Autobiographical Note: Kenneth M. Watson

January 8, 2004

Dr. Kenneth M. Watson, 9 April 1981

Personal Data

I was born September 7, 1921, in Des Moines, Iowa. My father was a minister in the Methodist Church. During my childhood years we lived first in Shenandoah and then in Creston, Iowa. These were both small

farming communities. I graduated in 1943 from Iowa State University (Ames, Iowa) with majors in Electrical Engineering and Physics.

In 1946 I married Elaine Carol Miller, who spent her childhood in Jeanette, Pa. We have two children, Ronald and Mark. Ronald is an optometrist living in the San Diego area. Mark is an author, living in Sedona, Arizona.

I retired at age 70 from UCSD in 1991 (70 was the mandatory retirement age at that time), but continued to receive research grants for another 10 years. We continued to live in the San Diego area after I retired. In the year 2000 we moved into our present home, a Continuing Care Facility for the Elderly.

I have had several hobbies: Sailing was a family hobby. We often sailed our boat from San Francisco to San Diego during the summer break. When I retired we purchased a power boat and a vacation home in the Sacramento River Delta area I constructed several model railroads. As the technology developed, I began doing video photography and animation using computer editing.

Employment and Professional Activities

1943-1946Naval Research Laboratory Washington, D. C.

1948-1949 Postdoctoral fellowship, Institute for Advanced Study, Princeton, N. J.

1949-1951 Postdoctoral position, University of Calif., Berkeley

1951-1954 Assistant Professor, Indiana University, Bloomington

1954-1957 Associate Professor, University of Wisconsin, Madison

1957-1981 Professor, University of California, Berkeley

1957-1981 Staff of Lawrence Berkeley Laboratory

1981-1991 Professor, University of California, San Diego

1981-1991 Director of Marine Physical Laboratory

During the 1960's I served as member of the Air Force Scientific Advisory Board

During the 1970's I was a member of the Defense Science Board

During the Eisenhower, Kennedy, and Johnson administrations I was a member of the President's Science Advisory Committee Military Panel.

During the Nixon administration I was a consultant to the National Security Council

During the 1970's and 1980's and early 1990's I was a member of the Navy's Planning and Steering Advisory Panel

In 1959 I worked with M. Goldberger, K. Brueckner, and M. Gell Mann to join J. Wheeler and C. Townes to form the Jason Group of government advisers. I remained in Jason until 1998.

In 1971 I joined E. Montroll, A. Hochstim, A. Thompson, and I. Oppenheim to form a private company, Physical Dynamics, Inc. I remained on the board of directors until 1981.

From 1981-2001 I was a consultant to Science Applications International Corp.

I became a member of the National Academy of Sciences, Section 13, in 1974.

Professional Data

In writing this I make no effort to be exhaustive in presenting my research publications. Instead, I have attempted to present "typical" work.

During World War II I was at the Naval Research Laboratory in Washington, D.C. During this period I went to night school at George Washington University. I received my PhD degree in physics in 1948 from the University of Iowa (Iowa City, Iowa). My thesis advisor was Josef Jauch.

I was a postdoctoral fellow at the Institute for Advanced Study during the 1948-49 academic year. This was a very exciting time for theoretical physicists. Robert Oppenheimer was director of the Institute. The observation of the "Lamb Shift" stimulated major developments in Quantum Electrodynamics. The new work of Schwinger, Feynman, and Dyson broke a barrier of infinities in quantum electrodynamics. My first encounter with the prediction of a "theory of everything" was Oppenheimer's optimistic suggestion that this might arise from a formula "long enough to wrap around Fuld Hall". During this year I collaborated with Joseph Lepore, using the new field theory techniques to study radiative corrections to nuclear forces (Phys. Rev. **76**, 1157-1163, 1948).

