our incorrect thinking to take account of the motion of the earth relative to the aether, if we recognized the experimental result as a fact. In effect, this is the first route that led me to what is now called the special principles of relativity. . . . I had just a chance to read Lorentz's 1895 monograph, in which he had succeeded in giving a comprehensive solution to problems of electrodynamics within the first approximation, in other words, as far as the quantities of higher order than the square of the velocity of a moving body to that of light were neglected. In this connection I took into consideration Fizeau's experiment. . ."

In his first paper on relativity, Einstein mentions "the failed attempts to detect a motion of the earth relative to the 'light-medium'" without specifying what attempts he had in mind.* Neither Michelson nor Fizeau are mentioned though he knew of both. Einstein's discontent with earlier explanations of first order effects may have made the mystery of Michelson-Morley's second order null effect less central to him. Yet this "unaccountable result" did affect his thinking and thus a new question arises: Why, on the whole, was Einstein so reticent to acknowledge the influence of Michelson on him? I shall return to this question in the concluding section of the next chapter.

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* In a thoughtful article on Einstein and the Michelson-Morley experiment [H2], Holton raised the possibility that Einstein might have had in mind other null effects known by then, such as the absence of double refraction [B2], [R1] and the Trouton-Noble experiment [T1].

(b) The precursors

1. What Einstein knew. Historical accounts of electromagnetism in the late nineteenth century almost invariably cite a single phrase written by that excellent experimental and theoretical physicist, Heinrich Rudolf Hertz (1857-1894): "Maxwell's theory is Maxwell's system of equations."* By itself this is a witty, eminently quotable, and meaningless comment on the best that the physics of that period had to offer. The post-Maxwell, pre-Einstein attitude which eventually became preponderant was, briefly put, that electrodynamics is Maxwell's equations plus a specification of the charge and current densities contained in these equations plus a conjecture on the nature of the aether.

Maxwell's own theory placed the field concept in a central position. It did not abolish the aether but it did greatly simplify it. No longer was "space filled three or four times over with aethers," as Maxwell had complained [M1]. Rather, "Many workers and many thinkers have helped to build up the nineteenth century school of plenum, one ether for light, heat, electricity, magnetism," as Kelvin wrote in 1893, in the introduction to Hertz's "Electric waves," [K3]. However, there still were many nineteenth century candidates for this one aether, some but not all predating Maxwell's theory. There were the aethers of Fresnel, Cauchy, Stokes, Neumann, MacCullagh, Kelvin, Planck, and most probably there were others, distinguished by such properties as degree of homogeneity, compressibility and the extent to which the earth dragged the aether along. This explains largely (though not fully) why there was such a variety of

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* See the second volume of Hertz's collected works [h3] which is also available in English translation [H4].

post-Maxwellian Maxwell theories, the theories of Hertz, Lorentz, Larmor, Wiechert, Cohn, and probably others. Hertz was of course aware of these options.* After all, he had to choose his own aether, (one which is dragged along by the earth). Indeed, his dictum referred to earlier reads, more fully: "Maxwell's theory is Maxwell's system of equations. Every theory which leads to the same system of equations, and therefore comprises the same possible phenomena, I would consider as being a form or special case of Maxwell's theory."

The most important question for all these authors of aethers and makers of Maxwell theories was to find a dynamical understanding of the aberration of light, of Fresnel's drag, and, later, of the Michelson-Morley experiment. In a broad sense, all these men were precursors of Einstein who showed that theirs was a task both impossible and unnecessary. Einstein's theory is of course not just a Maxwell theory in the sense of Hertz. Rather, Einstein's resolution of the difficulties besetting the electrodynamics of moving bodies is cast in an all-embracing framework of a new kinematics, based on the first of the two major re-analyses of the problem of measurement which mark the break between the nineteenth and the twentieth century, (the other one being quantum mechanics).

It is not the purpose of this section on precursors to give a detailed discussion of the intelligent struggles by all those men named above. Rather, I shall mainly concentrate on Lorentz and Poincaré, the precursors of the new kinematics. A final comparison of the contributions of Einstein, Lorentz and Poincaré will be deferred until the next Chapter.

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* This was particularly stressed in [M14].

I shall not discuss Lorentz' finest contribution, his atomistic interpretation of the Maxwell equations in terms of charges and currents carried by fundamental particles (which he called charged particles in 1892, ions in 1895, and, finally, electrons in 1899), even though this work by Lorentz represents such a major advance in the development of electrodynamics. Rather, I shall confine myself largely to the evolution and the interpretation of the Lorentz transformation:

$$x' = \gamma\,(x - vt);\; y' = y\;;\; z' = z;\; t' = \gamma\,(t - vx/c^2), \qquad (6.3)$$

$$\gamma = (1 - v^2/c^2)^{-1/2}, \qquad (6.4)$$

which relates space-time variables (x', y' z', t') to other variables (x, y, z, t). (For the purpose of this section it suffices entirely to consider only relative motion in the x-direction.)

The main characters who will make their appearance in what follows are: Voigt, the first to write down Lorentz transformations; FitzGerald, the first to propose the contraction hypothesis; Lorentz himself; Larmor, the first to relate the contraction hypothesis to Lorentz transformations; and Poincaré. It should also be mentioned that, before 1900, others had begun to sense that the aether as a material medium might perhaps be dispensed with. Thus Paul Drude (1863-1906) wrote in 1900: "The conception of an ether absolutely at rest is the most simple and the most natural -- at least if the ether is conceived to be not a substance but merely space endowed with certain physical properties," [D3]; and Emil Cohn (1854-1944), in 1901: "Such a medium fills every element of our space; it may be a definite ponderable system or also the vacuum," [C1].

Of the many papers which have been written on the subject treated in this section, the following in particular have been of great help to me: Tetu Hirosige (1928-1975) on the aether problem [H3]; McCormmach on Hertz [M14]; Brock [B3] and Brush [B4] on FitzGerald; and Miller [M15] on Poincaré.

As to Einstein himself, in his first relativity paper he mentions only three physicists by name: Maxwell, Hertz and Lorentz. As he repeatedly pointed out elsewhere, in 1905 he only knew Lorentz' work up to 1895. It follows -- as we shall see -- that, in 1905, Einstein did not know of Lorentz' transformations. He invented them himself. Also, at that time he did not know any of Poincaré's scientific papers. In fact, when I asked Einstein in the early 1950's what influence Poincaré had had on his thinking about relativity, he replied to me that he never had read Poincaré's papers on that subject.

2. Voigt. It was noted in 1887 [V2] by Woldemar Voigt (1850-1919) that equations of the type

$$\Box \phi = 0 \tag{6.5}$$

$$\Box \equiv \frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} - \frac{\partial^2}{c^2\,\partial t^2}, \tag{6.6}$$

retain their form if one goes over to the new space-time variables

$$\begin{aligned} x' &= x - vt \\ y' &= y/\gamma \quad , \quad z' = z/\gamma \ , \\ t' &= t - vx/c^2 \quad . \end{aligned} \tag{6.7}$$

These are the Lorentz transformations (6.3) up to a scale factor. Voigt announed this result in a theoretical paper devoted to the Doppler principle. As an application of Eq. (6.7) he gave a derivation of the Doppler shift, but only for the long familiar longitudinal effect of order v/c. His new method has remained standard procedure to this day: Voigt made use of the invariance of the phase of a propagating plane light wave under the tranformation (6.7), [P3]. Since the Doppler shift is a purely kinematical effect (in the relativistic sense), it is irrelevant that Voigt's argument is set in the dynamical framework of the long forgotten elastic theory of light propagation.* As far as I know, Voigt never came back to Eq. (6.7) in his later work.

Lorentz was familiar with at least some of Voigt's work. In 1887 or 1888 the two men were in correspondence -- about the Michelson-Morley experiment [V3]. However, for a long time Lorentz seems not to have been aware of the Voigt transformation (6.7). Indeed, Lorentz's Columbia University lectures, given in 1906 and published in book form in 1909, contain the following comment: "In a paper. . . published in 1887 . . . and which to my regret has escaped my notice all these years, Voigt has applied to equations of the form [Eq. (6.5)] a transformation equivalent to [Eq. (6.3)]. The idea of the transformations [Eq. (6.3)] . . . might therefore have been borrowed from Voigt and the proof that it does not alter the form of the equations for the <u>free</u> ether is contained in his paper," [L7]. Although these lines were written after Einstein's work of 1905, they still contain a reference to the aether. So does the second edition of Lorentz' book, published in 1915. I shall have more to say on this subject in the next chapter.

3. <u>FitzGerald</u>. The collected papers of the Irish physicist George Francis FitzGerald (1851-1901), published in the year after his death under the editorship of his friend Joseph Larmor (1857-1942) [L8], show that FitzGerald belonged to the small and select group of physicists who participated very

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* According to this theory light is propagated as a result of oscillations in an elastic incompressible medium.

early in the further development of Maxwell's theory. (In 1899 he was awarded a Royal Medal for his work in optics and electrodynamics by the Royal Society, of which he was a member.) However, this handsome volume does not contain the very brief paper for which Fitzgerald is best remembered, the one dealing with the hypothesis of the contraction of moving bodies. This paper appeared in 1889 in the American Journal Science [F3], under the title "The Ether and the Earth's Atmosphere." It reads, in full: "I have read with much interest Messrs. Michelson and Morley's wonderfully delicate experiment attempting to decide the important question as to how far the ether is carried along by the earth. Their result seems opposed to other experiments showing that the ether in the air can be carried along only to an inappreciable extent. I would suggest that almost the only hypothesis that can reconcile this opposition is that the length of material bodies changes, according as they are moving through the ether or across it, by an amount depending on the square of the ratio of their velocities to that of light. We know that electric forces are affected by the motion of the electrified bodies relative to the ether, and it seems a not improbable supposition that the molecular forces are affected by the motion, and that the size of a body alters consequently. It would be very important if secular experiments on electrical attractions between permanently electrified bodies, such as in a very delicate quadrant electrometer, were instituted in some of the equatorial parts of the earth to observe whether there is any diurnal and annual variation of attraction -- diurnal due to the rotation of the earth being added and subtracted from its orbital velocity, and annual similarly for its orbital velocity and the motion of the solar system."

Here, for the first time, appears in the literature the proposal of what now is called the FitzGerald-Lorentz contraction. The formulation is qualitative,and primitive by modern standards. Consider the statement: "The length of material bodies changes according as they move though the aether. . ." First of all, there is (of course) still an aether. Secondly, the change of length is considered (if I may borrow a later phrase of Einstein) to be objectively real, it is an absolute change, not a change relative to an observer at rest. Consider next the statement about the molecular forces being affected by the motion. The author clearly has in mind a dynamical contraction mechanism which presses the molecules together in their motion through the aether.

FitzGerald's hypothesis was referred to several times in lectures (later published) by Oliver Joseph Lodge (1851-1940) [B2]. Larmor too properly credited FitzGerald in the introduction to his collected works: "He [F.] was the first to suggest . . . that motion through the aether affects the dimensions of solid molecular aggregations," [L9]. Elsewhere in that same book we find FitzGerald himself mentioning the contraction hypothesis, in 1900. In that year Larmor's essay "Aether and Matter" [L10] came out. In a review of this book, FitzGerald wrote that, in the analysis of the Michelson-Morley experiment, "He [Larmor] has to assume that the length of a body depends on whether it is moving lengthwise or sideways through the ether" [L11], without referring, however, to his own suggestion made more than ten years earlier!

FitzGerald's curious silence may perhaps be explained in part by what he wrote to his friend Oliver Heaviside (1850-1925): "As I am not in the least sensitive to having made mistakes, I rush out with all sorts of crude notions in hope that they may set others thinking and lead to some advance,"

[F4]. Perhaps, also, he was held back by an awareness of those qualities of his which were described by Heaviside soon after his death: "He had, undoubtedly, the quickest and most original brain of anybody. That was a great distinction; but it was, I think, a misfortune as regards his scientific fame. He saw too many openings. His brain was too fertile and inventive. I think it would have been better for him if he had been a little stupid - I mean not so quick and versatile but more plodding. He would have been better appreciated, save by a few" [O2].

Lorentz was one of those who appreciated FitzGerald the way he was.

4. Lorentz. The first paper by Lorentz relevant to the present discussion is the one of 1886 - that is, prior to the Michelson-Morley experiment - in which he criticized Michelson's theoretical analysis of the 1881 Potsdam experiment [L3]. Since the main purpose of Lorentz' paper was to examine how well Fresnel's stationary aether fit the facts, he re-examined the aberration and Fizeau effects and noted in particular another achievement (not yet mentioned) of Michelson and Morley: their repetition of the Fizeau experiment with much better accuracy, which bore out Fresnel's prediction for the drag coefficient in a much more quantitative way than was known before [M16]. Since, at that time, Lorentz had a right to be dubious about the precision of the Potsdam experiment, he concluded that there was no particular source for worry: "It seems doubtful in my opinion that the hypothesis of Fresnel has been refuted by experiment," [L3].

