LEON KNOPOFF: AN ORAL HISTORY

Interviewed by William Van Benschoten

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Los Angeles

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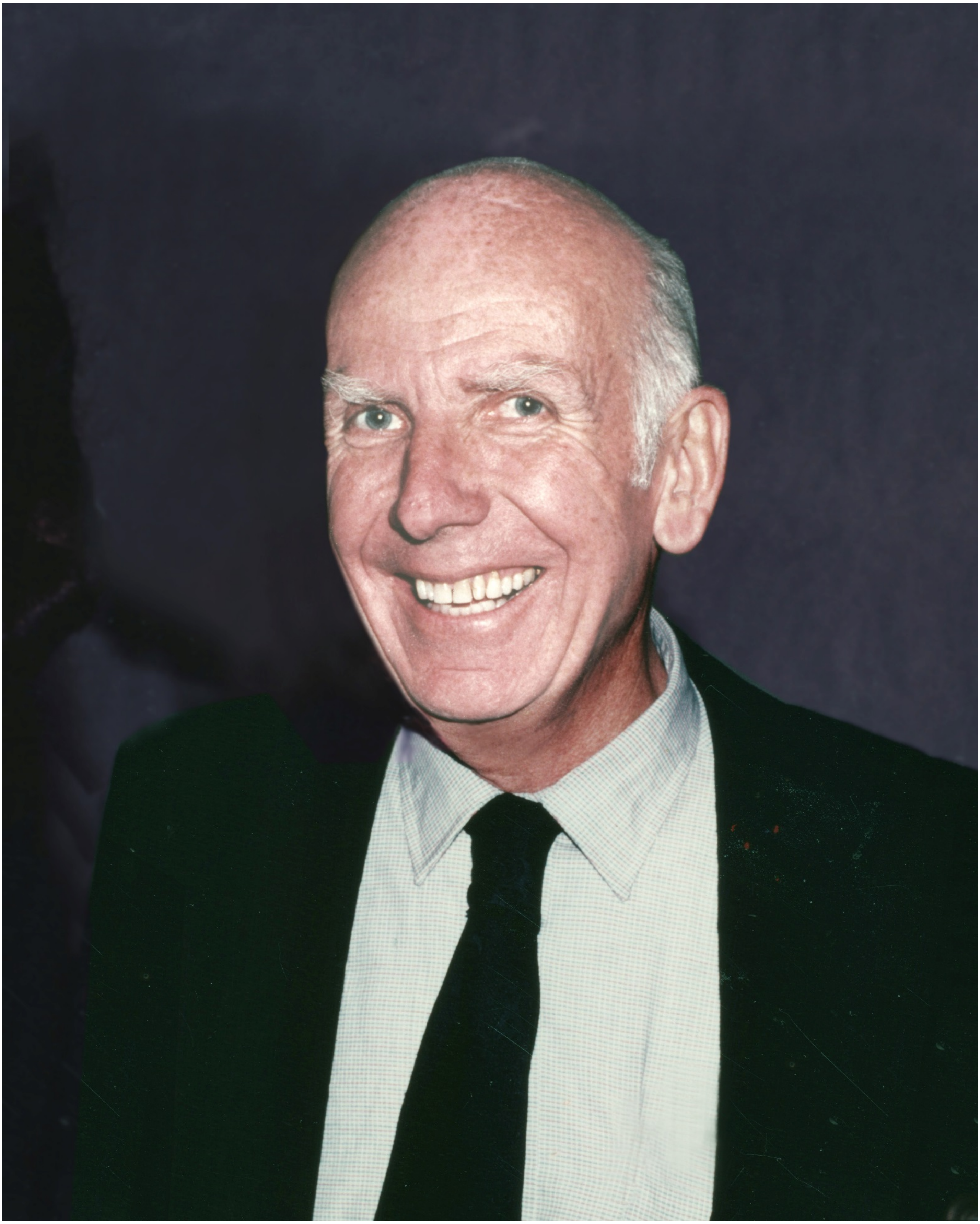
Photograph courtesy of Joanne Knopoff.



CONTENTS

LEON KNOPOFF, Ph.D., D.h.c.

Research Professor of Physics and Geophysics
and Research Musicologist
University of California, Los Angeles

EDUCATION

B.S. California Institute of Technology (Elect. Engrg.) cum laude	1944
M.S. California Institute of Technology (Physics)	1946
Ph.D. California Institute of Technology (Physics and Mathematics) cum laude	1949

ACADEMIC

Research Prof. of Geophysics, Institute of Geophysics, UCLA, and Research Prof. of Physics, Department of Physics, UCLA	1994-2011
Prof. of Geophysics, Institute of Geophysics, UCLA, and Prof. of Physics, Department of Physics, UCLA	1961-1994
Prof. of Geophysics, Institute of Geophysics, UCLA	1959-1961
Assoc. Prof. of Geophysics, Institute of Geophysics, UCLA	1957-1959
Research Associate in Geophysics, Institute of Geophysics, UCLA	1950-1957
Assoc. Professor of Physics, Miami University, Oxford, Ohio	1949-1950
Asst. Professor of Physics, Miami University, Oxford, Ohio	1948-1949
Professor of Geophysics, California Institute of Technology	1962-1963
Research Musicologist, Institute of Ethnomusicology, UCLA	1963-
Visiting Professor, Cambridge University	1960-1961, 1976-1977, 1986-1987
Visiting Professor, University of Karlsruhe	1966
Visiting Professor, Harvard University	1972
Visiting Professor, University of Chile	1973
Visiting Professor, University of Trieste	1984
Visiting Scientist, Laboratorio per lo studio delle dinamiche di grandi massi, Venice, Italy	1970

HONORS

Docteur Honoris Causa, Université Louis Pasteur, Strasbourg	2004
Honorary Professor, Institute of Geophysics, China Earthquake Administration, Beijing	2004
Fellow, Selwyn College, Cambridge University, 1986-1987	elected 1986
Member, National Academy of Sciences	elected 1963
Fellow, American Academy of Arts and Sciences	elected 1965
Member, American Philosophical Society	elected 1992

HONORS, Continued

The (H.F. Reid) Medal of the Seismological Society of America	1990
Gold Medal, Royal Astronomical Society	1979
Emil Wiechert Medal, Deutsche Geophysikalische Gesellschaft	1978
Golden Badge Award, European Geophysical Society	2001
NSF Senior Postdoctoral Fellow	1960-1961
Guggenheim Foundation Fellow	1976-1977
Fellow, American Association for the Advancement of Science	elected 1964
Fellow, American Geophysical Union	elected 1962
Honorary member, Seismological Society of America	elected 1990
Honorary member, Phi Beta Kappa	elected 1996
(Foreign) Associate, Royal Astronomical Society	elected 2003
Canadian International Cooperation Year Medal	1965
Listed in *Top 1000 Scientists: From the Beginning of Time to 2000 AD* by Philip Barker, Universities Press, Hyderabad, 2002	
Festschrift volume: ***Geophysical Journal International*** v. **143**, (2000) pp. 279-498.	
Honorary Symposium, UCLA	Sept. 14, 2000
Honorary Seminar, Strasbourg	June 15, 2004
Harold Jeffreys Lecturer, Royal Astronomical Society	1977
Beno Gutenberg Lecturer, American Geophysical Union	1992
Sidney Chapman Memorial Lecturer, University of Alaska	1988
Distinguished Geophysics Lecturer, Texas A & M University	1990
UCLA Faculty Research Lecturer	1972
Outstanding teaching awards, Physics Department, UCLA	1987-88, 1992-93, 2 awards in 1995-96
Oral History, American Inst. of Physics	1990
Oral History, UCLA	2003

REFERENCE LISTINGS

Who's Who in America
Who's Who in Science and Engineering
American Men and Women of Science
International Who's Who
Who's Who in the West
Who's Who in American Education
Who's Who in Science
McGraw-Hill Modern Scientists and Engineers
Chambers' Dictionary of Scientists
Dictionary of International Biography
Men of Achievement
Who's Who in Technology
Directory of American Scholars

ADVISORY BOARDS

Editorial Board, ***Science***	1985-1990
Educational Advisory Board, J.S. Guggenheim Foundation	1989-2006
Seismic Hazard Committee, City of Anchorage, Alaska	1999-2002

PUBLICATIONS

Author and coauthor of 232 scientific papers in refereed journals.

Author and coauthor of 134 other publications, including original papers in non-refereed journals, book chapters, and other reports and reviews.

Books:

The Crust and Upper Mantle of the Pacific Area
L. Knopoff, C.L. Drake and P.J. Hart, editors,
Geophysical Monograph No.**12**, American Geophysical Union,
xi+522 pp, 1968.

The World Rift System
L. Knopoff, B.C. Heezen and G.J.F. MacDonald, editors,
Tectonophysics, Vol. **8**, Nos. 4-6, 309 pp, 1969.

The Nature of the Solid Earth
E.C. Robertson, J.F. Hayes and L. Knopoff, editors,
McGraw-Hill Book Co., N.Y., xiv+671 pp, 1972.

The Upper Mantle
A.R. Ritsema, editor, K. Aki, P.J. Hart and L. Knopoff, assoc. editors,
Elsevier Publishing Co., Amsterdam; xii+644 pp., 1972.

Instabilities in Continuous Media
L. Knopoff, V.I. Keilis-Borok and G. Puppi, editors,
Contributions to Current Research in Geophysics, vol. **12**, Birkhäuser Verlag, Basel, 210 pp, 1985.

Earthquake Prediction, The Scientific Challenge
L. Knopoff, editor, Proc. National Acad. Sciences, vol. **93**, 3719-3842, 1996.

TEACHING

39 research students awarded Ph.D.
40 postdoctoral scholars from 17 countries

FIELDS OF EXPERTISE

Earthquakes, physics of earthquakes and of earthquake prediction, nonlinear dynamical systems, complex systems analysis, fracture mechanics and fracture dynamics, elastic wave propagation, structure of the earth's interior, plate tectonics, measurement of the earth tides at the South Pole, dating of ancient pottery by thermoluminescence, musical perception, etc.

MEMBERSHIP IN PROFESSIONAL SOCIETIES

American Association for the Advancement of Science (fellow)
Seismological Society of America (honorary member)
American Geophysical Union (fellow)
American Physical Society
Royal Astronomical Society

EDITORIAL BOARDS

Reviews of Geophysics and Space Physics	1963-1970
Tectonophysics	1973-1996
Wave Motion	1979-1984
Bolletino di Geof sica	1981-1986
Editor, Nonlinear Processes in Geophysics	1994-2000
Associate Editor, Journal of Geophysical Research	1996-2000

UNIVERSITY SERVICE (partial list)

Director, Institute of Geophysics and Planetary Physics, UCLA	1972-1986
Physics Department, UCLA, Graduate Admissions Officer	
Physics Department, UCLA, Chair, Space Assignment Committee	
Chair, University of California Systemwide Committee on Seismic Risk	1985-1989
Joint Chancellor's/Senate Committee on Seismic Risk, UCLA	1983-1988
Faculty Research Lectureship Committee, Academic Senate, UCLA Frequent Service since 1972, Occasionally Chair	
Budget Committee, Academic Senate, UCLA	
Committee on Computing, Academic Senate, UCLA (Chair, 1970-71)	1970-1972

SERVICE (Selected):

Secretary-General, International Upper Mantle Project	1963-1971
Chair, U.S. Upper Mantle Committee	1963-1971
Chair, International Union of Geodesy and Geophysics (IUGG) Committee on Mathematical Geophysics	1971-1975
Vice-Chair, IUGG Committee on Mathematical Geophysics	1975-1979
Organizer of Biennial Conferences of Committee on Mathematical Geophysics, IUGG	1972-1986
U.S. National Committee for the Geodynamics Project	1971-1975
U.S. National Committee for the International Union of Geodesy and Geophysics	1973-1977
Chair, U.S. Committee for the International Association of Seismology and Physics of the Earth's Interior (IASPEI)	1973-1977
Ad Hoc Committee on Seismology and Aftershocks, U.S. Atomic Energy Commission	1972
Governor's Earthquake Council, State of California	1972-1974
Committee on Earthquake Prediction, U.S. National Academy of Sciences	1973
U.S. Working Group on joint US-USSR Earthquake Prediction Program	1973
Earthquake Studies Advisory Panel, U.S. Geological Survey	1973-1977
Panel to Review US-USSR Scientific Exchanges, U.S. National Academy of Sciences, Board on International Scientific Exchange	1975
Senior Advisory Committee, Incorporated Research Institutions for Seismology	1985-1989
Steering Committee, Southern California Earthquake Center	1991-1999

PERSONAL

Place of Birth: Los Angeles, California
U.S. Citizen
Married to Joanne Van Cleef Knopoff; three children:
Katherine (Katie) Knopoff Wadley, Rachel A. Knopoff, Michael V.C. Knopoff

ADDRESS

Institute of Geophysics and Planetary Physics
or Department of Physics and Astronomy
University of California
Los Angeles, CA 90095

SUMMARY OF ACHIEVEMENTS

Knopoff and his students, postdoctoral scholars and colleagues have made seminal and pioneering discoveries and contributions in a wide variety of fields:

Nonlinear earthquake dynamics

- Studies of earthquake occurrence as nonlinear science
- Nonlinear dynamics of rupture in self-organization of earthquakes
- Highly popular model for earthquake and seismicity simulations
- Contributions to models of self-organized criticality
- Importance of geometrical inhomogeneities on self-organization of earthquakes
- Magnitude distribution of tectonically driven earthquakes is not self-similar
- Importance of subcritical creep on clustering of seismicity and to earthquake prediction
- Resurrection of the single-couple model of earthquake faulting

Earthquake Statistics

- Solution of the cause of the universality of the Gutenberg-Richter and Omori laws of earthquake distributions
- The inverse Omori law for the rate of occurrence of earthquake foreshocks
- Power law distribution of epicenter spacings
- Long-range correlations among large and intermediate-magnitude earthquakes

Geophysics

- Regionalization of the continental upper mantle into shields, rifts, stable younger regions
- Relatively cool “keel” beneath ancient continental shields to depths of about 400km
- Discovery of ultralow velocity channel under the Pacif c Ocean which is a zone of decoupling the convecting lithosphere from the middle mantle
- Attenuation of seismic waves: Earth’s mantle above 400km depth is much more attenuating than mantle below 400km.
- Attenuation of seismic waves: Introduced specif c attenuation factor Q into common seismological language
- Observation of 14-day and 28-day tides at South Pole
- Quantum mechanics calculations of the properties of metals at core pressures

Theoretical Elastodynamics

- Scattering and diffraction of elastic waves
- Properties of inhomogeneous materials, especially as a function of crack density

Archaeology

- Co-discoverer of thermoluminescence method of dating of ancient pottery

Music

- Studies of musical perception

HIGHLIGHTS OF THE RESEARCH OF LEON KNOPOFF

I. Theoretical Seismology

Knopoff has contributed to the theory of elastic wave propagation in inhomogeneous media with applications to seismology. This provided an understanding of scattering, diffraction, surface wave propagation. These results have application to the understanding and reduction of noise on seismograms, which are those often very large wiggles on the records that prevent the observation of desired signals on seismographic recordings for both earthquake and petroleum exploration purposes.

Gilbert and Knopoff published a theory of the diffraction of short wavelength elastic waves by curved obstacles with special reference to the diffraction and focusing of seismic waves by the core of the earth [24, 37]. This work laid the foundation for later studies of propagation in the presence of curved subduction structures in the earth and of waveform tomography in a spherical earth by Helmberger and colleagues.

Garbin and Knopoff published the first calculation of the average elastic properties of an elastic solid permeated by cracks [177, 190, 191]. Chatterjee, Mal and Knopoff published the first exact calculation of the elastic properties of a composite to second order in the concentration [224, 230]. The latter results are of importance in the field of non-destructive testing of materials permeated by hidden flaws. These papers showed that self-consistent methods of calculation were incorrect in the second order in concentration of extended inhomogeneities. Extension to the percolation limit at high densities was made by Davis and Knopoff [365].

Knopoff published a representation theorem for the full elastic wave equation. Buried in that solution was the demonstration that the displacement in the far field radiation from faulting is proportional to the velocity of slip on a fault [8].

Hudson and Knopoff published the theory of the scattering by statistical distributions of obstacles in a layered earth, and the nature of signal-generated seismic noise [80, 88, 90, 94].

II. Attenuation of Sound in Solids and in the Earth

Knopoff studied and analyzed the phenomenology and mechanisms for attenuation of elastic waves in solid materials [67]. Knopoff and MacDonald showed that the observation that the Q of solids at small strains is independent of the frequency is due to nonlinearity [11, 31]. Knopoff showed that attenuation of sound waves in solids, as measured in the laboratory, is due to non-linear grain-boundary sliding. Knopoff made the assumption that properties of attenuation in the laboratory could be extended to the earth's interior, and inverted surface wave and free mode observations to show that the upper mantle of the earth had a much greater attenuation than the lower mantle [67]. Hence the upper mantle may be closer to its melting point than the lower mantle. The use of the parameter Q to describe attenuation in the earth has become commonplace since Knopoff's paper with that abbreviated title [67].

III. The geophysical inverse problem

In the inverse problem, one wishes to determine the properties of an inaccessible region from measurements at selected locations (usually) on its surface. Knopoff showed that the geophysical inverse is non-unique, i.e. that one can never determine the properties of the inaccessible region with precision, and hence that it is incumbent upon geophysicists to indicate the range of uncertainties in their interpretations [40]. Keilis-Borok, Knopoff and a number of students developed the hedgehog method of inversion, which directly specified the class of solutions consistent with the data [178], and which is still occasionally used in the problems of geophysical inversion. Knopoff and Jackson constructed a solution to the problem of overparameterization of structure, i.e. how to fit the detailed structure (of the earth) with insufficient data [161].

Knopoff published the first computational solution to the fault-plane problem [41] and with Teng, the first computational solution to the travel-time problem [74], the first concerned with deriving the mechanism at the source of an earthquake and the second to determining both the location of an earthquake and the earth's structure between the earthquake and the seismograph.

IV. Observations of long-period earthquake surface waves

Knopoff made the first installation of temporary long-period seismic networks for the purpose of making synoptic measurements of earthquake surface waves [81, 82, 98]. Using these observations, he performed inversion of phase relations for surface wave observations to derive regional interior structure of the earth (see below). As far as known, Knopoff, Mueller and Pilant were the first to perform digital processing of long-period seismograms [81]. Nakanishi, Slichter and Knopoff developed an ultralong-period seismometer [205], installed at the South Pole and still in use today.

V. Interior Structure of the Earth

The seismic low-velocity channel at a depth of about 100 km and deeper in the earth, was identified and classified; this classification permitted the discovery of significant differences in the upper mantle of the earth between oceanic and continental provinces, the latter further divisible into shields, younger stable continents and active regions. Knopoff identified an almost global universality of upper mantle structure for these types of regions from local studies; this formed the basis for regionalizations used in later global studies.

Knopoff showed that continental upper mantle structure could be divided into three broad types, which are the ancient shields, tectonically inactive regions of intermediate age, and tectonically active regions including rifts [162]. This result depended on regional studies carried out with Biswas [178], Panza [236], Schlue [163], Fouda [189], Mueller [81].

Knopoff and his students Leeds, Kausel and Schlue, showed that the upper mantle in the Pacific had a waveguide with extraordinarily low S-wave velocities, fully 10 to 15% lower than at comparable depths in the shields, and that this must surely represent a zone of decoupling between the motion of the oceanic lithosphere and the deeper mantle [184, 202, 215]. The thickness of the oceanic layer was about 100 km at its oldest part. Implying the existence of very high temperatures at shallow depths under the Pacific; such high temperatures could only arise because of slip of the uppermost 100 km of the Pacific structure over the substrate during convection. From this and other information derived from surface wave analysis, the lithosphere is inferred to slide over the lower mantle over a decoupling layer which is the low-velocity channel. The low-velocity channel under the Pacific Basin is a zone of partial melting. The low-velocity zone is probably caused by partial melting, and the upper boundary of the channel is on the melting solidus.

Except for the purely oceanic Pacific and Nazca Plates, all major tectonic plates were discovered to have a very deep root or keel under the continental shields, extending to depths as great as 400 km, and hence there must be a large viscous drag force on these plates. These deep keels are zones of cooling, having cooled from the surface downward, and represent a major thermal constraint on models of convection in the earth's mantle [162, 276]. For example, part of North America and the Atlantic are joined in a common plate whose slip is retarded by the American keel.

Wielandt and Knopoff found that the structure under the East Pacific Rise has anomalous low velocities to a depth of at least 400 km and if there is a phase transformation at this depth, it must be significantly elevated under the Rise, thereby implying that convection in the mantle penetrates to at least this depth [262].

During the course of the work on surface waves, methods were developed with Biswas [140], Schwab [138, 149, 160] and Panza [168, 192], to synthesize seismograms by studying methods of superposition of higher modes of surface waves. To do this Knopoff developed an efficient method for solving problems of elastic waves in multilayered media [53]. Panza has applied these techniques with much success to the study of the strong ground motions observed in the near-focus region of large earthquakes, with relevance to the problems of response of buildings during earthquakes.

VI. Earthquake Statistics

The universality of the log-linear regularity of the Gutenberg-Richter magnitude distribution law suggests universality of process. Nevertheless, the strong geometrical heterogeneity of fault zones worldwide suggests that there should be large variations in these distributions from seismic region to region. Assumptions of universality derived from the distributions in complete catalogs, composed of both aftershocks and mainshocks, have been attributed to the properties of mainshocks. Knopoff has resolved the incompatibility [356] by showing that the aftershock population dominates the catalogs and that the statistical properties are mainly those of the aftershocks, which are in their turn mainly associated with the few largest earthquakes. The universality of the aftershock process has been shown to be due to the universality of the fragmentation of the neighborhood of major earthquake faults. The range of fragmentation distances is approximately 3 km in Southern California; the overwhelming majority of aftershocks are found in these low-strength zones. The distribution for mainshocks in Southern California with magnitudes greater than about 5 does not follow the standard Gutenberg-Richter law; scale-independent models are not applicable.

Knopoff's early belief was that the subject had advanced sufficiently to permit construction of models that could be used in either an earthquake prediction sense or in a risk analysis sense. To do this, Kagan and Knopoff began to study the statistics of earthquakes systematically and carefully. One of the important results from this work was the discovery that the barriers and asperities to earthquake rupture are geometrical in origin (rather than of geological origin), and the three-dimensional character of this geometry could be identified; in other words, earthquake faults are not plane surfaces!

Most small earthquakes occur in a damage zone nearby an earthquake fault. This zone is the residue of past epochs of major faulting. Thus most small earthquakes are not genuine predictors of subsequent instability on a major fault. However they do play the role of a stress gauge, and as such are a predictor of the onset of critical states on major faults.

Kagan and Knopoff were the first to establish the statistical validity of the 1/t law of increase of number of foreshocks before a main shock [227]. They showed that there is migration of earthquake epicenters, even among large earthquakes [204]. In the case of the largest earthquakes, there are correlations on a time scale of several years and at distances of up to 2500 km. These correlations were shown to be statistically significant. Thus even large earthquakes cluster!

Kagan and Knopoff were the first to identify the universal statistical regularities of spatial distributions of earthquakes. They identified the universal power law for the distribution of the distance between earthquake epicenters. Thus earthquake epicenters have a scale-independent self-similar distribution, with implications for the geometry of faults [244]. His recent work shows that this result is a property mainly of aftershocks which dominate the statistics.

Keilis-Borok and Knopoff showed that a certain class of intermediate sized earthquakes almost always precedes the strongest earthquakes in Southern California as well as in other parts of the world [241]. The time interval between the predecessor and successor earthquakes is about 3 years and the distances between them may be as large as several hundred km. Molchan showed that these correlations are statistically significant.

Kilston and Knopoff showed that there is a statistically significant correlation between the phases of the moon and strong earthquakes in Southern California. The cause of these correlations may be ocean loading off the Southern California coast [275]. These correlations are absent in the case of smaller earthquakes in Southern California, probably because most of the smaller earthquakes are aftershocks.

VII. Statistical Earthquake Prediction

Knopoff and Kagan incorporated geometry and creep-induced time delays into a stochastic model of a complex earthquake source, and succeeded in modeling short-term clustering of individual earthquakes as well [254]. They were able to apply this theoretical model of rupture with stochastic elements (which has only a small number of parameters) to earthquake risk analysis, i.e. to estimate the probabilities of occurrence of future earthquakes, at least on the short time-scale [295]. This work represents the first time a physical theory with a very small number of adjustable parameters has been applied to the extrapolation of an earthquake catalog for the purposes of prediction. As a gambling proposition, this short-term predictor offers about 1000:1 odds that about 40% of strong earthquakes will be identified in specified space and time windows; the time scale is from a few hours to a few days.

Keilis-Borok and Knopoff developed a systematic phenomenology for earthquake prediction. These systematic techniques were applied to the prediction in advance of the Armenian and Loma Prieta earthquakes [312]. The Loma Prieta earthquake was predicted to have a magnitude greater than 6.4, within a time window of four years starting from midsummer 1986, and in a roughly rectangular region about 600 km in linear dimensions, with a success rate of about 80%.

Knopoff and colleagues have shown that 28 out of 31 strong earthquakes in Northern and Southern California are preceded by a widespread increase in the rate of occurrence of intermediate-magnitude earthquakes over a time interval of from 2 to 10 years; the time interval is dependent on the size of the future great earthquake. However no such increase is found for small earthquakes [339]. What was most remarkable in the result was that seismicity was drastically reduced over a very long range compared with the size of the earthquake rupture. The dimensions of the future earthquake are so small compared with the size of the region of anomalous precursory activity, that a significant change in our attitude toward understanding earthquake stress fields is needed.

VIII. Modeling the Seismic Source and the Theory of Earthquake Prediction

Knopoff has been working on the development of a comprehensive theory of earthquakes, to explain from first principles the phenomenology of precursory quiescence, earthquake swarms, sudden increases in precursory seismicity, aftershocks, foreshocks. Knopoff has shown that there is a strong coupling of earthquake occurrence to modern developments in non-linear science, including the trappings of chaos, strange attractors, etc. There are four basic ingredients of any model of earthquake occurrence: plate tectonics to restore the energy dissipated in earthquakes, stress redistribution because of fracturing, precursory creep processes producing time delays between the times critical stresses are reached and the times to fracture, and the influences of inhomogeneity in physical properties, which are almost wholly geometrical.

Burridge and Knopoff constructed the first numerical model of a nonlinear dynamical system to simulate seismicity and faulting. In the last 12 to 14 years, this model has become a favorite of many groups for simulating self-organization and chaos in the earthquake dynamical system [92]. It has been cited many hundreds of times, both in the physics and geophysics literature on earthquake clustering. This paper has been a beacon in the non-linear modeling community for modeling earthquake processes.

Knopoff has argued that earthquake seismicity as a self-organizing process cannot be modeled on the basis of quasistatic models of fractures alone, but that the dynamics of the rupture process is extremely important in the analysis. His position is that what goes on during the brief moments of the rupture cannot be ignored, neither in the effects on human life and structures, but also in terms of the state of stress of the earth after the earthquake. He therefore began an extensive program of analysis of rupture dynamics, especially in the presence of the irregular geometry of faults. Irregular geometry can be modeled as a local increase in friction (fracture threshold) [350]. In the presence of fluctuating fracture thresholds, cracks grow subsonically; the fracture criteria must be applied on the moving edge(s), which are a form of Stefan problem [174, 259, 280, 359]. Of concern in these problems has been the decay of friction from the static state to the dynamic state and these have been explored analytically [357] as well as numerically [332].

Knopoff has applied these ideas to the problems of seismicity as a self-organizing system and has found that geometry is a pervasive influence on pattern formation, and indeed can lead to instabilities in the self-organization [326]. The properties of these systems are completely unlike systems that are popularly assumed to be spatially homogeneous. The postulate of spatial homogeneity has been popularized because of the scale-independence implicit in the Gutenberg-Richter distribution law of earthquake magnitudes. However Knopoff has shown that the scale-independence is not a property of the main-shock population that the modeling of (future) events is intended to simulate, but is instead a property of aftershocks, which dominate the statistics [356]. He has found that the mainshock population has a major transition scale at around earthquake magnitude 5, thus indicating a varying physics of rupture for earthquakes smaller and larger than these magnitudes.

In two widely quoted papers, Knopoff calculated the energy to be released in sliding on a source model of an earthquake [12], and Burridge and Knopoff [70] identified the equivalence between body forces and seismic dislocations (this work was independent of Maruyama's, but Maruyama published shortly before B+K; the B+K paper is the more often quoted, and is significantly more general). Among other results, it was established rigorously that a seismic source will radiate seismic waves with a double couple focal mechanism.

Knopoff observed that pre-shock creep is an extremely important influence affecting clustering of earthquakes. (If earthquakes do not cluster in time and in space, there is no hope for earthquake prediction; the problem is to unravel the observations of earthquake sequences to determine the mode of clustering.) Yamashita and Knopoff, and Chen and Knopoff have been concerned with physical models of stress corrosion in producing clustering such as aftershocks [296] and foreshocks [302]; they have even been able to reproduce complete clustering histories that start with earthquake swarms, followed by an extended period of seismic quiescence, followed in turn by foreshocks and a main shock [299]. Such sequences have been suggested as possible precursory histories before great earthquakes. Yamashita and Knopoff demonstrated that the intermediate-term, intermediate-magnitude clustering of earthquakes before very strong ones, is caused by some form of accelerated precursory creep, a two-dimensional spatial distribution of earthquake faults and a fluidized environment for faulting. This can generate precursory spatio-temporal quiescence under the proper geometry of a strongly faulted region [324].

Knopoff and his students have shown that the Gutenberg-Richter magnitude-frequency law is not a property of self-organization of stresses on a single fault, but is instead a characteristic of seismicity on a complex fault system. In their model, characteristic earthquakes and highly fluctuating spatiotemporal distributions take place in the presence of a distribution of inhomogeneity of the frictions that regulate faulting. Thus the earthquake self-organizing system is not in a critical state, heterogeneity is important in the evolutionary process for earthquakes, and spatial localization of earthquakes is a consequence of the geometry [325].

The phenomenology of precursory intermediate-magnitude, intermediate-time clustering before strong earthquakes (see above) has forced us to change our way of thinking about earthquake mechanism. Knopoff has proposed that the phenomenology can only be explained if it is supposed that the fault system of California is permanently permeated by water at high enough pressure to reduce the sliding friction to virtually zero. The fault is prevented from slipping by an array of very strong patches of "glue". When the earth is at a critical state for the onset of subcritical creep, the smaller patches begin to degrade by stress corrosion at an accelerated rate, and ultimately these break in intermediate-magnitude earthquakes. When the strong event occurs by the same mechanism of stress corrosion, but this time in the fracture of a major patch, the precursory process is switched off because the stresses are redistributed to great distance by triggered sliding along the fluidized faults that adjoin the patch(es).