During the winter of 1949 Oppenheimer introduced me to Edward Teller. This led to an invitation to spend two years as a postdoctoral fellow at the University of California, Berkeley (UCB). Here the environment was dominated by high energy experiments being done with the electron synchrotron and the 184" cyclotron. The Bevatron under construction . The production and properties of pi and mu mesons and nucleon-nucleon collisions were giving new insight into strong interaction symmetries and forces. During these postdoctoral years, I collaborated in research with several people, including Ernest Henley, Keith Brueckner and Robert Serber (the head of the theoretical group). During this period Brueckner and I began exploring the hypothesis of *charge independence*, often called *charge symmetry* at that time (Phys. Rev. **83**, 1-9, 1951). This was an exciting time in Berkeley with experimental studies of pi meson production, scattering, and absorption. I participated in several investigations, both phenomenological and theoretical, of these processes (Phys. Rev. **81**, 575-578, 1951 and Phys. Rev. **84**, 258-265, 1951 with Brueckner and Serber; Phys. Rev. **84**, 1084-1089, 1951, with S. Fernbach and T. A. Green; etc.).

In the fall of 1951 I joined the faculty of the Physics Department of Indiana University. During my three years here I did research primarily in nuclear and pi meson physics. I explored implications of *charge independence* (Phys. Rev. **85**, 852-857, 1952); pi meson-nucleon interactions (Annual Rev. Nuclear. Science, Vol.4, 219-270, 1954 with M. Gell-Mann); photo production of pi mesons (Phys. Rev. **95**, 228-239, 1954); pi meson field theory (Phys. Rev. **90**, 699-708, 1953 and Phys. Rev. **95**, 228-239, 1954 with Brueckner); etc.

While at Indiana I began to be interested in quantum mechanical collision processes. A paper "The Effect of Final State Interactions on a Reaction Cross Section" (Phys. Rev. **88**, 1163-1171, 1952) discussed the role of interactions between particles emerging from a reaction. In further work, I began an investigation of the scattering of a specific particle by a system of particles (gas, solid, atom, atomic nucleus, etc.). Phenomenological equations for wave scattering by systems of scatters had been proposed by Foldy and Lax. I developed a generalization of their models which I showed to be an *exact* solution of the Schrodinger equation. A further generalization provided a separation into coherent and incoherent scattering (Phys. Rev. **89**, 575-587. 1953).

In 1954 I joined the physics faculty at the University of Wisconsin. Here I continued to investigate pi-meson interactions with nucleons (Phys. Rev. **95**, 228-239, 1954 and Annual Review of Nuclear Science, vol.4,219-270, 1954 with M. Gell-Mann). At this time, I began a long term interest in statistical mechanics ("Equation of State of Gases and Liquids at Low Temperatures", Phys. Rev. **108**, 518-536, 1957 with W. B. Riesenfeld).

During 1956 I went to the Los Alamos Laboratory on leave from the Univ. of Wisconsin. Here I became interested in plasma physics and controlled fusion. One aspect was the use of the Boltzmann equation to analyze low density ionized gases (Phys. Rev. **102**, 12-19, 1956; Phys. Rev. **102**, 19-27, 1956 with K. Brueckner; Annals of Physics **2**, 435-470 and **5**,1-25, 1958 with S. Chandrasekhar and A. N. Kaufman; Proc. Roy. Soc. A **245**, 435-455, 1958 with Chandrasekhar and Kaufman). A different approach to plasma confinement was described with James Tuck (Phys, of Fluids **2**, 239-246, 1959). I also did some work studying ionized gases in space (Phys. of Fluids **6**, 480-488, 1958 with L. Kraus).

In 1957 I joined the physics faculty at the University of California in Berkeley. Here I continued an interest in meson and nuclear physics (see, for example, "Collective Excitations of Nuclear Matter" Annals of Physics **6**, 1-36, 1959 with A. Glassgold and W. Heckrotte) , but began to work more in statistical mechanics: "Statistical Mechanics of Relativistic Streams" (Phys. of Fluids **3**, 741-747, 1960 and **3**, 747-757, 1960 with S. Bludman and M. N. Rosenbluth); "Two-Stream Instability in Finite Beams"(Phys. of Fluids **5**, 196-209, 1962 with E. Frieman, M. Goldberger, S. Weinberg and M. Rosenbluth); " Statistical Mechanics for the Nonideal Bose Gas" Phys. Rev. **120**, 660-674, 1960 with A. Glassgold and A. Kaufman; "Linked-Diagram Expansion for the Equation of State of a Gas of Molecules" (Phys. of Fluids **4**, 655-662, 1961 with A. Kaufman); "Statistical Theory of the Dielectric Constant of an Imperfect Gas" (Phys. of Fluids **4**, 931-943, 1961); "Multiple Scattering of Electromagnetic Waves in an Underdense Plasma" (Jour, of Math. Phys. **10**, 688-702, 1969, see also Jour. Math. Phys. **11**, 1496-1504,1970)

In 1958 I attended the State Department Treaty negotiations in Geneva, Switzerland, on the detection of Nuclear Explosions in Space. An analysis of the technical issues was published with R. Latter and R. Herbst in

Ann. Rev. of Nuclear Science **11**,371-418, 1961. At about this time I collaborated with J. Bond and J. Welch to publish a book related to weapons technology: "Atomic Theory of Gas Dynamics". Addison-Wesley, 1964.