We move to 1892, the year in which Lorentz publishes his first paper on his atomistic electromagnetic theory [L12]. The Michelson-Morley experiment has meanwhile been performed and Lorentz is now deeply concerned (as was noted before): "This experiment has been puzzling me for a long time and in the end I have been able to think of only one means of reconciling it with Fresnel's theory. It consists in the supposition that the line joining two points of a solid body, if at first parallel to the direction of the earth's motion, does not keep the same length when it is subsequently turned through 90°," [L13]. If this length be ℓ in the latter position then, Lorentz notes, Fresnel's aether is alright if the length in the former position equals ℓ' where

$$\ell' = \ell \left(1 - \frac{v^2}{2c^2}\right) , \qquad (6.8)$$

the lasting answer to second order. In order to interpret this result, Lorentz assumes that molecular forces, like electromagnetic forces, "act by means of an intervention of the aether" and that a contraction effect $0(v^2/c^2)$ cannot be excluded on any known experimental grounds.

These conclusions agree in remarkable detail with FitzGerald's earlier proposal: Save the aether by its dynamical intervention on the action of molecular forces. In 1892, Lorentz was still unaware of FitzGerald's earlier remarks, however.

The fall of 1894: Lorentz writes to FitzGerald, telling him that he has learned of the latter's hypothesis via a paper by Oliver Lodge of 1893, informing FitzGerald that he had arrived at the same idea in his paper of 1892, and asking him where he has published his ideas so that he can refer to them [L14]. A few days later FitzGerald replies: His paper was sent to Science, "but I do not know if they ever published it ... I am pretty sure that your publication is prior to any of my printed publications,"(!) [F5]. He also expresses his delight at hearing that Lorentz agrees with him "for I have been rather laughed at for my view over here."

From that time on Lorentz used practically every occasion to point out that he and FitzGerald had independently arrived at the contraction idea. Already in his memoir of 1895 he wrote of "a hypothesis ... which has also been arrived at by Mr. FitzGerald, as I found out later," [L15]. This paper not only brings to a close the history of the FitzGerald-Lorentz contraction but also marks the beginning of Lorentz' road towards the Lorentz transformations, our next subject.

In the paper of 1895 Lorentz proved the following "theorem of corresponding states." Consider a distribution of non-magnetic substances described, in a coordinate system $\vec{x},t$ which is at rest relative to the aether, by $\vec{E}$, $\vec{H}$, $\vec{D}$, the electric, magnetic and electric displacement fields respectively. $\vec{D} = \vec{E} + \vec{P}$, $\vec{P}$ is the electric polarization. Consider a second coordinate system $\vec{x}'$, t' moving with the velocity v relative to the ($\vec{x}$, t)-system. Then, to first order in v/c, there is a corresponding state in the second system in which $\vec{E}'$, $\vec{H}'$, P are the same functions of $\vec{x}'$, t' as $\vec{E}$, $\vec{H}$, $\vec{P}$ are of $\vec{x}$, t, and where

$$\vec{x}' = \vec{x} - v\,t, \qquad (6.9)$$

$$t' = t - \vec{v}\vec{x}/c^2, \qquad (6.10)$$

$$\vec{E}' = \vec{E} + \vec{v}\mathrm{x}\vec{H}/c, \qquad (6.11)$$

$$\vec{H}' = \vec{H} - \vec{v}\mathrm{x}\vec{E}/c. \qquad (6.12)$$

Like Voigt had done before him, Lorentz regarded the transformations (6.9), (6.10) only as a convenient mathematical tool for proving a physical theorem, to wit, the independence to O(v/c) of terrestrial optical experiments from the motion of the earth, (a result already mentioned in Section (a)). Eq. (6.9) was obviously familiar to Lorentz but the novel Eq. (6.10) led him to introduce significant new terminology. He proposed to call t the "general time" and t' the "local time", [L16]. Although he did not say so explicitly, it is evident that to him there was, so to say, only one true time: t.

One last remark on the 1895 paper. It contains another novelty, the assumption that an "ion" with charge e and velocity v is subject to a force $\vec{K}$:

$$\vec{K} = e \left[\vec{E} + \vec{v} \times \vec{H}/c \right], \tag{6.13}$$

the Lorentz force (Lorentz called it the "electrische Kraft" [L17]).

As has been noted repeatedly, Einstein knew of Lorentz' work only up to 1895. Thus Einstein was aware of no more and no less than the following: Lorentz' concern about the Michelson-Morley experiment, his "first order Lorentz transformation," Eqs. (6.9) and (6.10), his proof of the first order theorem for optical phenomena, his need to supplement this proof with the ad hoc contraction hypothesis, and, finally, his new postulate of the Lorentz force, Eq. (6.13).

As a conclusion to the contributions of Lorentz prior to 1905, the following three papers need to be mentioned.

1898: Lorentz discusses the status of his work in a lecture given in Düsseldorf [L18]. It is essentially a summary of what he had written in 1895.

1899: He gives a "simplified version" of his earlier theory [L19]. Five years later he characterized this work as follows: "It would be more satisfactory, if it were possible to show, by menas of certain fundamental assumptions, and without neglecting terms of one order of magnitude or another, that many electromagnetic actions are entirely independent of the motion of the system. Some years ago [in 1899] I have already sought to frame a theory of this kind," [L20]. In 1899 he wrote down the transformations

$$x' = \varepsilon\gamma\ (x - vt) \tag{6.14}$$

$$y' = \varepsilon y,\ z' = \varepsilon z, \tag{6.15}$$

$$t' = \varepsilon\gamma\ (t - vx/c^2), \tag{6.16}$$

which are the Lorentz transformations Eq. (6.3) up to a scale factor ε. He noted (among other things) that "the dilatations determined by [Eqs. (6.14), (6.15)] are precisely those which I had to assume in order to explain the experiment of Mr. Michelson"! Thus the reduction of the FitzGerald-Lorentz contraction to a consequence of Lorentz transformations* is a product of the nineteenth century. Lorentz referred to t' defined by Eq. (6.16) as a "modified local time". Concerning the scale factor ε he remarked that it had to have a well defined value which one only can determine "by a deeper knowledge of the phenomena." Note that it is of course not necessary for the interpretation of the Michelson-Morley experiment to know what ε is.(As for all optical phenomena in free space, one may allow not only for Lorentz invariance but also for scale invariance, in fact for conformal invariance.) In 1899 Lorentz did not examine whether his theorem of corresponding states could be adapted to the transformations (6.14) - (6.16).

1904: Lorentz finally writes down the transformations (6.3) - (6.4), [L20]. He fixes ε to be equal to one from a discussion of the transformation

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* For the simple mathematics of this reduction see standard textbooks, e.g. [P4].

properties of the equation of motion of an electron in an external field. This time he attempts to prove a theorem of corresponding states (that is, Lorentz covariance) for the inhomogeneous Maxwell-Lorentz equations. He makes an error in the transformation formulae for velocities.* As a result he does not obtain the covariance beyond the first order in v/c.**

I shall return to this 1904 paper in the next Chapter. However, inasfar as the history of relativistic kinematics is concerned, the story of Lorentz as precursor to Einstein is herewith complete.

5. Larmor. Larmor's prize winning essay "Aether and Matter" [L10] was completed in 1898 and came out in 1900. Not only does it contain the exact transformations (6.3) - (6.4) but also the proof that one arrives at the FitzGerald-Lorentz contraction with the help of these transformations [L21]. Larmor was aware of Lorentz' paper of 1895 and quoted it at length. But he could not know the 1899 paper.

It is true that Larmor's reasonings are often obscured by his speculations (of no interest here) about dynamical interrelations between aether and matter. However, there is no doubt that he gave the Lorentz transformations and the resulting contraction argument before Lorentz independently did the same. It is a curious fact that neither in the correspondence between Larmor and Lorentz*** nor in Lorentz' papers is there any mention of this contribution by Larmor.

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* See Ref. [L20], Eq. (8).

** Compare Eqs. (2) and (9) in Ref. [L20].

*** This correspondence is deposited in the Ryksarchief in the Hague. I am grateful to Dr. A.Kox for information related to this correspondence.

The first time I became aware of Larmor's work was in the early 1950's when Adriaan Fokker (1887-1968) told me that Larmor had the Lorentz transformations before Lorentz. Alas, I never asked Fokker (an ex-student of Lorentz) what Lorentz himself had to say on this subject.

6. Poincaré. In 1898 there appeared an utterly remarkable article in the "Revue de Metaphysique et de Morale," written by Poincaré,- and entitled "La mesure du temps," [P5].* In this paper, the author notes that "We have no direct intuition about the equality of two time intervals. People who believe to have this intuition are the dupes of an illusion." The italics are Poincaré's. He further remarks: "It is difficult to separate the qualitative problems of simultaneity from the quantitative problem of the measurement of time; either one uses a chronometer, or one takes into account a transmission velocity such as the one of light, since one cannot measure such a velocity without measuring a time." After discussing the inadequacies of earlier definitions of simultaneity Poincaré concludes as follows: "The simultaneity of two events or the order of their succession, as well as the equality of two time intervals must be defined in such a way that the statements of the natural laws be as simple as possible. In other words, all rules and definitions are but the result of an unconscious opportunism." These lines read like the general program for what would be given concrete shape seven years later. Other comments in this paper indicate that Poincaré wrote this article in response to several other recent publications on the measurement of time.

In 1898 Poincaré did not mention any of the problems in electrodynamics. He did so on two subsequent occasions, in 1900 and in 1904. The style is

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* This essay is available in English as Chapter 2 in "The Value of Science," [P9].

again programmatic, and these times the aether questions are central. "Does the aether really exist?" He asked in his opening address to the Paris Congress of 1900 [P6].* "One knows where our belief in the aether stems from. When light is on its way to us from a far star . . . it is no longer on the star and not yet on the earth. It is necessary that it is somewhere, sustained, so to say, by some material support." Speaking of the Fizeau experiment, he remarked that there "one believes one can touch the aether with one's fingers." Turning to theoretical ideas, he noted that the Lorentz theory "is the most satisfactory one we have".** However, he considered it a drawback that the independence of optical phenomena from the motion of the earth should have separate explanations in first and in second order. "One must find one and the same explanation for one and for the other, and everything leads us to anticipate that this explanation will be valid for higher order terms as well and that the cancelation of the [velocity dependent] terms will be rigorous and absolute." His reference to cancelations would seem to indicate that he was still thinking about a conspiracy of dynamical effects.

In 1904, he returned to the same topics, once again in a programmatic way, in his address to the International Congress of Arts and Science at St. Louis, Missouri [P1]*. "What is the aether, how are its molecules arrayed, do they attract or repel each other?" He expressed his unease

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* This address is available in English as Chapters 9 and 10 in "Science and Hypothesis" [P7].

** During the period 1895-1900, Poincaré considered it a flaw of the theory that it did not satisfy momentum conservation in the Newtonian sense, that is, conservation of momentum for matter only. He withdrew this objection soon afterward.

*** This address is available in English as Chapters 7 - 9 in "The Value of Science", [P9].

with the idea of an absolute velocity: "If we succeed in measuring something we will always have the freedom to say that it is not the absolute velocity, and if it is not the velocity relative to the aether, it can always be the velocity relative to a new unknown fluid with which we would fill space." He gently chided Lorentz for his accumulation of hypotheses. And then he goes beyond Lorentz, in treating local time as a physical concept. He considers two observers in uniform relative motion who wish to synchronize their clocks by means of light signals. "Clocks regulated in this way will not mark the true time, rather they mark what one may call the local time." All phenomena seen by one observer are retarded relative to the other, but they all are equally retarded (Poincaré points out) and "as demanded by the relativity principle [the observer] cannot know whether he is at rest or in absolute motion". Poincaré is getting close. But then, with all respect, he falters. "Unfortunately [this reasoning] is not sufficient and complementary hypotheses are necessary [my italics]; one must assume that bodies in motion suffer a uniform contraction in their direction of motion." The reference to complementary hypotheses makes clear that relativity theory had not yet been discovered.

Poincaré concluded this lecture with another of his marvelous visions: "Perhaps we must construct a new mechanics, of which we can only catch a glimpse, . . . in which the velocity of light would become an unpassable limit." But, he added, "I hasten to say that we are not yet there and that nothing yet proves that [the old principles] will not emerge victoriously and intact from this struggle."