IX. Equations of state of earth-forming materials

MacDonald and Knopoff published a confirmation of the Birch hypothesis that the outer core is not pure iron [17, 30]. To do this, as far as is known, they were the first to use shock wave data to identify the deep composition of the earth. They proposed silicon as an alloying agent for the core; this early proposal was much criticized, but has recently re-emerged as a popular candidate. The problem is important for understanding how the earth condensed out of the primitive solar nebula. Bukowinski and Knopoff used quantum mechanics to determine the properties of iron [198] and potassium [219] at core pressures and found that a proposed outer shell electronic transition in potassium might justify its use as an alloying agent in the core; there are other, geophysical reasons why it is not a suitable alloying agent, but the potassium transition cannot be excluded on structural grounds.

X. Earth Tides

From observations made at the South Pole, Rydelek and Knopoff published the first accurate, direct observation of the amplitude and phase of the 14- and 28-day lunar earth tide [260, 266]. From the phase lead of the 14-day tide observed at the South Pole, they were able to infer that the worlds oceans are not equilibrium oceans at these periods [303], i.e. that the tides cannot be calculated from static attractions of sun and moon, but must instead be derived from a dynamical theory, even though these periods are far from the period of resonance of the oceans.

XI. Thermoluminescence Method for Dating Ancient Pottery

Kennedy and Knopoff were the first to date ancient pottery by thermoluminescence techniques [35, 36]. Without significant modification, it is the procedure that hundreds of workers in archaeology and art history are using today.

XII. Systematic musicology, Economics and Linguistic Structure

Knopoff and Hutchinson have published a number of quantitative tools for doing melodic stylistic analysis [218, 225, 230, 238, 255, 271, 298]. They have established perceptual alphabets for melodic temporal pattern recognition and have identified temporal windows for short term pattern recall. They have established that human physiological processes have strong influences on the way in which a society develops language.

In an application of his work on nonlinear dynamics of earthquakes, Knopoff has shown that personal decision making, psychological influences, and creativity have an inordinate influence on economic structure and as a consequence economic prediction by formal time-series analysis is not justified; economic systems are open systems and the human influence cannot be ignored.

In the area of written language as a complex system, grammatical structure, i.e. long-range correlation, plays a vital role in its self-organization, and hence nearest-neighbor structural analysis, such as through Markov process analysis, is not justified. In the area of musical structure, these problems are even more intricate because several sensory detectors (of tonal duration, pitch and loudness) are all in interactive play simultaneously [305].

INTERVIEW HISTORY

INTERVIEWER:

William Van Benschoten; B.A., History, University of California, Riverside, 1990; M.A., History, University of California, Riverside, 1991; C.Phil., History, University of California, Los Angeles, 1995.

TIME AND SETTING OF INTERVIEW:

Place: Knopoff's office at UCLA.

Dates of sessions: August 20, 22, 27, and 29, and September 3, 2003.

Total number of recorded hours: 9.

Persons present during interview: Knopoff and Van Benschoten.

CONDUCT OF INTERVIEW:

The interviewer supplied the spellings of proper nouns. Center for Oral History Research staff compiled the table of contents and interview history. Leon and Joanne Knopoff reviewed the transcript. They verified proper names, made a number of corrections and additions, and provided the Curriculum Vitae.

The transcript of this interview is a verbatim transcript of the audio recording. It was transcribed by a professional transcribing agency using a list of proper names and specialized terminology supplied by the interviewer. The interviewee was then given the opportunity to review the transcript in order to supply the missing or misspelled names and to verify the accuracy of the contents, and those corrections were entered into the text without further editing or review on the part of the Center for Oral History Research (COHR) staff.

In some cases the audio recording may differ slightly from the transcript, either because the transcriptionist did not accurately transcribe what was said or because of the changes the interviewee made at the time of their review.

This interview was generously supported by Gold Shield, Alumnae of UCLA.

For further information on the work of Dr. Knopoff, please consult the following website: leon.knopoff.com.

SUPPORTING DOCUMENTS:

The original tape recordings of the interview are in the university archives and are available under the regulations governing the use of permanent noncurrent records of the university. Records relating to the interview are located in the office of the Center for Oral History Research.

TAPE NUMBER: I, SIDE ONE

August 20, 2003

VAN BENSCHOTEN: Today is August 20th, 2003. I'm with Leon Knopoff. Did I say that correctly?

KNOPOFF: Perfectly.

VAN BENSCHOTEN: Okay, good. And this is tape one, side A. We should start with maybe if you could give us your full name.

KNOPOFF: My full name is Leon Knopoff, no middle names.

VAN BENSCHOTEN: Oh, okay. Where were you born and when?

KNOPOFF: I'm a native of Los Angeles, born July 1st, 1925.

VAN BENSCHOTEN: What part of Los Angeles were you born in?

KNOPOFF: East Los Angeles, born in Boyle Heights and lived most of my childhood in Boyle Heights and City Terrace.

VAN BENSCHOTEN: If you would, describe a little bit what it was like growing up there. What was Boyle Heights like?

KNOPOFF: Well, it was a very, very mixed community, people from many ethnic backgrounds. Today, it's almost 100 percent Hispanic, but in those days it was a very large Jewish community. I think they were the largest fraction, but there were people from other origins; Hispanics, blacks, Japanese.

VAN BENSCHOTEN: Describe maybe the neighborhood. What kind of neighborhood was it?

KNOPOFF: Working-class neighborhood. My parents were working-class people. My father [Max Knopoff] had been a carpenter until the Great Depression, and then was a milkman until the mid-thirties, and then he resumed carpentry.

As I look back on it, it was a very happy time for me to be growing up. There was a great neighborhood camaraderie among the kids, and the parents had a very, very close and protective attitude toward their children. It was a wonderful time to be a child.

VAN BENSCHOTEN: Let's start maybe with your grandparents, if you could talk a little bit about them. Maybe if you could start on your father's side.

KNOPOFF: I have no idea of my grandparents, except as they were told to me by my parents. My parents [Max and Ray Knopoff] immigrated to the United States in their teens, each individually, and met in Los Angeles and were married in Los Angeles, and they never returned to Eastern Europe where their parents were, and I have no information really about them.

My father was one of five siblings. He was the second youngest and made his way to the United States at the age of eighteen in 1914 to escape service in the czar's army, and just came months before war broke out and worked as an itinerant carpenter in the eastern part of the United States, mainly small, small cities, New Haven, Cleveland, Cincinnati, before coming to Los Angeles.

My mother emigrated from Eastern Europe also. She was at the age of sixteen when she came, in the company of an older half-brother Leib Singer. And she came also to New York in, I guess, 1912, and came to Los Angeles in the same year that my father did, 1921. They met here as part of the working-class movement on the Eastside of Los Angeles and were married in 1925. They were married in ’21—sorry. They were married in ’21, just at the time that they arrived, and in 1925 I was born.

I have all sorts of family anecdotes about the time— Well, on my mother’s side. I don’t know if you really want to hear about that.

VAN BENSCHOTEN: Oh, yes, that would be great.

KNOPOFF: My mother was one of four children of her mother Brindl Fluss Schor, but one of only two children of her father. Both my grandmother and grandfather were second marriages to each other. My grandfather wanted desperately to have a son to say the prayer, the Kaddish for him after his death, and he had no children by his first wife. And my grandmother was a recently widowed woman with two sons by her first husband, and therefore had a proven track record of having produced sons. And so my grandfather, Hersch Schor, divorced his first wife by a traditional method, which is just to say, “I divorce you. I divorce you,” you say it three times, and they were divorced in the front of rabbinical authorities, and he married my grandmother.

She bore him a son, and that was wonderful, and then to his great dismay, she bore him a daughter, my mother. And then my grandfather died, and my grandmother and her two children, her last two children, moved to across the border from Russian Poland onto the Austrian side of the border, and they went to work in Lemberg, now Lvov, and a big city in Eastern Europe. And they put together enough money so that two children could come to the United States.

VAN BENSCHOTEN: Just before the war?

KNOPOFF: 1912 for my mother. In 1914, for my father, yes.

VAN BENSCHOTEN: Let's talk a little bit. You mentioned that both of them came here, out to the West Coast, in 1921 as part of the working-class movement. What was that movement?

KNOPOFF: Well, in the 1920s, and undoubtedly before, and into the thirties, there was a great polarization between the working class and the— What's the right word?

VAN BENSCHOTEN: Entrepreneurs?

KNOPOFF: The entrepreneurial class. And there was great activity in terms of trying to organize labor, to work for better working conditions, and they were generally on the left wing. And on the Eastside of Los Angeles with the large number of people who spoke Yiddish as a language of origin, there was a Jewish labor movement as a subclass, and my parents were part of that.

My parents came independently to Los Angeles, because they each had a relative here in Los Angeles. My father had a first cousin, and my mother had a half-brother, one of the brothers from the earlier marriage of my grandfather, both living here, both working in non-entrepreneurial jobs; my father's cousin was a barber and my half-uncle, Sam Singer, was a tailor in the garment industry.

VAN BENSCHOTEN: Did they both end up in East L.A. then?

KNOPOFF: They both ended up in East L.A. And this was a time of great interaction. People who spoke the same language and had common interests would interact socially, and it was a very vibrant time.

VAN BENSCHOTEN: What kind of education did your father have, and your mother, for that matter, if they had much?

KNOPOFF: I don't think they had any formal education at all. I don't know what their education was like in Eastern Europe. My father was from White Russia, my mother from Russian Poland. I don't know. But I say with great pride that they were extraordinarily intelligent, intelligent people. They were skilled in communication, in written English, and my father in mathematics, with enough arithmetic to enable him to figure the cost and make proposals for jobs of construction. He was a contractor in his later years, and my mother was doing sewing in the ladies garment industry.

They spoke English with accents, and the family language was Yiddish. I spoke Yiddish until it was time for me to go away to kindergarten at the age of five. So that's my first language.

VAN BENSCHOTEN: Now, when you're a young boy growing up, did they talk much about the Old Country, about where they had come from?

KNOPOFF: Very little. My interpretation, my own interpretation of their youth, was that it was an effort to break away from the old traditional ways of living and that they wanted to set out for themselves on their own standards. So except for the preservation of the old language, I don't think that there was much effort made at trying to establish contact on a basis of traditions of the Old Country.

There was a certain amount of recall, but I don't— My father would sing, he was a very fine singer, and so he would sing some of the old songs once in a while.

But I know very little about the tradition, because I think that my parents tried to break away very forcefully from that old tradition.

I know nothing about the Jewish religion. I was brought up in a completely areligious household, and that was part of the breakaway process. It was really a polarization that put the traditions, the way people interacted socially in the Old Country, and separated it from the new life.

VAN BENSCHOTEN: So that was one of the questions I was going to ask you, you anticipated it, but the impact of religion on your family. So there was none?

KNOPOFF: Except insofar as my father was anti-religious.

VAN BENSCHOTEN: It worked the other way.

KNOPOFF: I can remember often as a child my father reading the newspaper, and when he would read that some religious person, a church official or somebody, would say something and would be quoted in the newspapers, my father would immediately react negatively. But I think that's about all. We had no experience.

VAN BENSCHOTEN: How active was your father in the labor movement?

KNOPOFF: I think fairly active. In the late twenties, the bottom had already fallen out of the building trades, construction, even before the Great Crash of '29, and my father reasoned that everyone needed milk. So he decided that the way to keep his family supported was to become a milkman, because people would— And he went to work for Borden's [Borden Dairy Co.]. Borden's was then a milk-delivering company in Los Angeles. And very shortly thereafter, the men went on strike against Borden's because Borden's refused to pay a bonus for the men bringing in customers, new customers, who were largely their own friends and acquaintances, and so the men went on strike.

But Los Angeles at the time, as I understand it, 1930, I was only five or six years old, had not only a— Well, it had the infamous Mayor [Frank L.] Shaw, who was later recalled, but it also had a police captain [William “Red” Hynes] who was very anti— He always saw Red whenever he saw anything that was anti-establishment. And they attempted to break up the strike, and ultimately Borden’s went out of business. I don’t know if this was the prime mover.

My father, I think, was in fact hassled on the picket line. A family legend has it that he was put in jail overnight, but that was for labor activities, but I never heard him verify that to me.

But, yes, there was a large agitation about working conditions and class disparity. This was part of the atmosphere in which I grew up.

VAN BENSCHOTEN: Exactly. It’s the Depression.

KNOPOFF: Oh, yes.

VAN BENSCHOTEN: Before we get to that, because I’d like to maybe learn a bit more about what it was like growing up in the Depression, especially in L.A., but describe, maybe, your father’s personality. How would you describe him?

KNOPOFF: [Pauses] Oh. He was a quiet man, a hardworking man, but not afraid to speak out on issues that were important to him.

I’m an only child. He and my mother both were very, very protective of me, they sheltered me, and they made a very strong effort to keep the family situation out of my sight. Whatever financial hardships there may have been, I really never knew about it. I can remember the day the banks closed, and that was— I can remember moving from one house to another house because the rents were cheaper as the houses got smaller, but the reasons for that were never exposed to me. I was really very sheltered.

VAN BENSCHOTEN: Yes, I understand. That's interesting.

KNOPOFF: They were very protective.

VAN BENSCHOTEN: Did your father have hobbies? Did he have interests?

KNOPOFF: I'd say that his main interest was his music. He belonged to a choral society called the Freiheit Gesangs Verein, and in the chorus they sang a lot of not only traditional music from Eastern Europe, but also Jewish songs of protest. There was a lot of poetry and a lot of music that was associated with the Jewish labor movement, and I heard all this, so it was fine. It was a wonderful time.

VAN BENSCHOTEN: So a strong sense of community then, too?

KNOPOFF: A strong sense of community, yes.

VAN BENSCHOTEN: We've talked a little bit about your father. Let's turn to your mother. Now, your mother comes here. What type of work did she do?

KNOPOFF: She was in the garment trade. She was working on ladies' coats, sewing ladies' coats. This was, and for a long time was, piecework. You were paid for by how fast and how much you could produce. It was hard work.

Shortly after her arrival in New York, her older half-brother had committed suicide, and that left her alone at the age of, I guess, seventeen. And she went to work in the needle trade, and she stayed in New York from 1914 upon her arrival, until she came to Los Angeles in '21 and then started again here.

But as soon as my parents were married, she became a homemaker. She was also a quiet woman, again with a very, very deep sense of right and wrong. She had a great sense of morality, and she knew what was going on that was right and what was not. I think it was a very, very strong influence on me, and she wasn't afraid to express herself about it. But she was a very, very intelligent woman.

My wife, Joanne [Van Cleef Knopoff], has said frequently how much my mother was ahead of the times by putting into practice personal health habits and the kinds of food we ate, and these are the way people prepare foods and tend to their personal health today, and she was doing this back in the twenties and thirties. She was way ahead of her time.

VAN BENSCHOTEN: I assume, like your father, she did not have that much formal education.

KNOPOFF: She didn't have any formal education either, but she was self-taught, went to night school. I don't know whether my father did, but she went a lot to night school.

I don't know if I'm supposed to let out family secrets at this point, but my father was a great pacifist. As I remarked, one of the reasons he left Russia was to avoid service in the Czar's army. And soon after, three years after, arriving in the United States, the United States was at war, in 1917. He did not obey the call, the draft call, and for a number of years thereafter, he was worried about his legal status in this country because he had disobeyed the national law.

But at the time of the start of the Second [World] War, he saw that as a highly moral and principled conflict, and so in the late thirties and through the war, he enlisted his talents in the war effort. He went to work at the shipyards doing carpentry on the ships. You wouldn't think there would be carpentry on the ships, but, in fact, Henry J. Kaiser operated a shipyard in Long Beach [California] in which he made concrete boats, and the carpenters had to build the forms to pour the concrete for the boats, and so my father worked for Kaiser Shipyards.

And to do that, there were some issues of citizenship, and my mother, who had already become a citizen, then got my father to become a citizen because he was married to one. I remember I was already a teenager at the time, but I did hear the conversation that he was very concerned about whether or not his past would be brought up. But it wasn't. He was very, very relieved.

VAN BENSCHOTEN: I know that during the Depression, FDR [Franklin Delano Roosevelt] and the New Deal, and many interesting, fascinating things were happening. It's a great time of upheaval and tumult. How politicized were your parents? Did they take part in the political elections? Were they citizens by then?

KNOPOFF: No, no.

VAN BENSCHOTEN: Not yet?

KNOPOFF: No, no, no. They didn't get citizenship until later in the thirties. But their sympathies, of course, were President— [Herbert C.] Hoover was a hated individual and their sympathies were all with Roosevelt, who was going to be the savior of mankind.

I can remember in the 1932 campaign, my mother took me out of school because Roosevelt had come on a campaign stop to Los Angeles, I think it was. It must have been a campaign stop. He was staying at the Biltmore Hotel. She took me out of school, and we went. She took me by the hand, we took the streetcar downtown, and we went to Pershing Square to see Roosevelt. He appeared on the balcony, and he gave a speech, a campaign speech. Pershing Square was absolutely jammed all the way from Hill to Grand Avenue, with humanity, and the closest we could get was one block away. Of course, I was small, and I couldn't see Roosevelt from a block away— There was some little figure on a balcony. But the enthusiasm was grand.

I remember going to school on Election Day in '32, and so I was then seven. The school was surrounded by a cyclone fence with a gate in the fence, and these two upper-class elementary school kids—they were much larger than I was—were taking everyone aside who walked in through the gate to the schoolyard, and I was included. When it came my turn, they took me, and they jammed me up against the fence outside the schoolyard, and they said, "Who are you for, Roosevelt or Hoover?"

And I said, "Roosevelt." I passed. [mutual laughter] But it was a—

VAN BENSCHOTEN: A lot of enthusiasm.

KNOPOFF: I don't think I knew of anyone who was pro-Hoover.

VAN BENSCHOTEN: Not a popular man at that time.

KNOPOFF: Not a popular man.

VAN BENSCHOTEN: He's been rehabilitated recently, especially under [President Ronald W.] Reagan, but still that's a long way away.

Okay. I wanted to ask another question about your family, and then we'll move on. How would you describe your parents' marriage?

KNOPOFF: In what way?

VAN BENSCHOTEN: Was it a fairly harmonious union? Did they have their own sort of separate worlds? There's many different ways that people conduct a marriage. I was wondering, how would you characterize their marriage? Did they do many things together?

KNOPOFF: [Pauses] After I was born, and it's really, of course, the only time that I know, my mother's life was centered on me, and so there were few times that she went off on her own. I can only remember one time where she went off and did her own thing.

But weekends, evenings, we were all always together. My father was a hardworking man, and so I think there wasn't a lot of time spent in family life, because he was often resting or relaxing. For example, a milkman in those days would sleep in the afternoons, so I had very quiet afternoons for fear of waking my father.

But we went on outings together. We went to concerts together. I can only remember one time where my mother went off by herself, and she was the more athletic. She could swim long distances, and my father's swimming skills were not as well developed. But she was also interested in hiking. They both had an interest in the mountains before they were married.

So I can remember one day, it's the only time it ever happened, that my father and I drove my mother in our pickup truck, because milkmen had their own pickup trucks to put the milk in the back, and we drove her to the foot of the Mount Wilson trail at Sierra Madre. She set off with a candle in her hand, which she would then light when it got dark in the late afternoon, so she could get up to the top of Mount Wilson by dark on Friday evening, because on Friday evenings they had astronomy lectures at the hotel, at the Mount Wilson Hotel, and she stayed overnight. And that's the only time that I remember that she ever went away and wasn't with us overnight. Then the following day, I guess it was, we drove to the trailhead in Sierra Madre, and there she appeared, stump of candle in hand. I don't know; that may be an exaggeration. I don't know.

My father never took a holiday without us. After I was born, he would go to chorus rehearsal on his own. That was his once-a-week activity. We were a strongly and closely associated family.

VAN BENSCHOTEN: It sounds like it.

KNOPOFF: And for me it was a very happy time, a very happy time.

My father was toil-worn. My mother was a hard-working woman, too, and she participated as much as she could in support of my father.

VAN BENSCHOTEN: Did you ever get to a point where she considered perhaps going back to the needle trade?

KNOPOFF: Oh, she did.

VAN BENSCHOTEN: Oh, she did?

KNOPOFF: My father died at a very early age. He died just two days short of his fifty-first birthday, and I was then a graduate student. Despite the extraordinary sadness of that event for both of us, she was resolute, she would not listen to anything I said, and she was back in the needle trades almost immediately, where she continued until her death fourteen years later.

VAN BENSCHOTEN: But before we move on and talk a little bit about yourself as a young child, we already have in part, but talk, maybe, about do you have any more memories of growing up in the Depression? The story you told about FDR I thought was fascinating, when the banks closed, you remembered that, and that's pretty good.

KNOPOFF: Oh, we were renting a house, one of our many houses. You have to understand that my childhood, we lived a very, very nomadic life. I've remarked that we moved from place to place frequently, I think as part of the impact of the Depression, during the Depression years. But in the later years when my father was building houses, we also— My father built houses on speculation, and he would buy a vacant lot. We had enough money so he could buy a lot. He would have all the— He would take out a loan so that he could build the house. Then we would move into the house until it was sold, and that gave enough of a bankroll so that he could pay off the loan, then go out and buy another lot and build another house with another loan.

Well, so we moved into the house. As soon as it was sold, we had to move out, and we would rent a house until the new house was completed, and then we would move in again to the new house. That would then be sold after a while, and we would— So in the thirties, later thirties, we were really moving from place to place.

But getting back to an earlier time when the banks failed, we lived on Rowan Avenue and I must have been— I was four years old, I'm certain, because— My mother didn't explain, but one morning she said, "You remain here in the house," which was very unusual, "and play by yourself. I've got to go to attend to something." And she walked— And so I was playing, and one of the curtains on a roller, roller curtain, you know, these roller shades, and I was tugging on it, and, of course, it came all the way down and off the roller, and I panicked. I'd made a misdeed. And I knew where she was going. She said she was going to the bank. And so I left the house and ran to the bank. This was about a distance of a mile, and I crossed a lot of streets.

I came to the bank, and there was this bank on the corner of Wabash and Evergreen, the United States National Bank, and there was this crowd. To a four-year-old child, it seemed like a great crowd, a huge crowd. But I'm sure it wasn't that big, and it was milling around the closed door of the bank. I found my mother, and I tried to tell her that I had pulled the roller shade down on me, which was my universe. [laughs] The bank's doors didn't open, of course, and finally the crowd dispersed and we walked home.

I asked what had been going on, and it was explained to me that we had a savings account in the bank, but the bank never reopened and we lost the money. But it really wasn't much. For us it was— I forget, I don't think it was as much as a hundred dollars, but that was a good sum in those days.

VAN BENSCHOTEN: Yes, it was at that time.

KNOPOFF: I couldn't tell you how much was involved, but it was explained to me.

VAN BENSCHOTEN: Any other strong memories of the Depression?

KNOPOFF: Oh, I have one strong memory. In one of our later moves to an even smaller house, and it was one that we lived in for about four or five years, which was a good long time. It was on Marengo Street, not far from the [Los Angeles] County Hospital, about two blocks east of there, and it was in a bungalow court.

We had one bedroom, a living room, kitchen, and a bathroom. I had the bedroom. My parents had the living room, with a pullout couch, pullout bed, which could be made into a couch. We ate in the living room. My mother had gone shopping and I was alone in the house, and it must have been summertime. The knock on the screen door—it may have been a doorbell; I don't remember—and I went to the door, and there was a young man on crutches, and he had only one leg, and he was trying to sell candy, and I told him to go away. He went away.

And when my mother came back, I told her about it, and very quietly she reproved me. She told me this had been a veteran of the First [World] War, and he was worse off than we were.

VAN BENSCHOTEN: Okay. I'm going to flip this over.

TAPE NUMBER: I, SIDE TWO

August 20, 2003

VAN BENSCHOTEN: This is tape one, side B now.

All right. We've talked about your family. Let's talk a little bit about your upbringing. We've already had sort of glimpses of that already in some of the stories that you've told us. Describe yourself as a young boy, and by "young," maybe five, six, seven. How would you describe yourself?

KNOPOFF: [Pauses] Well, I was the object of my parents' affection, being the only child. They were very protective, but they were in no way making me— They did not make me— They would in no way give in to my whims. I was very knowledgeable of the family situation, and I wanted very much to be a participant in it. I think I was not allowed to be a participant because of my age and strength and so on, but I very much enjoyed the family events.

I didn't interact much with other children my age, but that is associated with my educational experience, too. I'd been taught a little bit about reading, quite a bit of reading in English at home, which my mother and I were learning together, and when I went off to kindergarten, I must have arrived a few days after kindergarten began. I spoke only Yiddish, because that was the only language I'd really heard, and I hadn't played much with other kids my age. And I remember that the kids in the kindergarten yard were all scurrying around on some construction or other in the schoolyard, and I stood on the periphery, and I was not part of it.

By the end of the kindergarten half-year, in those days in school there were two sessions per year, and you could start the school year in September or you could start it in February. And by the end of the school half-year, I was underage to start first grade. I was only five-and-a-half, but my reading skills in English and my speaking skills were so profound that the kindergarten teachers, who had advanced all the other kids up to first grade, were left with this brilliant kid who was being left behind because of age. So a kindergarten teacher, with my mother in attendance, dragged me over to the principal of the elementary school, whose name was Wilhemina Van de Goorberg. [laughs]

VAN BENSCHOTEN: A good Dutch name.

KNOPOFF: It was a good Dutch name. I say this for your edification. And I performed, apparently. I don't remember anything about it. And it was decided to give me an IQ [intelligence quotient] test. So we went downtown to the Board of Education in downtown L.A., and I apparently passed the IQ test with some incredible score—I don't know what it was—and I was immediately placed out of kindergarten and into the upper second grade. And subsequently, I skipped the lower third grade.

So I was in school, and my playmates were always about two-and-a-half to three years older than I was, and in the early years, that was a significant age difference. And even into high school when people started developing their social skills as a teenager, I was still a twelve-year-old when the other kids were fifteen. I was not as strong physically as the other kids, and when one got into— When I was nine, ten, eleven, the other kids of my age had their own friends, acquaintances that they'd made in school. So I was pretty much on my own socially.

The children of my parents' social group turned out to be of a slightly older age themselves, these children, so they took care of me. They, in fact, liked me and protected me. I was well taken care of.

So I sort of went through— I was a kid who could— Anytime there was a task put before the— An intellectual task put before the group, it was always I who could do it. If it was a manipulative task, then it was pretty bad.

We were looking at my report cards from elementary school some weeks ago. They emerged in the family archives, which we still have. And I have bad scores in art, for example, in the fifth grade; just terrible scores. And I still have bad— I'm still unable to draw any pictures, but I don't attribute that to the— But at the time I was not up to the standards of the other kids in manipulative skills.

After school, in the early years, I continued to get training in a Yiddish-language, secular school. So I've never learned Hebrew. I don't know anything about anything in Hebrew, but I can read, write, and speak Yiddish to this day. But at the age of nine-and-a-half, we even stopped that. From the age of about six till about the age of about nine-and-a-half, I went to this Yiddish school. I have another anecdote to tell you about that later.

And then my father, who was a milkman, had a customer who couldn't pay his milk bill, and so it was decided that payment for the milk bill would be this customer's family upright piano and thirty-five dollars. Now, my father paid the thirty-five dollars, and I can't be sure that it was the upright piano and thirty-five dollars, or the bill was thirty-five; the milk bill was thirty-five dollars. I don't know.

So my father put the upright piano, which was very heavy, with someone else, in the back of his truck, brought it home, and then my father refinished the piano, because he was a carpenter. So he put a beautiful mahogany stain on the bench and the piano. It was a gorgeous work of art, much work, much more than the piano deserved. It had a good tone, he had finished it, and then there was nothing for me to do; I had to take piano lessons. But you couldn't do piano and Yiddish lessons at the same time. That would just be too demanding of my time. So we gave up the Yiddish.

In later years, after my father had died, I got in touch with his brother [Volodya Somerov], who had remained behind in then Leningrad, which is where that family, my father's family, had moved to before the Second War. And after [Joseph] Stalin's death, we got in touch and we started communicating, and the only common language was Yiddish, and I had to write my letters in Yiddish. And I realized by comparing the letters that my uncle sent to me in return with my handwriting, that my handwriting was that of a nine-year-old child.

VAN BENSCHOTEN: That's fascinating.

KNOPOFF: You know, your handwriting in English at the age of nine does not resemble—It's a childish— I cannot form the letters as an adult can. I just can't do it.

So I had to start taking piano lessons, which I did for six years. I became pretty good at it.

VAN BENSCHOTEN: Who gave you lessons? Was it a private instructor?

KNOPOFF: For one year, we had somebody come. Oh, yes. We had somebody come to the house once a week, and he wasn't awfully— My parents didn't think we were making an awful lot of progress, but my father had another customer in the same social circle who was taking music lessons at the L.A. Conservatory of Music and Arts, a very imposing name. They were down on Figueroa Street, which was at that time automobile row and just south of Pico [Boulevard], and so I started music lessons there. I had music lessons with Mrs. [Adeltha E.] Carter, and she ran the school. And I'd go once a week on the streetcar down to Pico and Figueroa and go up the stairs to the Conservatory and take my music lessons, come home and practice away, and we'd have the usual recitals that all these kiddies have, and it was a good time.

VAN BENSCHOTEN: So you did that for six years.

KNOPOFF: I did it for six years, and then when I was fifteen-and-a-half, it was time for me to go to college. I graduated from high school, and that was the end of the music lessons.

VAN BENSCHOTEN: Now, do you feel that the music lessons had any influence at all on cultivating your scientific ability or abstract ability to think abstractly or any of those things?

KNOPOFF: I don't think so directly, but I was much involved in— But the musical experience stayed with me and became an extraordinarily important part of my life and has to this day.

VAN BENSCHOTEN: In what way?

KNOPOFF: In high school I became involved— I took a music class—we were doing harmony—in high school, and two of my acquaintances wanted to organize a trio. It would have been clarinet, cello, and piano, except they didn't have a pianist. So they asked if I wouldn't play the piano with them. And the problem with kids who take piano lessons by themselves is that they cannot keep time. So these guys, these two guys, took me under their wing and kept showing me how to keep time. They actually trained me to keep time, and it was something that stuck with me for a long time. It was something that the piano teacher didn't do. She'd say, "All right, keep time," but she didn't tell me what keeping time really was.

My father was very musical. We, all three of us, went every summer to the Hollywood Bowl. My mother was musical. Even before she married my father, she was at the Hollywood Bowl in its first year of opening when they sat on the grassy hillside. There were no benches on the hillside in 1921. Or was is it '22? I don't know the exact year.

I can remember the great conductors coming to the Hollywood Bowl. I remember getting lost as a small child in the Hollywood Bowl because I went off to bathroom or something, and then came back at the wrong level, and having my father go hunt for me during the second half of the performance.

I can remember [Otto] Klemperer conducting. He was the permanent conductor of the L.A. Philharmonic in the old Philharmonic Auditorium, and he did a Beethoven cycle, and my father took me to several of these concerts. I remember I must have been already eight or nine years old, and Klemperer was doing all the

Beethoven nine symphonies, and we went— We took the streetcar down to the Philharmonic Auditorium and tried to get tickets for the Beethoven's Ninth Symphony, and there was one seat left in the mezzanine (which is not where we usually sat, which was way up in the gallery), an expensive seat. And my father said, "We'll take the last seat in the house, and my son will sit on my lap." And they refused. They said, "Oh, no, he's too big. He needs a seat of his own." But he persuaded them, and I remember Beethoven's Ninth with Klemperer.