In the mid-1960's I began a series of investigations, in collaboration with M. L. Goldberger of the observation of "entangled" quantum mechanical systems. We were concerned with sequential measurements and interference effects for correlated systems. Some papers published by ourselves on these issues are: Phys. Rev. **134**, B919-B928, 1964; **137**, B1396-B1409, 1965: **140**, B500-B509, 1965.

M. L. Goldberger and I collaborated to publish a book on collisions of quantum mechanical systems: "Collision Theory", John Wiley & Sons, Inc. 1964.

My interest in quantum mechanics of scattering by complex systems continued from the mid 1960's into the mid-1970's. Some relevant publications are: "Electronic Transitions in Slow Collisions of Atoms and Moleculses" (Phys. Rev. **174**, 152-164, 1968); "The Optical Model" (Advances in Theoretical Physics, **1**,115-194, 1965, Academic Press, N.Y.); a book "Topics in Several Particle Dynamics" with J. Nuttal (1967, Holden-Day Inc., San Francisco)

A continuing interest in plasma physics led to two papers authored with Norman Kroll: "Theoretical Study of Ionization of Air by Intense Laser Pulses" (Phys. Rev. A **5**, 1883-1905, 1972) and "Charged-Particle Scattering in the Presence of a Strong Electromagnetic Wave (Phys. Rev. a **8**, 804-809).

During the period 1970-1975 I conducted research in atomic and molecular scattering. With Yukap Hahn I demonstrated a method for constructing projection operators for electronic states of atoms. The semiclassical approximation was used. This method was successfully applied by several experimental groups investigating collisional ionization of atoms. [Phys. Rev. A **6**, 548-556, 1972 amd Phys. Rev. A **7**, 491-499, 1973] See also, "Electronic Transitions in Slow Collisions of Atoms and Molecules. IV with Chen and Joachain (Phys. Rev. A **5**, 1268-1285, 1972), etc.

In 1971 I joined E. Montroll, A. Hochstim, A. Tompson, and I. Oppenheim to form a private company, Physical Dynamics, Inc. In 1973 I took leave without pay from UC Berkeley to start a La Jolla, CA office of Phys. Dynamics. By 1975 we had, in addition to the La Jolla office, two offices in the Los Angeles area, an office in Berkeley, and an office in Seattle. In 1976 Montroll, Goldberger, Hochstim and I formed a nonprofit research organization in La Jolla, the *La Jolla Institute.* In 1981, because of pressure from other commitments, I resigned from the board of Physical Dynamics.

In 1979 the *La Jolla Institute* received a grant from the Navy to set up an Institute for the Study of Nonlinear Dynamics. We received space on the University of California's Scripps Institution of Oceanography campus to establish this. I took leave from UC Berkeley for two years to be director of this.

In 1981 I was offered the directorship of the Marine Physical Laboratory (MPL), a unit of the Scripps Institution of Oceanography. This included a professorship in the SIO department of the University of California, San Diego. My coming to MPL meant a transfer from UC Berkeley to UC San Diego (my retirement

and other benefits transferred with me). Research at MPL includes physical oceanography, marine chemistry, marine acoustics, etc. During my years at MPL I conducted research in physical oceanography.

In the late 1970's I began doing research in fluid mechanics related to oceanography. This evolved from an interest in nonlinear mechanics and a series of seminars at Berkeley shared with Andrew Sessler and Alan Kaufman. Examples are seen in "Some properties of deep water solitons" (Phys. of Fluids, **19**, 345-354, 197 6 with B. I. Cohen), Electromagnetic Fluctuations induced by wind waves on the deep-sea floor" (J. Geophys. Res. **83**, 431-442, 1978 with C. Cox, N. Kroll, and P. Pistek), and "Numerical analysis of weakly nonlinear turbulence" (Proc. Natl. Acad. Sci. USA **76** 2109-2113, 1979 with J. D. Meiss and N. Pomphrey).