The account of Einstein's presursors ends here, with a note of indecision. Lorentz transformations had been written down. Simultaneously had been questioned. Light velocity as a limit velocity had been conjectured. But, prior to 1905, there was no relativity theory. Let us now turn to what Poincaré did next, not as a precursor to Einstein but essentially simultaneously with him.

(c) Poincaré in 1905

All three papers just mentioned are qualitative in character. Poincaré, one of the very few true leaders in mathematics and mathematical physics of his day, knew of course the electromagnetic theory in all its finesses. He had published a book on optics in 1889 [P10] and one on electromagnetic theory in 1901 [P11]. In 1895 he had written a series of papers on Maxwellian theories [P12]. From 1897-1900 he wrote several articles on the theory of Lorentz [P13]. All this work culminated in two papers which he completed in 1905. Both bear the same title: "Sur la dynamique de l'électron." The occurrence of the term "dynamics" is quite significant. So, for other reasons, is the following sequence of dates:

June 5, 1905. Poincaré communicates the first of these two papers to the Académie des Sciences in Paris [P14].

June 30, 1905. Einstein's first paper on relativity is received by the Editor of the Annalen der Physik.

July, 1905. Poincaré completes his second paper which appears in 1906 [P15].

The first of the Poincaré papers is in essence a summary of the second much longer one. The content of articles is partly kinematical, partly dynamical. Here I shall only discuss their kinematic part, leaving the remainder until the next Chapter.

The June paper begins with the remark that neither the aberration of light and related phenomena nor the work of Michelson reveals any evidence for an absolute notion of the earth. "It seems that this impossibility of demonstrating absolute motion is a general law of nature."

Next, Poincaré refers to the contraction hypothesis and to Lorentz' paper of 1904 [L20] in which -- as Poincaré has it -- Lorentz had succeeded to modify the hypothesis "in such a way as to bring it in accordance with the complete impossibility of determining absolute motion." This statement is not quite correct, since (as was mentioned earlier) Lorentz had not succeeded in proving the covariance of the inhomogeneous Maxwell-Lorentz equations. Poincaré was to return to this point in July. However, already in June he had the correct transformation properties of the velocities, the point Lorentz had missed. "I have been led to modify and complete [Lorentz' analysis] in certain points of detail."

Poincaré then turns to the transformations (6.14) - (6.16), "which I shall name after Lorentz" and continues: "The ensemble of all these transformations, together with the ensemble of all spatial rotations must form a group; but in order for this to be so it is necessary that* $\varepsilon = 1$; one is thus led to assume that $\varepsilon = 1$, a result which Lorentz had obtained in another way."

The final topic discussed in this paper concerns gravitation. Following Lorentz' dynamical picture, Poincaré reasons in a more general and abstract way that all forces should transform the same way under Lorentz transformations. He concludes that therefore Newton's laws need modification and that gravitational waves should exist which propagate with the velocity of light! Finally, he points out that the resulting corrections to Newton's law must be $O(v^2/c^2)$ and that the precision of astronomical data does not seem to be so high as to rule out effects of this order.

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* I use the notation of Eqs. (6.14)-(6.16), Poincare used the symbol ℓ instead of ε.

The July paper of Poincaré gives many more details. Its section 1, entitled: "Lorentz transformation", contains the complete proof of covariance of electrodynamics. "It is here that I must point out for the first time a difference with Lorentz," [P16]. Section 4 contains a discussion of "a continuous group which we shall call the Lorentz group." Poincaré explains his argument for $\varepsilon = 1$: start from Eqs. (6.14) - (6.16). Consider the inverse of these transformations, that is, replace v by -v. Clearly

$$\varepsilon(v) \; \varepsilon(-v) = 1 , \qquad (6.17)$$

Moreover from a rotation of 180° around the y-axis it follows that

$$\varepsilon(v) = \varepsilon(-v), \qquad (6.18)$$

so that

$$\varepsilon(v) = 1. \qquad (6.19)$$

Once it is settled that $\varepsilon = 1$, the Lorentz transformations have the property:

$$x^2 + y^2 + z^2 - c^2t^2 \text{ remains unaltered.} \qquad (6.20)$$

Insert 6.36a

Poincaré did of course not know that, in June, someone else had independently noted the group properties of Lorentz transformations and had derived Eqs. (6.19) - (6.21) by an almost identical argument.

I shall return later to the efforts by Lorentz in 1904 and by Poincaré in 1905 to give a theory of the electron. However, I believe to have now presented all the evidence which bears on the roles of Lorentz and of Poincaré in the development of relativity theory. I shall now let their case rest for a while until the discussion of Einstein's first two papers on the subject will have been completed. Thereafter an attempt will be

Insert for p. 6.36

In showing the group property of Lorentz transformations, Poincaré remarked that the "product" of two transformations (6.3), one with velocity v_1, the other with v_2, results in another Lorentz transformation with velocity v given by

$$v = \frac{v_1 + v_2}{1 + v_1 v_2/c^2} \qquad (6.21)$$

made to compare the contributions of all three men.

As a last step preparatory to the account of this work by Einstein I should like to mention what little we know about his thoughts on the subject prior to 1905.

(d) Einstein before 1905

Einstein's curiosity in electromagnetic theory goes back at least as far as his Pavia days of 1895, which followed his escape from the hated high school in Munich.* His interest in doing his own aether drift experiment was aroused while being a student at the ETH. The following series of brief and rather disconnected remarks bear on his concern with electrodynamics in the decade preceding his creation of the special theory of relativity.

1. The Pavia essay.** In 1895 Einstein sent a manuscript entitled "Über die Untersuchung des Ätherzustandes im magnetischen Felde" ("On the examination of the state of the aether in a magnetic field") to his uncle Caesar Koch in Belgium. This paper -- which Einstein never published*** -- was accompanied by a covering letter in which he wrote: "[The manuscript] deals with a very special theme and is . . . rather naive and incomplete, as can be expected from a young fellow." In the opening lines of the essay he asks the reader's forbearance: "Since I completely lacked the material for penetrating deeper into the subject, I beg that this circumstance will not be interpreted as superficiality."

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* See Chapter 3.

** In 1950 Einstein dated this manuscript to be from 1894 or 1895. It was sent to Caesar Koch in 1895, since in its covering letter Einstein tells of his intent to go to the ETH and adds: "In the next letter I shall write you what may come of this."

*** Both the essay and its covering letter were reproduced in a paper by Mehra [M17].

The main questions raised in the essay are: How does a magnetic field, generated when a current is turned on, affect the surrounding aether? How, in turn, does this magnetic field affect the current itself? Evidently Einstein believed in an aether at that time. He regarded it as an elastic medium and wondered in particular how "the three components of elasticity act on the velocity of an aether wave" which is generated by turning on the current. He came to the following main conclusion: "Above all it ought to be [experimentally] shown that there exists a passive resistance to the electric current's ability for generating a magnetic field; [this resistance] is proportional to the length of the wire and independent of the cross section and the material of the conductor." Thus the young Einstein discovered independently the qualitative properties of self-induction, a term not used by him (a phenomenon of course known by then)! In this paper he mentions "the wonderful experiments of Hertz". I do not know how he became aware of Hertz' work. At any rate, it is clear that he knew already then that light is an electromagnetic phenomenon. It is equally clear that he did not yet know Maxwell's papers however.

2. The Aarau question. In his final autobiographical note Einstein wrote: "During that year* in Aarau the question came to me: If one runs after a light wave with [a velocity equal to the] light velocity, then one would encounter a time independent wave field. However, something like that does not seem to exist! This was the first juvenile thought experiment which has to do with the special

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* That is, sometime between October 1895 and the early fall of 1896, see Chapter 3.

theory of relativity," (and he added: "Invention is not the product of logical thought , even though the final product is tied to a logical structure"), [E12]. Also in his more extensive autobiographical notes, published in 1949, Einstein remarked that "After ten years of reflection such a principle [special relativity] resulted from [this] paradox upon which I had already hit at the age of sixteen," [E6].

3. The ETH student. Since Rudolf Kayser, Einstein's son in law and biographer, was himself not a physicist, it is hard to believe that the following lines from his biography could have come from anyone but Einstein himself. "He encountered at once, in his second year of college,* the problem of light, ether and the earth's movement. This problem never left him. He wanted to construct an apparatus which would accurately measure the earth's movement against the ether. That his intention was that of other important theorists, Einstein did not yet know. He was at that time unacquainted with the positive contributions, of some years back, of the great Dutch physicist Hendrik Lorentz, and with the subsequently famous attempt of Michelson. He wanted to proceed quite empirically, to suit his scientific feeling of the time, and believed that an apparatus such as he sought would lead him to the solution of the problem, whose far reaching perspectives he already sensed. But there was no chance to build this apparatus. The skepticism of his teachers was too great, the spirit of enterprise too small. Albert had thus to turn aside from his plan, but not to give it up forever. He still expected to approach the major questions of physics by observation and experiment," [R2].

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* That is, the academic year 1897-1898.

As to electromagnetic theory, Einstein was not offered a course on this subject in his ETH days. As was already noted in Chapter 3, he learned this theory from Foeppel's textbook.

4. The Winterthur letter. A letter by Einstein to Grossmann, written in 1901 from Winterthur, informs us that aether drift experiments were still on Einstein's mind. "A new and considerably simpler method for investigating the motion of matter relative to the light-aether has occurred to me. If the merciless fates would just once give me the necessary quiet for its execution!" [E13]. Since there are no preliminaries to this statement, one gains the impression that Grossmann knew something about a previous method which Einstein must have had in mind when both men were together at the ETH.

This letter also shows that Einstein still believed in an aether as late as 1901.

5. The Bern lecture. On the evening of December 5, 1903, Albert Einstein, technical expert 3rd class with provisional appointment, held a lecture in the conference room of the hotel "Storchen" in Bern before the Naturforschenden Gesellschaft Bern. He had been elected to membership of this society on May 2, 1903. The subject of his December lecture was "Theorie der elektromagnetischen Wellen," [F6]. It would obviously be extraordinarily interesting to know what Einstein said that evening. However, to the best of my knowledge, no record of his talk exists.

6. The Kyoto address. Finally I quote another part of the Kyoto address which Einstein gave in 1922. Before doing so, I should point out that I do not know what precise times are referred to in the statements "I then thought. . ." and "In those days. . .".

"I then thought I would want to prove experimentally to myself in any way the flow of the aether to the earth, that is to say, the motion of the earth. In those days when this problem arose in my mind, I had had no doubt as to the existence of the aether and the motion of the earth in it. Meanwhile I had a plan to try to test it by means of measuring the difference of heats which were to appear in a thermocouple according as the direction along or against which the light from a single source was made to reflect by suitable mirrors, as I presupposed there should be a difference between the energies of reflected lights in the opposite directions. This idea was as similar as the one in the Michelson experiment, but I had not carried out the experiment yet to obtain any definite result," [01].

7. Summary. In the same lecture Einstein remarked: "It is never easy to talk about how I got to the theory of relativity because there would be various concealed complexities to motivate human thinking and because they worked with different weights," [01]. Even with this admonition in mind, it would seem that the following is a fair summary of Einstein's work and thoughts on electrodynamics prior to 1905.

Einstein's first important creative act dates from his high school days, when he discovered self-induction. It is of course not a contribution which should be associated with his name. At least twice he had an idea for a new experimental method to measure the aether drift. He intended to perform these experiments himself but did not succeed in doing so, either because his teachers would not let him (Kayser biography) or because he did not have enough free time (letter to Grossmann). He believed in an aether at least until 1901 (same letter). Sometime during 1895 or 1896 the thought struck him that light cannot be transformed to rest (Aarau). He knew of the Michelson-Morley experiment which, however, was not as crucial for his formulation of specail relativity as were the first order effects, the aberration of light and the Fresnel drag (statement to Shankland, Kyoto address). He knew the 1895 paper of Lorentz. He did not know the Lorentz transformations. He did not know any of Poincaré's scientific writings.

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6.8 too cryptic
6.34 notion

I. INTRODUCTORY

1. Purpose and Plan

One day, it must have been around 1950, as I accompanied Einstein on a walk from The Institute for Advanced Study to his home, he suddenly stopped, turned to me and asked me if I really believed that the moon only exists if I look at it. The nature of our conversation was not particularly metaphysical. Rather, we were discussing the quantum theory, in particular what is doable and knowable in the sense of physical observation. Obviously, the twentieth century physicist does not claim to have the definitive answer to this question. He does know, however, that the answer given by his nineteenth century ancestors will no longer do. They were almost exactly right, to be sure, in so far as conditions of every day life are concerned, but their answer cannot be extrapolated to things moving nearly as fast as light, or things that are as small as atoms, or in some respects to things that are as heavy as stars. We know better now than before that what man can do under the best of circumstances depends on a careful specification of what those circumstances are. That, in very broad terms, is the lesson of the theory of relativity which Einstein created and of quantum mechanics which he eventually accepted as (in his words) the most successful theory of our period but which, he believed, was nonetheless only provisional in character.