So in the high school years, I really began to learn music, not because of my piano-playing, but this same group of kids with whom I was playing trios and quartets were ushers down at the L.A. Philharmonic. They used high school kids as ushers, and I was very young, but I was fourteen or fifteen, but I went down. I decided, with my parents' permission, I was going to go be an usher at the L.A. Philharmonic. And you had to dress appropriately and show people to their seats, but after that was done, I heard the most wonderful music that I'd never heard before, and part of it was my father's taste in classical music, and he loved [Peter I.] Tchaikovsky and didn't appreciate [Johannes] Brahms. And then I discovered Brahms and [Robert A.] Schumann. I can remember [Sir John] Barbirolli, then the conductor of the L.A. Phil, and I can remember him conducting a Schumann symphony, and I just— I was enraptured. It was the third symphony of Schumann. I remember to this day, I was a teenager, I just discovered all this beautiful music that I'd never heard before.

In those days, we still had wind-up Victrolas and the [Enrico] Caruso records, but there was stuff out there that you didn't get because you were playing these piano exercises.

Incidentally, I had an excellent memory. I won the Bach Prize at the Conservatory every year because I could memorize more Bach than anybody else. But it was easy for me. It wasn't a matter of playing it well; it was a matter of just seeing something, and I could remember what it was. It wasn't a photographic memory, but it was very close to it. And so everybody knew that the Bach Prize was sort of— It was not something that anybody else wanted to compete for, because it was given under the wrong boundary conditions. It was given to people who could memorize more pieces, which is— [laughs]

VAN BENSCHOTEN: In what other way did that incredible memory show itself?

KNOPOFF: Oh, I could study for exams where you had to repeat information and not be very original, and that was very, very helpful. I remember at L.A. City College, where I started my college career, just staying up all night before a final exam in history and, just memorizing facts, did very well.

VAN BENSCHOTEN: So, returning then to the music and the importance of music in your life, I know later on you're going to become a research musicologist. What is the connection between— I mean, it seems obvious in one way, but I would like you to maybe elucidate it.

KNOPOFF: After I finished my graduate studies, I went off to Miami University in Oxford, Ohio, and we had a supper club of unmarried people who would go off to The

Huddle restaurant, which was the only place in town where you could— And have our dinner. Among the members of that group were a man in the Poli Sci [political science] Department, who had a marvelous baritone voice, and a member of the Music faculty, and he'd recently come to Miami from UCLA, as a matter of fact.

Since I was on the Physics faculty there, he asked me if I wouldn't give a course in the Music Department in musical acoustics, which I did. It was pretty traditional acoustics. It's not a course I would give today, but that was fifty years ago, over fifty years. And he and I formed a two-piano team, and he was a very good pianist. But professors of music are supposed to be good pianists. And I had kept up my music playing four hands while I was a student, especially as a graduate student at Caltech [California Institute of Technology]. And so we formed the two-piano team, which was a lot of fun. We played some great pieces together.

And after two years at Miami, I decided to come to UCLA, and Linc [Lincoln] Spiess, this Music faculty member, said, "Oh, you must look up Boris Kremenliev on the Music faculty when you get to UCLA," and I did. We became friends, he and his wife, Elva [Kremenliev], in my bachelor days here in Los Angeles. And Boris was interested in— He was not only a composer, but he was an ethnomusicologist, because his origin was Bulgarian, and he knew Bulgarian folk music and was much involved in that. He was instrumental in organizing an Institute of Ethnomusicology, now no longer at UCLA.

And the director of that was a man by the name of Mantle Hood, who is a brilliant ethnomusicologist, and Mantle became very— And I was already by then trying to think in terms of music as a communicable medium, as the fact that there are recognition processes that say, yes, this music is classical; this music is oriental; this music is baroque; this music is modern; this music is this composer; this music is that. There are pattern recognition procedures.

So I became very interested in music as the problems of organization of sound into language, and that even predated my association with the Institute of Ethnomusicology, although I can say that it hadn't been really formalized in my mind as a problem until I became involved with the Institute. Mantle persuaded me and several other people, Bill [William] Hutchinson and Charlie [Charles] Seeger, to form a sort of a faculty nucleus of this Institute of Ethnomusicology. His was the only formal appointment on the professorial faculty in the Institute, but he had a seminar that he carried out every week with a large number of students all interested in the music of different cultures.

I saw my position in that seminar as trying to see what were the common features, what were the ingredients that caused one culture to organize their sound in this way and another one to organize their sound in another way. I still worry about those problems. I don't understand the answers, but nevertheless, very interesting. So it was a chain of circumstances.

VAN BENSCHOTEN: Returning to your upbringing and your schooling in particular, you mentioned the importance of music and that you weren't that good, let's say, in art, or the manipulative sort of science.

KNOPOFF: Yes.

VAN BENSCHOTEN: But what subjects did you shine at?

KNOPOFF: Oh, I'd say I was best in mathematics, and I was good in language, in the social sciences, but really very, very strong in mathematics and science.

There was a point in my life where at the age of twelve, my left leg, my left knee, suddenly collapsed on me. I never knew of any injury to it, any physical injury prior to the collapse, and then the collapse became repetitive at intervals of weeks or so, and the knee became badly swollen. And from that time forward, I was in the hands of doctors who analyzed and probed and did nothing but drain the fluid from my knee and set me back on my feet.

So I didn't— I was unable to participate in athletics, and the problem wasn't resolved until nine years later. Why I raise that question— I've forgotten what direction I wanted that to take me.

VAN BENSCHOTEN: I was talking about subjects that you shined at, and you had mentioned math.

KNOPOFF: Oh, and at one point, the solution of these medical people was that they would put me in a leg cast from hip to toe and immobilize the leg, and maybe the problem would go away. So they did. They put me in plaster, and I couldn't go to school. So they sent me— I was at home and I was given a teacher who would come to my home for the half-semester, the ten weeks that I was laid up at home. But that teacher could only teach English and history, couldn't teach math or science. And I was a high school student, and I wanted— So apropos of that, to show you the insensitivity of the times, many years later I got a copy of my high school transcript, and there it shows for that half year that I had withdrawn from Roosevelt High School and I had enrolled in "Cripple" High School. [laughs]

VAN BENSCHOTEN: My god, that is a little insensitive.

KNOPOFF: Very. [mutual laughter]

And so on my return, I returned in mid-semester, and I said that I needed to take algebra and chemistry because that was necessary for me to get on my college track. That must have been eleventh grade then. And they refused to let me take chemistry because they said, "It's hard to break in. But we'll give you a tutor and let you take the algebra." Well, by the time a week or two had passed, I had done all that, the first ten weeks of algebra, and I was well on to the— I was a regular student and finished up with very good grades. I took all the math. I doubled up on chemistry and physics in the twelfth grade.

But they were— The subjects were algebra— High school mathematics was exciting for me because it was a challenge. I don't think I ever had a deep motivation to do something in the high school years and even into college years because of a tremendous love for the subject. I enjoyed the challenge of solving puzzles. That was much more interesting than trying to find out why something worked. If somebody said, "Here's a problem. Can you solve it?" I could do that. So I really enjoyed mathematics for that reason because there was always a set of problems at the end of the chapter, and you could whiz your way through them.

And if there was a math competition, I could do very well at it. I found I enjoyed the competition. I didn't see mathematics as a— I didn't want— I never liked abstract mathematics, which I started to encounter in graduate school. It wasn't until I got into graduate school that I really began to appreciate the depth of science. But I've always been interested in competition. I don't know if that's a— I hope that doesn't come through as a negative statement. But I have liked the idea of solving puzzles, and doing it before somebody else solves the puzzle.

Attached to that is a certain snob appeal, snob influence, of the difficulty of the puzzle, and so when I— I must say that I didn't have an awful lot of advice in the public school system about career directions. Nobody in the community knew anything about—had ever heard of physics, and engineering, even, was—it had been mentioned to me as a career direction. Engineering had been mentioned as a career direction in high school. So when I went home and discussed this with my parents, — "Oh, yes, there's someone over there, and we know someone who— A very distant acquaintance whose son is a civil engineer."

So off I went to L.A. City College and enrolled in civil engineering, the only thing I'd ever heard of as an engineering career. I did extraordinarily well at L.A. City College, took all the engineering prerequisites. Engineering was not then offered as a bachelor's degree at UCLA. You could take two years at UCLA, and you had to finish your last two years at [University of California,] Berkeley. This was in 1941, '42.

I couldn't afford to go to UCLA, and besides, there was no transportation out this far from the Eastside of L.A., but I could take the streetcar to L.A. City College, which was a very good school as academic preparation. The plan was, before the war broke out, that I would spend two years at L.A. City College, and maybe we could find the means to go to Berkeley for the last two years.

I did extraordinarily well in physics. I got all As in all the subjects that I took at L.A. City College. I suddenly saw the challenge, and that was a lot of fun, so I got good grades, while I had fair grades in high school, but sensational grades in City College. And my physics professor that taught me first-year physics had been a Caltech Ph.D., and he said, “Why don’t you take the entrance exam for Caltech.”

I figured, “Well, entrance exam.”

VAN BENSCHOTEN: Another challenge. [laughs]

KNOPOFF: Another challenge. So I took the entrance exam, and I went off to Caltech, got the interview, and the interviewer said, “Well, are you interested in coming to Caltech?”

And I said, “Well, I can’t afford it.” Tuition at that time was three hundred dollars a year, a lot of money.

So I went and I actually spent a year and a half at L.A. City College, and I took the entrance exam the second year for entrance as a sophomore to Caltech, having worked some in the summer and got some money together. I again took the entrance exam and I got admitted. This time I could pay my way for one year.

Then I discovered that among the engineering students there was a pecking order, and the electrical engineers were at the top and the civil engineers were at the bottom, and that was not acceptable to me. So I quickly changed majors to electrical engineering, and I completed my bachelor’s degree.

But I had a very close friend, Leon Shenfil, who came with me at the same time from L.A. City College, the only other one, and he was a physics major, and he kept telling me that physics was much harder than electrical engineering. So that was clearly a challenge that I couldn't withstand. So by my senior year at Caltech, I was taking the physics courses as well, and got my degree in engineering and changed to physics as a graduate student.

I think the graduate program was the first time I really came face to face with trying to understand why things behave the way they do, rather than just as symbolic or mathematical descriptions of what things are. And a lot of physics, unfortunately, these days is taught as a series of mathematical expositions of the physics, but the physics ought to be described in language, not in mathematics. Mathematics is a device for expressing the predictive elements of physics, to predict quantity, to predict what's going to happen in a certain hypothetical or real experiment, but nevertheless, the understanding is in terms of language, and the language doesn't necessarily imply spoken language. It implies a language that we— When you think, you think in the English language. It doesn't have to be expressed.

VAN BENSCHOTEN: Well, let's talk about that a little bit more when we get— I have some questions about some earlier things, but we'll get back to that because it sounds very interesting.

KNOPOFF: Okay.

TAPE NUMBER: II, SIDE ONE

August 20, 2003

VAN BENSCHOTEN: This is tape two, side A.

We were talking— We had ended on our first tape with you being at Caltech, and what I'd like to do is back up just a little bit with a few questions I had. One of them was on parental expectations. You mentioned that you were going to go to college. Was it always that way, you knew you were going to go to college, or was it something else?

KNOPOFF: Yes, yes. I was always the smart kid on the block. The other parents, I remember— I'd remarked earlier about how I'd been advanced from kindergarten into the second half of the second grade. When other parents heard that, they thought that their children should have that privilege as well. They didn't get it. But I was never— I don't think I was ever smart-alecky. If I ever was, I was told not to repeat that, gently, but nevertheless. But I don't think I— I can't remember that that ever happened.

I can remember wonderful times at the beach, at Santa Monica and in the summers, playing in the sand and learning how to swim. That was the place where I learned how to swim. I was recognized as being a smart kid by all standards, either formal or informal, and my parents realized that this was something that they had to make sacrifices for, and I'm convinced that they did.

So it was a given that no matter what the economic status of the family might be, I was going to go ahead to college. I think when I was still in elementary school, I was already asking, "What comes after college?" I was told, "The university." I didn't realize that they may have been the same.

I never had an attitude about it that I was going to be some sort of toy that was going to be groomed for this. I always wanted to be a participant in helping out with the family, to whatever level of skill I had. Summers, I tried to work on my father's construction projects. I mentioned that— Well, to return, by the mid-thirties—it must have been '35 or '36—my father declared that the Depression— We were coming out of the Depression. He could foresee that.

VAN BENSCHOTEN: So did FDR.

KNOPOFF: Well, FDR told us, told everybody, that he was going to accomplish that. I think that times were hard until the Second War ended, and then the war had obliterated the other hard times.

So my father decided to leave the milk business. They had formed their own company. After Borden's went defunct, the men, who were largely in the building trades themselves, decided to organize their own milk delivery company, and in the spirit of the labor movement they called themselves the United Independent Dairies Cooperative. United and Independent and Cooperative was all in the title of the— But they did not own dairy cows. That was an experiment that my father tried also in the late twenties, but not successfully, by trying to have a dairy farm with a few cows when we lived for one year in Arcadia.

But the men began delivery. They would buy the milk from producers, and in the earliest years, the producers would even bottle it for them. And the only identifying symbol was the bottle cap, the little paper cap you put in the neck of the glass bottle.

When we lived on Eastern Avenue, one of our many houses, it must have been when I was three years old or four years old, the gathering place for these milkmen—there were only a half a dozen of them or so—was our family living room, which was where the crates of the milk bottles, filled milk bottles, were brought by the big truck from the dairy farm. And then the men came at two and three in the morning to pick up the crates of milk and put them on the trucks, pickup trucks, and my father among them would go and deliver the milk to the front doorsteps of the houses, or back doorsteps, and then the excess milk that had been undelivered would be returned to our living room. My mother made cheese in the backyard, butter. We had all the extra milk that was unreturnable.

I couldn't participate in that, but when my father started— Later the group actually bought their own, had their own bottling plant, and they still brought the milk in, but they had their own bottling plant, no longer in our living room. We'd long since moved out. And then my father decided to— He was the leader of this cooperative group, and he decided to give it up and went into— Went back not to carpentry working for somebody else, but to start building houses.

And my mother was the interior designer, because she always claimed that my father didn't have very well-developed artistic talents. So she chose the color schemes and the choice of the plumbing fixtures or whatever.

VAN BENSCHOTEN: He became a homebuilder. This was the latter part of the Depression, then?

KNOPOFF: The later part of the Depression. My mother just told him, "Have your painter paint this thing this color," or that color, and that was all right. It was a small part of the whole operation.

So, summers I was then in, what, young teens, in high school. I would work for my father hauling lumber from the curb where the lumber truck had dropped the stacks of two-by-fours over to the building site, which is only a few feet, you know, in these houses that were single-family dwellings, not very large constructions. It was tiring work, but I appreciated the fact that he was doing a lot of manual labor associated with the carpentry skills, which were very well developed. He was a master carpenter.

I remember later when I was in high school, I was taking trigonometry in my senior year in high school, and my father asked me, "What's this trigonometry?"

So I said, "Well, it's concerned with angles."

So he said, "Ah. Here is a pitched roof," a roof inclined at an angle. "How would you lay out this roof?"

And so the pitch was four in twelve or something like that, so I did the calculations, sines and cosines of angles and that sort of thing, and he said, "This is the way I did it, I would do it," and he took his carpenter's square and drew a line like this, and drew a line like that, that's where you— [laughs] So he didn't know trigonometry, but he was better at it than I was in a practical situation.

So I tried to help when I could, but, basically, well, I was very conscious of the family situation. I had finished a half year in spring semester at Los Angeles City College, and I got a job. I had a course in surveying. I can't remember if it's— No, it must have been at the end of my first full year. It must have been in the summer of '42. I'd already been— I had taken a course in surveying in preparation for my civil engineering career.

And there was an ad that somebody wanted a surveying assistant down in San Diego, so off I went to San Diego and got a summer job on a surveying crew. They were putting in housing for military families. This was early in the War; big housing development. And I worked the entire summer. That's where I got the money for Caltech. It was a way to pass the summer, but I got some money for it. It was already the beginning of the wartime, and they needed people. They would even take sixteen-year-olds to work on summer projects.

VAN BENSCHOTEN: Okay, good. You anticipated another question, which was about jobs. I was wondering, too, what about influential teachers? Did you have people who were helping you or shaping you or pointing you in certain directions?

KNOPOFF: In the public school system, I don't think I really did. I think the teachers enjoyed my presence. I don't have any— With one exception, I don't think I have any strong association of having anyone take a personal interest in me or try to move me in any particular direction.

The only case— The only exception to that, which was not strong, was the fact that I had had to delay a— Well, I was interested in constructing a device in my high school physics class that would move a mirror in response to soundwaves. And I couldn't figure out how to silver a mirror, silver a piece of glass that I was going to suspend with a thread. So I went to my chemistry teacher, and she did that, showed me how to do that. And that's about the only instance I can remember of having some extracurricular contact, and I enjoyed that, but it was very brief.

At City College, Dr. Ralph Winger, who was the physics teacher, took a very, very— He recognized my talent in physics and strongly encouraged me to go to Caltech, as I've remarked.

I worked while I was at L.A. City College, and I felt the financial need, not because the money wasn't there in the family. The family always— My parents would always have given me the money, but I felt a personal need to contribute. So I went to work for the NYA, National Youth Authority, which was a program—I suppose one of the Depression-era programs of the Roosevelt administration—to stimulate and provide jobs. And I went to work for thirty-five cents an hour reading calculus papers. Having already taken the first course in calculus, I could then be enrolled in the second course and grade papers for the first course.

I fell under the spell of a man by the name of Alexander Hood, who was very devastated when the battleship *Hood* was sunk by the Japanese. I don't think he was a particularly great teacher, but he was full of anecdotes, and I enjoyed sitting and hearing him speak, and he was my first encounter with somebody with a British accent, so it was very interesting to hear.

I don't think that I— During the War, which had already begun after I'd spent a year at L.A. City College, I didn't have any strong interaction with teachers at Caltech. Caltech was mainly a V-12 school, mainly full of Navy students studying the regular Caltech subjects, but in Navy uniform, in preparation for going into naval service after their graduation. And I was a civilian. So the Navy students occupied all the dorms. I couldn't afford to live in the dorms, anyhow. I traveled by [Pacific Electric] Red Car from my home to Caltech daily. But I was a civilian, and underage in the early years. In the later years of my undergraduate career, I was put into 4F because of my knee. My knee was— I couldn't use it in any kind of demanding physical circumstance.

But it was a military environment. The military recruits, the naval students, were— We were egalitarian in the classroom, and they were very, very good students, but they also had to do their marching at times when I didn't have to march. And they did their PE, and I tried to do as much of it as I could.

VAN BENSCHOTEN: Now, the nonmilitary students there, what percentage was that?

KNOPOFF: There was, oh, a handful of them. I couldn't tell you how many there were, but there were some.

VAN BENSCHOTEN: You were in the minority, at least, yes.

KNOPOFF: Definitely in the minority.

And there was an accelerated program, so we didn't have summer vacations at that time, because they ran a continuous program. So as soon as we had a summer term, a summer quarter, that was part of the regular school year. So we had an accelerated program, and I graduated in October of '44.

VAN BENSCHOTEN: All of nineteen, I think, right?

KNOPOFF: All of nineteen, nineteen, a little— A few months after my nineteenth birthday, but that was because I had already been pushed ahead in the elementary school, and then we had an accelerated program without summer breaks in the undergraduate program for the bachelor's. I don't think I had any strong influences there among the teachers. I even did some work part-time on a little project of one of the faculty members, but he wasn't a strong influence on me.

And then I went off to graduate school, and the War wasn't over for another year, and then people started coming back, and it started becoming a different organization, a different environment. I'd say of the faculty at that time, my thesis advisor, Bill [William H.] Pickering, I never got close to him, really.

The one faculty member that I had an enjoyable rapport with was a man whose name was Smythe, William R. Smythe, and he was well known for having put together a course in electromagnetic theory that was very difficult. He was putting together an expansion of his book, and the tasks set by the prof, the homework problems at the ends of each chapter in Smythe's course were legendarily difficult. He had had the last three chapters of this second edition that he had in unpublished form, and the problems there were very difficult, and I tackled the second part of his course, which was very difficult, and I encountered one problem, and I solved the problem, and where he had had an answer that was extraordinarily long, and it was a series solution, I summed the series and got the answer in a compact form. He was extraordinarily impressed.

From then on, he and I had a very fine— I was no longer one of the troops. He had a very stern face, but he always had a smile in his eye for me, and I enjoyed it. He allowed me to relax, and we'll get to that a little bit later on, in one very tense environment, event.

VAN BENSCHOTEN: You said that, and I just want to follow up on this before I forget, is that you mentioned that there was sort of two different schools. There was a school, if I understood correctly, when the War was still on and it was mostly military, people being trained for the military for work there, and then afterward when the War was over. How was the mood different then when the War was over? How was Caltech?

KNOPOFF: Oh, it was getting back to normal. The faculty was no longer, themselves, working on war projects. The faculty was heavily involved in military projects in the area, and they were not very accessible then. The courses went on, but it wasn't an environment where the faculty saw the education at the institution and the peer research that would normally go on there as the principal focus.

There was no doubt that there was— I don't want to tear down the quality. The education was excellent, it was wonderful, but I'm not sure that there was a lot of— How shall I put it? What I want to say is that we faculty at UCLA live a life of teaching in the classroom and a life of research in the laboratory, and the life of research in the laboratory spills over into the classroom because the students can hear what it is that you're doing once in a while and the faculty have time to spend. Hopefully they will spend time with students, both undergraduates and especially with graduate students.

And that wasn't being done. They were teaching, but then they had other— The faculty had other obligations, and the students had— And there was the ever-presence of the War. One was concerned about what was going on outside Pasadena.

VAN BENSCHOTEN: Right, and there was a big war.

KNOPOFF: It was a big war. There were things that we traveled to, three or four of us. One of us had a car. I didn't have a car, but one of the civilians had a car, and we pooled together our A stamps and we had enough gasoline that we could travel back and forth, if I wasn't traveling by Red Car. I would hitchhike sometimes. Hitchhiking was good. People were very kind, and it was different times than today.

VAN BENSCHOTEN: Well, I wanted to ask, too, I mean, you talked about some of the classes you took, you talked about the mood of Caltech. What about your social life and about friends and to what degree did you have a social life?

KNOPOFF: I don't think I had much of a social life at all when I was an undergraduate. I was really very focused on my studies. I don't think— I can remember going to movies or something like that, with my parents mainly. But my compatriots were— People my own age were off at war and—

VAN BENSCHOTEN: You were absorbed in your studies.

KNOPOFF: I was absorbed in my studies, yes.

VAN BENSCHOTEN: Well, you mentioned films, and this is a question I should have asked earlier, but I'll ask it now. During the Depression, I know, just from having read several books, the radio was very important for many people and films were very important. It was sort of the golden era of film. How often did you see films or how important was radio in your life, in your family's life?

KNOPOFF: I think I can remember the first radio we got, which was in the early thirties, I think. I can remember it as a tabletop radio with a cloth cover to the loudspeaker and a wooden grill across the cloth. I think it was an Atwater Kent.

We used to hear the radio. My father couldn't abide the childish serials that went on at just about or just before dinnertime, and so we didn't have the radio on during dinnertime. But before, I heard some of the serials, but they didn't play much of a role. But what did play a role, and which was very important to me, was that, on Sundays, especially, there would be good music on the radio, and my family would tune to the radio, and we took every chance we could to hear the New York Philharmonic—I think it was the New York Philharmonic—or the NBC Symphony.

And then on some weekday evenings there was the Standard Symphony Hour sponsored by the Standard Oil Company, and they would have a rotating West Coast symphony. One week it would be the San Francisco, and one week it would be the L.A., and one week it might be, if they had one, in Portland or Seattle or something like this. I've forgotten now. And the music that you could get on the radio, especially on Sundays, and we would have Sunday dinner and turn on the radio, and this was a very important time. But the music was very important.

The movies. We went to the movies a lot, I'd say. We went frequently, like once a week, and typically Saturdays, Saturday evenings. And you could go to the Olympic Theater downtown and see a double feature for fifteen cents.

VAN BENSCHOTEN: Those were the days.

KNOPOFF: Those were the days. And we only went to good movies. We never went to junk. I never went with the kids on the Saturday afternoon matinees. We always went after the matinees as a family event. The floor would be littered with the popcorn after the kids had gone through and sat through the serials before the main features.

I was very timid as a child, and I couldn't stand shooting, and if there was shooting, I would slide off my seat and put my head down below the chair in front of me, and I would ask— Or if there was violence, I would ask if it was finished before I would emerge. By the same token, I, like most children, I thought that the love scenes were pretty mushy, and I wanted to get on with the real story. But we went on— We went to what would pass for art films these days, but that was— Hollywood was putting out good stuff.

One film that sticks in my mind, one film evening sticks in my mind, was my father and I went to see New Year's Eve downtown on Broadway in downtown L.A., and we left my mother home. I don't know why. She didn't feel like going or something. And there was a double feature playing, and it was *Ramona*, and I think it had Loretta Young and Tyrone Power, but I can't be sure, and the other feature was *Midsummer Night's Dream*, and it had— I remember Mickey Rooney was Puck, but I can't remember who some of the others were in it.

And we saw the double feature, and it was, I think, the first time I ever saw color, a color movie. And then we emerged, and there was this mob scene, blowing blowers on Broadway, like you see in Times Square these days. But I remember seeing *Ramona* and *Midsummer Night's Dream* that evening.

But we saw other shows. The Olympic Theater used to love to play English movies that featured Jessie Matthews, who was sort of a Jeanette MacDonald of England. Neither of them had a particularly good voice, but—

VAN BENSCHOTEN: The story was good.

KNOPOFF: Well, it was operetta, set to the movies, Nelson Eddy and Jeanette MacDonald and that sort of thing.

VAN BENSCHOTEN: Now, were all these at the Olympic Theater, or did you go to the other theaters?

KNOPOFF: Oh, no, no, no, we went to the local theaters, too. We went to the Meralta, which was close by. We went to the Brooklyn Theater, which was up on Brooklyn Avenue. It was a few miles away from where we lived. My father knew the manager, a man by the name of Jack Berman. I don't know why I remember the name.

And in the Depression, they used to have keno, which is the same as bingo, I guess. Or no, I guess it was— I don't know what the difference was. And you signed up for it, and you threw your card in a bin, and then on keno night, which was usually a night that the theater would be normally not very full, they would have a drawing or something like this and give some money away.

We never went on a Tuesday night or whenever it was, but Jack Berman insisted that my father fill out a card. So the whole family filled out cards, put it in the hopper, and as luck would happen, my name was drawn on one of these Tuesday nights, and the following day the whole school knew it, the elementary school knew about it. "How come you were not there? You would have had" some large sum. I don't know what it was.

And I said— It never meant anything to me, because it was a gesture for Mr. Berman, and it was never a loss to me because it was never a— It was a zero option of my ever winning.

VAN BENSCHOTEN: It had no possibility of it.

KNOPOFF: You know, that's life.

VAN BENSCHOTEN: Okay, so radio and film. Another question, too, before we move on to Caltech, about your sort of family's nomadic life. What effect do you think that nomadism had upon you as a boy and then later on as an adult, if any?

KNOPOFF: I think it increased the focus on the family and decreased the kinds of interactions children might have with their neighbors, because you never— I was not in a place long enough to establish strong connections. I had acquaintances, they were not zero, but it was somewhat of a small journey to get together with them, and so it was not as though they lived in the same block. So I don't think I had a lot of close— I didn't have close friends, because of that nomadism.

I can also remark that when I went to Hollenbeck Junior High [School], it was a walk of several miles across city streets, and we did that as a matter of course. Now kids don't seem to— They think it's an imposition if they're asked to walk several miles. Well, I don't know. And when we moved from the Boyle Heights area back into City Terrace, I would then take the streetcar, the Yellow Car, first to Hollenbeck and then to Roosevelt, including the "Dinky" car. There was a half-sized streetcar that they ran on a feeder line from the end of the B-car line, the one I went on, Brooklyn Avenue, and we would take that. The end of the line was just where we lived, in City Terrace. "Dinky" cars, or whatever they called it, the "Dinky." It had some other name, official name, but I don't remember.

VAN BENSCHOTEN: Let's see. I had another question about competition, because you had mentioned how important competition was, problem-solving, and how this was tied up with competition as well. Were there notable competitions that— I know you mentioned the musical one. But were there other competitions, academic or otherwise, that you participated in?

KNOPOFF: I only participated in one competition in high school. The American Chemical Society ran a chemistry contest of some sort, and the high schools that had chemistry programs participated in it. Actually, they all did. Roosevelt High School put together a chem team, and I got on it because I was out of phase. I had missed the first half-year of chemistry, and I was in a catch-up mode, and so when they put the team together, they put the team together mainly of people who were a half-year behind me. But I was in that class in chemistry.

There were five of us who went up to do it, and we did well. I think we were the best public school team, but I wasn't the best on our team. I was, I think, third or something like that. There wasn't anything special, I mean. But as you say, competition, I loved examinations, and I liked examinations in the classroom. They were good, fun challenges.

VAN BENSCHOTEN: Okay. I'm near the end of the tape here.

KNOPOFF: Okay.

[End of August 20, 2003, interview]

TAPE NUMBER: III, SIDE ONE

August 22, 2003

VAN BENSCHOTEN: Today is August 22nd, 2003. This is tape three, side A. I'm with Leon Knopoff.

I had a few follow-up questions. First of all, is there anything that you would like to add to the record in thinking about it over the last two days, I guess, any other anecdotes or stories or anything?

KNOPOFF: No, I think that I'd like to reinforce the impression that I tried to give two days ago, and that is that I see my childhood as a very happy one. I lived in a very protective environment, but one in which I was not pampered. We never went without. There was always food on the table. We had a car. My father always made sure that we were provided for. We were not rich, but it was a very happy time for me, and the stresses that my parents might have felt were never, never communicated to me.

VAN BENSCHOTEN: At one point in the last tape, and I should have maybe prepared you for this a little bit better, but you mentioned that there was an anecdote about the Yiddish school that you attended, that you wanted to talk about, and I'm wondering, do you remember that at all?

KNOPOFF: I had one very emotional experience. I'd mentioned the cousin that my father had been invited to come and visit and stay with in Los Angeles. I mean, my father moved from the East to Los Angeles to be with this first cousin, Isaac Knopoff.

And I think you've seen the photographs, the wedding photographs of my parents, and the principal guests at the wedding were Isaac and Regina Knopoff. They had two children, and their oldest boy, by the name of Usher, U-s-h-e-r, was attending the same Yiddish school that I did, but he was a class behind me.

So one day their class had finished, and I came into my after-school class in Yiddish, and partway through the lesson, through the hour's activities, the class became very— The students became rather tense and turbulent. There were mutterings flying around the students that a student had been— That an accident happened a couple of blocks down the street in a shopping area just beyond where the school was. As I recall it, the class became pretty much uncontrollable, and it was decided to dismiss the class because they had heard.