In the early 1980's I became interested in applying methods of statistical mechanics to internal wave turbulence. An early effort was given in "Description of Nonlinear Internal Wave Interactions Using Langevin Methods" (J. Geophys. Res. **85**, 1085-1094, 1980 with N. Pomphrey and J. D. Meiss). A model using Brownian motion theory to study a "test" wave in a field of waves was developed in "Relaxation processes for a three-wave interaction model" (Proc. Natl. Acad. Sci. USA **78**, 2029-2032, 1981 with J. D. Meiss and Proc. Natl. Acad. Sci. USA **80**, 1144-1146, 1983). An analysis of the accuracy of various models for internal wave turbulence using the Taylor-Goldstein equation was given in "Internal-wave interactions in the induced diffusion approximation" (J. Fluid. Mech. **117**, 315-341, 1982 with J. D. Meiss).

An investigation of the interaction of internal waves with oceanic mesoscale currents led to the conclusion that this led to a transport of internal wave energy to high vertical wave numbers. See "Interaction between Internal Waves and Mesoscale Flow" (J. Phys. Oceanography **15**, 1297-1311, 1985).

In the latter part of the 1980's I developed an interest in the persistence of patterns waves on the ocean surface. An example is the Kelvin wake of a ship. A publication analyzing this phenomenon is given in "Persistence of a Pattern of Surface Gravity Waves" (J. Geophys. Res. **91**, 2607-2615, 1986).

In the early 1990's I returned to an analysis of the coupling of surface and internal gravity waves. Several earlier publications had discussed this to answer the question "Are surface waves a major source of energy for internal waves?" Major disagreements were found among these publications. I analyzed these works and found that the underlying formulations in all of them were equivalent and that the discrepancies were due to unjustified numerical approximations. I showed that calculations without these numerical approximations led to identical results for all the published theories ("The Coupling of Surface and Internal Gravity Waves: Revisited", J. of Phys. Oceanogr. **20**, 1233-1248, 1990). Detailed calculations made using archived environmental data for the North Pacific Ocean showed a significant energy transfer from the internal wave to the surface wave field ("Energy transfer between surface and internal waves in the North Pacific Ocean", J. Geophys. Res. **99**, 12,549-12,560, 1994).

In the mid 1990's my interest in nonlinear classical mechanics and ocean surface waves led to a study of capillary waves (few centimeter wavelengths) interacting with longer waves (10 cm to a meter wavelengths). A

series of radar observations of capillary wave modulation by internal waves found a pronounced effect (for example: R. Gasparpvoc, J. Apel, and E. Kasischke, "An overview of the SAR Internal Wave Signature Experiment" J. Geophys. Res. **93**, 123-4-12316, 1989; B. Hughes and T. Dawson, "Joint Canada-U.S. Ocean Wave Investigation Project: An Overview of the Georgia Strait Experiment" J. Geophys. Res. **93**, 12219-12234, 1988). Due to the rapid relaxation rate for capillary waves, it was assumed that the capillary waves from (which the radar scattering occurred) were being continuously modulated by longer waves, which in turn were being modulated by internal wave currents. This was referred to in the literature as the "long wave effect", but there was no detailed understanding of the mechanism for this.

Ocean surface wave dynamics can be formulated as nonlinear interactions among a set of harmonic oscillators. The Hamiltonian formulation of this is mathematically very similar to the equations of classical and quantum mechanical field theory that I had encountered at the beginning of my career. I developed a canonical transformation technique which greatly simplified numerical integration of the equations. Calculations of the "long wave effect" agreed with observations of the radar scattering. Various other calculations were made of capillary spectra. Comparison made with phase averaging models showed serious limitations of these models. ["Interaction of capillary waves with longer waves. Part!. General theory and specific applications to waves in one dimension", J. Fluid Mech. **321**, 87-120, 1996, with S. B. Buchsbaum; "Interaction of capillary waves with longer waves. Part 2. Applications to waves in two surface dimensions and to waves in shallow water", J. Fluid. Mech. **397**, 99-117, 1999.]