We walked on and continued talking about the moon and the meaning of the expression "to exist" in so far as it refers to inanimate objects. When we reached 112 Mercer Street I wished him a pleasant lunch, then returned to the Institute. As had been the case on many earlier occasions, I had enjoyed the walk and felt better because of the discussion even though it had ended inconclusively. I was used to that by then, and as I walked back I wondered once again about the question: Why does this man, who contributed so incomparably much to the creation of modern physics, remain so attached to the nineteenth century view of causality?

In order to make that question more precise it is necessary to know what Einstein's credo was in regard not just to quantum physics but to all of physics. That much I believe I know and will endeavor to explain in what follows. In order to answer the question one needs to know not only what the credo was but also how it came to be adopted. My conversations with Einstein taught me little about that, not because the issue was purposely shunned but because it was not raised. Only many years after Einstein's death did I see the beginnings of an answer when I realized that, nearly a decade before the discovery of modern quantum mechanics, he had been the first to understand that the nineteenth century ideal of causality was about to become a grave issue in quantum physics. However, while I know more now about the evolution of his thinking than I did when I walked with him, I would not commit the folly of saying that I now understand why he chose to believe what he did believe. When Einstein was fifty he wrote an introduction to a biography of him by his son-in-law Rudolph Kayser in which he said: "What has perhaps been overlooked is the irrational, the inconsistent, the droll, even the insane, which nature, inexhaustibly operative, implants in an individual, seemingly for her own amusement. But these things are singled out only in the crucible of one's own mind." Perhaps this statement is too optimistic about the reach of self-knowledge. Certainly it is a warning, and a fair one, to any biographer not to overdo giving a firm answer to every question he may legitimately raise.

I should briefly explain how it came about that I went on that walk with Einstein and why we got to talk about the moon. I was born in 1918 in Amsterdam. In 1941 I received my Ph.D. with Léon Rosenfeld in Utrecht. Some time thereafter I went into hiding in Amsterdam. Eventually I was caught and sent to the Gestapo prison there. Those who were not executed were released shortly before V.E. Day. Immediately after the war I applied for postdoctoral fellowships at Niels Bohr's Institute as well as at The Institute for Advanced Study where I hoped to work with Pauli. I received a letter from Pauli saying he would support my application. I was accepted at both places and first went for one year to Kopenhagen. Some time after my arrival there came a period of several months during which I worked together with Bohr. The following lines from my account of that experience are relevant to the present subject. "I must admit that in the early stages of the collaboration I did not follow Bohr's line of thinking a good deal of the time and was in fact often quite bewildered. I failed to see the relevance of such remarks as that Schroedinger was completely shocked in 1926 when he was told of the probability interpretation of quantum mechanics, or a reference to some objection by Einstein in 1928, which apparently had no bearing whatever on the subject at hand. But it did not take very long before the fog started to lift. I began to grasp not only the thread of Bohr's arguments but also their purpose. Just as in many sports a player goes through warming up exercises before entering the arena, so Bohr would relive the struggles which it took before the content of quantum mechanics was understood and accepted. I can say that in Bohr's mind this struggle started all over every single day. This, I am convinced, was Bohr's inexhaustible source of identity. Einstein appeared forever as his leading spiritual partner - even after the latter's death he would argue with him as if Einstein were still alive," [P1].

In September 1946 I went to Princeton. The first thing I learned was that Pauli had gone to Zürich in the meantime. Bohr also came to Princeton that same month. Both of us attended the Princeton Bicentennial Meetings. I missed my first opportunity to catch a glimpse of Einstein, walking next to President Truman in the academic parade. However, shortly thereafter Bohr introduced me to Einstein who greeted a rather awed young man in a very friendly way. The conversation on that occasion soon turned to the quantum theory. I listened as the two of them argued. I recall no details but remember distinctly my first impressions: they liked and respected each other. With a fair amount of passion they were talking past each other. And, as had been the case with my first discussions with Bohr, I did not understand what Einstein was talking about.

Not long thereafter I encountered Einstein in front of the Institute and told him that I had not followed his argument with Bohr and asked if I could come to his office some time for further enlightenment. He invited me to walk home with him. So began a series of discussions which continued until shortly before his death.* I would visit with him in his office or accompany him (often together with Kurt Gödel) on his lunch time

* My stay at the Institute had lost much of its attraction because Pauli was no longer there. As I was contemplating returning to Europe, Robert Oppenheimer contacted me and informed me that he had been approached for the directorship of the Institute. He asked me to join him in building up physics there. I accepted. A year later I was appointed to a five-year membership and in 1950 to a professorship at the Institute, where I remained until 1963.

walk home. Less often I would visit him there. In all I saw him about once every few weeks. We always talked German, the language best suited to grasp the nuances of what he had in mind as well as the flavor of his personality. Only once did he visit my apartment. The occasion was a meeting of The Institute for Advanced Study faculty for the purpose of drafting a statement of our position in the 1954 Oppenheimer affair.

Einstein's company was comfortable and comforting to those who knew him. Of course he knew well that he was a legendary figure in the eyes of the world. He accepted this as a fact of life. There was nothing in his personality to promote such attitudes. Nor did he relish them. Privately he would express annoyance if he felt that his position was misused. I recall the case of Professor X who had been quoted by the newspapers as having found solutions to Einstein's generalized equations of gravitation. Einstein said to me "Der Mann ist ein Narr" and added that in his opinion X could calculate but could not think. X had visited Einstein to discuss this work and Einstein, always courteous, had said to him that if his results were true they would be important. Einstein was chagrined to have been quoted in the papers without this proviso. He said that he would keep silent on the matter but would not receive X again. According to Einstein the whole thing started because, in his enthusiasm, X had told some colleagues who saw the value of it as publicity for their university.

To those physicists who could follow his scientific thought and who knew him personally, the legendary aspect was never in the foreground -- yet it was never wholly absent. I recall an occasion, in 1947, when I gave a talk at the Institute about the newly discovered π and μ mesons. Einstein walked in just after I had begun. I remember that I was speechless for the brief moment necessary to overcome a sense of the unreal. I recall a similar moment during a symposium* held in Frick Chemical Laboratory in Princeton on March 19, 1949. The occasion was Einstein's seventieth birthday. Most of us were in our seats when Einstein entered the hall. Again there was this brief hush before we stood to greet him.

Nor do I believe that such reactions were typical only of those who were much younger than he. There were a few occasions when Pauli and I were both together with him. Pauli, not known for an excess of awe, was just slightly different in Einstein's company. One could perceive his sense of reverence. Bohr too was affected in a somewhat similar way, differences in scientific outlook notwithstanding.

Whenever I met Einstein our conversations might range far and wide but invariably the discussion would turn to physics. Such discussions would touch only occasionally on matters of past history. We mainly talked about the present and the future. When relativity was the issue he would often talk of his efforts to unify gravitation and electromagnetism and of his hopes for the next steps. His faith rarely wavered in the path he had chosen. Only once did he express a reservation to me when he said, in essence: I am not sure that differential geometry is the framework for further progress, but if it is, then I believe I am on the right track. (This remark must have been made sometime during his last few years.)

The main topic of discussion was quantum physics, however. Einstein never ceased to ponder the meaning of the quantum theory. Time and time again the argument would turn to quantum mechanics and its interpretation.

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* The speakers were J.R. Oppenheimer, I.I. Rabi, E.P. Wigner, H.P. Robertson, S.M. Clemence and H. Weyl.

He was explicit in his opinion that the most commonly held views on this subject could not be the last word, but he had also more subtle ways of expressing his dissent. For example he would never refer to a wave function as "die Wellenfunktion" but would always use a mathematical terminology: "die Psifunktion." I was never able to arouse much interest in him about the new particles which appeared on the scene in the late 1940's and especially in the early 1950's. It was apparent that he felt that the time was not ripe to worry about such things and that these particles would eventually appear as solutions to the equations of a unified theory. In some sense he may well prove to be right.

The most interesting thing I learned from these conversations was how Einstein thought, and, to some extent, who he was. Since I never became his co-worker the discussions were not confined to any particular problem. Yet we talked physics, often touching on topics of a technical nature to which I shall return in later chapters. In more general terms, we did not talk much about statistical physics, an area to which he had contributed so much but which no longer was in the center of his interests. If the special and the general theory of relativity came up only occasionally, that was because at that time the main issues appeared to have been settled. Recall that the renewed surge of interest in general relativity began just after his death. However, I do remember him talking about Lorentz, the one father figure in his life; also we once talked about Poincaré. If we argued so often about the quantum theory, that was more his choice than mine. It had not taken long before I grasped the essence of the Einstein-Bohr dialog: complementarity versus objective reality. It became clear to me, from listening not only to Einstein but also to Bohr, that the advent of quantum mechanics in 1925 represented a far greater break with the past than had been the case with the coming of special relativity in 1905 or of general relativity in 1915. That had not been obvious to me earlier, as I belong to the generation which was exposed to quantum mechanics in ready-made fashion. I came to understand how wrong I was in accepting a rather widespread belief that Einstein simply did not care anymore about the quantum theory. On the contrary, he wanted nothing more than to find a unified field theory which would not only join together gravitational and electromagnetic forces but which would also provide the basis for a new interpretation of quantum phenomena. About relativity he spoke with detachment, calling his discovery of the special theory "den Schritt," "the step." About the quantum theory he spoke with passion. The quantum was his demon. I learned only much later that Einstein once had said to his friend Otto Stern: "I have thought a hundred times as much about the quantum problems as I have about general realtivity theory,"[J1]. From my own experiences I can only add that this statement does not surprise me.

We talked of things other than physics, the news of the day, politics, Europe, America, the bomb, the Jewish destiny, and also of less weighty matters. I recall that one day I told Einstein a Jewish joke. Since he relished that very much I began to save good ones I heard for a next occasion. As I would tell him such a story, his face would change. Suddenly he would look much younger, almost like a naughty school boy. When the punch line came he would let go with a contented laughter, a memory I particularly cherish.

An unconcern with the past is a privilege of youth. In all the years I knew Einstein I never read a single one of his papers, on the simple grounds that what to a physicist was memorable in them I knew already and what was superseded I, as a physicist, did not need to know. It is now clear to me that I might have been able to ask him some very interesting questions, had I been less blessed with ignorance. I might then have known some interesting facts by now, but at a price. My discussions with Einstein never were historical interviews, they concerned live physics. I am glad it never was otherwise.

I did read Einstein's papers as the years went by and my interest in him as an historical figure grew. Thus it came about that I learned to follow his science and his life from the end to the beginnings. I gradually found out what is the most difficult task in studying past science: to forget, temporarily, what came afterward. The study of his papers, discussions with others who knew him, access to the Einstein Archives, personal reminiscences -- those are the ingredients which led to this book. Without disrespect nor lack of gratitude I have found the study of the scientific papers to be incomparably more important than anything else.

At this point the promised tour of this book starts. For ease I introduce the notation, to be used only in the next few pages, of referring to Chapter 3 as (3), and to Chapter 5, Section (c) as (5c). To repeat, symbols like [J1] indicate references which are found at the end of this chapter.

I shall begin by indicating how the personal biography is woven into this book. The early period, from Einstein's birth (1879) to the beginning of his academic career as Privatdozent in Bern (February 1908) is discussed in (3) which contains a sketch of his childhood, his school years, his brief religious phase, his student days, his difficulties in finding a job, and most of the period he spent at the Patent Office in Bern, a period that witnesses the death of his father, his marriage to Mileva Maric and the birth of his first son. In an addendum to (3) those Einstein biographies are noted which were particularly helpful to me. Most of these were read by Einstein himself. In (10a) we follow him from the time he began as a Privatdozent in Bern to the end (March 1911) of his associate professorship at the University of Zürich. His second son was born in that period. The next phase (11a) is his time as full professor in Prague (March 1911-August 1912), which is followed by his years (August 1912-April 1914) as a professor at the Federal Institute of Technology (ETH) in Zürich. The circumstances surrounding his move (April 1914) from Zürich to Berlin, his separation from Mileva and the two boys, and his reaction to the events of the first World War which started only months thereafter are described in (14a). The story of the Berlin days is continued in (16), ending with Einstein's permanent departure from Europe. This period includes years of illnesses which did not noticeably affect his productivity; his divorce from Mileva and remarriage to his cousin Elsa; and the death -- in his home in Berlin -- of his mother (all in (16a)). (16b) and (16c) are devoted to the abrupt emergence (in 1919) of Einstein (whose genius had by then already been fully recognized for some time by his scientific peers) as a charismatic world figure and to my views on the causes of this striking phenomenon. (16d) is devoted to Einstein's position as a public figure in German life, to his espousing of Zionism, and to the few years during which he traveled extensively. His years in Princeton are briefly described in (25b), the last years of his life in (26) and (27). The book ends with a detailed Einstein chronology (30).