And so I walked. I had a walk of about two blocks to the shopping center, corner of Wabash and Evergreen, and then I had another three blocks after crossing the street to get home. Isaac and Regina lived around the corner from us, and when I arrived at the movie theater, which was a couple of blocks away from the Yiddish school, there I saw the body of a small boy who had been laid in the lobby, just outside the locked doors of the movie theater because it was the afternoon, and I recognized from a distance that it was my cousin Usher. And people were milling around.

He had been struck by a car. He had been in the crosswalk and a hit-and-run driver had hit him at very high speed. The newspaper said he'd been thrown an incredibly high distance in the air. And there were adults milling around, and they kept saying, "Who is he? Does anybody know who he is?"

And on the periphery of this crowd, I said, "Well, his mother lives at such-and-such an address," and I hung around, again on the periphery of the crowd.

And some time later, about fifteen, twenty minutes later, I saw Regina pushing the baby carriage, and she was running down the street, screaming at the top of her voice. [Cries] I never told anyone who it was who had identified the body, but I'll never forget. It wasn't so much my dead cousin, but the voice of his mother shrieking was— I was six or seven, I don't know. It stayed with me for my whole life.

VAN BENSCHOTEN: All right. I had a—

KNOPOFF: And because of that, they had another child who would not have been born, I guess, had that not have happened.

VAN BENSCHOTEN: Okay. I had a few questions about Caltech that were follow-up, but they segue quite nicely into the picking up of our chronology for this session, I think.

You had mentioned that under the influence of a friend at Caltech that you changed the focus of your study from electrical engineering to physics, and I guess I wanted to know what was your rationale for doing that? I remember you had mentioned partly that it was you were moving away from descriptions of the physical world.

KNOPOFF: No, that came later. That came later. I really thought that electrical engineering was not challenging. I could do anything that was— Any problem that was set before me in electrical engineering was easy to do, and nothing was challenging. And this friend of mine, Leon Shenfil, with whom I'd come from L.A. City College, we'd transferred to Caltech in our second year at Caltech at the same time, he told me in many discussions that physics was much more challenging.

So I thought, gee, that's where I should be going. It wasn't the fact that I was more motivated to find out, to get deeper insights into the way the universe worked; it was just that these problems, these challenges that were put up before the students by the—thrown up by the—teachers were puzzles to be overcome, and these were much more difficult puzzles than the ones that I would get in electrical engineering.

So by my senior year, I realized that I wanted to become a physics student. I was too far along to complete the degree requirements for physics, because they had to take languages, whereas engineering students didn't have to take languages and that sort of thing. Well, actually, I did— I took extra mathematics, I took extra physics courses, and got a degree in electrical engineering. And immediately upon receiving the bachelor's, I changed my major in graduate school to physics.

VAN BENSCHOTEN: You had also mentioned Smythe, William R. Smythe.

KNOPOFF: Yes.

VAN BENSCHOTEN: And you said he had allowed you to relax in a very tense environment.

KNOPOFF: The Ph.D. requirements at the time at Caltech involved taking a certain number of core graduate courses which were very difficult, I found, therefore very interesting, and after a significant amount of that kind of formal preparation, one took a Ph.D. oral exam. It's not done these days at UCLA, but at that time it was essentially an oral review of all of fundamental physics. It was not a review of your research program; it was intended to be the gateway to the research program once you had passed this screening event, and it was a frightening episode for all the students. They prepared extraordinarily intensely.

The committee consisted of about seven senior faculty, and the exam lasted, as I recall, three hours, and we covered quantum mechanics and thermodynamics, classical electricity and magnetism, a number of subjects. And one of the members— I had no say in who the members would be. They were selected by— I've forgotten the process now, but anyhow. And one of the members of the committee was Professor [Paul S.] Epstein, Paul Epstein, who taught an incredibly beautifully organized series of courses that I just adored, because they were beautifully organized. He was a very formal man. He'd been trained in Germany.

I raise this because the— But all the students wrote down their experiences from the exam in a book, which was handed from student to student in anticipation, and it was well known that if you had Epstein on your committee, one of the first questions he would ask is, what is the second law of thermodynamics? And he had written a book.

VAN BENSCHOTEN: Which is on your shelf.

KNOPOFF: Which is on my shelf right now, on thermodynamics. And the statement was that—in the "bone" book, in the book that was handed from student to student—was that you must give Epstein a statement of the second law of thermodynamics, because he's certain to ask it of you. You must give it exactly as worded in his book, without changing a word.

And Epstein, I think, was the second on my series of questioners, and, of course, his first question was, what is the second law of thermodynamics? And I, being an independent spirit, decided to give my own version of it, which I still think is correct. And after I had made my statement, he said, in his German accent, "No." That was it, and I stuttered a bit and I tried to rephrase, and he said, "No."

And we went on from there, and we did a little bit more in thermodynamics, and we switched to quantum mechanics, which I did extremely well. I think he was asking quantum mechanics as well.

And then it came Smythe's turn, and Smythe asked me a really rather elementary question about the direction of the magnetic field due to a current in a conductor—circuit geometry. And I gave absolutely the wrong answer. I was so rattled at that point. And Smythe laughed and smiled and he says, "Relax. Think about it for a second." And so I thought, and from then on, the exam went well.

Then the committee retired for its deliberations, and the committee emerged and the conclusion was that I had passed everything except thermodynamics, which was a blow. I had to go through another examination, but this time it would only have to be with Professor Epstein in his own private office at a time of my own choosing.

I was in a funk. I was really thrown by this, and for weeks, a few months thereafter, if I saw Epstein coming down the sidewalk, or down one side of the central garden of Caltech, I would quickly cross over to the other side. I just couldn't bear to face the man. I had been mortified. It was just terrible.

But there was nothing to do for it. I had to face up to my fate, and so one day I made the appointment with Epstein, and I went in. And the first question he asked is, what is the second law of thermodynamics? And I repeated it like a parrot, exactly as it was stated in his book, and we went on from there. And after ten or fifteen minutes or so of questioning about thermodynamics, he grunted and said, "You have obviously learned some thermodynamics in the time since we last met." I passed. [mutual laughter]

VAN BENSCHOTEN: Wow, that's an interesting story.

KNOPOFF: It was a terrible time of my life. [laughs] It was my obstinacy and his obstinacy. It had to be done precisely in his way, and I was determined to do it, in my sense of independence, in my own way.

VAN BENSCHOTEN: Now, do you feel after all these years that your first definition was the better definition?

KNOPOFF: No, no, no.

VAN BENSCHOTEN: It was just different.

KNOPOFF: No, no. Epstein was an authority, and I was still wet behind the ears in physics. But he wasn't willing to compromise and perhaps lead me into a better, more rigorous, more precise statement of the second law from the position that I had taken. That was not his style. He was a very formal individual, and it came through in those beautiful lectures that I mentioned. They were beautifully organized. Epstein was a personality who could never tolerate questions from the students from the floor. They threw him off the track that he had planned while preparing for the day's lecture. So the students knew this, and he gave these perfectly organized presentations. They were just marvelous, and they were logical. They were great.

VAN BENSCHOTEN: Let's pick up the chronology, just back up a little bit.

KNOPOFF: Okay.

VAN BENSCHOTEN: I want to talk a little bit about the transition from an undergrad at Caltech to a graduate student. Was it a given that you would attend Caltech then for graduate school?

KNOPOFF: Yes. I didn't attend Caltech as an undergraduate because of the prestige of Caltech. I attended it because it was financially convenient for me to attend Caltech. It had never occurred to me in my high school years that I would go there, mainly because I knew very little about it. Berkeley was the more visible and prominent engineering school.

But when the opportunity came to go to Caltech and I learned quite a bit about it from Dr. Winger, I resolved to go there, and it was clearly to my financial advantage to go there, because basically all I had to pay— I could live at home, I could take the Red Car or hitchhike, and all I had to do was pay the tuition. So the same was true for graduate school, and graduate school even more because I graduated with my bachelor's in October '44, and that was the depth of the War. We were deeply into the War, and the idea of travel and living out of town was not an option.

The graduate classes were not offered as frequently as one would want, so the early year of the program was— It was a scramble to try and find courses because the faculty was off doing war work. But everyone looked with optimism after August ’45, after V-J [Victory over Japan] Day and the beginning of a new school year, that it was a year that we’d have a full series of offerings, and it was good. It was good.

And I experimented. I tried my hand at some— I heard that mathematics was really challenging, so I tried my hand at some mathematics and found for the first time that it was too abstract for me, so I decided that wasn’t a direction I wanted to go to, advanced mathematics.

So I stuck with physics, passed my oral exams, started my research under the direction of Dr. [William H.] Pickering. It was an experimental thesis. FM, frequency modulated radio, had just come out in ’45. It was still an experimental thing. Up to that time, we’d only had AM, amplitude modulation, and FM was short wavelengths and it was only line-of-sight from the transmitter to the receiver. FM transmission into the canyons of Los Angeles was very difficult, and it was plagued by all sorts of interference effects. The signal would be very dependent on where you were in the canyon.

So with the aid of one radio station, at that time it was KHJ-FM, with a transmitter on Mount Lee, just next to Griffith Park, they were induced to at a certain time of day sending out a test signal that I would specify. They wouldn’t be broadcasting twenty-four hours a day as stations do today, but at noon they would send out a test signal. And I had a van and an antenna and a receiver, and I would record these radio signals in canyons of the Santa Monica Mountains and study their interference effects. I wrote a dissertation on the interference of frequency modulated waves, and I must say it was a terrible thesis.

VAN BENSCHOTEN: Why?

KNOPOFF: I think I was too immature, really. Didn't know how to write. I didn't know how to organize the material well. I had material. It was just not presented well.

I got my degree in '49. By '48, my father had died. My father died on December 31st, 1946. That winter, that Christmas vacation, I had decided to have surgery on my knee. I had had this very unreliable knee, very painful at times, for nine years, since I was twelve years old, and nothing ever showed up on the x-rays. And bed rest, we've talked about that. Aspiration to take out the fluid from the knee took place many times. The doctors wanted to do exploratory surgery, and my mother said, "No. No one's going to do exploratory surgery" on her child.

By '46, I had my majority, I was twenty-one years old, and I decided that I was going to have the surgery done. And so over Christmas I had it done, and I was on crutches and I came home from the hospital. My father didn't pick me up, and I wondered why. Someone picked me up at the hospital and took me home, and my father was in bed and he had been diagnosed with pleurisy. That must have been—And he was in bed, and we chatted. He had some sort of pneumonia or cold or something and pleural infection.

And on New Year's Eve, his heart— He had a heart attack and died instantly. It was within seconds. We had the funeral in the first week of the new year, and I went back to school. My mother at that point said that she was going back to work, and there was no dissuading her from that.

But by the end of '48, I was getting very worried that I was putting a great financial burden on her, and I decided to leave Caltech with thesis unfinished. I had all the material, and I looked around for a job, and I heard of a teaching job at Miami University in Ohio. In fact, I heard of it late in the year. It turned out that they had hired somebody, and that somebody had decided not to show up for the new school year in September of '48 and to go off into industry somewhere else. I don't know where that was.

They passed word of mouth, and I heard about it and I applied. I left Caltech, and among all my bundles of clothing and so on, I had the materials for my dissertation. So here I was about to undertake a new job, heavy teaching load, and write a dissertation all at the same time. We'll talk about the experience in Ohio.

But while in Ohio that first year, I did write the dissertation, but the pressure of holding a full-time job was a major stress. I don't think I knew how to write, and I had difficulty organizing the material, but I wrote something and I submitted it and it was accepted.

A number of years later, a very fine and respected colleague of mine, who was on the Caltech faculty, whom I love enormously, went into the archives of the Caltech

Physics Department and looked at my thesis, and he said, "Did you write something that bad?" [laughs]

And I said, "Yes, I did." I don't have a copy of the thesis. I've refused to look at it ever since. I don't even know what's there. I've tried to put it out of my mind.

VAN BENSCHOTEN: Now, your advisor was Pickering. How did you choose Pickering, or did Pickering choose you, and then how did you come by your project?

KNOPOFF: I liked Pickering. He was a young man in his thirties. He was enthusiastic. I felt that I could generate some sort of a personal relationship. In '46, when I was starting my studies, looking around for a thesis problem, '47, I was twenty-two years old and I was still immature. I didn't have much experience in the outside world, and I looked for somebody young, I think, principally.

Pickering had been a student of [Robert A.] Millikan's and his own work had been on Millikan's research in cosmic rays. After Millikan got the Nobel Prize for measuring the charge of the electron, he turned to cosmic rays, and Millikan had the wrong idea, as it turned out, that the cosmic rays coming from the sun were electrons. It turned out that they were protons, hydrogen nuclei. And he mounted several expeditions to measure the charges of the cosmic rays, measured from electroscopes sent up in balloons to high elevation. You couldn't do experiments the way you can these days.

Pickering was one of these people, and he was also in electrical engineering. But he also, it turned out— And he had this interest in this question of interference of frequency modulated waves, and he suggested the problem to me. We did reasonably well, except at that point Pickering was becoming involved with the Jet Propulsion Laboratory, which was operated by Caltech for the problems of space exploration, and more and more Pickering became involved in those activities. He ultimately became the director of Jet Propulsion Laboratory, a position he held with great distinction for many, many, many years. And basically he was not around much in later years of my dissertation. It was hard to get in touch with him. So we didn't really have a particularly strong and intimate relationship. I was pretty much on my own, and I think it showed.

VAN BENSCHOTEN: So you were on your own. Were there other people in the Pickering lab or other people on campus whom you were working with?

KNOPOFF: No, no. Really, my contacts were my fellow physics graduate students, and they were off doing other things, mainly in modern physics, and I was still in this nineteenth-century electromagnetic theory universe. Nineteenth-century in the sense that classical physics is not the twentieth century.

But it was an interesting problem, and it really— I think I really went there because of the exposure I'd had to Smythe's course in electromagnetic theory. And I think it may have been Smythe who suggested I go over and speak with Pickering, but I'm not sure about that. I don't remember that. But I really liked the beauty of the organization of and the logic and the symmetry of electromagnetic theory, and this was really an opportunity to continue an exploration in that direction, but it was not twentieth-century. It would not have been an advance in twentieth-century physics.

In ’48 I made an effort to find a job here in the area. I wanted to be closer to my mother, and the one thing I didn’t want to do was to go into the war industry or the post-war industry, didn’t want to go into defense work.

VAN BENSCHOTEN: Why was that?

KNOPOFF: Political reasons. I didn’t have an enthusiasm for the kinds of things that would produce more and more destructive and more devastating weapons. I don’t know. I wasn’t really attuned to that. It was partly my upbringing, but I don’t know. One gets hardened as one gets older, but I was very sensitive, and I really didn’t care for violence. I still— It disturbs me enormously, and I can’t— The things that you read in the papers are so horrible. Anyhow, it was a principle. It was a principle.

I explored several other places that might have a use for an almost-Ph.D. physicist, but they didn’t, and so I decided that maybe teaching was the appropriate thing to do. So off I trundled in my dad’s 1939 Dodge car now. We had a sedan, because he didn’t need a pickup truck in his war work, and he had left me the ’39 Dodge, which we, after a few blown radiators on the way back to Ohio across the desert in Arizona—or no, one radiator and a couple of blown tires—finally made it to Ohio and embarked on teaching.

VAN BENSCHOTEN: If you would, talk about that transition, then, when you get to Ohio. First of all, you know, climatically speaking, you have winters now in Ohio.

KNOPOFF: Yes, and you have— My first impression of Ohio and the Ohio River Valley in September was one of heat and mugginess. I had never encountered all that humidity before. The winter I wasn’t yet ready for.

But what I found in Ohio was, first of all, I'd been brought up and lived my entire life, never been out of Los Angeles before, except for one family trip to San Francisco, but I hadn't encountered a rural community before, and here we were thirty-five miles out of Cincinnati and I was in this small town. I think it was 5,000 population and a school with 2,500 population, or maybe it was reversed; I've forgotten which way it went. It was rural. It was WASPish. I felt uncomfortable.

My landlady was the head of the local chapter of the DAR [Daughters of the American Revolution]. She had a housekeeper. My landlady was one of the owners of one of the two banks in town. She was quite well off, and she had a housekeeper and a handyman gardener. And when I drove up that first summer day, all three were waiting for me at the head of the driveway. I greeted them, and I was introduced to Mrs. Stewart, my landlady and the owner of the house; Mrs. Moore, the housekeeper; and Mr. Kelly, the handyman gardener.

And she said, "Well, after you've taken your suitcases up to your room, why don't you come down and we'll have a cup of tea," or something like that. So I thought that was fine. So we came in for the cuppa. Mrs. Moore brought in the tea and we sat down, and Mrs. Stewart said to me, "You know, that was a very nice thing you did when you arrived."

I said, "What did I do?"

"Why, you shook hands with Mr. Kelly." You see, he was black. This was 1948.

I also must tell you that some— Along about July, I got a letter from the chairman of the department, who turned out to be a princely fellow, but I hadn't yet met him. This was still two months before my travels. And he said, "I want to pass along a recommendation from the Vice President for Academic Affairs of Miami University to you. This might be a good opportunity for you to think of changing your name."

VAN BENSCHOTEN: I'm going to flip over the tape.

KNOPOFF: Sure.

TAPE NUMBER: III, SIDE TWO

August 22, 2003

VAN BENSCHOTEN: This is tape three, side B.

You were talking about Miami and your first day there, or rather also the letter you got from the man who hired you.

KNOPOFF: Yes, well, there's nothing more to say. My first reaction was to ask what names would I choose, and they were all Jewish names. [laughs] Then I decided they'd have to accept me on my terms. I didn't do anything about it. I don't even know if I still have that letter or not in the archives.

But the teaching load, I think we were a faculty of three or four people in the Department. The Department had a very fine reputation. They had the reputation that they sent more of their graduates on to graduate schools in physics than any other university or school in the country, but did not itself offer a graduate degree. This was a place that offered a master's degree, didn't offer a Ph.D., and I was given the assignment of teaching the advanced courses for juniors and seniors, which I shared with Dr. Ray L. Edwards. He was a very warm, friendly man.

Teaching was demanding. I think I approached my teaching with too many expectations. I don't think it was— I wanted them to be at my level, and I wasn't patient enough. I should have realized that Miami students weren't Caltech students. I tried hard, but I don't think it was— It was not a success.

VAN BENSCHOTEN: Is this the first teaching that you had done?

KNOPOFF: Except as a teaching assistant at Caltech. But teaching assistants teach labs. But organizing a course, I'd never had that experience before.

Anyhow, the winter was not as rigorous as I expected. My fellow bachelor friends on the faculty, junior faculty, one in chemistry, one in music, one in political science, I, formed a little social group, and we went off to Cincinnati frequently to hear a concert or go to the movies. My landlady was scandalized. One didn't— Cincinnati was a major trip for her. She did not travel frequently. But we went to restaurants. That was the weekend activity.

And I submitted my thesis, had it typed up by a woman who did some typing in Oxford, and pasted my photographs into the thesis. One didn't— Duplication was a problem, because we didn't have an opportunity to rewrite much, because you were paying by the page when you had somebody typing your stuff for you.

Submitted my thesis, it was accepted, and I showed up. I drove back across as soon as school was out, and we had the Ph.D. ceremony, and I was free. I had not had a summer vacation for a long, long time. Even before the war, I had been working in the summer, so this was a good long time, '49 at that point, and I would go to the beach almost daily, went up to Caltech a few times to visit with my old classmates who had still not yet finished their degrees, and chatted. And, to tell the truth, I was getting a little bit bored.

One evening—I remember it as a Friday evening—the telephone rang. I was going out very shortly. The telephone rang, and this voice on the other end said this was Professor......,, and I didn't catch the name, at UCLA, and "I've been talking to some of my colleagues at Caltech, and I'm looking for somebody who might like to undertake a summer job of research. Your name was mentioned, and would you be interested? I've been talking to my colleagues in physics up at Caltech."

And I said, "Well, I need to know a little bit more about this." So he launched into a description of the research over the telephone. I didn't understand anything he was saying. It didn't mean anything to me. So I said, "All right, look. I don't quite follow all you're saying. Can I come in tomorrow, Saturday, and we can talk about it?" I'd been used to this country environment where there was nothing else to do but go into the office on Saturdays or Sundays, because what else do you have to do in Oxford, Ohio? [mutual laughter] And besides, I was working seven days a week on my thesis, except the times I went to the movies or a restaurant or something like that. And before that, I'd been to Caltech, and when you're a student, you're a full-time student. So I said I'd come in on Saturday.

He said, "Well, around here, we don't— There are very few of us here on Saturdays."

So I said, "Okay, I'll come in on Monday."

He said, "Yes, you do that on Monday, but I have to leave in about a half an hour for Canada, and I'll be back in six weeks. But you come in to such-and-such a place at UCLA, and you speak to Mr. So-and-so." And again, I didn't get the name. So-and-so, and I didn't get that name, and a third name that I didn't get, "And they will tell you what it's all about."

So I came in on the following Monday. It turned out that I'd been speaking to Professor [Louis B.] Slichter. And I spoke to these three people. One was a beginning graduate student and two were undergraduate assistants, and they didn't have any idea of what Professor Slichter had in mind, except that he had been on a recent visit to Canada, and at the University of Toronto he had seen a demonstration in one of the laboratories in which the people had taken a solid block of material and put some transducers on the surface of the block of material, and had been able to transmit seismic waves through the block and record a series of seismic signals, much as you would do on a much grander scale in the Earth, but with explosions or with earthquakes, and recorded at distance on seismometers placed on the surface.

So it was a model of the Earth, except the details of the structure were different. It was just a homogeneous block. And they thought that might be the problem that he had in mind.

So I decided to go to work on that without direction, and I played around a bit and did a little reading. And after six weeks, the day was announced that Slichter was returning.

The faculty of the Institute at that time consisted of four people. There was Slichter and [David T.] Griggs, [Clarence E.] Palmer, and [Robert E.] Holzer. H-o-l-z-e-r. Holzer. Palmer and Holzer were atmospheric physicists and Griggs was a solid-earth experimentalist working on high-pressure experiments, totally outside the field that I was. All were outside the field.

Palmer, who had a wry sense of humor, came in the day before Slichter's arrival and said, in an attempt to needle me, "Professor Slichter's arriving tomorrow, and oh, you're going to be put under such scrutiny that you wouldn't…" I've forgotten the precise words.

Well, the day came, and in walked this kindly man, white hair, smile, and I showed him what I'd done. He was extraordinarily pleased, and all this tension that Palmer had tried to put into me evaporated. It was marvelous.

Slichter wanted me to stay on. He said, "Wouldn't you stay on?" And I told him, no, I had a commitment to go back to Ohio, I'd promised, and it wasn't going to be the same as it'd been with my predecessor at Ohio who had left just after agreeing to come.

So I went back to Ohio, and that winter was a difficult winter. It was not only a winter with— The snow was manageable, but I wound up with a couple of episodes of pulmonary infection, one put me in the hospital, mild pneumonia, and I decided this was not for me. It wasn't just the weather, and it wasn't just the encapsulated environment of Ohio, the people's attitudes and the distance from the big city, no great libraries, no great art galleries. I could go weekly in to hear the Cincinnati Symphony Orchestra, which was a pretty good orchestra. My first exposure to [Giuseppe] Verdi's *Requiem* was there. It was a marvelous experience.

But I looked around at my colleagues, and the fact that Miami was not a research institution, it was strictly a good preparatory school, at least for preparing those students who wanted to go on to advanced degrees, and I looked around at my colleagues, and these were people for whom teaching was going to be the end of their career. They were not going to be involved in anything creative.

At that point, I remembered the faculty at Caltech. It wasn't difficult to remember them. They were still a great presence, and these were people who were not only teachers, but they were creative individuals. And I said, "I've got to do more than— What I'm doing now in '49 is what I'm going to be doing forty years from now if I remain here." And I said, "I've got to change."

And so I wrote Slichter, and I said, "Is that offer still open?"

And he said, "Yes."

And I accepted, and they had promoted me from assistant professor to associate professor in the first year, after the first year at Miami.

VAN BENSCHOTEN: That's fast.

KNOPOFF: Forty-eight hundred dollars a year in my second appointment for nine months. Slichter offered me fifty-two hundred or was it fifty-four hundred for eleven months at UCLA, which was a decline in salary. Miami said they couldn't meet it. I said all right. My mind had already been made up, and I packed my belongings and trundled back to Los Angeles, and here I've been ever since, except for some times away, which we'll talk about.

I started in on a program of research under Slichter's supervision. I was his postdoc. I must say that in '49, when I was still here for the summer, I forget whether it was eight weeks or six weeks or ten weeks. I've forgotten the time I was here. I didn't know anything about geophysics. I'd never had any exposure to geophysics.

Slichter asked if I would be interested in geophysics and interested in coming here to UCLA.

And I said, "Is there a future in geophysics?" It was sort of a brash effrontery of young people who don't understand diplomacy.

And a twinkle appeared in his eye, and he said, "Well, some of us think so." [mutual laughter] And here was this great man who was already a member of the National Academy of Sciences, and he was a very distinguished scholar.

VAN BENSCHOTEN: Well, this would be a good place maybe to talk a little bit about Slichter as a man, his personality. I already have sort of an idea, a rough idea, I think, of what his temperament was like, but maybe if you could elaborate on that a little bit more. Talk about his achievements, too, through your eyes.

KNOPOFF: Slichter came from an academic environment. His father had been professor of mathematics and later dean of the graduate school at the University of Wisconsin. Slichter was one of four brothers. All went on to achieve great distinction, both in the academic and in the manufacturing or financial world. One was a famous professor of economics, Sumner Slichter, at Harvard [University]. One [Donald S. Slichter] was the president of Northwestern Mutual Life Insurance Company, and one became president of a steel company in Cleveland; I've forgotten which one. So they were all very, very distinguished.

Slichter was brought up in this environment of ideas, but developed a great individuality. He fell under the aegis of Max [Maxwell] Mason, who went on later, who was professor of physics and mathematics at Wisconsin, and who went on later to become the president of the University of Chicago. And then when he moved out here and— He had a great distinguished record. He was the head of the Mount Wilson Observatory Council, the advisory council for the Mount Wilson Observatory. He was a very important guy.

Slichter got his degree at Wisconsin in 1917 in June, and the First [World] War had already broken out in April, and Mason was involved in antisubmarine work, and he enlisted Louis to become involved in that program of research. Mason had the idea of detecting enemy vessels by setting out an array of microphones, essentially, of receiving devices that would allow one to focus the sound and therefore minimize the noise of what's coming in from— It's a principle we still use today. We have array detectors.

They did some experiments in Lake Mendota on the University of Wisconsin campus, and Slichter went off very quickly as a commissioned officer to the East Coast to do the antisubmarine work, and after the war, came back to Wisconsin, did a Ph.D. under Mason as an experimentalist, and Mason then got a—I'm sorry for that long prologue—but Mason got a contract from a copper mining company to detect ore bodies, mineral or metallic ore bodies, conductors, under the surface of the earth using methods of physics. That was the dream.

Mason formed a company, and it was called Mason, Slichter, and Gauld—G-a-u-l-d, Gauld—and the company was in business, and they were partners. Their job was to apply physics methods to detecting ore bodies, and they got contracts. They developed new techniques, and they were quite successful.

Mason went shortly afterward, within two or three years, in the twenties, to the University of Chicago where he became the president, and the responsibility for the company was now Slichter's. He was then in his late twenties.

Then the Great Depression came and no one was interested in finding more minerals, and the company went out of business, and Slichter was on his own. He was well known as a prospector, not in the mule train version, but in a very high-class way, and this was sophisticated stuff. He got a position as a research associate at Caltech for a year, and he says in some of the correspondence, which I have here, that he used it to sharpen up his skills in mathematics. He studied with all the great names at Caltech in 1930.

On the campus at that time was the chair of the Geology Department at MIT [Massachusetts Institute of Technology] who wanted to get geophysics started at MIT, and he invited Slichter to come to MIT to initiate a geophysics program, which Slichter did. Then he did some extraordinary experiments, some extraordinary calculations and some extraordinary experiments that won him international recognition and election to the National Academy of Sciences; very imaginative work, groundbreaking.

The war came, and away he went again to war. Again, submarine detection, torpedo-entry problems. He never really discussed these things, but if you scrounge, you can find descriptions of them. At the end of the war, he decided to return to Wisconsin, his place of origin, took a professorship there, but he said that he was going to just relax and kick back for a while, and not make any serious decisions about new work until he'd had a chance to reflect. It had been a very intensive time.

He got the program at [the University of] Wisconsin started, and at that time UCLA had the idea of starting an Institute of Geophysics. That idea had its genesis in discussions among four or five members of the UCLA faculty, principally Joseph Kaplan, Joe Kaplan in the Physics Department; [Harald] Sverdrup—I'll remember Sverdrup's first name—who was the Director of the Scripps Institution of Oceanography. You have to understand that in the forties, Scripps was administratively part of the UCLA campus. The UCSD [University of California, San Diego] campus did not exist, and the Scripps Institution of Oceanography was administratively and academically part of the UCLA campus. There was a laboratory, enormous laboratory, 120 miles away or 100 miles away, whatever the distance is. But all of the paperwork, and if a student wanted to get a Ph.D., the Ph.D. was awarded through UCLA, and that sort of thing.

Sverdrup in Oceanography. Kaplan in Physics, whose specialty was high-atmosphere physics. [Jacob A.B.] Bjerknes, who was in our Meteorology Department, [Jørgen] Holmboe, who was his colleague in Meteorology, and [James] Gilluly of the Geology Department— I think these were the principals.

Geophysics at that time didn't exist as a discipline. The people who were doing geophysics were doing other things, but you could see that the motivation was mainly from the atmospherics people and the oceanographic people. They went to work, and largely through Kaplan's energy, they persuaded the University to put together a committee to develop this idea.

An Institute of Geophysics was, in fact, approved in '45, and a committee of eleven members was appointed, six from the south and five from the north, five from Berkeley and five from UCLA, and Sverdrup from Scripps, also from UCLA, with Vern Knudsen, our dean of Graduate Studies here at UCLA and professor of physics, as the chair. And their task was to decide what this new institute was to be.

They decided that the headquarters of the Institute should be at UCLA, not at Berkeley. They decided what the purpose of the Institute was to be, and it was to study the physics of the environment of the Earth, the solid Earth, the atmospheric Earth, the fluid Earth—space physics didn't yet exist as a discipline—and to identify a list of candidates for the directorship of this Institute, and Slichter was the number one choice. The first two alternates were Bjerknes and then Kaplan.

The inquiry went out to Slichter, would he be willing to undertake the directorship, and he replied from Wisconsin that he'd just arrived two months earlier at Wisconsin, and he couldn't think of undertaking a new task. A subsequent inquiry went out about four or five months later, with the same reply.

The Institute was formally put in existence in July '46 with Kaplan as Acting Director. The view of Kaplan and really of Sverdrup was that of a genuine statewide institute with localized function that oceanography should be done at Scripps, in La Jolla, that UCLA, with its power in meteorology and upper-atmospheric physics, Kaplan, Bjerknes, and Holmboe, would be doing atmospheric science. Solid earth geophysics would be at Berkeley where there was great strength, where there was the Seismological Laboratory headed by Perry Byerly. Perry Byerly was a great seismologist.