Before I turn to a similar tour of the scientific parts I should like first to make a few comments on topics I will rarely touch on in what follows: Einstein and politics, and Einstein as a philosopher and humanist.

Whenever I think of Einstein and politics, the encounter comes back to me which I had with him in the late evening of Sunday, April 11, 1954. On the morning of that day a column by the Alsop brothers had appeared in the New York Herald Tribune, entitled: "Next McCarthy target: the leading physicists" which began by stating that the junior senator from Wisconsin was getting ready to play his ace-in-the-hole. I knew that the Oppenheimer case was about to break. That evening I was working in my office at the Institute when the phone rang and a Washington operator asked to speak to Dr. Oppenheimer. I replied that Oppenheimer was out of town. (In fact, he was in Washington.) Thereupon the operator asked for Dr. Einstein. I told her that Einstein was not at the office and that his home number was unlisted. The operator told me next that her party wished to speak to me. The Director of the Washington Bureau of the Associated Press came on the line, who told me that the Oppenheimer case would be all over the papers the next morning and that he was eager for a statement by Einstein as soon as possible. I quickly saw that there would be pandemonium on Mercer Street the next morning which might be avoided by a brief statement that evening. So I told my party that I would talk it over with Einstein, and that I would call back in any event. Then I drove to Mercer Street and rang the bell. Helen Dukas let me in, I apologized for the strange hour and said it would be good if I could talk briefly with the Professor, who meanwhile appeared at the top of the stairs dressed in his bathrobe, and asked: Was ist los? What is going on? He came down and so did Margot. After I had told him the reason for my call, Einstein burst out laughing. I was a bit taken aback and asked him what was so funny. He said that the problem was simple. All Oppenheimer needed to do was to go to Washington, tell the officials that they were fools and then go home. On further discussion we decided that a brief statement was called for, we drew it up and Einstein read it over the phone to the Washington man. The next day Helen Dukas was preparing lunch when she saw cars in front of the house and cameras being unloaded. In her apron (she told me) she ran out of the house to warn Einstein who was on his way home. When he arrived at the front door he declined to talk to the reporters and went in.

Was Einstein's initial response correct? Of course it was, even though his suggestion would not and could not be followed. I remember once attending a series of seminars by Bertrand de Jouvenel in which he told us that a main characteristic of a political problem is that it has no answer, only a compromise. Nothing was more alien to Einstein's scientific oeuvre and his extra-scientific life than to settle for compromise. He often spoke out on political problems, always steering to their answer. Such statements have often been called naive. In my view Einstein was not only not naive but highly aware of the nature of man's sorrows and his follies. His utterances on political matters were not always concerned with the practicable and I do not think that on the whole they were very influential. However, he gladly payed the price of sanity. As another comment on political matters I should like to relate a story I was told in 1979 by Israel's president Navon. After the death of Weizmann in November 1952, Ben Gurion and his cabinet decided to offer the presidency of Israel to Einstein. Abba Eban was instructed to proceed from Washington to

Oppenheimer's description: "There was always with him a wonderful purity at once childlike and profoundly stubborn," shows the writer's talent for almost understanding everything.

Princeton to transmit the offer. Shortly thereafter, in a private conversation, Ben Gurion turned to Navon (who at that time was his personal secretary) and said: "What are we going to do if he accepts?"

Einstein often lent his name to pacifist statements, for the first time in 1914 (14a). In 1916 he gave an interview to the Berlin paper die Vossische Zeitung about the work on Mach by his friend Friedrich Adler, who was then in jail for having shot and killed Karl Stürgkh, the prime minister of Austria [E1]. After the death of Leo Arons, a physicist he admired for his political courage but did not know personally, he wrote an obituary in the Sozialistische Monatshefte [E2]. After the assassination in 1922 of his friend Walther Rathenau, foreign minister of the Weimar republic and educated as a physicist, Einstein wrote of him in the Neue Rundschau: "It is no art to be an idealist if one lives in cloud cuckoo land. He, however, was an idealist even though he lived on the earth and knew its smell better than almost anyone else," [E3]. In 1923 Einstein became a co-founder of the Association of Friends of the New Russia. Together with people like Lorentz, Mame Curie, Henry Bergson he worked for a time as a member of the League of Nations' Committee for Intellectual Cooperation. Among those he proposed or endorsed for the Nobel prize for peace (30) were Masaryk; Herbert Runham Brown, honorary secretary of War Resisters International; Carl von Ossietzky, at the time the latter was in a German concentration camp; and the organization Youth Aliyah. He spoke up about the plight of the Jews, and helped. Numerous are the affidavits he personally signed in order to help Jews from occupied Europe emigrate to the U.S. He was ceaseless in his advocacy of disarmament.

Einstein's political orientation which (for simplicity) may be called leftist stemmed from his sense of justice, not from an approval of method or a sharing of philosophy. "In Lenin I honor a man who devoted all his strength and sacrificed his person to the realization of social justice. I do not consider his method to be proper," he wrote in 1929 [E4], and, shortly thereafter: "Outside Russia, Lenin and Engels are of course not valued as scientific thinkers and no one might be interested to refute them as such. The same might also be the case in Russia, but there one cannot dare to say so," [E5] . I shall not return to Einstein's interests in and involvements with political matters which are described at length in the important book "Einstein on peace," [N1].

Einstein was a lover of wisdom. But was he a philosopher? The answer to that question is no less a matter of taste than of fact. I would say that, at his best, he was not but would not argue strenuously against the opposite view if only because my knowledge of philosophy in the technical sense is flimsy. It is certain that Einstein's interest in philosophy was genuine. He studied philosophical writings throughout his life, beginning in his high school days when he first read Kant (3). In 1943 Einstein, Gödel, Bertrand Russel and Pauli gathered at Einstein's home to discuss philosophy of science [R1], about half a dozen times, Helen Dukas recalls. "Science without epistemology is -- insofar as it is thinkable at all-- primitive and muddled," he wrote in his later years, warning at the same time of the dangers to the scientist of adhering too strongly to any one epistemological system. "He [the scientist] must appear to the systematic epistemologist as a type of unscrupulous opportunist: he appears as <u>realist</u> insofar as he seeks to describe a world independent of the acts of perception;

as idealist insofar as he looks upon the concepts and theories as the free inventions of the human spirit (not logically derivable from what is empirically given); as positivist insofar as he considers his concepts and theories justified only to the extent to which they furnish a logical representation of relations among sensory experiences. He may even appear as a Platonist or Pythagorean insofar as he considers the viewpoint of logical simplicity as an indispensible and effective tool of his research," [E6]. Elements of all these isms are clearly discernable in his own thinking. In the last thirty years of his life he ceased to be an "unscrupulous opportunist," however, when, much to his detriment, he became a philosopher by freezing himself into realism or, as he preferred to call it, objective reality. That part of his evolution will be described in detail in (25). There can be as little doubt that philosophy stretched his personality as that his philosophical knowledge played no direct role in his major creative efforts. Further remarks by Einstein on philosophical issues will be deferred until (15e) except for his comments on Newton which will be discussed shortly.

The men whom Einstein at one time or another acknowledged as his precursors were Newton, Maxwell, Mach, Planck and Lorentz. As he told me more than once, without Lorentz he would never have been able to make "den Schritt," the discovery of special relativity. Of his veneration for Planck I shall write in (18a), of the influence of Mach in (15e), of his views of Maxwell in (16e). Since we are on the subject of philosophy I should perhaps note that I do not quite share Isaiah Berlin's opinion [B1] that Mach was one of Einstein's philosophical mentors and that Einstein first accepted, then rejected Mach's phenomenalism. Einstein's great admiration for Mach stemmed entirely from the reading of the latter's book on mechanics in which the relativity of all motion is a guiding principle. On the other hand Einstein considered Mach to be "un déplorable philosophe," [E7] if only because to Mach the reality of atoms remained forever anathema.

Einstein's deep emotional urge not to let anything interfere with his thinking dates back to his childhood and lent an unusual quality of detachment to his personal life. It was not that he was aloof or a loner, incapable either of personal attachments or of deep anger, as in his attitude toward Germany during and after the nazi period. When he spoke or wrote of justice and liberty for others, called the Jews his brothers or grieved for the heroes of the battle of the Warsaw ghetto, he did so as a man of feeling at least as much as a man of thought. That, having thus spoken and thus felt, he would want to return to the purity and safety of the world of ideas is not an entirely uncommon desire. Truly remarkable, however, was his gift to effect the return to that world without emotional effort. He had no need to push the everyday world away from him. He just stepped out of it whenever possible. It is therefore neither surprising that (as he wrote shortly before his death) he twice failed rather disgracefully in marriage nor that in his life there is an absence of figures he identified with -- with the exception, perhaps, of Newton.

It seems to me that when in mid-life Einstein wrote of "The wonderful events which the great Newton experienced in his young days...Nature to him was an open book...In one person he combined the experimenter, the theorist, the mechanic and, not least, the artist in exposition...He stands before us strong, certain and alone: his joy in creation and his minute precision are evident in every word and every figure..." [E8], he described his own ideals which included in particular the desire for fulfillment not only as a theorist but also as an experimental physicist. Earlier he had written that "[Newton] deserves our deep veneration" for his achievements, and that Newton's own awareness of the weak sides of his theories "has always excited my reverent admiration," [E9], (the weak sides including the action of forces at a distance, which, Newton noted, was not to be taken as an ultimate explanation).

"Fortunate Newton, happy childhood of Science!" [E8] . When Einstein wrote these opening words in the introduction to a reprinting of Newton's "Opticks" he had especially in mind that Newton's famous dictum "hypotheses non fingo," ("I frame no hypotheses") expressed a scientific style of the past. Elsewhere Einstein was quite explicit on this issue: "We now know that science cannot grow out of empiricism alone, that in the constructions of science we need to use free invention which only a posteriori can be confronted with experience as to its usefulness. This fact could elude earlier generations, to whom theoretical creation seemed to grow inductively out of empiricism without the creative influence of a free construction of concepts. The more primitive the status of science is, the more readily can the scientist live under the illusion that he is a pure empiricist. In the nineteenth century many still believed that Newton's fundamental rule 'hypotheses non fingo' should underly all healthy natural science..." [E10]. Einstein again expressed his view that the scientific method had moved on in words only he could have written: "Newton, forgive me; you found the only way which, in your age, was just about possible for a man with the highest powers of thought and creativity. The concepts which you created are guiding our thinking in physics even today, although we now know that they will have to be replaced by others farther removed from the sphere of immediate experience, if we aim at a profounder understanding of relationships," [E11].

However, in one respect Einstein forever continued to side with Newton and to quote his authority. That was in the matter of causality. On the occasion of the bicentenary of Newton's birth he wrote to the secretary of the Royal Society: "All who share humbly in pondering over the secrets of physical events are with you in spirit, and join in the admiration and love that bind us to Newton," went on to comment on the evolution of physics since Newton's days and concluded as follows: "It is only in the quantum theory that Newton's differential method becomes inadequate, and indeed strict causality fails us. But the last word has not yet been said. May the spirit of Newton's method give us the power to restore unison between physical reality and the profoundest characteristic of Newton's teaching--strict causality," [E12].

What is strict Newtonian causality? As an example, if I give you the precise position and velocity of a particle at a given instant, then you are able to predict from Newton's laws the precise position and velocity of that particle at a later time. Quantum theory implies, however, that I am unable to give you that information about position and velocity with ideal precision, even if I had the most perfect instrumentation at my disposal. That is the problem I discussed with Einstein about the existence of the moon, a body so heavy that the limitations on the precision of information on position and velocity I can give you are so insignificant that for all astronomical intents and purposes you can neglect the indeterminacy in the information you obtained from me, and may continue to talk of the lunar orbit. It is quite otherwise for things like atoms.

In the hydrogen atom, the electron does not move along an orbit in the same sense as the moon moves around the earth. For, if it did, then the hydrogen atom would be as flat as a little pancake while actually it is a little sphere. As a matter of principle, there is no way back to Newtonian causality. It will be evident that this recognition never diminished Newton's stature. Einstein's hope for a return to that old causality is an impossible dream. It will be evident that this opinion, held by modern physicists, has not prevented them from recognizing Einstein as by far the most important scientific figure of this century. His special relativity includes the completion of the work of Maxwell and Lorentz. His general relativity includes the completion of Newton's theory of gravitation and incorporates Mach's vision of the relativity of all motion. Planck, Einstein and Bohr (in order of appearance) are the founders of the quantum theory. In all these respects Einstein's oeuvre represents the crowning of the work of his precursors. He added to and revised the foundations of their theories. He is a transitional figure, perfecting the past to such an extent that he made the future look different.