It was also decided early that UCLA would not be doing seismology as a branch of solid earth geophysics, and in the Kaplan model would never be at UCLA anyhow. Because seismology was already concentrated at Caltech in the persona of Gutenberg, and Gutenberg and Byerly had divided up the State of California into the northern part and the southern part, and they were doing the seismology for these two parts, why have another institution of seismology when Caltech was already a world-class institution, which it was? That was the Kaplan model.

There was an inconsistency. If you had the headquarters at UCLA and you brought Slichter in, who was a solid earth geophysicist, then how could you have a solid-earth geophysicist at the head of an organization that was to do atmospheric science?

Slichter, himself, in the summer of '46, went on a seven-month tour of geophysical organizations, departments, both in the U.S. and in England. He came to Caltech. He came to Berkeley. He was at Stanford [University] for a few weeks. He spent some months in the Cambridge area—MIT and Harvard—went to Cambridge, England, spent some time there with Teddy [Edward, later Sir Edward] Bullard.

In January, early January, of '47, even before returning to Wisconsin for the beginning of school, he'd already telephoned or got in touch with, I don't know, telephoned, but he certainly got in touch with Vern Knudsen and said he was interested. It's clear to me that this was a time that Slichter was considering should he take the job at UCLA, should he not take the job at UCLA? What is geophysics? How would he organize an institute if he were to take the job? He wanted to explore what was being done in many places. It was an education for him, and he made his decision even before coming back to Wisconsin.

The committee convened. I have to tell you one other bit of this story, and that is that James Gilluly, Jim Gilluly, was the chair of the Geology Department here. He was a member of the committee as well, and he was negotiating with Dave Griggs, who was by then a very, very famous solid-earth experimentalist. Dave had got his Ph.D. under P. W. Bridgman, Percy Bridgman, who got the Nobel Prize in physics, at Harvard, and he was experimenting with the deformation of rocks under high pressures when the war came and had published some extremely important papers and, after the War, found himself for a while at the RAND [Research and Development] Corporation. Jim wanted to bring him to UCLA, and after negotiations Griggs signed onboard at UCLA as a member of the Geology Department.

But Gilluly, who was a member of the committee, said, "This is the guy. He's world-famous. This is the guy you want in your Institute of Geophysics." And once that was done, the idea of a localized institute focusing only on atmospheric science was— It was no longer a possibility. And this took place before Slichter had agreed.

So with Slichter and Griggs onboard, the Kaplan model was no longer a possibility. Kaplan set to work to implement his model, and he sent out letters of appointment and invitation to Holzer and Palmer, who were— Palmer was a tropical meteorologist, and meteorology of the tropics is very different from the meteorology of temperate latitudes. And Holzer, who was very much concerned about thunderstorms, lightning strokes, very important parts of atmospheric science, later went on to a distinguished career in the magnetism of outer space.

The chronology, as best as I can put it together, was that the committee agreed—ten of the eleven members of the committee agreed—that the appointment of Slichter should be made with all possible speed. The eleventh was Sverdrup, who agreed that Slichter should be appointed to the faculty at UCLA but that the question of the directorship should be kept open for a little bit more deliberation.

So the committee agreed. They wrote a letter. It was a letter to the president, President [Robert G.] Sproul, that the appointment should be made.

Kaplan knew of all this. Of course, he was a member of the committee and knew. And before the letter was sent, according to the chronology that I have been able to identify, Kaplan wrote or contacted Sproul and asked to be appointed Director, not the Acting Director. And the letter from Sproul arrived appointing Kaplan Director.

The committee letter went up to Sproul and then started the difficult machinery of generating an appointment. It had to go through all the committees, which we know about as part of the procedure. At that time, apparently, one could get an appointment through in four or five months. Now it takes much longer. And the formal letter of Sproul came back after the committee letter was received. The formal documents for appointing Kaplan to his new position as Professor of Physics and

Director of the Institute came, and the appointment as Director of the Institute was for one year starting in April ’47, retroactive to the July 1st preceding. So that one-year appointment would be up in July.

The letter of invitation went out to Slichter in July, and Slichter accepted and Kaplan— I have a letter from Perry Byerly to Louis Slichter in which he says that Kaplan was very unhappy about this whole thing. I think Kaplan anticipated that the appointment would be renewable, but Sproul, in his wisdom, realized that he had been put in a very awkward position of having appointed a director contrary to the committee’s wishes. In fact, there are letters in the file from the members of the committee, and they are extremely concerned, and Kaplan is somewhat on the defensive in his letters as to why he has taken on this job.

In any case, there was a rift that developed with Kaplan and Slichter, both on the same campus, and there was never any great friendship, which is perhaps an understatement. And the rift never was healed.

But that was an environment that I knew nothing about. I arrived here in ’50 as Slichter’s postdoc, and as a junior person, twenty-five years old, I had no experience in university politics, and I didn’t have an academic appointment, so I didn’t even hear about these discussions in faculty meetings. I worked in the laboratory.

Our lab was in Site 1. No one knows of the existence of Site 1 anymore, but the Physics Department was in the then-called Physics-Biology Building, it was Physics slash or Physics dash Biology, and that’s now Kinsey Hall. The Biology

Department and the Physics Department shared the building. And just south of the building, where Knudsen Hall now stands, was a set of shacks, literally, wooden shacks with some on the ground with concrete floors and some up on—with an elevated floor, a little bit—with some on piers.

The Institute [of Geophysics] had one building on the floor, on the ground, and I had Slichter's lab, which was one-half of that area, of that building, and Griggs had the other half of that building. Palmer and Holzer shared one of the buildings that were elevated, and Slichter and the Institute office was in another building. So we had two-and-a-half buildings. Slichter shared his with some other organization in the social sciences, but I don't remember who it was. And that was it.

Palmer and Holzer had their postdocs. I was Slichter's postdoc. Griggs had his postdoc.

VAN BENSCHOTEN: Let's hold it right there, and then we'll pick it up again with a little bit about your research. Let's get a new tape in the machine.

TAPE NUMBER: IV, SIDE ONE

August 22, 2003

VAN BENSCHOTEN: This is tape four, side A.

I was hoping to remain on the subject of the Institute of Geophysics. If you could talk maybe a little bit about Slichter's conception of what the Institute's about, its aims, its funding, especially in those early years.

KNOPOFF: Okay. Let me first indicate that Slichter had a wonderful personality. He interacted wonderfully with people, had an extraordinary sense of humor. He was jovial. He was kind. He taught by letting— He understood that people learned if they could relax, and I thought that that was a remarkable quality that he had.

When Slichter arrived here in the summer of '47, he found that all the FTE [full-time equivalent] had been spent. There were four senior faculty members: Griggs, Slichter, Holzer, and Palmer. The Institute had been allocated, as I understand it, 5.2 FTE, and there were bits and pieces here and there. But by the time he arrived, there was no flexibility. So he had an institute, but he couldn't make any appointments to it.

There is a record of an appointment of an assistant professor to the Institute in '49, I believe, in atmospheric sciences, I think in Holzer's field, and that was for three years. He doesn't appear in the annals. But by '54, so that's now seven years after Slichter arrived here, two appointments were made in geochemistry, and these are the first two major appointments. One was Bill [William] Fyfe, in geochemistry, an assistant professor, and the other was George [C.] Kennedy, who had come from Harvard, and he was a full professor in geochemistry.

If Slichter had a program of how the various subdisciplines should be represented in the Institute, I never knew about it. Slichter often, often remarked that the success of the Institute was to be found in the appointments that it made. He wanted to appoint people of distinction. He also had a talent for identifying people who would become distinguished. But his idea was eminence first, no matter what discipline, and he would let the Institute develop around the people in the Institute and its new appointments.

Griggs and Slichter were members of the National Academy of Sciences. Kennedy was an absolute genius. He was, I think, the greatest experimentalist I've ever met, but he was also unbelievably vigorous and without diplomatic skills. He antagonized people. I think with a different personality he would have been elected to the Academy. He did wonderful, wonderful things.

The Institute grew significantly under Slichter's guidance, but I think we also discussed the unique position of the Institute on this campus, and let me mention— make a few remarks, about that. The Institute was endowed, is endowed, is still endowed, with faculty positions. The coin of the realm in the academic world is the FTE, full-time equivalent. All right. And these are budgeted positions, budgeted. Ultimately the budget flows from Sacramento, but these are budgeted positions, and they are the coin of the realm. They have to be— You have to get them by pleading and negotiating with and by showing a need, with the administration.

We were a statewide institute and not beholden to the campus, so our appointments are statewide appointments, appointments through the vice president for research. The president sits at the top of the university, but there are a number of different threads. The most populous and the major thread of this institution, of the university, is the instruction and research program. That accounts for the departments of teaching and the research that's done in the departments.

The departments offer academic degrees, but there's also a research line in the flowchart, and that research line originally focused on two institutions in the state. One was the Scripps Institution of Oceanography and the other was the Agricultural Experiment Station, and these two were to fulfill the responsibility of the University of California to the State to allow for major advances in the technology that would improve the industry, improve the financial presence of the State of California. One was fisheries and the other was agriculture, two major industries. So these were endowed with professorships, but without teaching responsibilities, because they could not offer degrees.

The Institute was formed as the fourth of these organizations. The third was the Lick Observatory. The Institute was formed as the fourth of these organizations, reporting directly to the president of the university at that time—we no longer do that—with faculty who were members of the Academic Senate, but with no teaching responsibility because they were not members of any teaching department. This is a condition of great privilege to be a member, for some people on this campus and many other campuses. They'd love to have a full salary, devote their life to research, and not have to go into the classroom.

So now I must say that our appointments are approved not by the president, but through the regular appointments procedure on the UCLA campus. The UCLA campus gives us space, which is also an important component of the balance sheet of any organization. A large part of our nonacademic support budget comes from UC-statewide. It comes from Oakland, formerly from Berkeley.

I think the Institute was always an object of envy, and many people on the faculty always said, "Why can't I have that kind of position, when these other people have this kind of position?" And Slichter's method of insulation was to appoint people of incredible quality so that the Institute could not be attacked, because it was just not doing things that could be otherwise done in departments.

He also saw the Institute in terms of its directions as pursuing the ideas in the original proposal, to explore the academic interstitial space between the mainstream departments that were concerned with the issues of the environment, the natural environment of the oceans, atmospheres, and solid Earth, and later space. And the Departments of Physics and Chemistry and Meteorology and Mathematics and Engineering and Astronomy were all interested peripherally in these areas, in this area of the Earth and its environment, but it wasn't their main focus.

The physicists were off doing high-energy physics and quantum mechanics and the physics of the twentieth century. The geologists were interested in the surficial structure of the Earth at those times, in those days, and issues of the deep interior of the Earth were not really their province.

The chemists weren't interested in why minerals of one chemistry should appear in one place and not in another place. How do the various chemical reactions produce different kinds of minerals happened to be George Kennedy's specialty, under high pressures and temperatures. So that has continued to be an important, pervasive direction of the Institute. That's a very broad blanket, to explore the natural sciences of the Earth and its environment. That is extraordinarily broad, and no one does that in its entirety. We all do small pieces of it.

The general idea was that, I think, in Slichter's mind—I'm reading his mind—was that you should appoint people who were of imagination who were able to attack these issues on the fringes of the mainstream areas, that you should appoint people of distinction because that would establish the reputation and the security of the Institute.

And I think through Slichter's work, at the time of his retirement we had achieved— He had achieved those goals. When he retired—he retired as director in '62; he retired from the faculty in '65, I believe— there were seven members of the Institute who were members of the National Academy of Sciences, if you want to use that as a criterion. I've referred to that frequently. I don't think that's the proper criterion, but nevertheless, it's a measure. It's one measure. And I think the Institute since that time has continued to do extremely well. There's some fluctuations, nevertheless, of necessity.

Let me tell you about two more appointments to the Institute, the next two appointments after Fyfe. Fyfe didn't spend too many years here. And Kennedy. But the next appointment after Kennedy. And it goes back to my own research at that point. Slichter, he kept hands off. I never had direction, I don't think, from him in my own research. He brought me in, and I don't think I saw very much of him. I never reported to him. I sort of decided, well, maybe seismology is a— I had this idea of the seismic model with the transducers on the block of— And I went from there.

I started reading in seismology. I saw a strong connection between seismology, which was wave propagation, and electromagnetic theory, which was where I'd been brought up as a grad student, except the problems were rather more complicated for technical reasons.

So I started to develop a set of ideas about developing seismology, seismic wave theory, in directions parallel to what had been developed for electromagnetic theory, which was a beautifully and highly developed subject dating back to the beginning of the twentieth century. And I had this general idea. There were fragments here and there before me, but I had that idea.

A bit of personal history here. I was doing so much reading and so much exploring and trying all sorts of things out. I'd come in '50. By '52, nothing had come out. I was still doing this exploration, and this secretary to Dr. Slichter, one secretary did everything. She did all the financial business, all the academic administrative business, all the correspondence for the whole Institute. Her name was

Gertrude Perrine. I mention that because her name will appear a little bit later in our story.

But she came to me and said, “Let me whisper something in your ear. I think you ought to submit something to a journal, so publish a paper.” I had zero publication record, nothing from my thesis, nothing in all the— Zero, absolutely.

Well, I ginned up a one-page paper and submitted it to a journal, and it was published, and I thought that would palliate things. And by ’54, I had published this one one-page paper.

VAN BENSCHOTEN: That’s amazing.

KNOPOFF: And I’d been here four years. And then it started to come out, and I started to publish a few, a couple of important papers, and they got recognition. And I started going to meetings, and I think it was ’54 or ’55, it must have been ’55, I was invited to go to MIT, give a seminar, in what was obviously recruiting. Universities and departments and institutes are always looking for possible appointments, talent. And I was invited to MIT, and I encountered a young man there who was, I think, an assistant professor. He may have been an associate professor,
Gordon J.F. MacDonald. He was three years my junior, and he was a whiz. He was a genius. He could do anything, experimentalist, theoretician.

He had a student working on attenuation of elastic waves of the Earth. And I went there, and I had been working on attenuation of elastic waves of the Earth, and it was clear to me that what they were doing was completely wrong. I knew “the truth,” and we had intense discussions there, and they came around to my point of view. He did. The student finished his thesis in the direction he'd already started. But Gordon came around to my point of view, which was that attenuation, at least near the surface of the Earth, was an inherently nonlinear process.

And I came back, and without caution said to Professor Slichter that I had met the most amazing scientist at MIT. He had received his Ph.D. from Harvard, and Kennedy knew him, Gordon MacDonald. And there was one position open in the Institute at the time, and there was competition among the various postdocs in the various labs in the Institute, and we all wanted to get that academic appointment. And I thought that I would— I sort of dreamt that I would have that. And they went after Gordon MacDonald. [laughs] And they got him.

There was negotiation between Slichter, and apparently the price level kept getting raised as the negotiations went forth, and Gordon accepted and left MIT and came to UCLA. And this was a revelation. We published a number of papers together, and I think that it was a year later that I got an appointment to the faculty, to the ladder faculty. I came in as an associate professor at that point, but that was in '57. So those were the sixth and seventh appointments to the institute.

VAN BENSCHOTEN: Lucky seven.

KNOPOFF: Lucky seven. And you should know that in the National Academy of Sciences there was a youth movement. The National Academy of Sciences has mainly a bunch of greybeards. They always speak, "We have to have a youth movement," but they can't resist electing the persons who deserved to be and that sort of thing.

But in the early sixties, they had a youth movement, and in three years in succession— I can verify these dates, but I think in ’58 they elected Frank Press, who is six months older than I am. In ‘62 they elected Gordon MacDonald to the Academy. And the following year, ’63— So we had ’58, ’62, and the following year in ’63 they elected me to the Academy. So, in fact, Gordon was elected the year before I was. I think that that’s the case. And so Slichter appointed well in that case, too. He was really a— Gordon was a brilliant, brilliant guy.

VAN BENSCHOTEN: And what was your working relationship like when you were putting out these papers together after he joined the Institute full-time?

KNOPOFF: Well, we worked well together. That was, I think, with one other brief exception, the only time I ever worked closely with a member of the Institute, of the Institute faculty. It wasn’t my style. My style was always to work with my students and my own postdocs, and it was basically a direction that encapsulated my own, or our own, interests, and these interests really didn’t overlap significantly the directions that other faculty members, other groups, were taking in the Institute. So we made our own directions.

But in the case of Gordon, we played out these issues, first of attenuation, and secondly of the chemical composition of the deep, deep, deep interior, the core of the Earth. I had some ambitions in those days of applying quantum mechanics to the problems of the Earth. Let me say that I didn’t fulfill those ambitions, mainly because the seismology became so engrossing and so demanding that I just— But I did publish a few papers, and one of the best ways of studying that area is to try and understand the properties of materials at very, very high pressures, governed by the electronic properties of the atoms under high pressures. And Gordon and I explored those issues together, because he was extraordinarily good in those issues of the early composition of the Earth, the composition from which the Earth would have been formed earlier. But he was extraordinarily broad. He played— He did many, many things.

In the mid-fifties— Well, let's back off a little. Slichter had made significant contributions to the study of heat flow and the thermal evolution of the Earth, to the geophysical inverse problem, which is how you go from measurements made at the surface of the Earth to its internal structure, and to the use of chemical explosions as sources of seismic waves to determine the structure of the Earth's crust, a technique that was widely adopted. Almost every university in Germany in the sixties and seventies was doing crustal seismology by the methods that Slichter developed in the thirties. And all seismic explorations by the petroleum industry use his methods, modernized of course, to this day. He also invented a technique to do prospecting for minerals using electromagnetic waves.

Slichter had never lost his contacts with the petroleum and minerals exploration industry, and he had the idea in the fifties to find funding for my work, in the mid-fifties, and he put together a consortium of twelve—I think it was twelve—organizations—eleven oil companies and one minerals company—each to contribute a few thousand dollars to a fund to support work in what was called the Seismic Scattering Project. It was Slichter's Seismic Scattering Project, and the science was essentially my responsibility. I did the whole thing. He was not involved scientifically.

VAN BENSCHOTEN: This is when you're still a postdoc?

KNOPOFF: I was still a postdoc. And I got together— I had six graduate students. At the beginning, I was not allowed to be a member of a dissertation committee, but I was, after '57, a member of the faculty, and I was on the dissertation committee of all these students.

But I had six graduate students. I had a postdoc, as well. I was essentially developing my ideas of seismic theory along the lines from which electromagnetic theory had been developed and to use this as a prototype. But now there were much more difficult mathematical issues. And that was really the first, my first— I've had three major episodes in my career, and that was really my first career, which I think lasted until the early sixties when it started to move in a— It began in a different direction.

So Slichter was instrumental. I couldn't have gone out— I was a junior person, and I couldn't have got that kind of support, but he went out and did it for me, really. My gratitude to him has never stopped. It was an incredible opportunity, and he gave me that opportunity.

VAN BENSCHOTEN: Good. Did you want to continue or—

KNOPOFF: Well, do we still have some time?

VAN BENSCHOTEN: Yes, we do, definitely.

KNOPOFF: Okay. Let me tell you what happened in— Let me jump ahead a few years, because we've mentioned Gertrude Perrine. Gertrude had moved from the Institute to become the chief office person in the Physics Department. We had moved out of the shacks, incidentally, at Site 1, and we moved into this building, the Geology Building. We are in the new wing of the Geology Building. The old wing— The Geology Building at that time had been only extended— Well, I can show it to you, but it didn't extend out this far as where we're sitting now. And we had rooms that were assigned to us in that building.

So I'll bring us up to 1957 at this point, in terms of my personal life. I was a bachelor. I was having a social life, but not very intense commitments in any direction. No commitments in any direction, despite pressures from my mother to develop them. At the age of thirty-two, I was still without connections. And in '57, Gertrude Perrine, who had a very eligible daughter herself, threw a Christmas party. To throw a Christmas party with eligible men to meet the one daughter would be quite unbalanced, so the daughter invited some of her friends, and I was one of the people identified by Gertrude to come to this Christmas party. It was a very pleasant party, very nice.

And I saw this very beautiful girl who was not the prime focus of the party. I saw this other beautiful girl, and that was it, and we were married. Three years later, but we were married.

VAN BENSCHOTEN: Well, good. I was going to ask you about that.

KNOPOFF: And there are other features of that, but, anyhow, that was a beginning of a marriage and a family that began in ’57, December ’57.

VAN BENSCHOTEN: Just for the record, and we’ll talk a little bit more about your children, but you had shown me some photographs. You have a son and two daughters.

KNOPOFF: Yes, yes.

VAN BENSCHOTEN: We’ll definitely mention them as we go along.

Okay. We’re almost near the end of the tape here, but before we do, maybe we can close out this session at least. Tell me a little bit more, if you would, about sort of early UCLA when you come here. You’ve already talked about Site 1. Where did you live when you came here?

KNOPOFF: When I first arrived in ’50, we were still living in the house in City Terrace on the Eastside of Los Angeles that my father had built and in which he had died. I had mentioned that we had lived this nomadic life, and somewhere along the line, I guess it was just before the beginning of the Second [World] War, my mother put her foot down and said, “We’re not going to move again.” And so we settled in that house. We then had enough capital so that my father could do his building without us having to move and sell and move and sell.

But then the war had intervened, and my father had gone off to war work where he was no longer building, and we were settled in that house and we lived there. My father died, and I was traveling up to Caltech from there. And when I came back from Miami to UCLA, we were still there. My mother and I, we shared the house.

She was working in downtown Los Angeles, and I was commuting across downtown Los Angeles to UCLA, and so we agreed that it would be better to move west, and we found a place that was halfway between, and bought a duplex just off of La Cienega Boulevard, and so my mother could travel downtown from the Westside, and I could travel easily to UCLA, and that worked well.

I was living there. I lived there from— I couldn't tell you with any precision when we moved there, but probably around '53, '54, I would guess. We lived there. My mother died in 1960, the year we were married, '61. Forgive me. She died in '61. We were married.

We were married in April of '61. I had been on a year's leave of absence at Cambridge, England, came home. That's another story I'll tell you about later. But I came home and took my bride back to Cambridge. My mother became extremely ill and died in August of that year. We returned just in time, just before her death. And then Joanne and I settled in that house for one year before we then moved to our own house. So, in fact, we occupied that house after my mother had died.

VAN BENSCHOTEN: Describe, if you would, too, since it's fifty years, over fifty years ago, but describe what was the mood of the campus in 1950 when you first arrived at UCLA. I know that's kind of a hard question.

KNOPOFF: I think I'm the wrong person to answer that. I wasn't much interactive with the campus.

I think we have to stop for a moment.

VAN BENSCHOTEN: Okay.

[interruption]

VAN BENSCHOTEN: All right. You wanted to say just a few more words about UCLA.

KNOPOFF: Sure. The Miami time was a useful time for me. Somehow or other, I did start to do some reading outside of physics. I became very much interested in various areas of philosophy, scientific philosophy. I was doing a lot of exploration. It was the first time I could really afford to do that, even though I had great pressures from my thesis and from the teaching.

And one of the people that I read was Hans Reichenbach, who had written— Who was one of the— I don't know if he was *the* foremost, but Reichenbach was writing on the philosophy of science, but especially on the role of quantum mechanics, which, of course, was a tremendous revolution in physics, and people would write in the old days about the impact of Newtonian physics. Reichenbach was introducing philosophy and trying to discuss quantum mechanics in this context, and he was a very impressive author for me to be exposed to.

When I came to UCLA, Reichenbach was on the faculty in the Philosophy Department. So one of the first things I did was to get the schedule of classes, and I saw that Reichenbach was teaching a course in inductive logic to freshmen. And so I went to Royce Hall, in this vast lecture hall, I remember, and there was Reichenbach, my idol, and he was talking about inductive logic and using in his examples, which he did, Sherlock Holmes, the tales of Sherlock Holmes.

Now, I had been a Sherlock Holmes buff, and the reason was that while we were at Caltech, some of the graduate students who always ate together—lunches and dinners—had a little society of self-education, and one would quote Shakespeare. And I learned all of Gilbert and Sullivan with my— I had heard them, heard the records on the radio, and I had bought a copy of the complete plays, and I had such a quick ear that I could remember them all. That was part of that ear memory that I talked about before. One of the members of our group was an expert on Sherlock Holmes, and he knew every character and every— He could send us all an examination saying, "Which train did Sherlock Holmes take to go to—," whatever. And he tried to induce me to become an expert, so I read all of the Sherlock Holmes. And here was Reichenbach talking about Sherlock Holmes, and I really was thrilled that this kind of literature would appear in the discussions or the conversations of this great man.

What I saw at UCLA was an incredible opportunity to expose myself to stimuli that were outside my field. I especially enjoyed the concerts. There were noontime concerts at UCLA that you could go to. They seem to have disappeared now, I think.

TAPE NUMBER: IV, SIDE TWO

August 22, 2003

VAN BENSCHOTEN: I'm sorry. This is tape four, side B now.

Did you want to add anything else to that? We were talking about—

KNOPOFF: I think the library is an incredible resource, but I was doing it on my own. I wasn't doing it except in the one Reichenbach case. I wasn't going over and taking classes.

But the reason I mention that is that in '62, my friend Frank Press, whom I've mentioned before, was then at Caltech, and he invited me to join the Caltech family—faculty. And that was a really tremendous stimulus to my ego, to be invited to go back there and join the faculty of an institution where I'd done my own studies, and I thought very seriously about that, and I took a year at Caltech.

I never resigned my position at UCLA, to the great distress of the people at Caltech. They insisted that I do so, but I didn't. And I spent the year as a regular— Not as a visiting professor, but as a full professor at Caltech, and discovered that UCLA was more to my taste, that I couldn't get the breadth of exposure. The resources were not available at Caltech that I needed to develop my own inner self, outside of my science. The science there was fabulous, but I needed more, and so I resigned Caltech and came back to UCLA. I wasn't "coming back"; I'd only taken a year's leave of absence.

But what UCLA had to offer me was much more than Caltech could offer me. Isn’t that—

VAN BENSCHOTEN: Okay. Let’s leave it there.

[End of August 22, 2003 interview]

TAPE NUMBER: V, SIDE ONE

August 27, 2003

VAN BENSCHOTEN: Today is August 27th, 2003. This is tape five, side A.

I had a few follow-up questions. You mentioned when you had gone to Oxford, Ohio, shaking hands with Mr. Kelly.

KNOPOFF: Yes.

VAN BENSCHOTEN: Mr. Kelly was African American. I was wondering, and I think it's a fascinating story, but you also mentioned a suggestion made by your supervisor, I believe, that you perhaps consider changing your name.

KNOPOFF: Yes.

VAN BENSCHOTEN: And I wanted to ask, had you come across any anti-Semitism in L.A. or Caltech or earlier in your life?

KNOPOFF: Personally not, never. I don't think I ever encountered it either at Caltech or at UCLA. I think I was much more aware of it in the Midwest than I ever have been here.

VAN BENSCHOTEN: I wanted to ask too about the loyalty oath. These questions come from all different sides; they have no order, really. But the loyalty oath comes in '51, '52, I believe, and I was wondering what was the impact of that on you. Nothing?

KNOPOFF: No. I was employed essentially on a research grant as a postdoc, and I was not a member of a teaching faculty, and I didn't have much—

VAN BENSCHOTEN: Do you remember that being talked about, though, at all?

KNOPOFF: Oh, yes. It was going around a lot, and I remember, of course, that Dave [David S.] Saxon left the Physics Department and went to work for SWAC. At the time the National Bureau of Standards had two experimental computer facilities. One was called Standards Eastern Automatic Computer, and the other one was Standards Western, and Standards Western was located on the UCLA campus. And Dave went to work for the Bureau of Standards in their developmental computing facility; this was long before anybody had any decent computing, but in protest against the loyalty oath. So that was well known and quite familiar to us, and there was a lot of conversation about— I think it was Regent [John Francis] Neylan who was instrumental in getting that going. It was a time of great discussion, and people were discussing it, yes.

VAN BENSCHOTEN: Do you remember, if you had any, what your own view of that loyalty oath was, if you had to face it?

KNOPOFF: My own view of it was that it was ridiculous, along with, I think, almost 99.9 percent of everyone that was involved, present at the time. It was nonsensical to single out the university faculty for special consideration. It was a terrible time of leading up to the time of [Senator Joseph] McCarthy and all the ferment to try and divert attention away from where the problems that affected the country were.

VAN BENSCHOTEN: The other thing that comes up very often in our Gold Shield-sponsored university history collection is the attempt of UCLA to sort of get out from under—

KNOPOFF: The thumb of Berkeley?

VAN BENSCHOTEN: The thumb of Berkeley. Exactly. In some of the things that you've said so far, I get an echo of that. For instance, when the Institute was created how, you know, very often professors come from both, and I think there's sort of an attempt to sort of be evenhanded about it. It had to be a coup for UCLA at the time to get the Institute. Is that how it was sensed at the time, rather, you know, that it was centered here even though there was a branch up at Berkeley as well?

KNOPOFF: There was no branch at Berkeley.

VAN BENSCHOTEN: Oh, there wasn't?

KNOPOFF: There was never a branch, and there still is none to this day. There are branches today at UCLA; University of California, Irvine; University of California, San Diego; and at the two labs, at Los Alamos and Livermore. There may be one at University of California, Santa Cruz and University of California, Davis, but I'm not sure. But there has not been one at Berkeley. Some of that goes back to the Kaplan days, but I don't have too clear an image of what those details are.

It may have been a coup, but I certainly don't think it was viewed as one by the rest of the faculty. I think I've already mentioned that the faculty, especially in the near and associated departments at UCLA, saw it as a point of envy rather than a point of admiration that they wanted to understand why this faculty was privileged without teaching responsibilities and why they couldn't participate freely in devoting 100 percent of their activities to research. So I don't think it was viewed that strongly as a coup for UCLA.

VAN BENSCHOTEN: What about the general competition or rivalry between the two schools? Did that work itself out in the Institute in the early days?

KNOPOFF: I think Slichter made a serious effort to generate some sort of communication between geophysicists on the various campuses. One of his early acts was to inaugurate an Annual Conference on Geophysics, sponsored by the Institute, and it rotated to— Was held one year at La Jolla, one year at UCLA, and one year at Berkeley in cyclic order for many, many years.

It was a time, as I've already indicated, that geophysics had yet not emerged as a major discipline on its own on the national scene. There wasn't a national organization, but the annual meetings were extraordinarily small. They could be held in one or two rooms. This meeting was a great meeting, and it was probably the best meeting on geophysics on the West Coast, certainly for a number of years.

Louis also had a small grants-in-aid program that was part of his funding from Statewide, and he used that to set up seed research, to seed programs of research at all the campuses, not just UCLA. I think it was a very serious effort at outreach within the University of California community. I thought it was very successful.

VAN BENSCHOTEN: One final question about the chancellors. In 1950, the chancellor is [Clarence J.] Dykstra. In '52, Earl Hedrick comes along. Did you get to meet either of these two gentlemen?

KNOPOFF: No.

VAN BENSCHOTEN: The great change in stature of UCLA takes place with Franklin [D.] Murphy. That's clear. He's the man with the vision and the man with the know-how, and Murphy's the one who puts UCLA on the map. He was a very powerful individual.

KNOPOFF: I don't know. Have we talked about the invitation that was issued to me to come and join the faculty at Caltech? And while we were in the process of negotiating for my return to UCLA from Caltech, I met with Murphy, and he was an interesting man to try to resist his persuasive powers. He was very interesting, very good, I thought.

VAN BENSCHOTEN: All right. We were talking, when we finished up last time, about the first phase of your research into seismic wave theory, and I was wondering if you wanted to say a few more things about that, and then we'll take it from there.

KNOPOFF: I don't know. It was an interesting time. We did some very good, nice things. We studied the scattering of elastic waves by asking questions about how seismic waves would be affected if they traveled through a medium in which there were inhomogeneities whose sizes were smaller than the wavelengths of the seismic waves. I had a very good postdoc at that time, John Hudson, from Cambridge [University]. He's still at Cambridge. And together we did, and he did separately, some very interesting things on the questions of how the elastic wave velocities would be affected by traveling through media with a large number of inhomogeneities.

And then we also, Freeman Gilbert and I, looked at questions of the opposite end of the spectrum, what would happen if seismic waves hit curved obstacles, curved in three dimensions, obstacles whose dimensions, whose curvatures, were large compared to seismic wavelengths, with particular reference, ultimately down the line, toward understanding how seismic waves would wrap themselves or be influenced by the Earth's core, which is a very large object compared to seismic wavelengths, and that was very interesting.

Then I started to develop my interest in attenuation at this time, and the attenuation there was very much influenced by the experiments that I encountered in the Physics Department here at UCLA that were being carried out by [Professors] Bob [Robert] Leonard and Joe [Joseph A.] Rudnick. Bob Leonard and Joe Rudnick. Sorry. Not Joe Rudnick, but Izzy [Isadore] Rudnick, Joe's father, and Bob and Izzy were measuring attenuations mainly in metals.

I started to do essentially a literature search, and I found that almost all materials, whether they be metals or ionic solids or plastics or rocks, had a very peculiar frequency dependence for their attenuation compared to liquids. And MacDonald and I developed a model that said that this was due to nonlinear processes. Nobody in the geophysics community really thought that nonlinear processes were responsible, that they had to be linear processes, because the processes were taking place at very small amplitudes. I still feel that way.

And then there was a strong leap by the geophysics community that I was not prepared to make, although I must say I waffled a little bit about that in my resolution. But nevertheless, all these observations had been made in the laboratory or at very, very low pressures compared to pressures at the center of the Earth, very, very low pressures in these materials. There was no guarantee that the properties of this anomalous attenuation would be valid at greater depth in the Earth.

There has been a continuing argument whether or not this would be applicable to the deeper Earth. I have not felt so. I wrote a paper with the provocative title—and it had only one letter in the title—the letter *Q*. Q is a symbol that represents a specific measure of attenuation in anything. And the paper has been much cited. Measurement of Q in the mantle of the Earth, the mapping of attenuation in the interior of the Earth is an active, active area today. It's being done by a large number of people.

You have to understand that the evolution of the surficial structures of the Earth—the oceans, the mountains, the Great Plains and the shields of all the continents—is related in some way to internal structures in the Earth. If there are internal structures in the Earth, it implies that there's inhomogeneity in the Earth. If it's something that is completely amorphous and having no structure, it's lifeless, it has no evolutionary properties on its own. So there is a great, great activity today trying to delineate the structure of the interior of the Earth and how that might be used to determine the evolutionary history of the Earth. That's another area of work that's going on in the discipline.

Most of the work of identifying inhomogeneities involves identifying the density and elastic wave velocities in the interior of the Earth. But now we're finding that identification of the attenuation properties, the ability to convert wave motions into small amounts of heat because the amount of absorption by solids is small—that's what contributes to the attenuation—that is now becoming a very prominent mapping area as well.

As for attenuation in general, where you have higher attenuation, you also have higher temperatures, closer to the melting point. Materials closer to their melting points can absorb sound waves more easily. So, clearly this is an effective way to identify temperature differences in the Earth. The connection is not easy, and most of the people who identify attenuations are content to leave it there. They don't want to make the next step to temperatures, because that's hard.

VAN BENSCHOTEN: Let's go to the second phase. You talked about long-period seismometers and setting them up, which eventually leads to the World-Wide Standard Seismographic Network and other important things. I guess the first thing would be, how do you make the transition from the earlier research that you just talked about to this second phase? How did you get there?

KNOPOFF: Well, in 1960, I'd been at UCLA for ten years, seven of them as a postdoc, and I felt that it was time for me to take a leave of absence. I wasn't entitled to a full sabbatical, because I'd only been on the faculty for three years.

[interruption]

KNOPOFF: At the time, 1960 was the time of intense discussions between the Soviet Union and the United States on the questions of underground nuclear testing. A treaty had already been in place forbidding testing in the oceans, the atmospheres, and in outer space. But testing underground became a very large activity and was essential to the development of nuclear weapons, and the development of a treaty forbidding testing underground was never in place, but there were discussions going on.

Discussions were being mainly held at Geneva. I was not a member of the U.S. delegation to Geneva.

It was realized that a scientific program had to be instituted to develop the technology and the understanding to identify and discriminate nuclear explosions carried out underground. I say "discriminate" because you must also, if a nuclear explosion causes seismic waves to travel through the entire Earth, how do you know those seismic waves didn't come from an earthquake?

And so the search began for signatures. The national program was assigned to the U.S. Air Force, and the Air Force, in its great wisdom, recognized that it didn't have the immediate capability of carrying out such a program, because the know-how wasn't there. So the Air Force began a program of education. They supported, through a grants program, activity in developing the fundamental knowledge to carry out such a program in the future.

This kind of seismology wasn't active at this time. Geophysics, in most universities, except for a few institutions, was a minor adjunct of geology departments. The program for detection and identification and discrimination was a significant event in allowing solid earth geophysics to develop nationwide. It became an extraordinarily important program, and we started developing academic programs at almost all institutions worldwide and nationwide and at a wide variety of universities.

I wanted to get in on this activity. I went to my close colleague Frank Press, who was at the time at Caltech, and who had been a member of the delegation to

Geneva, and I asked, "I'd like to be involved in this learning process. What do you recommend?"

He said, "If you're going abroad, how about the following. Why don't we put together a collection of four—." Scrap the word *four*. It turned out to be four. "A collection of long-period seismographs to study how seismic waves travel through a particular localized chunk of the real estate of the surface of the Earth."

And since I was going to Europe—I was going to Cambridge—I decided that I'd write a proposal for funds to the Air Force to acquire four long-period seismographs. We used galvanometers and photographic recordings and drums. That's all done electronically these days.

So I shipped a half a ton of equipment to each of four stations from the United States, and we set up around existing observatories around the Alps, and Frank helped me learn how to install these. He went with me to Europe in '60—he was there for other reasons—and we installed the four instruments at four stations around the Alps.

I began to write some papers at Cambridge on the inverse problem; that is, how one goes from measurements at the surface of the Earth to structure, interprets this structure in the interior. And when I came home, we began to process these data and found an unusual low-velocity channel under the Alps, a structure which transmitted elastic waves very slowly.

Shortly thereafter, by '64, the Air Force had begun a program of the installation of the World-Wide Standard Seismic Network. World-Wide Standard Seismograph Network, I think is the right set of words. And at each of these stations all over the world, except in the Soviet Union and China, I think, we had instruments all over the world, mainly concentrated in the United States, including this same quality of long-period seismographs. It was no longer necessary for us, for me, to go out and set up these instruments in a synoptic way on a temporary basis operating the instruments for a year and then bringing them home and installing them somewhere else. That was my original intention, but now that the density of such instruments in permanent observatories was high enough, I didn't need that kind of individual kind of network.

And so thereafter, my students and I used the films, the records that were coming out of these stations. We used data from— There were a hundred or more such stations all over—I've forgotten the precise number—and we could get data from Egypt or from South America as easily as we could from the station in Pasadena.

VAN BENSCHOTEN: So almost overnight this was done, practically, it was there.

KNOPOFF: Well, overnight in— It means something different today than it does then.

No. The data were sent. The films, you understand, were sheets of film almost a meter long and some twenty centimeters wide, and I can give you tales of trying to get film or trying to get instruments into Spain, which was part of our second installation, after the one we did around the Alps. In the second project, we installed those same four seismographs around the western Mediterranean and found some very interesting structure below the western Mediterranean in its mantle. But I can give you tales of trying to bring film into a country where it—

TAPE NUMBER: V, SIDE TWO

August 27, 2003

VAN BENSCHOTEN: This is tape five, side B.

KNOPOFF: We did an analysis of the transmission of elastic waves, long-period elastic waves, concentrating on the long-period instruments of the WWSSN [World-Wide Standard Seismograph Network] in as many regions as we could, continental as well as oceanic. Under the Pacific, we found an extraordinarily remarkable structure. Beginning at a depth of about 100 kilometers and extending to about 200 kilometers, we found an unusually low-velocity channel. The velocity of transmission of elastic waves through this channel was much lower than had been observed anywhere else in the world.

Under the most ancient parts of the continents, those that are several billions of years old, which are extraordinarily stable structures—and ancient shields are found under every continent of the Earth: Australia, Antarctica, North America, South America, Africa, Eurasia—there we found almost no channel at all. If we associate low velocities with high temperatures, then in terms of the model of continental drift and plate tectonics, each continent has a very deep keel because it's cool and has no low-velocity channel. Whereas the Pacific and, to a lesser extent, the Atlantic, is extremely mobile, the surficial parts of the Pacific are essentially decoupled from the lower mantle, perhaps at this zone, perhaps between 100 and 200 kilometers under the Pacific. So it says that the Pacific and the mantle beneath it has a great mobility, and in terms of the model of plate tectonics, the motions under the Pacific are essentially driving the motions of the entire globe in the mantle.

So this was a very exciting set of discoveries for us, and I wrote a paper called *Observation and Inversion* [Knopoff, L., Observation and inversion of surface-wave dispersion, *Tectonophysics* 13:497-519, 1972.], and in that I outlined the fact that I thought that I could— That the transmission of elastic waves in the mantle of the Earth was sufficiently similar among ancient shields everywhere on every continent, among active mountain-building regions on almost every continent, on younger regions that had been active in more recent times, and we thought we could do a regionalization and split off the oceans as something unusual and different. So it was an exciting time to make these kinds of observations.

We had no data on the South Pole, and moving ahead to a much later time, there was an effort to try to develop an international cooperative program between the United States and the Soviet Union on Antarctica. The Antarctic continent was supposedly neutral ground, and there's the Antarctic Treaty in which the continent is not supposed to be developed for any material purpose, only as a scientific laboratory. There was a station at the South Pole, which Professor Slichter was operating in from the mid-sixties— Well, early seventies onward, I guess.

I got a contract or a grant from the NSF [National Science Foundation], who was administering this joint U.S.-U.S.S.R program, and we put up a seismic station at a Russian station on the coast of Antarctica, and we had a similar station at the Pole, which is in the interior of the continent, and we used the phase velocities of elastic waves to identify that, yes indeed, Antarctica was also a shield. The data were not really good, but we had to do the best we could.

VAN BENSCHOTEN: Did you actually have to go there, then, to Antarctica, or did you have people do this?

KNOPOFF: I went to the Pole once. Louis, even before he retired, he began a program of studying the Earth tides. The Earth tides had been suggested back in the late nineteenth century by Lord Kelvin as a method of studying the strength of the Earth, because the solid Earth responds to the fluctuations in the motion of the sun and moon relative to the Earth. We're not running in perfect circles around each other; we're in elliptical motions. And these cause the tides. Not only are there tides in the oceans, there are also tides in the solid Earth.

You are sitting here at UCLA, and you are rising and falling about thirty centimeters, about a foot, twice every day, because it's a twelve-hour tidal cycle. But, of course, it's very slow, and we're not aware of it, and everything is rising and falling at the same time. So the only flexure takes place on the global scale, not on the scale of this building.

But we know that the Earth is slightly flattened at the Poles, compared to the equator, and this gives the impression that the Earth is a fluid rotating in space. And a rotating fluid, spinning fluid, ought to be slightly elongated in its equatorial dimensions compared to its polar dimension. So somewhere there has to be a change in the strength of the Earth between very short periods such as with seismic waves, where the Earth appears to be quite strong, and at periods of the order of, let's say, ten thousand years, where it appears to be almost a fluid and therefore weak.

So I think principally through Louis' activity, interest was generated in going to longer and longer periods away from seismic periods, so there was the diurnal period and the semi-diurnal, twenty-four and twelve hours. The Earth appeared to be strong at those periods. That's been known for some time.

Then it was decided to try and measure the flexure of the Earth, was it strong or weak at fourteen-day and twenty-eight-day periods, which are the periods of the fluctuations of the moon and its orbit and causing the Earth tide? But there you have a difficulty of measuring that at temperate latitudes, such as at UCLA, because the diurnal fluctuations are extraordinarily large, and it would be extremely difficult to measure the small fortnightly and bi-fortnightly fluctuations.

Louis had a collection of extraordinarily sensitive instruments for measuring the gravitational attraction at the surface, and two instruments operated always at UCLA, and he sent some out to other places. He sent two instruments to the South Pole. Why the South Pole? Because at the South Pole, there is no twelve- or twenty-four-hour day. There is no day and night on a twenty-four-hour time scale, and therefore these diurnal tides would be absent. And the only tides, if present at the Pole, would be the fortnightly and bi-fortnightly tides.

But to get a long run of data to measure something that wiggles every fourteen days, you need a lot of "fourteen dayses" to be able to analyze the data. You can't do it from just one fourteen-day cycle. So you had to have a long run, and the South Pole is a very, very austere place to try and make measurements. He set up these stations.

We sent a graduate student from UCLA. In fact, we advertised and sometimes got people that way who wanted to go. We would send one person who spent the entire year at the South Pole and was then replaced at the end of that period of service with another one, and so we tried to keep the process continuous.

The South Pole is at 9,400 feet about sea level and the first 10,000 feet are ice. So it's at high elevation. You're not on bedrock, you're on ice, and the ice flows very slightly. These all have their effects. And Louis, who was a very brilliant instrumentalist, figured out how to compensate for all these things.

When Louis died, the program was still going on, and I took over the program.

VAN BENSCHOTEN: At '65, '66, in there?

KNOPOFF: This would have been— Well, he died in '78, and I assumed responsibility for the program to keep it going. Along about—I'm guessing—'80, but I can't be sure…

Louis had never been able to travel to Antarctica. It was one of his great dreams. The experiment had been going for fifteen years or thereabouts, maybe not quite that long, and it was his dream. But he was elderly and suffering from adult-onset diabetes, and he was not allowed to go by the Navy doctors who were administering the transportation in Antarctica.

Along about '80, someone came to me and said that UCLA should somehow fly its flag at the Pole, that a senior member of the research program had never been actually physically present at the Pole. So it was represented to me that I should go. Of course, I accepted immediately because I loved travel— It was a place that I had never been and I wanted to see it. And it was an exciting experience to fly from Christchurch in New Zealand to McMurdo Sound and then to fly in a ski-equipped C-130 giant transport turbo-prop with enormous skis landing on the ice at the South Pole.

I spent four days at the South Pole. The students and the technicians that we had down there for the polar summer were perfectly able. They didn't need my help. I just was a tourist, and I enjoyed the experience. It was an exciting experience.

VAN BENSCHOTEN: Yes, it must have been great.

KNOPOFF: To see this vast sea of ice, it's one of the great deserts of the world, and as far as the eye can see, it's flat and white. There's not a— The nearest rocks are a thousand kilometers away. There's not a hill. There's not a structure that you can see. It's just flat. And quiet. If you walk a few hundred meters away from the polar station where there are generators going all the time, it's extraordinarily quiet. It's marvelous.

VAN BENSCHOTEN: So this second phase of your research— By the way, is this program still under way, this project in Antarctica?

KNOPOFF: I don't do it anymore. Oh, we lost our grant. There were two reasons to go to Antarctica. I've told you one of them; the tides. The other was the discovery made here at UCLA by Louis and his group, and simultaneously by a group at Caltech and a group at Lamont Observatory at Columbia University, from the great May 1960 earthquake, the Chilean earthquake. This was probably the greatest earthquake in the twentieth century in the western hemisphere, probably in the world.

The Earth was excited and began to vibrate, and the analogy has always been stated that it rang like a bell. I'm not sure that that's properly drawn, but nevertheless, the Earth resonated. It vibrates in many, many different modes with different frequencies. It's a very complicated structure. And the frequencies of vibration were measured here at UCLA with the gravity meter, the sensitive gravity meter, used as a seismograph, and it was a famous pioneering result. I was not involved in that process.

The spectral distribution— The spectral description of those modes at UCLA and at other temperate latitudes, temperate and equatorial latitudes, is that the modes have a very, very complex structure due to the Earth's rotation and due to its flattening. But that spectrum would be much simplified at the South Pole. In technical language, the spectral lines become split into multiplets, and that splitting is due to the rotation and the flattening, also some due to the inhomogeneities in the Earth.

But at the Pole you're on the axis of rotation, you're on the axis of symmetry, so all the splitting effects are minimized, and you have the pure spectrum of the Earth. And so the proposal was made that we could use the very sensitive instruments at the South Pole to measure the resonance spectrum, the vibration spectrum of the Earth, when the next very, very great earthquake would occur. During the lifetime of the experiment, that next great earthquake didn't occur. We waited for years and years and years, and it just didn't happen, and finally the NSF said, "We're cutting off your funding," and that was it. Then, a few years later, in fact, that earthquake did occur, but we missed it. Somebody else got it.

Let me tell you a little bit more, if I may, about the history of some of the Institute activities.

VAN BENSCHOTEN: Okay.

KNOPOFF: In the later fifties, Gordon MacDonald and I especially felt the need to go out and have contact with students. The Institute was set up as a research organization, and we were, therefore, without contact, formal contact with students and, in fact, recruiting students to come and do doctoral research in the laboratories was difficult because the students didn't know about us. They were off in their own departments taking courses and being exposed to the faculty of the departments, and it would be natural for a student to say, "Well, I really enjoyed the material in that particular course. I hear about the research of that faculty member. I'll do my thesis work under that faculty member," and they didn't know about the Institute.

So Gordon and I wanted to establish contact, and the Physics Department very generously allowed us to begin teaching courses in the Physics Department. He and I taught a course in statistical mechanics, a graduate course. I had been teaching undergraduates in the Physics Department for a few years, and I had been appointed to the faculty of the Institute in '57.

By '60, I don't know the politics that went on behind it—I've never known—but just about the time that I was leaving for my leave in England, I was appointed professor jointly in the Department of Physics and the Institute of Geophysics. I was the only one in the Institute that had that joint appointment at that time.

In the mid-sixties, our Institute became the leading organization on the campus that was focusing on space research, and space research was a very, very active area of research all over the country, both in the U.S. and Soviet Union, and our Institute was the place where space research was being developed. And we attracted a large number of graduate students. I was not involved in that, in their training, because I was in solid earth.

But here were a large number of graduate students who had no department. We developed an interdepartmental program, but contract resources were being used—I probably shouldn't say this but somebody had to take care of the academic needs of these students. The program became so large, I don't know how many. My image is of the order of fifty, but it must have been less, graduate students, which is very large, so that a Department of Geophysics was formed, an academic department.

To staff that department, you needed academic faculty. Well, the academic faculty came from the Institute of Geophysics, so FTE were transferred from the Institute to UCLA to the Department, and now a number of the faculty had joint appointments with the teaching Department of Geophysics and a research Institute of Geophysics.

Within one or two years after the Department was formed, maybe longer, it underwent fusion with the Department of Geology and formed the Department of Earth and Space Sciences, which exists to this day. At that point, the Institute had most of its faculty, except for two, Kennedy and Griggs, as joint appointees with teaching departments.

Then the appointment process reversed after the death of Kennedy and death of Griggs, the death of both of them. The Institute only has joint appointments now. There are no appointments wholly within the Institute. That has some advantages, among other things, because it takes the available resources of the Institute and effectively doubles the number of people who fill the same number of slots. Because half the salaries are being paid by the academic departments, people fulfill their responsibilities to those departments, and half of the salaries come from the Institute's own resources, which are free of the teaching responsibilities because they are supposed to focus on the research contributions. So now it is a complete reversal of the way the Institute was organized in those days, and I think I was the first one to take that route.

In '62, I was very heavily recruited by Caltech. Frank Press was the director at the Seismological Laboratory. He was and is a very, very good friend. He and the chair of the Division of Geological Sciences at Caltech pressured me extraordinarily heavily to leave UCLA and go to Caltech, and I must say it was very attractive. To go to a place of such international scientific reputation and a place where I had been a student and looked up in awe at the faculty, to be invited to go there, that was some sort of dream, and I couldn't resist it.

I'd wanted to do it in an exploratory way, but at the same time, I didn't want to leave UCLA, and part of it was because of my love of Louis Slichter, a man who had done so much for me that I couldn't leave him. I couldn't leave him. And I must say that the Caltech effort was high pressure. Louis was low pressure, as he always was, and it was clear that I would be hurting Louis. So what I did was I took a leave of absence from UCLA. Caltech didn't like that. They wanted me to resign at UCLA. But I didn't take a visiting professorship at Caltech; I took a regular ladder faculty position.

I spent a year at Caltech and I made my decision. I preferred UCLA after having explored the Caltech environment.

Caltech, for me, was too narrowly focused. It was science, science, science. And what made UCLA attractive for me, we had good science here, maybe not as famous physics or as famous chemistry. Our geophysics had developed in a different way, but it was really terrific. But the fact that we had a music department here, we had a research library, we could browse, I could go to the research library and browse to my heart's content and read all sorts of— Pick up a book and just read in areas that were completely unknown to me. To hear lectures or the weekly noon organ concerts in Royce Hall, to hear lectures on subjects far, far from science, that was exciting. That was exciting. So I left Caltech.

VAN BENSCHOTEN: I'm sorry. Just leave it here for a sec.

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August 27, 2003

VAN BENSCHOTEN: This is tape six, side A.

We were talking about Caltech, after making your decision not to stay there.

KNOPOFF: Well, one of the disadvantages at Caltech was that the Seismological Laboratory was a good mile or more west of the campus. The Seismological Laboratory was in the hills just above the Rose Bowl, and the Caltech campus was east of the arroyo a good mile, and that meant that you were either doing your research in the Seismological Laboratory, with the students at the Seismo Lab, or you were teaching down on the main campus, and it wasn't the same thing as having the whole operation in one place. It didn't serve the right educational purpose. The environment at the Seismo Lab was wonderful. It was just wonderful.

VAN BENSCHOTEN: So you returned to UCLA.

KNOPOFF: And later they sold the Laboratory building. It was a wonderful mansion, and the Seismo Lab moved back to the Caltech campus itself, but many years later.

But I had to hurt somebody. The people at Caltech were very saddened. I left and returned to UCLA. I've been here ever since except for sabbatical leaves.

Another ingredient in the whole process— Can I make another—

VAN BENSCHOTEN: Sure, go ahead.

KNOPOFF: Along about '57, George Kennedy came down here to this office from his office on the second floor, and George was, in his personal life, a great collector of species orchids and a great collector of pre-Columbian pottery. He had been exposed to a method that was in practice in the oil company laboratories, and other places as well, of the dating of rocks by the method of thermoluminescence, and he had the idea that we could apply this to the dating of ancient pottery, which is on a much shorter time scale than ancient rocks, obviously.

He asked if I couldn't develop the instrument to do it. So I played around with it, and, in fact, did it. We were quite successful. And he found a collection, a suite of pottery fragments—

[interruption]

VAN BENSCHOTEN: You were talking about thermoluminescence and coming up with that.

KNOPOFF: He had found a suite of potsherds from the Agora, in Athens, and these had been dated stylistically, and our dates and the stylistic dates matched wonderfully. I gave two lectures on this in Chicago. We got ourselves identified and on the books as having done this. And I went off to my sabbatical, or my leave, in England [in 1960], and I gave a lecture on thermoluminescence at Oxford. In the audience, [M.J.] Aitken at Oxford heard my talk, and that was the reason he developed his world-famous thermoluminescence laboratory at Oxford.

In the audience was a man by the name of John Marco Allegro, who was a reader in Semitic languages, I think at Manchester [University], and he asked if I would want to go to the Dead Sea on an archeological expedition. I was unmarried, and I agreed. I said I knew nothing about archaeology, and he implied that that was not a serious issue. And there were six of us: two Americans and three British and one Belgian.

Allegro was quite a promoter. He promoted tickets for us on Middle East Airlines. He promoted the money to organize the expedition by selling the rights to the London *Sunday Times* and the BBC [British Broadcasting Corporation] so they could send their correspondents to join us. And he promoted dehydrated foods from Bachelor's Foods, who was a manufacturer.

When we arrived on the Jordanian side of Jerusalem, which was then a divided city, we traveled to— We found and hired the men who had done the excavations at Qumran, where the Dead Sea scrolls had been found, and they were from Bethlehem, and they were the diggers on our site at the shore of the Dead Sea. And I have many tales of that, but I won't disturb you with those anecdotes.

But as it turns out, Joanne and I were to be married in early February of 1961. I went across the border from Jordan into Israel, circuitously, but I did it. I gave my lecture on thermoluminescence in Jerusalem, to one of the largest audiences I've ever seen, because everyone in Jerusalem, or Israel, probably, for that matter, is a weekend archaeologist. So the crowd overflowed the auditorium. I got samples from [Yigael] Yadin, who had by then discovered Ein Gedi, but had not yet discovered Masada, and these were samples of cloth and pottery, which I brought back to UCLA for dating, cloth for radio-carbon dating.

I started feeling ill in Israel, and by the time I got back to England, it was discovered that I had infectious hepatitis, which I'd clearly acquired in Jordan. It's an archaeologist's disease, drinking bad water and eating tainted food. And I was in the hospital, and my bride-to-be was telephoned that I couldn't make the wedding. I missed the wedding date, came home to recuperate, and then the two of us returned in April, after the real wedding, back to England.

I have mentioned that I had not been publishing much in the fifties. In fact, I was in a state of some depression. Not in a clinical sense, but I was depressed about where my scientific future was going with all this, and I started to flirt with perhaps going into music as a profession, and I made contact with Boris Kremenliev on the music faculty here, who had been recommended to me by my piano-playing partner at Miami. We talked a little bit about that, and I even took some courses in music here, some orchestration and other courses. But then my research blossomed and I put that in the background, but my contact with the Music Department continued. I even taught some courses in musical acoustics to music students here.

In 1961, while I was still recovering, Bill [William] Hutchinson and Mantle Hood—that was my first contact with Bill Hutchinson—who were in the Music Department, came over to my house where I was convalescing, and we started talking. They were very interested in my joining an Institute of Ethnomusicology, which was forming on the campus at the time. And I might say that that was also an instrumental ingredient in my returning to UCLA from Caltech. I joined this wonderful Institute of Ethnomusicology headed by Mantle Hood, and it had Bill Hutchinson and Charlie

Seeger [Charles Louis Seeger, Jr.] and myself in it as the senior faculty and with these graduate students.

I saw my role in that activity as trying to identify universals of thought of musical organization from all the contributions that the students were making, each of them expert in the music of a different culture of the world, and I was trying to understand how this could be synthesized in some way, because all human beings have essentially the same thought processes. They think about the influence of music in their daily activities, and music develops in us all in a similar way, and musical activity develops in similar ways. It's basically an abstraction. It's not needed directly for the basic elements of human survival. Indirectly, of course, it is.

I don't think I understood the problems as well then as I do now, and it was only a few years ago that I began to ask "Do earthquake prediction and music have any relationship?", and I decided they have a very close relationship. Earthquake prediction is the search for pattern. The search for pattern takes place in an environment where nature does its very, very best to hide the existence of pattern. It's still not clear whether these patterns exist. I spend a lot of my time trying to find them.

On the other hand, in the musical area, the patterns are obvious, but we don't know what organizes, what is the cause of the pattern. We don't know what the basic building blocks are and how you put them together to form a musical pattern that says this is Brahms and this is Japanese. Yet the basic elements are similar.

So I see music and earthquakes and a large number of other areas, including economics, which I've dabbled in but not seriously, as areas of what are called "complex systems" these days, the problems of building up large-scale structures from small-scale building blocks. How do you assemble these building blocks under certain local rules, rules of assembly that determine how one object attaches to another object at short range? Assemble them under this set of rules—you'll get music. Assemble them in this set of rules—you'll get earthquake patterns. Assemble them in this set of rules—you'll get frogs' noises or whatever.

So this is a— I think I've at last rationalized in my own mind that I think I can come to grips with two disparate areas of my very deep involvement and make them seem as though they are part of the same problem.

VAN BENSCHOTEN: So the hidden stitch is complex systems.

KNOPOFF: That's the overlying umbrella. That's really the broad area.

VAN BENSCHOTEN: You anticipated a question that was going to come a little bit later on, but that's fine.

I want to double back, if I could just briefly, to the research you do in the structure of the upper mantle. I think I know the answer to some of this, but what were the consequences and applications of geological inversion on your work here?

KNOPOFF: I'm not sure I understand. Consequences on my work?

VAN BENSCHOTEN: The consequences just generally on the field and applications of it as well.

KNOPOFF: Well, I think that this work led at the time to— Was a contribution to an effort to try to understand the processes of mountain-building. The model of plate tectonics emerged a few years after we began this work. We began in the late sixties.

VAN BENSCHOTEN: So it helped to confirm.

KNOPOFF: And the model of plate tectonics at the time was a description of the relative motions of the various plates that cover the surface of the Earth, but an effort to define the dynamics, to define the driving processes that cause these plates to move would require an understanding of where and how much the inhomogeneities are, where the thermal differences are in the Earth, and so I think we were contributing to that.

Can I change the subject here? Because I think while we're on this, you wanted to talk a little bit about my contributions to the discipline and to international scientific cooperation. In the late fifties, at a time of great international crisis, well, *crisis* isn't the right word, certainly during the time of the Cold War, the International Geophysical Year took place. It was a remarkable event. It took place largely in an attempt to break down international barriers through science, through geophysics, and it was not just a matter of everybody in each different country doing a piece of research, but it involved people internationally getting together and discussing and explaining and proposing and arguing about science. It was a way to get the Russians out of their shell. It was still in the times of Stalin, don't forget, in the late fifties.

The International Geophysical Year came and went. By '63, [Nikita] Khrushchev was now in the Kremlin. Stalin had died, and [Vladimir V.] Beloussov from Russia proposed an International Upper Mantle Project, proposed it at the international assembly of the IUGG, International Union of Geodesy and Geophysics, at Berkeley in '63. The idea was that, as it turned out, Beloussov turned out to be the president and the conduct of the Upper Mantle Project, the day-to-day and long-term organization was going to be the— Was proposed to be the responsibility of the United States.

Merle Tuve was the chair of the Geophysics Research Board of the National Academy of Sciences, wanted very much to have the U.S. take on the responsibility, a big responsibility, in the Upper Mantle Project, and he turned to me and asked me to become the Secretary-General of the International Upper Mantle Project.

I replied that I wasn't very good at— I was never a very good international diplomat. I was much less good at organizing international programs. I'd never had any experience in organizing national programs. I was just a research scientist and worked on a much smaller than this celestial scale of international organization. I said I would do it, but that Merle would have to give me an administrative assistant of the highest quality who would actually do the work. In other words, I was content to lend my name to it. I would do a lot of the work, but I could not attend to the day-to-day details of a major, major international project.