Einstein deserves to be given the same compliment he gave Newton: He too was an artist in exposition. His talents for language, the German language, are second only to his gifts for science. I refer not so much to his proclivity for composing charming little rhymes as to the quality of his prose. He was a master of nuances, which are hard to maintain in translation. The student of Einstein should read him in German. It is fitting that several of his important papers, such as his scientific credo in the Journal of the Franklin Institute of 1936 or his autobiographical sketch which appeared in the Schilpp book [E6] should appear side by side in the original German and in English translation. He wrote all his scientific papers in German, whether or not they eventually appeared in that language. Not only his mastery of language but also his perceptiveness of people (which I already mentioned) are evident in his writings in memory of colleagues and friends, of Schwarzschild and Smoluchowski, of Marie Curie and Emmy Noether, of Michelson and Thomas Alva Edison, of Lorentz, Nernst, Langevin and Planck, of Walther Rathenau and, most movingly, of Paul Ehrenfest. These portraits serve as the best cure against the opinion that Einstein was a naive man. In languages other than German he was less at ease. On his first visit to Paris, in 1922, he lectured in French, however [Kl]. He spoke in German when addressing audiences on his first visits to England and the United States, but became fluent in English in later years.

Music was his love. He neither cared for twentieth century nor for many of the nineteenth century composers. He loved Schubert, but was not attracted to the heavily dramatic parts of Beethoven's oeuvre. He was not particularly fond of Brahms and disliked Wagner. His favorite composers were earlier ones, Mozart, Bach, Vivaldi, Corelli, Scarlatti. I never heard him play the violin. Most of those who did attest to his musicality and the ease with which he read scores on sight. About his predilections in the visual arts I quote from a letter by Margot Einstein to Meyer Schapiro: "In visual Art he preferred, of course, the old Masters. They seemed to him more 'convincing' (he used this word!) than the Masters of our time. But sometimes he surprised me looking at the early period of Picasso (1905, 1906)...words like cubism, abstract painting...did not mean anything to him...Giotto moved him deeply...also Fra Angelico...

Piero della Francesca...He loved the small Italian towns...He loved cities like Florence, Siena (Sienese paintings), Pisa, Bologna, Padua and admired the architecture...if it comes to Rembrandt, yes he admired him and felt him deeply..." [E13].

As a conclusion to this introductory sketch of Einstein the man I should like to elaborate on the statement made in the preface that Einstein was the freest man I have known. By that I mean that more than anyone else I have encountered he was the master of his own destiny. If he had a God it was the God of Spinoza. He was not a revolutionary, as the overthrow of authority was never his prime motivation. He was not a rebel, since any authority but the one of reason seemed too ridiculous to him to waste effort fighting against. (One can hardly call his opposition to nazism a rebellious attitude.) He had the freedom to ask scientific questions, the genius to ask so often the right ones. He had no choice but to accept the answer. His deep sense of destiny led him farther than anyone before him. It was his faith in himself which made him persevere. Fame may have on occasion flattered him but never deflected him. He was fearless of time and to an uncommon degree fearless of death. I can not find tragedy in his later attitude to the quantum theory or in his lack of success in finding a unified field theory, especially since some of the questions he asked remain a challenge till this day (3)--and since I remember what he looked like.

I now turn to a tour of Einstein's science.

Einstein never cared much for teaching courses. He did not deliver a single Ph.D.. But he always was fond of discussing physics problems, whether with colleagues his age or people much younger. All his major papers are his own. Yet in the course of his life he often collaborated with others. A survey of these collaborative efforts, involving more than thirty colleagues or assistants, is found in (28). From his student days until well in his forties he would seek opportunities to do experiments. As a student he hoped to measure the drift of the aether through which (as he then believed) the earth was moving (6d). While at the Patent Office he tinkered with a device to measure small voltage differences (3); this led to a collaboration with the Habicht brothers in the laboratory of the University of Zürich (28). In Berlin he did experiments on rotation induced by magnetization (14b), later measured the diameter of membrane capillaries (28), and was involved with patents for refrigerating devices and for a hearing aid (28). But, of course, theoretical physics was his main devotion. There is no better way to begin this brief survey of his theoretical work than with a consideration of what he did in 1905.

In that year Einstein produced six papers, on the following subjects:

(a) The light-quantum and the photoelectric effect, completed March 17, see (19c) and (19e). This paper which led to his Nobel prize in physics was produced before he wrote his Ph.D. thesis, the second in this series of six.

(b) A new determination of molecular dimensions, completed April 30, his Doctor's thesis which was to become his paper most often quoted in modern literature; (5e).

(c) Brownian motion, received* May 11, a direct outgrowth of (c); (5d).

(d) The first paper on special relativity, received* June 30.

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* By the editors of the Annalen der Physik.

(e) The second paper on the same subject, containing the $E = mc^2$ relation, received* September 27.

(f) A second paper on Brownian motion, received* December 19.

There is little if anything in his earlier published work that hints at this extraordinary creative outburst. By his own account, the first two papers he ever wrote, dating from 1901 and 1902 and dealing with the hypothesis of a universal law of force between molecules (4a) were worthless. Then followed three papers of mixed quality (4c-d) on the foundations of statistical mechanics. The last one of these, written in 1904, contains a first reference to the quantum theory. None of these first five papers left much of a mark on physics. I believe they were very important warming up exercises in Einstein's own development. Then came a year of silence, followed by the outpour of papers (a)-(f). I do not know what were his trains of thought during that year. In that period his personal life changed in two respects: his position at the Patent Office changed from temporary status to permanency. And his first son was born. Whether these events helped to promote the emergence of Einstein's genius I cannot tell, though I believe that the arrival of the son may have been a profound experience. Nor do I know a general formula for what genius is, except that it is more than an extreme form of talent; and that the criteria for genius are not objective. I note with relief that the case for Einstein as a genius will cause less of an argument than the case for Picasso and much less than the case for Woody Allen, and I do hereby declare that in my opinion Einstein was a genius.

Einstein's work before 1905 as well as his papers (b), (c), (f), resulted from his interest in the two central early twentieth century problems of statistical physics, the subjects of part II of this book.

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* By the editors of the Annalen der Physik.

The first problem: molecular reality. How can one prove (or disprove) that atoms and molecules are real things? If they are real, then how can one determine their size and count their number? (5a) contains an introductory sketch of the nineteenth century status of this question. During that period the chemist, member of the youngest branch of science, argued the question in one context, the physicist in another, and one paid little attention to what the other was saying. By about 1900 many though not all leading chemists and physicists believed that molecules were real. A few among the believers already knew that the atom did not deserve its name which means uncuttable. Roughly a decade later the issue of molecular reality was settled beyond dispute, since in the intervening years many proposed methods for counting these hypothetical particles all gave the same result, to within small errors. The very diversity of these methods and the very sameness of the answers gave the molecular picture the compelling strength of a unifying principle. Three of these methods are found in Einstein's work of 1905. In March he counted molecules in his light-quantum paper (19c). In April he made a count with the help of the flow properties of a solution of sugar molecules in water (5c). In May he gave a third count in the course of explaining the long known phenomenon of Brownian motion of small clumps of matter suspended in solution (5d). The confluence of all these answers is the result of important late nineteenth century developments in experimental physics. His March method could only be worked out because of a breakthrough in far-infrared spectroscopy (19a). The April and May methods were a consequence of the discovery by Dr. Pfeffer of a method to make rigid membranes (5c). Einstein's later work (in 1911) on the blueness of the sky and on critical opalescence yielded still other counting methods (5e).

The second problem: the molecular basis of statistical physics. If atoms and molecules are real things, then how does one express such macroscopic concepts as pressure, temperature, and entropy in terms of the motion of these submicroscopic particles? The great masters of the nineteenth century, Maxwell, Boltzmann, Kelvin, van der Waals, and others, did of course not sit and wait for the molecular hypothesis to be proven before broaching problem number two. The most difficult of their assignments was the derivation of the second law of thermodynamics. What is the molecular basis for the property that the entropy of an isolated system strives toward a maximum as the system moves toward equilibrium? (4) is devoted to a survey of the contributions to this problem by Einstein's predecessors as well as by Einstein himself. In those early days Einstein was not the only one to underestimate the mathematical care which this very complex problem rightfully deserves. When Einstein did this work, his knowledge of the fundamental contributions by Boltzmann were fragmentary, his ignorance of Josiah Willard Gibbs' papers complete. This does not make any easier the task of ascertaining the merits of his contributions.

To Einstein himself the second problem was of deeper interest than the first one. As he said later, Brownian motion was important as a method for counting particles, but far more important because it enables us to demonstrate the reality of those motions we call heat by simply looking into a microscope. On the whole, Einstein's work on the second law has proved to be of less lasting value than his earlier-mentioned investigations on the verification of the molecular hypothesis. Indeed, in 1911 he himself wrote that he would probably not have published his papers of 1903 and 1904 had he been aware of the work of Gibbs.

Nevertheless, Einstein's preoccupation with the fundamental questions of statistical mechanics were extremely vital since they led to his most important contributions to the quantum theory. It is no accident that the term "Boltzmann's principle," coined by Einstein, appears for the first time in his March 1905 paper on the light-quantum. In fact the light-quantum postulate itself grew out of a statistical argument concerning the equilibrium properties of radiation (19c). It should also be remembered that the main applications of his work in 1904 on energy fluctuations (4c) are in the quantum domain. His analysis of these fluctuations in blackbody radiation led him to become the first to state (in 1909) long before the discovery of quantum mechanics that the theory of the future ought to be based on a dual description in terms of particles and waves (21a). Another link between statistical mechanics and the quantum theory was forged by his study of the Brownian motion of molecules in a bath of electromagnetic radiation. This investigation led him to find the momentum properties of light quanta (21c). His new derivation (in 1916) of Planck's blackbody radiation law also has a statistical basis (21b). In the course of this last work he observed a lack of Newtonian causality in the processes called spontaneous emission. His discomfort about causality originated from that discovery (21d).

Einstein's active involvement with statistical physics began in 1902 and lasted until 1925 when he made his last major discovery in physics: his treatment of the quantum statistics of molecules (23). Again and for the last time he applied fluctuation phenomena, with such mastery that they led him to the very threshold of discovering wave mechanics (24b). The links between the contributions of Einstein, de Broglie and Schroedinger, discussed in (24), make clear that wave mechanics has its roots in statistical mechanics -- unlike matrix mechanics where the connections between the work of Boler, Heisenberg, Dirac followed in first instance from studies of the dynamics of atoms (18c).

Long periods of gestation are a marked characteristic in Einstein's scientific development. His preoccupation with quantum problems beginning shortly after Planck's discovery of the blackbody radiation law, late in 1900, bore its first fruit in March 1905. Questions which lie at the root of the special theory of relativity dawned on him as early as 1895 (6d), the theory saw the light in June 1905. He began to think of general relativity in 1907 (9); that theory reached its first level of completion in November 1915 (14c). His interest in unified field theory dates back at least as early as 1918 (14c). He made the first of his many proposals for a theory of this kind in 1925 (17d). Insofar as the relativity theories are concerned these gestation periods had a climactic ending. It took no more than about five weeks between his understanding of the correct interpretation of the measurement of time and the completion of his first special relativity paper (7a). Similarly, after years of trial and error he did all the work on his ultimate formulation of general relativity in approximately two months (14c).

I focus next on special relativity. One version of its history could be very brief: In June 1905 Einstein published a paper on the electrodynamics of moving bodies. It consists of ten sections. After the first five sections the theory lies before us in finished form. The rest, to this day, consists in applying the principles stated in those first five sections.

My actual account of that history is somewhat more elaborate. It begins with brief remarks on the nineteenth century concept of the aether (6a), that quaint hypothetical medium which was introduced for the purpose of explaining the transmission of light waves and which was abolished by Einstein. The question has often been asked whether or not Einstein got rid of the aether because he was familiar with the Michelson-Morley experiment which to high accuracy had demonstrated the absence of an anticipated drift of the aether as the earth was moving through it without obstruction (6a). The answer is that Einstein undoubtedly knew of the Michelson-Morley result (6d) but that it probably played only an indirect role in the evolution of his thinking (7a). From 1907 on, Einstein often emphasized the fundamental importance of the work by Michelson-Morley but continued to be remarkably reticent about any direct influence of that experiment on his own development. An understanding of that attitude lies beyond the edge of history. In (8) I shall dare to speculate on this subject.

Two major figures, Lorentz and Poincaré, take their place next to Einstein in the history of special relativity. Lorentz, founder of the theory of electrons, co-discoverer of the Lorentz contraction (as Poincaré named it), interpreter of the Zeeman effect, acknowledged by Einstein as his precursor, wrote down the Lorentz transformations (so named by Poincaré) in 1904. In 1905, Einstein, at that time only aware of Lorentz' writings up to 1895, rediscovered these transformations. In 1898, Poincaré, acknowledged as one of the greatest mathematicians of his day, consummate mathematical physicist, had written that we have no direct intuition of the simultaneity of events in two different places, a remark almost certainly known to Einstein before 1905 (6b). In 1905 Einstein and Poincaré stated independently and almost simultaneously (within a matter of weeks) the group properties of the Lorentz transformations and the addition theorem of velocities. Both Lorentz and Poincaré missed discovering special relativity because they were so deeply steeped in dynamical considerations. Only Einstein saw the crucial new point: the dynamical aether must be abandoned in favor of a new kinematics based on two new postulates (7). Only he saw that the Lorentz transformations, and hence the Lorentz-Fitzgerald contraction, can be derived from kinematical arguments. Lorentz acknowledged this, developed a firm grasp of special relativity, but, even after 1905, never quite gave up the aether, nor his reservations concerning the velocity of light as an ultimate velocity (8). In all his life (he died in 1912) Poincaré never understood the basis of special relativity (8).

Special relativity brought clarity to old physics and created new physics. Einstein's own main application of the new postulates was his derivation of the $E = mc^2$ relation (7b). It took some years until the first main experimental confirmation of the new theory, the energy-mass-velocity relation for fast electrons (7e). After 1905 Einstein paid only occasional attention to other implications (7d), mainly because from 1907 on he was after bigger game: general relativity.

The history of the discovery of general relativity is slightly more complicated. It is a tale of a tortuous path. No amount of simplification will enable me to match the four-line mini-history of special relativity given earlier. In the quantum theory Planck had started before Einstein. In special relativity Lorentz inspired him. In general relativity he alone started the long road. His progress is no longer marked by that light touch and deceptive ease so typical for all his work published in 1905.

The first steps are made in 1907 as he discovers a simple version of the equivalence principle, understands that matter will bend light, and that the spectral lines reaching us from the sun should show a tiny shift toward the red compared with the same spectral lines produced on earth (9). During the next three and a half years his attention focuses on that crisis phenomenon, the quantum theory, rather than on the less urgent problems of relativity (10). His serious concentration on general relativity begins after his arrival in Prague in 1911, where he teaches himself a great deal with the help of a model theory. He gives a calculation of the bending of light by the sun. His result is imperfect since at that time he still believes that space is flat (11). In the summer of 1912, at the time of his return to Zürich, he makes a fundamental discovery: space is not flat, the geometry of the world is not Euclidean. It is Riemannian. Ably helped by an old friend, the mathematician Marcel Grossmann, he establishes the first links between geometry and gravity. With his habitual optimism he believes to have solved the fifty-year-old problem (13) of finding a field theory of gravitation. Not until the fall of 1915 does he fully realize how flawed his theory actually is. At that very same time Hilbert, too, starts his important work on gravitation (14d). After a few months of extremely intense work Einstein presents the final version of his theory on November 25, 1915 (14c).

One week earlier he had obtained two extraordinary results. Fulfilling an aspiration he had since 1907, he found the correct explanation of the long-known precession of the perihelion of the planet Mercury. That was the high point in his scientific life. He was so excited that for three days he could not work. In addition he found that his earlier result on the bending of light was too small by a factor of two. Einstein was canonized in 1919 when also this second prediction proved to be correct (16b).

After 1915 Einstein continued to examine the problems of general relativity. He was the first to give a theory of gravitational waves (15d). He was also the founder of general relativistic cosmology, the modern theory of the universe at large (15e). Hubble's discovery that the universe is expanding was made in his lifetime. Radio galaxies, quasars, neutron stars, and, perhaps, black holes were found after his death. These post-Einsteinian observational developments in astronomy largely account for the great resurgence of interest in general relativity in more recent times. (15) is devoted to a sketchy account of the developments after 1915 up till present times.

I return to earlier days. After 1915 Einstein's activities in the domain of relativity became progressively less concerned with the applications of general relativity than with the search for generalization of that theory. During the early years following the discovery of general relativity, the motivation for and the aim of that search appeared to be highly plausible: According to general relativity the very existence of the gravitational field is inalienably woven into the geometry of the physical world. There was nothing equally compelling about the existence of the electromagnetic field, at that time the only field known to exist besides gravitation (17a). Riemannian geometry does not geometrize electromagnetism. Should one therefore not try to invent a more general geometry in which there was room for electromagnetism at a level equally fundamental as gravitation? That, in the early days, was the program for a unified field theory. Its purpose was neither to incorporate the unexplained nor to resolve any paradox. It was purely a quest for beauty.

On his road to general relativity Einstein had found the nineteenth century geometry of Riemann waiting for him. In 1915 the more general geometries which he and others would soon be looking for did not yet exist. They had to be invented. It should be stressed that the unification program was not the only spur for the search of new geometries. From 1916 on mathematicians, acknowledging the stimulus of general relativity, began the very same pursuit for their own reasons. Thus Einstein's work directly caused the development of a new branch of mathematics, the theory of connections (17c).

During the 1920's and 1930's it became evident that there exist forces other than those due to gravitation and electromagnetism. Einstein chose to ignore those new forces although they were and are not less fundamental than the two which had been known longer. He continued the old search for a unification of gravitation and electromagnetism, following one path, failing, trying a new one. He would study worlds with more than the familiar four dimenstions of space and time (17b) or new world geometries in four dimensions (17d). It was to no avail.

In more recent years the quest for unification of all forces has become a central theme in physics (17e). The methods are new. There has been distinct progress. But Einstein's dream, the joining of gravitation to other forces has so far not been realized.

In concluding this tour I return one more time to Einstein's contributions to the quantum theory, many of which have already been mentioned in the foregoing. I need to add that, late in 1906, he became the founder of the quantum theory of the solid state by giving the essentially correct explanation of the anomalous behavior of hard solids like diamond at low temperatures (20). It is also necessary to enlarge on the remark made previously concerning the statistical origins of the light-quantum hypothesis.

Einstein's paper of March 1905 does not contain one but two postulates: First, the light-quantum was conceived as a parcel of energy insofar as the properties of pure radiation (no coupling to matter) are concerned. Secondly, Einstein made the further assumption -- he called it the heuristic principle -- that also in the coupling to matter (that is, in emission and absorption) light is created or annihilated in similar discrete parcels of energy (19c). That, I believe, was Einstein's one revolutionary contribution to physics (2). It upset all existing ideas about the interaction between light and matter. I shall describe in detail the various causes for the widespread disbelief in the heuristic principle (19f). This resistance did not weaken after other contributions of Einstein had been recognized as outstanding. Nor did it abate after the predictions for the photoelectric effect, made on the grounds of the heuristic principle, turned out to be highly successful (19e).

The light-quantum, a parcel of energy, slowly evolved into the photon, a parcel of energy and momentum (21), a fundamental particle with zero mass and unit spin. No proposal for a new fundamental particle was resisted more strongly than that of the photon (18b). No one resisted the photon longer than Bohr (22). All resistance came to an end when experiments on the scattering of light by electrons (the Compton effect) proved that Einstein was right (21e, 22).

Quantum mechanics was born within a few months after the photon issue was settled. In (25) I describe in detail Einstein's response to this new development. Initially he believed that quantum mechanics contained logical inconsistencies (25a). That phase did not last long. Thereafter he reached the conviction that quantum mechanics is an incomplete description of nature (25c-e). Nevertheless he acknowledged that the non-relativistic version of quantum mechanics did constitute a major advance. His proposal of a Nobel prize for Schroedinger and Heisenberg is but one expression of that opinion (30).

However, Einstein never had a good word for the relativity version of quantum mechanics known as quantum field theory. Its successes did not impress him. Once, in 1912, he had said of the quantum theory that the more successful it is the sillier it looks (20). When speaking of successful physical theories he would, in his later years, quote the example of the old gravitation theory. Had Newton not been successful for more than two centuries? And had his theory not turned out to be incomplete? (25d).

Einstein himself never gave up the search for a theory which would incorporate quantum phenomena but which would nevertheless satisfy his craving for causality. His version of a future interplay of relativity and quantum theory in a unified field theory is the subject of the last scientific chapter (26) in which I return to the picture drawn in the preface.

Finally I may be permitted to summarize my own views. Newtonian causality is gone for good. The synthesis of relativity and the quantum theory is incomplete (2). In the absence of this synthesis any assessment of Einstein's vision must be part of open history.

The tour ends here. General comments on relativity and quantum theory are next. Then follows a sketch of Einstein in his early years. Then the physics begins.

References to Chapter 1.

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R1. B. Russell, introduction to ref. [N1].

cc. PW

PRINCETON UNIVERSITY - INSTITUTE FOR ADVANCED STUDY

Circle - Prof. [illegible] calendar - [illegible] file PW

THREE LECTURES ON EINSTEIN

by

Professor A. Pais
Rockefeller University

The Princeton Physics Department will host a series of three talks on Einstein's contributions to physics, in room A-10, Jadwin Hall.

I. Monday, March 30, 4:30 p.m.
"How Einstein Got His Ph.D."

II. Thursday, April 2, 4:30 p.m. (The Physics Colloquium)
"Einstein as a Revolutionary: The Light-Quantum" nyc or Chicago

III. Friday, April 3, 2:30 p.m.
"Subtle Is the Lord: The Birth of Relativity" [illegible]

Hew

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY 08540

Telephone 609-734-8000

SCHOOL OF NATURAL SCIENCES

April 26. '81

Dear Harry

I am leaving for Switzerland later today and take with me many interesting thoughts arising from our recent conversations. As I told you, it seems quite fitting and natural to me that you would wish to speak with Sam Treiman and with Marshall Clagett. I am also confident that you will know whom else to consult if other perspectives on the questions will seem useful to you.

In any event I look forward to our next meeting. I return from Switzerland on May 11. and will be more or less steadily at the Institute for the subsequent two months. I will of

/over

course be often in Princeton thereafter. So there are opportunities enough not only for us to talk but also for me to be available to talk to "third parties" if you so desire.

Since my present position is of a kind which carries peculiarly specific responsibility it would be essential for me to know definitively whether things can work out not later than by December 1981.

And so, dear chauwer, au revoir —

Bram

January 9, 1981

Dr. & Mrs. A. Pais
23 Jefferson Road
Princeton, New Jersey 08540

Dear Bram and Sara:

There is an ancient Puritan admonition that the path to heaven is not the less true if of late discovered, and I hope you will accept that tonality at least for this belated thakk-you note to you both for the absolutely stunning book on a subject that all of us enjoy.

The book, as you say, will give us joy, but much beyond that context for your thoughtfulness and your friendship.

Cordially yours,

Harry Woolf

PAIS

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY 08540

Telephone 609-734-8000

SCHOOL OF NATURAL SCIENCES

April 9, 1981

Dear Harry

April is upon us and I am thinking of future plans. Some months ago we spoke of my possible return to the Institute and the conceivable advantages that might have for the Institute and for myself. At that time you said you might have a first judgment on this issue in a few months time. My interest in your judgment is undiminished and I naturally am curious whether you might have some comment for me.

As I mentioned to you earlier I have had private discussions about this matter with Sam Treiman, who is to be the next physics dept. chairman at PU, and who is an old and trusted friend of mine*. I thought I'd mention this since I know that Sam would be glad to share his views with you if you so desire.

All is well this side, and I hope it is likewise with you.

As ever
yours

Bram

---

* And of the Institute!

PAIS

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY 08540
Telephone 609-734-8000

SCHOOL OF NATURAL SCIENCES

4/14/81

Dear Harry

Thank you for a wonderful talk!

I enclose (for you to keep) a draft of my first chapter. It gives a plan of the book and, in a more personal way than what comes later, gives my reminiscences and general appreciation of who that man was.

Perhaps these pages may be of practical help for what we talked about, because their purpose is to give the non-specialist a flavor of what I am trying to do.

Thanks again.

As ever
Bram

March 6, 1981

Dr. Abraham Pais
School of Natural Sciences

Dear Bram:

Just a short note to tell you how absolutely thrilling I found your Table of Contents for the Einstein book to be. Whether you are dealing with "pots of pfeffer," or scaling to tensors, or the second coming of h, I found myself going through the skeleton of what will become a mighty morphology with extraordinary pleasure.

Perhaps even more than you, if that is possible, I look forward to having the book in hand.

Cordially yours,

Harry Woolf

Dear Harry

Thank you for your nice note!