He assigned from his staff a wonderful, wonderful person whose name is Pembroke J. Hart, and "Pem" Hart was terrific. He had his Ph.D. in geophysics from Harvard and was an administrator in Washington [D.C.] on Merle Tuve's staff, not doing active geophysical research himself. He was implementing geophysics, planning programs, making contacts, establishing liaisons within the U.S., and he became the organizer. He was the Secretary-General in fact of the International Upper Mantle Project.

I accepted, began in ’63, and it was extraordinarily successful. I think it was successful because of Pem Hart. He traveled all over the world making sure that national programs were in line. He got the U.S. to— We even got a message from President Lyndon Johnson giving his blessing, but no money, to the project, but we did have a special allocation within the National Science Foundation for additional research on the Upper Mantle Project within the United States. We took a leading role and it was a great success. It finished in ’71.

VAN BENSCHOTEN: I had a question.

KNOPOFF: Okay.

VAN BENSCHOTEN: This is your first experience with an international group of nations and scientists as well.

KNOPOFF: Yes.

VAN BENSCHOTEN: What do you think—

KNOPOFF: Well, I had made presentations at international meetings, but I had never done anything in an organizational sense. It was not, never has been, my style. I function better in the environment of this room, rather than function on a scale of a radius of 6,400 kilometers, which is the radius of the Earth.

VAN BENSCHOTEN: In looking at your résumé, in 1975 you’re on a panel to review U.S. and Soviet scientific exchanges for the U.S. National Academy of

Sciences as well, and it just brought to my mind—this is a question maybe that ties in here, that's good here—is what are the strengths and weaknesses of international scientific inquiry and exchanges? Are they overall? Is this the trend for the future? Are they absolutely necessary now in order to do good science?

KNOPOFF: No. I think at the time when there was great encapsulation of the countries of the West and the countries of the East, there were these very strong barriers, these were really valuable means for getting communication going between East and West. It was extraordinary. And I think some of the fallout of the Upper Mantle Project and its successors was to allow a flux of people back and forth. We started getting Russian scientists coming here to this country. Nowadays, these barriers are down; they don't exist anymore, and they've been down for some time. And I think that travel to international meetings is straightforward enough.

There's a lot of international participation in laboratories, and I don't think that heavily organized international programs at this level have any function any longer. You may still find this, I'm sure, in the medical areas, especially where large numbers of disease-affected areas take place in underdeveloped countries that don't have the resources to attack the problems medically. But the resources are in the developed nations, and so international cooperation there becomes very, very valuable because then it involves many nations coming in through international organizations frequently. There are international health organizations that have the ability to produce some sort of communication.

But I think in the physical sciences, certainly in the solid earth physical sciences, and probably physics and chemistry as well, that's probably less developed. I could imagine that in space physics there is still a large amount of international cooperation.

VAN BENSCHOTEN: With this international project that you just described to study the upper mantle, was the political component of it as explicitly stated as it seems to be? I mean to sort of bring these two super powers together in some way?

KNOPOFF: Well, it wasn't just the super powers; all the countries of the world were invited, too. We had a large number of countries participating in the program, and I think it was an opportunity to develop conversation among scientists using the science as a platform, from a large number of countries.

We had annual meetings, and we were divided into a large number of subdisciplines. People entered into it with a large amount of enthusiasm. It was good. Certainly the political motivation was there. I don't think it was overtly for that reason. But everybody wanted to talk to people from other countries. Not everybody, no. There were some who said, "No, I can do my own thing in my own laboratory, and I don't need to." There were some.

VAN BENSCHOTEN: All right. Did you want to— I think that's more or—

KNOPOFF: I wanted to move on from there. By '72, there had been one of the occasional—and it's supposed to be every five years; they never are—reviews of the Institute of Geophysics. The Institute had developed by then into an institute with three branches: [University of California,] Riverside, [University of California,] San

Diego, and [University of California,] Los Angeles. Bill [Willard F.] Libby was the director of the Institute here at UCLA and he was also the statewide director. He held both positions.

The opinion of the review committee was that this was too big a load, and that the job should be split into two parts, and so Bill Libby took on the job of the statewide director, and I was asked if I would not become the director for the UCLA campus. It was called the Associate Director. So for a while, there were associate directors. The adjective has now been dropped. The director on this campus is the Director for the campus.

I accepted, so I then began a new career in administration, which I did for fourteen years, from '72 to '86, and this was a different kind of activity. I took it, as I did the Upper Mantle Project responsibilities, with the understanding that it would not impact on my own research. I didn't have the advantage of a Pem Hart at this Institute, but I did have the advantage of a wonderful nonacademic staff in the persons of, first, Steve [Steven] Lawrence and then Keith Olwin, our laboratory business management officers. Keith's official title is Management Service Officer. He's just retired. And this was very, very important.

The purpose of the job of the Director is twofold. One is to define new programs within the Institute and find the resources and fill the resources with new professorial faculty; and, second, to make sure that the faculty in-house are happy and content so they can be concerned with their normal advancement and the usual contentious issues of space, which are a departmental and institute resource that very frequently are very, very contentious issues.

I think that my— I brought in a lot of new people to the Institute. Bill Libby's great contribution was the development of space physics, and he's responsible for getting the money for Slichter Hall. That money was all NASA [National Aeronautics and Space Administration Agency] money, and that had a wonderful impact on the rest of us, whether space physicists or not, because when we wrote a proposal to Washington for research to whatever organization, NSF, NASA, whatever, our overhead rate was less than for the rest of the campus because we were not paying off the mortgage on the building. The building had been paid for with government funds, but it had not been built with state funds.

So we had a lower overhead rate, which is a very significant ingredient in getting people to put their research grant proposals through the Institute rather than through a department, which has its own research activity, and everyone had a joint appointment. So there would be no perceptible advantage had the overhead rates been equal. That has just disappeared this year. After all the years that the building has been in existence, I think they've finally decided that the overhead rate should be equalized. So that was very valuable, and Bill was very, very vigorous in doing it.

VAN BENSCHOTEN: As fourteen years' director of the Institute, what do you feel are your most important achievements in that time?

KNOPOFF: Well, I'd say there were— I made— "I made." The Institute made, but I was instrumental in making two outstanding appointments. First was that I thought that the Institute should move in the direction of understanding the origin and evolution of life on the planet, and I recruited Bill [J. William] Schopf from the Department of Earth and Space Sciences and gave him a half appointment in the Institute of Geophysics, and that has been a wonderful, wonderful addition to the Institute.

I never felt that the Institute should be constrained to the areas of its original charter to study the structure of the solid earth and earthquakes, and to study the atmosphere.

[interruption]

VAN BENSCHOTEN: All right, we're back.

KNOPOFF: But I've always thought that geophysics—and I think Louis felt the same way—that geophysics should fill the interstitial academic space between or among the mainstream departments. So somewhere, if you imagine that here is physics and here is chemistry and here is mathematics and here is geology, that the Institute sits somewhere in the space amid these environments, prepared to undertake research into areas that are not mainstream to the departments. The departments have a responsibility to expose their students to what are really mainstream activities. These mainstream activities change over the decades.

But someone should be attacking—that's the Institute—should be attacking areas that could provide overlap, or disciplines that couldn't be attacked by the mainstream departments or that they were unwilling to, and once these become themselves sufficiently vigorous that they become mainstream, then the Institute should possibly leave these areas to the relevant departments, to change the boundaries and to define new areas.

So I thought— I recognized in my own mind that there were at least two areas that I could think of that I would like the Institute to move in, and one was the question of the relationship of life on the planet to the environment of the planet. What makes the evolutionary history of the planet so wonderful that life could appear on it? The basic outline is well known. Yes, we've got water, and the temperatures are just about right, and all the rest of that stuff, but, nevertheless, what allows one building block to be assembled in a complex system—my favorite "complex systems"—into another building block to make it a complex organism? And what was the transition from simple organisms to complex organism? Bill Schopf was, and continues to be, the natural person in that area.

A second area that I wanted to move toward was an interface between the Earth and the stars. We know that there is a solar wind. The Sun is ejecting protons that stream down and hit the Earth, and they affect the Earth in many vital ways. In an entertaining way, they cause the auroras. More vitally, they have a strong influence on climate.

The space physicists have studied well the organization of the instabilities in the space environment of the Earth. Charlie [Charles] Kennel, who was in the Physics Department, had an interest in developing relativistic astrophysics, relativistic plasma astrophysics. He was an expert on plasmas, which are a basic constituent of our spatial environment, charged particles in space, and he wanted to study what would happen if these charged particles began to move at relativistic speeds, and in particular he studied the explosion of the supernova of the Crab Nebula.

He was very, very good. He was very good because he has a talent for communication that is unsurpassed. He can take very complicated physics and make you think you are not just listening to something wonderful, you are a participant in the activity. He's that good. Regrettably, we've lost Charlie. He's moved on, and he's become the director of the Scripps Institution of Oceanography, to my great sadness. But he's a wonderful organizer on his own. I think he would have been my choice for the director of the Institute after I retired.

But I think these were the two principal areas. We also— I also began to establish a liaison with the Department of Atmospheric Sciences, and we brought in Michael Ghil, who later became the director of the Institute of Geophysics, because Michael has become interested, is working in the field of climate dynamics, which has become a very, very hot topic. I can't say that it was foreseen at the time that I did all that. That is a later development. I wasn't involved in that. And I think these are the three principal areas, and now we have a new institute activity in the field of astrobiology, and this continues the tradition of exploration of new areas, and they're still out there. There's still a lot of exciting things stuff going on out there.

VAN BENSCHOTEN: Okay.

[End of August 27, 2003 interview]

TAPE NUMBER: VII, SIDE ONE

August 29, 2003

VAN BENSCHOTEN: Tape seven, side A. Today is the 29th of August.

I had a few follow-up questions I wanted to ask. You had mentioned at several points in the interview your election to the National Academy of Sciences, and I was wondering what did this mean to you to be voted into this organization.

KNOPOFF: Oh, it was a great thrill. At the time, I never thought that I was worthy to be part of such a famous and remote organization, and it was very exciting. I was elected in '63 as part of a youth movement in the Geophysics Section, and there were three of us selected almost in succeeding years in our thirties. We were each in our own thirties, but it has never since been duplicated, because the Academy tends to be an organization of old fuddy-duddies, trying to elect people who have made significant contributions over their entire career, and so to be elected in one's thirties was a great, great thrill and honor. I didn't think I had really done enough at the time, but I was very complimented that my senior colleagues thought so.

Another exciting ingredient of that election was that my— I think my first formal meeting of the Academy and under Academy auspices was not an annual meeting, but it was an extraordinary meeting called to commemorate the one-hundredth anniversary of the Academy. The Academy had been organized in 1863 with a charter by President [Abraham] Lincoln, and in October, I think, of '63, they convened a special convocation of the Academy to have a centennial commemoration, and President John [F.] Kennedy addressed the group, and that was only a month before he was assassinated. So it was both a thrill to be in the presence of the President and to have, essentially, an address before a smaller group, some hundreds of people, but also before that great tragedy that came so soon after. It was very exciting.

VAN BENSCHOTEN: Has the Academy changed much since ’63?

KNOPOFF: Oh, it’s become much larger, much larger. In those days, they elected thirty-five people across the country each year. Now the number has been sixty for a number of years. I’m sorry; has been sixty people per year across the country, across all fields, and temporarily they occasionally increase it to seventy-five people when they want to include specialty groups of science or encourage minorities or that sort of thing. So I don’t think the young people are represented as a large enough minority yet, and the Academy still retains its elderly demographic.

But the annual meetings are extremely crowded, and the Academy, when it was smaller, lent itself to much personal contact across disciplines. Now it’s very much more difficult to do in the meetings.

VAN BENSCHOTEN: We had talked, or you had talked, rather, about the Institute, and you gave, I thought, a really good history of it and its aims and goals, the people who were a part of the Institute. But we didn’t get to challenges, I guess, the future. What challenges do you think face the Institute over the next couple of years, decades even?

KNOPOFF: Well, we faced an immediate challenge in the last year, and that's the extraordinary reduction in the budget of the Institute, and it has suffered more than departments have suffered, because it's a research organization, and research was particularly singled out for budgetary cuts. It took a 10 percent cut last year, and it's taken a 10 percent cut this year, and that's a large, a very significant sum.

The Institute has to— I don't think the goals are any different than they were in the early years. I think the Institute has to recognize areas that are forefront areas. They have to bring outstanding people to UCLA to work in these interdisciplinary areas and to persuade the departments that these are important subjects that have great promise for the future, I think. So it requires an Institute with imagination.

And I must say that administrations, which are— The people in the administration don't necessarily see that this is the most enterprising thing to do with their resources, since they have to support a lot of mainstream work, and so a large part of the task of the Institute is to persuade the administration of the virtue of the Institute and certainly its past record and the promise that it will continue to develop with such a record. And to succeed in that, it has to do as Louis Slichter always thought, and that is to staff with people with absolutely superlative credentials. I think that that's the only way that ensures the Institute's survival and ensures its visibility.

I think the Institute is, as Louis said often, it is an ornament to UCLA, and I think it is— And that's not meant in a derogatory sense. It's a genuine singular organization that brings attention to UCLA, perhaps as much as its athletic teams do.

VAN BENSCHOTEN: Right.

[interruption]

VAN BENSCHOTEN: All right. We're back. I wanted to ask about in our discussion yesterday, or, rather, on Wednesday, and in earlier sessions, Frank Press's name has come up several times in the interview, and he's a mover and shaker, so to speak, in the field of seismology and other fields. He was [President James E.] Carter's Science Advisor. I was wondering if you could maybe just give us a brief profile of Frank Press, from the knowledge that you have of him and his career and life.

KNOPOFF: Well, Frank and I are the same age. Frank is extraordinarily creative as a geophysicist and as a seismologist and thinks deeply about a lot of things. He's also a superb administrator and organizer of people. He was recognized early on to be the director of the Seismological Laboratory at Caltech, and when I went to Caltech for that one year plus a follow-on year to wind up my work, it was an amazing thing to work together with him. He's very generous with his time.

When we installed the seismographs around the Alps, he was there to teach me how to do it. He had been the inventor of the long-period seismograph, the instrument that we used and was later used in the World-Wide Standard Seismographic Network, co-inventor. Later, in the seventies, my family and I went on a sabbatical leave to Venice in Italy, and Frank and [his wife] Billie came and joined us for a brief time, and we worked together in the laboratory on the Grand Canal.

He has direction. He knows how to make decisions, and the decisions are almost always right. I can't imagine a case in which he's made bad decisions. He knows how to make decisions, how to make choices, and he follows up. He never dilly-dallies. From the directorship of the Seismo Lab, he went up to the chairmanship of the Department of Earth and Planetary Sciences at MIT. From there he went to become the Science Advisor to President Carter, as you've mentioned.

He was the first of the triumvirs in the youth movement in the Academy, of which I was the last, in the Geophysics Section, and after a year or so after he left the Carter administration and went back to MIT, he was the President of the National Academy of Sciences, which he was for the usual two terms, which I think goes for twelve years all told, and he's now six, or whatever it is, eight years past that.

He's an extraordinarily creative, incisive, direct, purposeful, decision-making, able leader, and yet has a charm and an ability to work with people that doesn't make him seem isolated and austere. So he's a good guy to work with.

VAN BENSCHOTEN: You mentioned his creativity. My next question has to deal with creativity. When you were talking about the dating method that you helped devise, your pioneering work in geological inversion also, I was interested, where did the ideas come from? Where do your ideas come from, in these and in other projects that you've handled?

KNOPOFF: I think most of the ideas are my own, and I can't tell you how they pop into my mind. I think I'm extremely versatile in terms of thinking of ideas. I've got, always have had, more ideas than I have ever had time to work them out.

In the case of thermoluminescence and the dating of ancient pottery, that was not my idea. That was George Kennedy's idea. But the other areas, I think that they're— I can't tell you how this works. It's not an organizable, codifiable process, but it's a train of thought. You're exposed to one idea, and you work on something for a bit, and then all of a sudden, three more ideas, or whatever number, will pop into your head while you're still working on the first one, and you have to make notes on a scrap of paper to write them down and remember what they are.

But it's something that has developed more and more strongly and more and more vividly as I've gotten older. I think that in my twenties, certainly, and in my early thirties, I was too immature scientifically to be able to identify good problems and important ones to work on, but as I've gotten older, I think my sense of criticism, self-criticism, and analysis has matured and has developed strongly.

You asked a bit earlier who my important teachers were. I'd like to mention two whose names I've mentioned, but not in the context of teaching, and that didn't happen until I came to the institute in 1950. One was Louis [B.] Slichter and the other was Dave [David T.] Griggs.

Louis Slichter taught me one important thing, among many things that he taught me, but he taught me to write. The essence of science, I think—well, one of the important ingredients of science—is communication. If you discover something or if you work on something, you've got to be able to tell somebody else about it, and that is done through the printed word with published papers.

And Louis taught me how to write the English language. I think I have a skill in that that is derived directly from him. Louis kept his hands off of me and let me develop in my own way my first important paper, which I called *Diffraction of Elastic*

Waves [Knopoff, L., Diffraction of elastic waves, *J. Acoust. Soc. Amer*., 29, 217-229, 1956.]. It was an idea that was my own. We've talked about my thought about developing seismic wave theory along the directions of physical optics theory, electromagnetic theory, and I did that. That was my idea. But how to put the words down, that was Louis' strong influence.

The other strong influence was Dave Griggs, and Dave taught me to be critical at first and self-critical more significantly afterward. In my early years, when we shared adjoining laboratories in that dust-filled shack on Site 1, I would get a terrific idea, that I thought was a terrific idea, and I would work it out and I would run next door to Dave, and he was very patient and listened to me. I'd explain this on the blackboard, and Dave immediately saw where I'd overlooked something or had made a misstep or something, and he would jump in immediately and pinpoint that, and I would recognize it for what it was worth. I wouldn't stop to defend it, and I would sort of slink back to my room next door and work at it again. I'd patch things up and go back to Dave, and the process was repeated a number of times until finally after some iterations, Dave would say, "Now, that's pretty good. You ought to publish that."

Dave taught me to be critical of myself. After a while, I could finally do that myself to my own product. That's led to a lot of delay in publication of ideas. I haven't wanted to go out and publish immature loophole-filled ideas. And sometimes to my detriment. Sometimes I've been scooped, but that's okay. There are other ideas out there.

And it's something I've tried to give to my students as well. First, when we identify a problem, we first try to see what literature there is out there, and the student comes typically with an attitude, "Ah, it's been published. That means it's been refereed, and it must be good." And I point out that more than 90 percent of the stuff that's in the journals is not worth the paper it's printed on, and they're amazed. And it takes a while, it takes a long time, for them to overcome the barrier between what they perceive to be authority and authority as the personification of wisdom and genuineness of reality, of correctness, and it takes a while for me to try to break that obstacle down.

After a while, it's more personal. I want them to start to try to find the loopholes in my ideas. After, when the process is finished, what I want them to do is to be able to apply a process of self-criticism. I want them to do it to themselves, and when that happens, our positions are reversed; they are the teacher, I am the student. Then, unfortunately, they usually leave UCLA and they go off somewhere else, and that was a brief moment where I sat in their shadow. But it's fun.

I think the essence of the— I don't think one can teach geophysics. I don't think one can teach anything. Everyone has to be self-taught, in my opinion. But you can show people how to develop a product of which they themselves will be proud, confident that it has some value, in their own opinion. Creativity is a different issue. Some of these students aren't creative; they just don't have the talent. But anyway, creativity, I think, is a product of maturity rather than something that goes on in these early years when a student is in his or her late twenties or early thirties and working on an advanced degree. I think they're still young, in general. There are exceptions, of course.

So you've asked about creativity. I don't think it's something that I can identify. It's a non-quantifiable thing.

VAN BENSCHOTEN: Do you think it can be cultivated, though? Do you think it can be?

KNOPOFF: Oh, yes, oh, yes, and a great university is the place in which it can be cultivated, because you run into so many people, you hear so many ideas, and I hear so many seminars. Now at my advanced age, I can go up there into that seminar, I can tell that, "Oh, that's nonsense what this person is saying. I know how to do that better." And sometimes I'll tell them, and sometimes I'll come back and make a little note of it in my notebook. I don't keep a notebook. I've never kept a notebook.

VAN BENSCHOTEN: Really?

KNOPOFF: I'm quite disorganized, as you can see from this room. Things are done on bits of paper and then buried under the other bits of paper.

VAN BENSCHOTEN: So do you keep it mostly, then, in your head?

KNOPOFF: Yes, and it's a very disorganized head, I'm afraid.

VAN BENSCHOTEN: I wanted to get back to a point you made about Slichter, how he was a person who helped you to write. What was his method? How would you describe Slichter's method?

KNOPOFF: I think mild criticism. He was an extraordinary— It wasn't that I was misusing the English language, but he taught me how to organize material and to put it in logical form, and it was again a process of criticism. His criticism was much more gentle than Griggs's, in this other style. Griggs was quite direct.

VAN BENSCHOTEN: One of the very first things that we talked about that you mentioned, I think, in this interview was the relationship between language, words, human language, and mathematics. I was wondering could you maybe expand upon that a little bit. You said that human language was much more versatile, if I understood you correctly.

KNOPOFF: Well, there are some people who want to look at the problems of analysis of complex systems. You've asked before, was there a connection between earthquakes and music in my own mind, and I think I have been able to rationalize in my own mind that there is a connection and that they are both parts of a more general area of study that's quite visible these days, called complex systems, studies of complex systems. How does one assemble small building blocks into large-scale structures that have pattern? In the case of earthquakes, nature does an extraordinarily good job of hiding patterns, you have a lot of small earthquakes, and the small earthquakes seem to be happening at more or less random times, and if there's a pattern, it's very difficult to identify, so we spend a large amount of time trying to identify that pattern.

I've taken the point of view in that work that if one understands what an earthquake is, whether it be a small earthquake or a large earthquake, that means, translated into more technical terms, one understands the physics of an earthquake.

What is a rupture? Why should one earthquake influence the occurrence of another earthquake in order that it form a pattern?

If one earthquake does not influence another earthquake, the time, the location, the size of the next earthquake, then there's no hope for the development of pattern. And pattern is what we need for prediction. We can't do prediction unless there is a pattern. So in the case of earthquakes, we haven't been successful in identifying too much about pattern, and those of us who work on the nature of the earthquake source and the attempt by nature to organize an ensemble of earthquakes into pattern structures, well, that's an exciting subject for the research.

In the case of music or language, we know that there is pattern. You play some music, and you say, "Ah, that is jazz," or "That's oriental," or "That is classical from the nineteenth century." We do this by some intuitive process. We don't quite know what the details of the organization in our head are, but the patterns are there. You read a text, and you say, "Ah, that text is German," or "That text is English," or "That text is Italian," and you do it without knowing the meaning. You do it from the pattern.

You hear some speech, and you say, "Ah, they're speaking French," and you don't have to know what is being said. You do it from the parodies, in some cases from the sound. You know what the comedian does when he imitates French, and you do it, in fact, not from the context, the meaning, but from the sound, from the music of the spoken language.

But what are the rules for the assembly of the same set of symbols, assembling this set of syllables into a structure, and it forms Italian? Assemble the same set of syllables, because there's only a fixed number of syllables that the human speaking apparatus can form, we have to have consonants and we have to have vowels, and there's only a certain number of consonants. Except for the click languages of Africa, basically we use the same sets of symbols, no matter what culture we're in, but we put them together in different ways.

So there the activity is one where we know that the patterns exist, contrary to the problems of earthquakes, where we don't know what the basic building blocks are. If we go down to the letters of the alphabet on the written page, we know what the basic building blocks are, but we don't think in terms of letters when we read or speak. We think in terms of words. But the number of words in the vocabulary is enormous, even in a child's vocabulary. It's enormous for quantitative purposes. And the purpose of doing quantitative analysis is, indeed, that; it's analysis.

The people who do this kind of thing aren't doing it to do synthesis. I don't think they will ever be able to write another [Ludwig van] Beethoven symphony or to compose another Shakespearean sonnet, even though they may do the right kind of statistical analysis on Beethoven or [William] Shakespeare. And the reason is that basically any translation from the real text into sound is fluctuations of air pressure on the ear, which is then translated into a set of nerve impulses that are transmitted to the auditory cortex in the brain and are interpreted. We don't record 256 vibrations per second on middle-C in the brain. We record C-ishness, and it lasts for so long and it's so loud, basic elements of pitch, duration, and loudness of a sound, plus overtone quality, which is extremely difficult to quantify.

But the brain operates on an abstraction of the sound. It doesn't operate on the actual physical ingredients of the sound. The brain operates on an abstraction of the printed page. It doesn't operate on the exposure of all the little photoelectric elements in the retina of the eye as they're exposed to the black and white structures on the printed page.

So there's an abstraction. Because the brain is inaccessible to direct measurement, we do not know what that process of abstraction is. So we try to do something else. We try to apply the most convenient techniques that are available to us from other fields; namely, we know how to count things. So I can count the number of letters of the alphabet that appear on the printed page, how many times As appear and Bs and Cs and Ds. And you'll find that, in fact, there are slight differences between languages, and the lengths of the words are greater in German than in English. Average length of word is about a little under six letters in English, and it's little over six in German. That's not enough to go about doing a synthesis. It's not enough to go about saying, "All right. Now I will count these letters, and now I will program a computer and it will assemble these letters. And if I assemble them with this set of probabilities, out will come German." That doesn't happen.

There are correlations. There are ways you put letters together to form words. There are ways you put words together to form sentences. And one would like to understand how one goes to meaning from quantity. For example, the word *the*, which is the most frequently used word in the English language, is not used at all in Russian. There is no article in Russian or Chinese. The most frequently used word, if you were to program the monkeys and the typewriters to plunk down words according to their frequency of usage in English, you'd get a whole bunch of articles in your output text, because *the* and *a* and *an* and *if* and some prepositions, they all appear the most frequently. And the nouns and the verbs that make the richness of the language are much less frequently used than the little short words.

So we perform analysis. Analysis is a reduction. It's an abstraction of the real thing, and we can never hope to take the abstraction back into a program of synthesis, because the synthetic product will hardly resemble art.

VAN BENSCHOTEN: There's always loss in the translation.

KNOPOFF: There's a loss in a twofold— Well, in the translational process, partly because any abstraction that I make in terms of numerical analysis, which is not the way that the brain does it, any abstraction that I make in terms of analysis, must perforce leave some of these rules out, because they are very subtle rules. They may seem to be unimportant in terms of the more visible prominent rules, but, nevertheless, they are important. So, the problem's tough.

VAN BENSCHOTEN: The problem's tough. [laughs] That's the conclusion.

KNOPOFF: And that makes it fun.

VAN BENSCHOTEN: Yes. All right. That's the end of my follow-up questions. We were going to talk today about the third part of your research, at least the way that we had broken it down before, and that would be earthquake prediction statistics. And

I was wondering, again, as I asked the first time, sort of the transition between the first phase and the second, what was the transition? You've already talked a little bit about this, the paper that you had written, I believe, in 1966 [R. Burridge and L. Knopoff, Model and theoretical seismicity, *Bulletin of the Seismology Society of America* 57:341-71, 1967.]. It was sort of a forerunner then of the work that you were going to take up more vigorously in the mid-seventies. Is that about right? Did I get my years correct?

KNOPOFF: Yes, yes.

VAN BENSCHOTEN: Talk about after that paper in '66. How did you sort of ease yourself into this third phase of research?

KNOPOFF: Well, let me say that it was a natural outgrowth of the first phase, where I was thinking in terms of elasticity systems quantitatively, developing a formalism for the treatment of elastic waves, wave propagation, but there was also an interest in just the way materials deformed. If you want to have an earthquake, and an earthquake is a rupture, if I want to tear this piece of paper, and I find a piece of paper here to tear, if I want to tear this piece of paper, I have to pre-stress it. I have to deform it before the event, and when the deformation that I apply to this material exceeds its strength, then the material will tear. When the material tears, all the forces that I applied before in this case— I shouldn't say "all," but in the case of this material in air, all the forces that I applied before are completely relaxed. Now I can— There is no force left on this torn piece of paper.

And the same is true of the Earth. And so by studying stresses in the Earth, sometimes these stresses travel from place to place as waves. That was the first phase. But sometimes the stresses are just localized and remain fixed in place by other forces, which are preventing them from relaxing, and that, too, could be studied within the framework. It was not unique to me. It had been done for a number of years, number of decades before, and by very great students of the problem.

I wrote a paper in the late fifties that I called “Energy release in earthquakes” [Knopoff, L., Energy release in earthquakes, *Geophys. J. Roy. Astron. Soc.,* 1:44-52, 1958.], and there for the first time I was able to relate the size of the earthquake to the amount of energy that had been stored before the earthquake and released in the earthquake.

It turned out that it had a very unusual result, an unbelievable result, that turned out to be, for me, very important forty years later. And that was that if I took the displacements that had been measured in the 1906 San Francisco earthquake—and there had been surveying measurements of fixed sites before and after the earthquake—and interpreted them in terms of a fracture in the Earth, the depth of that fracture was only about three kilometers. Yet we knew from other information that the depth of that fracture should have been fifteen kilometers, which is the depth of the greatest earthquakes, the depth of almost all the earthquakes in California.

All the earthquakes in California are very shallow, confined to the uppermost fifteen kilometers of the Earth, compared to the Earth’s radius, which is 6,400 kilometers. They take place in a very thin, brittle skin. But we found something even shallower, and that was unbelievable. And that result lay quietly until about five or six years ago, for me, and I returned to that problem, I think, very fruitfully.

Another ingredient was I had a very wonderful postdoc whose name was Bob [Robert] Burridge, and he had the idea of trying to understand the development of a fracture as a dynamic process. Fractures just don't occur in zero time. They have to take place over a period of time. There has to be one place that is weaker than all the other places along an extended fracture, and that has to break first. And then the fracture extends, it grows, as a causal process in which one object, one bond breaks, and then a neighboring bond breaks, and the fracture enlarges.

So even though this piece of paper tore in zero time on the scale of human perception, it doesn't tear in zero time. It tears in some finite time, and in order that the crack grow, stresses have to be transferred from the broken part to the unbroken part in the neighborhood of the edge of the crack. And those stresses are now increased by the dynamics of the crack itself so that the stresses now exceed the breaking strength at the edge of the crack. So that is a dynamic process.

VAN BENSCHOTEN: Let me flip it over real quick.

KNOPOFF: Sure.

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VAN BENSCHOTEN: Side B of tape seven.

KNOPOFF: And Bob had the idea that we should be studying the process of stress transfer, which was a natural outgrowth of our interest in elastic wave propagation. That's what elastic waves are; they're stress waves. And since we couldn't attack this thing quantitatively, except in extremely simplified circumstances—and nothing in nature is smooth or homogenous; you're not smooth, I'm not smooth, certainly I'm not smooth—we wanted to study inhomogeneous fractures, fractures taking place in inhomogeneous environments with variable breaking strengths to try to mimic, perhaps, irregularities in earthquake faults and that sort of thing where you might have stronger and weaker and stronger and weaker patches along earthquake faults. It was decided to do it numerically.