I thought you might like to get a bit of an overview of what I am doing. All but those parts marked | have now been written in an advanced first draft — this table of contents need neither be returned nor is it private —

As ever

Bram

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THE INSTITUTE FOR ADVANCED STUDY

JOHN HUNT
Associate Director
and Secretary of the Corporation

January 27, 1981

To whom it may concern:

Professor Abraham Pais, Chairman of the Physics Department at Rockefeller University, is presently a Visiting Member at the Institute for Advanced Study. While here, he is completing a biography of Albert Einstein.

Professor Pais' scholarly attainments are widely known and greatly appreciated by his peers. His qualifications for the preparation of an Einstein biography are of the highest. This letter is written in support of his request to the FBI for a copy of the official FBI file on the late Professor Albert Einstein.

The particular interest of the Institute in this matter stems from the fact that it was here that Albert Einstein lived and worked as a member of the Institute faculty from the time of his definitive move to the United States in 1933 until his death in 1955. Our recommendation of Professor Pais is made in full awareness of our responsibility to the memory of Albert Einstein.

Sincerely yours,

John Hunt

Princeton, New Jersey 08540 Telephone 609-734-8203

Dear John - This is a draft of my intended letter to the FBI - I am now ready for a "To whom it may concern" when you are - Yrs Bram

(1)

¶ Pursuant to the Freedom of Information Act, 5 U.S.C. 552, I hereby request a copy of the F.B.I. file on the late Professor Albert Einstein.

¶ I shall undertake to reimburse any expenses incurred in connection with this request.

¶ If you determine that some portions of this file are exempt from release, I request that you provide me with the remainder. I hereby reserve ~~the~~ my right to appeal any such decisions.

¶ If you do not grant this request within 10 working days, I will consider my request desired. If you should wish to discuss the matter, my business telephone number is (609)-7348068.

¶ The present request is made in connection with the preparation of a biography of A. Einstein which I am writing at this time, supported by a grant from the Sloan Foundation. Concerning my bona fides, I am a member of the National Academy of Sciences, the American Academy of Sciences, a Fellow of the American Physical Society, Chairman of the Physics Department at the Rockefeller University in New York, and, at this time, ~~a memb~~ a visiting member of the Institute for Advanced Study on a one year's leave which I devote to the completion of the biography mentioned above

¶ In further support of ~~my app~~ the present application I enclose a letter by

¶ Thanking you in advance for your time and consideration, I am,

Sincerely yours,

December 17, 1980

Professor Abraham Pais
School of Natural Sciences
Institute for Advanced Study

Dear Bram:

I thought you might like to know that at the Nobel ceremony in Stockholm on 10 December, which it was my pleasure to attend, Gösta Ekspong, who presented the work of Jim Cronin and Val Fitch, made the following statement in passing, "as early as 1955 Gell-Mann and Pais had analyzed the neutral K-mesons and found that they are strange, indeed unique in their ambivalence with respect both to matter and antimatter."

Cordially yours,

Harry Woolf

December 18, 1980

Professor A. Pais
The School of Natural Sciences
The Institute for Advanced Study

Dear Bram:

Thank you for the copy of the letter from the Princeton University Press. I have sent it on to James Brown, who has now answered me as follows concerning your book:

> I shall be happy to read his manuscript at any point he wishes, preferably not until after the close of the year. If he wishes to wait for the finish I shall look forward to it in the spring.

Brown's address is as follows:

James Oliver Brown, Esq.
James Brown Associates, Inc.
Room 1520
25 West 43 Street
New York, New York 10036

Good luck!

As ever,

John Hunt

THE INSTITUTE FOR ADVANCED STUDY
Princeton, New Jersey 08540

MARY WISNOVSKY
Assistant to the Director

19 May 1980

TO: Jim Barbour, Personnel Services

FROM: Mary Wisnovsky Mary

One of our visiting members next year, Dr. A. Pais, has requested a four-drawer, lockable filing cabinet for his office which will be in Building D. Val has informed me that we may charge this to the School of Natural Sciences. By the way, we need the cabinet in early June.

Many thanks.

Val says they have one that Pais can have

12 May 1980

Mary—

Pais letter being circulated in normal manner. It should come to you from Lorne.

Aida

cc. Allen Rowe
Mary Wisnovsky
Valerie Nowak

May 12, 1980

Dr. A. Pais
The Rockefeller University
1230 York Avenue
New York, New York 10021

Dear Bram:

Thank you very much for yours of 6 May 1980, concerning your stay at the Institute. I am forwarding a copy of your letter to Mr. Rowe so that the pattern of payment that you request can be brought about. Equally, I am asking Mrs. Wisnovsky to see to it that a four drawer lockable cabinet can be placed in the office to be assigned to you.

It will be a pleasure to have you among us again.

Cordially yours,

Harry Woolf

PRO·BONO·HUMANI·GENERIS
The Rockefeller University 1901

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE · NEW YORK, NEW YORK 10021

~~HS~~ aw Mr. Rowe
~~SM~~ Mrs. Wisnovsky
NS ~~Soc. Sci.~~
Pls. retn. to D.O.

May 6, 1980

Dr. Harry Woolf
Director
Institute for Advanced Study
Princeton, New Jersey 08540

Dear Harry:

This is to acknowledge your letter concerning my stay and fellowship at the Institute for '80 -'81. I have meanwhile obtained additional funds so that I can realize my plan to stay at the Institute for that full academic year. I would like to ask that the Institute grant be made available to me in five monthly portions, starting September 1. I would also like to ask if the office to be assigned to me could be made available some time in June so that I can transport some of my papers over there. I have one last question. Could I please have one four drawer lockable filing cabinet?

I look forward to my stay!

With warmest regards,

Bram

A. Pais

AP/rbb

Not sent per phone conv with Pais 5/5/80 He will not need apt

1 May 1980

Dr. Abraham Paid
Department of Physics
Rockefeller University
New York, NY 10021

Dear Dr. Pais:

We are now able to let you know the address of the apartment you will occupy when you come to the Institute. We are reserving a one-bedroom apartment for you. Your address will be 109E So. Olden Lane, Princeton, NJ 08540. The rent will be $350 per month which includes heat and utilities. (You may note, that this is less than the $400 previously stated on your housing application due to an adjustment by the business office.) A map of the housing project is enclosed and your apartment location marked in red. Also enclosed is some information you may find helpful.

When you know the exact date of arrival, please notify my office. Your apartment will be open and ready for you after September 1st for fall arrivals and after January 2nd, 1981 for second-term members. Your keys will be on the kitchen table.

Sincerely,

Mary Wisnovsky

Mary Wisnovsky

Encls.

cc: Business Office

Dr. Abraham Paid
Department of Physics
Rockefeller University
New York, NY 10021

Dr. Pais:

109E So. Olden Lane,

1 bdrm
350

CHARGE TO DIRECTOR'S FUND

cc: Allen Rowe
Mary Wisnovsky
Valerie Nowak

March 19, 1980

Dr. Abraham Pais
Department of Physics
Rockefeller University
New York, New York 10021

Dear Dr. Pais:

On the recommendation of the Faculty in the School of Natural Sciences it is my pleasure to formally offer you a membership in the Institute for Advanced Study for the academic year 1980-81. The term dates are: first term, Monday, 22 September to Friday, 19 December 1980; second term, Monday, 5 January to Friday, 3 April 1981.

If you are able to accept this invitation we can offer you a stipend of $15,000, and would be happy to arrange Institute housing for you during your visit. In this connection, would you be kind enough to complete and return the enclosed housing application.

Please let me hear from you as soon as possible.

Cordially yours,

Harry Woolf

Enclosures

109E

NS My

HOUSING RESERVATION

THE INSTITUTE FOR ADVANCED STUDY

MEMBER'S NAME A Pais NATIONALITY US

CURRENT ADDRESS Rockefeller University
New York NY 10021

HUSBAND'S OR WIFE'S FULL NAME ______ NUMBER OF ADULTS IN RESIDENCE 1

NUMBER OF CHILDREN IN RESIDENCE ____

CHILDREN'S NAME	DATE OF BIRTH	SEX

A LIMITED NUMBER OF CRIB-BEDS ARE AVAILABLE FOR SMALL CHILDREN FROM OVERSEAS FAMILIES
PLEASE INDICATE IF YOU DESIRE A CRIB FOR ANY CHILDREN ______

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(PLEASE SELECT APPRORIATE SIZE APARTMENT FOR YOUR NEEDS.)

☐	ONE ROOM STUDIO*	\$350.00	(studio, kitchenette, bath)
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☐	TWO BEDROOM APARTMENT	\$450.00	(living room, study, two bedrooms, kitchen & bath)
☐	THREE BEDROOM APARTMENT	\$500.00	(living room, study, three bedrooms, kitchen & bath)

FIRST TERM: (BEGINS) 9/22/80.....(ENDS) 12/19/80
SECOND TERM: (BEGINS) 1/15/81.....(ENDS) 4/3/81

When an apartment is assigned to a one-term or a one-year member, it is understood that the tenancy expires at the conclusion of the term or academic year. First-term members will be required to vacate their apartment no later than December 27th. However, it is possible to arrange a continuance of tenancy beyond the end of the second term. All requests for extensions must be in writing and approved by the Director's Office.

*ALL STUDIO APARTMENTS INCLUDE MAID SERVICE

OTHER SERVICES: For linen service and telephone service see page 2

CHARGES: A furniture deposit of \$200 is required of everyone (this can be deducted from your stipend check and is returnable when you depart). Also, arrangements can be made with the business office to deduct your monthly rent from your stipend.

LINEN PURCHASE AND/OR BLANKET RENTAL

NAME ____________________________
(LAST) (FIRST)

Please ✓ plan desired:

Linen purchase plan.......$9.00 x number of packages______ = $________

Blanket Rental Plan.......$4.00 x number of blankets______ = $________

Total Charge......$________

AUTHORIZATION FOR TELEPHONE SERVICE

Please have telephone service in my apartment on the date indicated. I agree to assume full responsibility for the installation charge and the monthly telephone bill. (Each apartment has a standard wall-mounted dial-type telephone in the kitchen. We allow no substitution of equipment or location.)

Installation charge..........$25.00
Monthly service charge.......$ 6.90

Signature A Pais

Installation date (about) September 1, 1980

(If this is not filled out and returned prior to your arrival, telephone service will take an additional month to be operational in your apartment.)

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

Application for Membership

School of Natural Sciences

Academic Year 1980 - '81 Both Terms ✓ First Term ______ Second Term ______

Name in Full A. Pais Date of Application MARCH 15 '80

Permanent Address Rockefeller University Citizenship US

Present Address 23 Jefferson Rd Place of Birth Amsterdam

Academic Degrees (Please give date and place received) Date of Birth 5-19-18

B.S./A.B. ~~A.B.~~ Feb. 1938 Amsterdam — Marital Status M

M.A. ~~M.A.~~S April 1940 Utrecht — Number of Children 1

Ph.D. July 1941 Utrecht — Ages of Children 22

Please indicate whether your family will accompany you:
[✓] Spouse [✓] Children

Former and Present Professional Positions (Please give dates, place, rank or title)

Dates	Place	Rank
~~Professor~~		
1950-1963	Inst. f. Adv. Study	Professor
1963-	Rockefeller U.	"

Current Salary 62,000

Intended Research: Please submit with this application, in duplicate, a brief outline of your current and intended research.

Scientific Biography of A Einstein

Publications: A list of publications should be attached in duplicate.
Please submit also a copy of available reprints and/or preprints.

Grant-in-aid: Only a limited number of memberships are available each year with grants-in-aid. It would therefore be helpful to us to know whether: (please check)

(✓) you will require financial assistance from the Institute partial

(✓) you plan to come to the Institute with funds from another source: (please specify) partial

( ) you are applying to other sources for funds: (please specify) ______

Other: ______

Please return this form in duplicate to: Administrative Officer, School of Natural Sciences,
Institute for Advanced Study, Princeton, New Jersey 08540

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY 08540
Telephone-609-924-4400

SCHOOL OF NATURAL SCIENCES

March 11, 1980

Memo to: Aida LaBrutte
From: Valerie Nowak

Dear Aida,

The School of Natural Sciences would like to invite Dr. Abraham Pais for the 1980-81 academic year. As you probably already know, Dr. Woolf has agreed to pay a stipend of $15,000 to Dr. Pais from his Director's Fund. Dr. Pais will try to find outside funds to supplement his salary and if he fails he will at least come for Term I. He will need a bachelor apartment for his stay.

Steve also asked that I extend our thanks to Dr. Woolf for his generosity.

Regards,
Valerie
Valerie

February 26, 1980

Cable sent to HW today -- at request of Stephen Adler

Dr. Harry Woolf
NORFOLK
Nairobi, Kenya

Adler would like invite Pais one term $15,000. Can you put him on Director's Fund. On your decision depends NS inviting additional postdoc.

(signed) Aida La Brutte