At the time in '67, computing power was so small by comparison with present-day computing power that we just didn't have the capability to attack large-scale problems. So we divided up our cracks into small pieces. We essentially had ten objects, and they would interact with one another in such a way that they would transmit stresses by springs from one moving mass element to another moving mass element, and we tried to apply how the system would be loaded under the motions of the plates. Plate tectonics was just coming in, a little bit later, actually, but we knew enough about the elastic rebound theory derived from the 1906 earthquake that we knew how to load the system. And we solved the problem involving ten masses.

We learned a lot. We published the paper, it was a good paper, and it lay dormant for essentially many years as, I guess, somewhat of a curiosity. I remember we also set up a little small-scale laboratory model of the same process, and when I showed that to a visiting colleague, he said, “Oh, that’s a lovely toy. How do I get one for my grandchildren?” And I was, of course, offended, and I thought, “Can’t you see what this is doing?” And of course he couldn’t see.

I started getting interested in going back to these issues of the interactions of crack formation and the influence on the large-scale organization of earthquakes in the seventies. I had a sabbatical leave, one of my three sabbaticals in Cambridge, in England, and I spent that time learning about nonlinear processes. Fractures are nonlinear. They are irreversible. Linear systems can always— If you stress something, it will return to its original configuration if there’s no dissipation. There are qualifications. If you tear something, it’s Humpty Dumpty, and you can’t put him back together again. And that’s a nonlinear process, an example.

Fracture is nonlinear. It involves large deformations rather than microscopic deformations, and it’s an area of mathematics that is not nearly as well developed as the mathematics of linear systems, which are very well studied, even in the first part of the twentieth century.

In the late eighties, a group at UC Santa Barbara, Jim [James] Langer and Jean Carlson, constructed an earthquake model. A model these days doesn't mean a physical piece of—

VAN BENSCHOTEN: Theoretical model.

KNOPOFF: It was a theoretical model which they implemented on a computer, and they were very excited about it, and they went across the street and said to the geophysicists, "Isn't this an interesting model for earthquakes?"

And they took one look at it and they said, "That's the model that Burridge and Knopoff published in 1967." Of course, the physicists, Langer and Carlson, who are very, very brilliant people, didn't know the paper because it had been published in a seismological journal. And they very generously called their work the Burridge-Knopoff model, and so it's become ever since. And our paper has had a remarkable renaissance, or, as the British say, renaissance [pronunciation: "re-nay'-sahnce"], and it has had an extraordinary visibility.

Freeman Gilbert at UC San Diego gave a talk at which I was present, and he remarked that, in his opinion, that paper, the 1967 paper of ours, was the most heavily cited paper in all of seismology, in seismological history. That may be true, I don't know; I've never counted. It's certainly had hundreds of hits.

I think the model has been misused. And I had literally forgotten about the model. Once Langer and Carlson had published their paper, I immediately went to my students and we said, "We must revive the model," and we did. We did it better than anybody else had been doing. We did it better than Langer and Carlson. We found new things that had to be incorporated in the model to make it good. We now have large computing power.

The model is still restrictive. The models never reproduce. There're still abstractions—abstractions mainly connected with dimensionality. The fact is that I can construct a model representing an earthquake fault as a one-dimensional string of stressed objects. I can do it even in two dimensions. But the real world is a two-dimensional surface of fracture—fractures are two-dimensional surfaces—imbedded in a three-dimensional Earth, and when the slip takes place very rapidly on the earthquake fault, then it radiates out elastic waves—that I studied in my first period—out to a distant seismograph. And so the waves emanating from the earthquake and traveling to a distant seismograph and being recorded there could be studied by the linear theory of my first period.

The actual generation of the waves was the subject of the new work, and here we've spent a lot of time in the last fifteen years. It was slow for the first ten years or so, and it was starting in Cambridge and coming back here, but it moved rapidly in the nineties, and I think it's moving rapidly now. So I think we're having a lot of fun with it.

We've introduced causality into the models; that is, to say that fractures can't travel faster than the fastest velocity of elastic waves. We've introduced some—made some—I think, important contributions to the numerical stability of the programs, which other people haven't done, and now we're in the position of drawing important conclusions relative to the nature of earthquakes themselves.

So I think I know a lot about the phenomenology of earthquakes. We even go out and do some of the phenomenology ourselves, because some of the things that we want to know about how the Earth organizes itself aren't there in the literature, so we go out and do it ourselves.

But basically, we spend— That's only in the service of the process of synthesis. So we start with some presumptions about the basic building blocks. We try and refine them as much as we can. I think one of the foci of our work has been that the physics of earthquakes occurs on many scales, the deformation of the Earth in the large, and on that scale it seems to you and me that the Earth is fairly homogenous, except for an earthquake fault here and there. But as you get closer and closer into an earthquake fault, then you start to see other structures coming in, all the way down to small-scale structures, which are little grains and pebbles and things that would normally be thought at very large scales not to be identifiable.

And one of the questions we've been trying to ask is, "How deeply into the physics must we go?" Is it enough to just be concerned about models of the large scale, on the large scale, without worrying about what goes on on the small scale? And that may be true also in the case of human beings. The nature of social interactions of, let's say, seven million people in a city, doesn't depend on what takes place on the scale of interaction distances of the order of, let's say, the height of a human being, one and a half meters or something like this, or the space between human beings, ten meters on the average, I don't know what it is. Similarly, on that scale, is the fact that a cell in the human body is one micron long, and the way they interact to form the shape of a human being, does this influence the sociology? And most sociologists would say probably in most circumstances no, and most geophysicists would say, “Well, don’t bother me with the details of what goes on on the small scale.” I’m not convinced of that, but that’s an arguable point.

And so a lot of our work has been focused on trying to see the influence between the small scale and the smaller scale. To be truthful, we don’t go down to the smallest scales. And part of the difficulty there has been that computers aren’t large enough. We do most of our work computationally, and incidentally, it is difficult to draw broad conclusions from computer output, and the reason is that it’s very easy to generate computer output, but the computer output is parameter-sensitive. That is, any computer output that you get is for that specific set of parameters, numerical quantities that you insert into the model, and in order to try to develop broad generalities, the results should be more or less independent of the parameters that you stick in.

So you tend to look at the output from a particular numerical experiment, and you say, “Oh, that’s very interesting. Does it lead to generalizations?” And the literature is rather full of presumed generalizations and, in my spirit of hypercriticality, I don’t necessarily buy all of the stuff that’s in these very thick journals these days, because anybody can write a paper and get it published, I think.

To return to the question of linearity, the most obvious statistic in earthquake occurrence is the fact that most of the earthquakes are small earthquakes. [Beno] Gutenberg and [Charles F.] Richter discovered in the forties that there was a relationship, and they defined earthquake magnitude, for which most people nowadays have an intuitive feeling, and they discovered— They, in fact, also did the magnitude scale. They discovered that there were ten times as many Magnitude 3s as Magnitude 4s, ten times as many Magnitude 4s as Magnitude 5s, ten times as many Magnitudes 5s as Magnitude 6s. There's also a correspondence in the size of the earthquake to these magnitudes, and it also follows this power law. The size of the fracture is related to the size of the earthquake. If I pre-stress the material, and I pre-stress it over a dimension of a kilometer, I'm going to release energy over a kilometer. I can store a large amount of energy in a pre-stressed material having a kilometer in dimension. I can store a small amount of energy in a pre-stressed material whose size is the size of a pencil—you know, a few millimeters. So the greater the magnitude of the earthquake, the greater the linear dimension of the fracture, even if the— By linear dimension, let's say the radius or some length measure of this surface.

The size of a Magnitude 5 earthquake or Magnitude 6 earthquake is about ten kilometers, a relatively big distance. The size of a Magnitude 4 earthquake is about one kilometer. The size of a Magnitude 2 earthquake is about a hundred meters. So it drops by a factor of ten in length for every two magnitudes, about. If a seismologist heard me say all this stuff, he'd quibble with me, but that's a rough estimate.

So if I want to model earthquakes, let's say, from Magnitude 2, 100 meters, up to the largest earthquakes in California, and the San Francisco earthquake had a rupture length 450 kilometers long, call it 500 kilometers just for round numbers, 500 kilometers is 500,000 meters. Right? And a Magnitude 2 earthquake is 100 meters.

So we’re talking about a range of 5,000 from the smallest earthquake to the largest earthquake.

So if I imagine that an earthquake fault has on it a population of, at different times, small earthquakes from Magnitude 2s up to Magnitude 8, the big San Francisco earthquake, and I want to model this in three dimensions and assign to each little element in the system, make each element represent 100 meters on my computer, I’ve got to develop a computer whose dimensions are 5,000 by 5,000 by 5,000. That’s enormous.

So my earthquake sizes increase logarithmically by factors of ten for each two magnitudes, but my computer has to increase according to the cube of the linear length, and I don’t have the computing power. I’m not a programmer, I don’t know how to program. That’s where my students teach me. But if I want to model a large earthquake system leading to large earthquakes, I can’t model the small earthquakes at the same time rigorously, so I have to average across the small earthquakes to represent them by a larger clump of things, a larger unit, in order to reduce the demands on my computing power. And therein lies the same problem that we saw in the problem of language. I’m already constructing an average, and I don’t know if I’m doing the right kind of averaging because the processes are nonlinear and they behave in unexpected ways. They *may* behave in unexpected ways.

So I make assumptions, and somebody else will make different assumptions, and therein lies the versatility and the variability of the kinds of papers that you see published in the literature, because they all make different kinds of assumptions about the things that you can presumably neglect in trying to construct an earthquake model. I must say that most of the papers in the literature these days neglect the dynamics. They assume that the earthquake takes place in zero time, and it takes in the transition from a state that is the precursor state of stress to the presumed post-earthquake state of stress. And we have argued that dynamics is an important ingredient. It's not something to be thrown away. And we spend a lot of time trying to understand how one goes from smaller, from one scale size, to the next larger scale size, and that's a technical detail, but it's an important one for us.

But what have we learned? I must say that part of it, of this game, is the criticism game again, and I tend to be critical of just about everything else that has been published. Sorry to expose you to my nihilism here. And one of the popular models these days, it's been around now for thirteen or fourteen years, is a model that says that because you have ten times as many Magnitude 2s as Magnitudes 3s and ten times as many Magnitude 3s as Magnitude 4s and so forth, the system is without any obvious scale. That is, if you looked at the sizes of craters on the moon, you would see a similar distribution. You would see ten times as many craters of small size as the next larger size and so on and so on and so on until you got to the largest size. It's a reversal of Jonathan Swift's quatrain, if you recall. Jonathan Swift had a quatrain that said— Now I've forgotten the precise statement, but it was something like big fleas have little fleas on them to bite them, and then the rhyme goes on and so on and so on to the smaller scales ad infinitum.

If I gave you a spectrum of sizes of craters on the moon, and there was not an astronaut standing on the moon or a meter stick to tell you how large that crater was, you would not know if you were looking at a crater that was ten meters across or a hundred meters across or ten kilometers across, because there was no ruler, no meter stick.

The same is true for earthquakes, so the presumption there was that the processes that produce small earthquakes are the same processes that produce the larger earthquakes, because they're distributed according to the absence of scale. That's not quite true. There is a cutoff at the largest earthquakes. We don't have earthquakes that are larger than Magnitude 8. The precise definition of Magnitude 8 seems to fluctuate as people redefine their scales, but that's a technical point.

As a consequence of the absence of scale, I won't go through the detail, but one can draw a conclusion that on the basis of one model, a certain model, it is impossible to predict the future time, location, and magnitude of the next earthquake. The entire Earth sits in an unstable, almost critical state, and the dropping of a little— The occurrence of a small initiating event here might initiate a small event, it might initiate a big event, and when that's likely to happen is also unknown, or where.

And that means that if you follow that argument, you will then conclude that earthquakes are not predictable, and that has had some following. I'll show you a recent paper that says exactly that. And that depends on the absence of scale. Scales exist on a number of levels, but basically we're talking about a distance scale, a meter stick. Are there critical distances? If there are critical distances in the Earth, then there will be critical changes in the statistics of the earthquakes, because earthquakes that take place on faults whose dimensions are larger than on these critical scales will organize themselves into structures according to a different kind of physics than earthquakes that are influenced by a smaller scale.

Well, we know that there are certain critical scales. One is the thickness of the zone in which earthquakes occur in California, and that's about fifteen kilometers. I think the depth of the Northridge earthquake began at seventeen kilometers, but we know no aftershocks are found below about fifteen kilometers, so that's a good round number.

I've been spending a lot of time in the last five years trying to see if there are smaller scales, and I mentioned one earlier this morning, that I think that there is a real three-kilometer scale. Now, bear in mind that my inversion of the triangulation data in the 1906 earthquake gave a depth of fracture of three kilometers. It wasn't that the fracture was three kilometers deep, but there was another structure whose dimensions were about three kilometers that gave the apparent depth of fracture to be three kilometers, because I had made the assumption that the structure wasn't there, in my original calculation.

I have now found much evidence favoring a structure of dimensions from two to four kilometers straddling an earthquake fault, not a horizontal structure as in the case of a horizontal slab, as in the case of the depths of earthquakes, but in fact a vertical structure paralleling an earthquake fault. And this means if there is now a structure, there has to be some sort of a difference in the organization of earthquakes on the larger scale—larger than two to four kilometers—which is around a Magnitude 5 earthquake. So the larger earthquakes organize themselves, in the models, according to one process, with different statistics than earthquakes with magnitudes smaller than five, which organize themselves differently and give a different kind of statistics.

So we went back to the original statistical literature. That's easy to get. Lists of earthquakes are extraordinarily easy to get. That's what archival institutions have been doing for years, and therein lies the difficulty. Once you have a list of numbers—

[interruption]

KNOPOFF: Here is a list of earthquakes. The archival institution, in this case Caltech, publishes thousands and thousands of pages of earthquakes, and most of them are very, very small. And you see here is a numbering, here is 18,393, in this particular catalog that I downloaded off the Internet, and this is in a very small region of California that I happen to be studying. Once you have a long list of numbers, it's easy to be deluded that you want to do statistics on them. You can count things. And there we are back again to counting things.

But the counting is not done, in my opinion, intelligently, because earthquakes, I think, have— There are two different kinds of earthquakes. Suppose you had a population of men and women, and you wanted to study the statistical distribution of the heights of women, but you compiled a list of the heights of all the human race, and then you said, "Ah, let's assume that the heights of all the human race therefore give us the heights of the women." Of course, you would say that's nonsensical. What you should do is identify the men, subtract them from the population, and study what's left.

Well, we have two populations of earthquakes. After every great earthquake there are aftershocks. They occur with great frequency, but in a relatively short time compared to the time between the larger earthquakes. So are there two populations? Are there aftershocks, which are the fallout of a large earthquake, and another population, which are the response to the slow deformation of a faulted earth and which creaks as the stress increases, ultimately to be released in large earthquakes?

I find that the aftershock population dominates the statistics. There are many more aftershocks than main shocks, what I call main shocks. So if you try your best to strip off the aftershocks from the complete population, that's a place where you become extraordinarily sensitive, because everyone says, "You didn't use the right definition of aftershocks."

All right. If I apply my definition of aftershocks, I find that, lo and behold, the main shock catalog has a kink in it that the small earthquakes, small main shocks, distribute themselves according to our usual power law, but the ones whose magnitudes are greater than five, corresponding to a critical dimension of about three kilometers, distribute themselves according to a different rule. The implication of all this is that the presence of a critical size in the system must ultimately negate the argument that earthquakes are not predictable.

VAN BENSCHOTEN: Right. That's the upshot.

KNOPOFF: That's the consequence, and that's what I— I'll show you the paper that I'm trying to send off this week; that's the last sentence of the paper.

VAN BENSCHOTEN: Okay. And the theory you're attacking, just to be clear, is the self-organizing criticality.

KNOPOFF: That's the <u>model</u> of self-organizing criticality. And of course, it's dangerous to attack things that are very established. I'm not saying that earthquakes are predictable. I say that they are not not-predictable, as the SOC model says.

VAN BENSCHOTEN: Exactly. You define predictability in a particular way.

KNOPOFF: I think that's an easy one. That's an easy one. Predictability is the formation of pattern, and the problem is to identify the pattern. There we have model dependence also, because we do not do nearly a very— We can't do a very good job of simulation on the Earth. Nature does a very good job of hiding her patterns from us, and so we have to do an extensive amount of analysis to identify that pattern, and therein lies the difficulty of identification of pattern. We're subject to whatever schemes we, the individual, set up in trying to remove the noise from nature's obfuscation, nature's cloud that is passed around the world.

VAN BENSCHOTEN: Sharpen the signal.

KNOPOFF: Exactly. To improve the signal-to-noise ratio.

So that's a very long harangue, and I apologize.

VAN BENSCHOTEN: No, that's okay. You covered a lot of questions that I had in that.

Let's talk a little bit about the politics of earthquake prediction, and maybe a question to start with is how effective do you think both federal and state agencies have been in both collecting data on earthquakes and then reducing hazards of earthquakes? And by that I mean, too, over the period that you've been very active in the field, say from—

KNOPOFF: Okay. Let me give an answer in my own terms, if you don't mind.

VAN BENSCHOTEN: Okay.

KNOPOFF: The national program in earthquake prediction began in 1975. At the congressional level, it began through the agency of Senator [Alan] Cranston of California and as a response to the 1971 San Fernando earthquake. After every great earthquake, there's always a flurry of activity that says, "Yes, we've got to improve our earthquake prediction capabilities," and after a few years that enthusiasm declines as other emergencies supersede and impose their visibility on the problem. But earthquakes are frightening things for humans. They cause a lot of material damage. They cause, in underdeveloped countries, large casualties. So it's a worthwhile societal goal.

When the program was funded, I personally don't think that the problem was attacked with the same insight that the Air Force used to undertake the program for the detection and identification of underground nuclear explosions. The attitude in the late seventies was "We have been successful in putting a man on the moon, we have extraordinary progress in many areas in bridging the gap between theory and practice, biological areas, physics areas. Look at the wonderful technology that we've built.

All we’ve got to do—.” I think that my vision of the attitude was “All we’ve got to do is throw enough technology at the problem, and the solution will fall into our laps.”

And so the direction of the program on earthquake prediction was placed in the hands of the U.S. Geological Survey, rather than what was done in 1960 with the problem of detection of clandestine underground explosions, which was to put the program in the hands of the universities, saying, “We don’t know what the problems really are. Let’s undertake a program of study to identify the problems and to find solutions.”

So the program was put in the hands of the U.S. Geological Survey, which has no experience, in my opinion, in the study of earthquakes, and you wouldn’t find a university geology department or earth science department capable of handling these problems, because the problems are nonlinear in the fundamental physics.

There was a bifurcation in physics that took place in the 1920s at the time of quantum mechanics coming to the fore, and up to that time geophysics had been a respected part of physics, didn’t exist as a discipline, but Lord Kelvin, Lord Rayleigh, the great physicists, were doing geophysics up until the twenties.

The excitement of quantum mechanics pulled the physicists in the direction of particles of very small objects on the atomic scale, the nuclear scale, the subnuclear scale, and the geophysicists went into wave propagation. The solid earth geophysicists, at least, went into wave propagation, which was linear. And that became the responsibility of earth science departments. The students were educated to solve linear problems; even today, very few geology departments or earth science departments have the capability of doing nonlinear dynamics. I think we're a little bit better off at UCLA than other places, but in my opinion, it's still quite unsatisfactory.

The U.S. Geological Survey was the wrong place to put the program, and they said, "Let's go out and just improve the sensitivity of our ability to record smaller and smaller and more and more earthquakes, and presumably, if we have enough data, the answer will fall into our laps." So the thrust there, and I think still continues, is data recording. To record more sensitive data, you had to put more instruments into the field and improve the quality of your data processing to get more and more precision. That was done.

But the insights never were there. The amount of effort that went into understanding, that went into asking what is an earthquake, which has great ramifications, and asks what are the physical processes that cause earthquakes, we've talked about this, how do earthquakes organize themselves into patterns? These insights were not there.

[End of August 29, 2003 interview]

TAPE NUMBER: VIII, SIDE ONE

September 3, 2003

VAN BENSCHOTEN: This is tape eight, side A. Today is September 3rd, 2003.

When we left last time, I had asked the question, what has been the role of the state and federal governments in assisting science in earthquake prediction, and then you had talked a little bit about self-organizing criticality and the paper that you had written disputing some of the claims, I guess, of that theory. And then you had talked about the Geological Survey and the problems with, again, lacking insights into the problem of the origins of earthquakes, the dynamics of earthquakes. I was wondering, did you want to perhaps take it from there?

KNOPOFF: Yes. Let me take it from a slightly different perspective. We have an extraordinarily long state. The State of California has earthquakes along its entire length, and it's an extraordinarily long front, in the military sense, to defend against earthquakes.

It's clear that we can't put out instruments to record pre-earthquake activity along the entire length of this region. What we know now is that the density of instruments has to be extraordinarily great. When the program began, there was no idea of how to develop a concerted focused attack on problems, because we didn't know then the scale of the problems. We didn't know whether or not we should be making measurements on a regional scale or making measurements with a spacing of only one or two kilometers. And on the scale of one or two kilometers, it's an extraordinarily long, long stretch from Cape Mendocino all the way to the Mexican border. And this still remains a problem.

So the focus was on measurements, measurements generally on a broad scale to compile a lot of data, but specific areas, specific targeted areas, were not looked at. I can see our own research, my own research and that of colleagues, I think I now know that precursors to earthquakes may exist. I think they do, but that's a biased opinion, precursors to large earthquakes, but they are extremely localized, so that most of the data being gathered on a large scale aren't really relevant.

So now is the time to begin to redesign the kinds of measurements and experiments that should be undertaken to study active faults, but funding for any kind of program like this has to come from the federal government. It's not going to come from the state government, for obvious reasons these days, with regard to the financial state of our own government.

And what we learned in California, which is the state with the greatest hazard, can ultimately, I'm sure, be applied to other states and to other nations. This is a marvelous laboratory. The great difficulty has always been that earthquakes recur on a time scale that isn't the human time scale; that is, a big damaging earthquake may occur once every few decades, one, two, three decades, and that's a long part of the human life scale, so people's memories are short. Other issues of great emergency intervene, and preparedness for earthquakes is not what it should be. I don't want to get into the sociological issues of earthquake preparedness and what one does with a warning. Those are deep issues that I don't have any competence in handling. But I think there is now emerging at last a glimmer of where the problems are and how we ought to be able to focus attention on them.

There is another issue, and that is, if a great earthquake happens on the scale of, let's say for the sake of conversation, every ten years— It's not a precise number by any means. Let's say a Magnitude 7. The Northridge earthquake was a Magnitude 6.7, and it caused great havoc and some casualties as well and deaths as well. And if a large earthquake happens on a fault such as the Landers Fault, which was an earthquake that happened in 1992, the recurrence interval on those earthquakes is of the order of about five thousand years. The San Andreas Fault is the most rapidly slipping, has the greatest frequency of earthquakes on it, but most of the damaging earthquakes since 1857 in Southern California have been on faults that were not the San Andreas Fault.

If a great earthquake of the order of a Magnitude 7 occurs somewhere in California several times in a lifetime, and an earthquake fault, an individual earthquake fault, ruptures once every five hundred years, that means that each Magnitude 7 is likely to occur on a different fault of, what, five hundred different faults in California in our lifetime. And the geologists haven't mapped five hundred faults, and, basically, we haven't developed a strategy to try to understand the possible occurrence on faults whose locations we don't know as yet, and we don't know how to anticipate where, when, and with what strength the next large earthquake is going to be. So it's a serious problem. I can almost guarantee that the next earthquake in the

Los Angeles area isn't going to happen on the same fault that ruptured in 1994 in the Northridge earthquake. It's going to be somewhere else.

So the problems are difficult, and I think my own hope was that there would always be some connection being made between an understanding of the physics of earthquakes and the data-gathering and interpretation enterprise. That really hasn't taken place yet, and I think that that part has a long way to go. But I say that out of self-interest, because I'm very much interested in the physics of the earthquake.

I might say that people in my area who deal with the physics of earthquakes aren't very knowledgeable about the data, and that's an unfortunate part of that. And people who are involved with the data aren't very knowledgeable about the physics.

VAN BENSCHOTEN: What do you think could be done to sort of close that gap, this chasm between that?

KNOPOFF: I think it can be done, but it requires a different kind— I think I have remarked earlier, I think it requires a different— It requires a change in our educational system. The traditional kinds of things that are being taught in departments of earth sciences focus on the phenomenology. The kinds of things that are being taught in physics departments focus on the physics, including models of self-organized criticality that I consider to be overly, overly simplistic, and the gap is very, very large at this time. Hopefully, it will change, but no one has really embarked on the issues of trying to expose the physicist to greater insights into the wealth of information that exists phenomenologically, and no one has really taken a hand at trying to expose the earth scientist to the issues of not only the dynamics of rupture, but also the wonderful ideas that have been generated in recent years in the field of complex systems, with buzzwords like *chaos* and so forth. These things aren't being done in our departments of earth sciences mainly because the departments of earth sciences have been focused on issues of exploration, which are linear in nature rather than the nonlinearity of violent ruptures on the Earth's faults.

VAN BENSCHOTEN: Just out of curiosity, in the 1994 Northridge earthquake, what was your experience with that?

KNOPOFF: We were awakened at 4:30 in the morning, as everyone else, and our thoughts were immediately with regard to the safety of our family and coping with the immediate issues. Yes, I was interested in it as a phenomenon of seismology, but—

VAN BENSCHOTEN: You're a homeowner; you had to be concerned.

KNOPOFF: Of course. Of course. No, that was within the day or so after the earthquake.

But that also is another issue for— I think I remarked on this before, and correct me if I have. If the focus is on earthquake prediction, then one ought to be interested in the events leading up to the earthquake, rather than the events which follow the earthquake. We're all concerned about aftershocks and the damage resulting from the earthquake.

You might also be apprised of the fact that there has been a change in the direction of earthquake studies in the twenty-eight years since the program began, and that is that the focus has gone away from prediction and toward hazard; that is, instead of predicting the times and locations of individual events, ask what are the long-term probabilities, what are the long-term risks. And so you develop a different strategy. If you're going to build buildings, let's say on the university campus and they're to last seventy-five years or something like this, what's the probability that a very strong earthquake will affect those structures, and what's the probability that they will survive? And you devise different building strategies for building buildings that are to stand ten years than for buildings that are to stand one thousand years.

VAN BENSCHOTEN: So it's a shift in the dynamics.

KNOPOFF: There's been a shift away from the focus on individual events and toward regional studies and estimating what are probabilities of events taking place within a certain time frame or a certain magnitude frame or a certain space frame.

VAN BENSCHOTEN: Especially if there are building codes in many respects, too. Isn't there legislation [inaudible]?

KNOPOFF: Building codes seem to get changed every time there's a big earthquake. The first building code in the field came after the 1933 Long Beach earthquake, and that was focused mainly on the schools, because some schools were very heavily damaged in the 1933 earthquake. But usually after great earthquakes, there is an effort to change the building code.

VAN BENSCHOTEN: Do you approve of that shift, then? Do you feel that the pie, the funds, in other words, that are laid out for study of earthquakes and hazard reduction, are they divided fairly equitably?

KNOPOFF: No, of course not, but that's done with a lot of personal self-interest in that statement.

VAN BENSCHOTEN: Okay. I'm looking at your résumé, and you're on the Joint Chancellor-Senate Committee on Seismic Risk at UCLA, and I was wondering how prepared is UCLA, and then in a larger frame, L.A., do you believe, for the next big quake?

KNOPOFF: I can't speak for L.A. We submitted that report in 1985. By great coincidence, it was submitted on the very day of the Mexico City earthquake, a very serious earthquake in Mexico City. And we recommended that— There were two reports. One was the statewide committee of which I was the chair, and there was a UCLA committee of which Sam [Samuel L.] Aroni in the Department of Architecture, Architecture School, was the chair. I was a member of that committee.

On the statewide level, we identified buildings that were at risk associated with the potential of a large destructive earthquake, and we had assessments of buildings on all the nine campuses. We submitted our report and met with President [David P.] Gardner, and I think basically the response was, "It's a meritorious enterprise, but we haven't got the money for it." And he said that the university would try to go out and support a bond measure for earthquake rehabilitation of the statewide buildings, but I don't think anything ever came of that. And he said basically that the responsibility for rehabilitation, which is expensive, has got to come out of local budgets, and, of course, that doesn't sit well with local budgets, local campus administrations.

When we submitted the UCLA report to the chancellor, Chancellor [Charles E.] Young, Chancellor Young, I think, initially put up the usual resistance that comes with the realization that this is an additional budgetary demand, but he came around very, very quickly and almost immediately started a program of bolting down bookcases. One of the great hazards in these rooms is that the bookcases will fall over, and if you're sitting under a bookcase, that could be very, very dangerous. And we have experience of that kind of thing happening in earthquakes.

He initiated a program of bolting down all bookcases. All these bookcases you see in this room are bolted to the walls. He initiated a program, and it wasn't a very expensive program in both cases, the bookcases and of putting restraining cords across lab chemical shelves. The danger of spillage of lab chemicals onto a room, falling off of a shelf, is very serious.

Since that time, he and Chancellor [Albert] Carnesale have— There's been a great program of rehabilitation of structures all over the campus, the buildings, the four old buildings, around the quad. Now the two gyms are being rehabilitated. At the time of the '94 earthquake, Powell Library was already under rehabilitation. The east tower of Royce Hall cracked visibly in the '94 earthquake, and that building was then closed and has been rehabilitated, including renovation of the auditorium. Haines Hall has been completed. Kinsey Hall is still to be done, but it's going to be starting. It's on the books, as I understand it. Kerckhoff [Hall] was done, the Math Sciences Building. And so it's moving.

You don't do all the buildings at once for obvious reasons, but this campus, I think, has been doing a very, very nice job, and I think it's to the credit of the administration that it's either made the resources available or has gone out and found the resources to do the job. These are not inexpensive programs and require a large amount of shifting of resources, including classrooms and student activities and so on.

VAN BENSCHOTEN: In 1972, you're on the Governor's Earthquake Council, and I thought I'd throw this in here, because, again, it sort of falls under the role that you played at UCLA. Is that worth— No? Okay.

KNOPOFF: No.

VAN BENSCHOTEN: All right. Let's move a little bit to some general questions on science. In talking about your career, we've learned quite a bit about, I think, the evolution of geophysics over the last thirty, forty years. How do you see the future of solid earth geophysics? What are some of the major challenges facing the field?

KNOPOFF: Well, I think there're some very difficult and exciting problems that one can imagine even now. But to try and predict where the problems will be a few decades from now, the fields always grow and mature and evolve, and the attacks on problems always generate new problems. That's in the way of the nature of things.

I think that there are exciting problems trying to understand— To me, the earthquake problem remains an exciting puzzle to be unraveled and to be understood. We have very little understanding of how faults interact. Does the activity on one earthquake fault influence the time and location of an earthquake on another possibly neighboring fault? Let me give you an example. It's been often quoted that the recurrence time of earthquakes on the San Andreas Fault, at least at one site not far from here on the San Andreas Fault, is about 135 years on the average. This has been done for about nine or ten big earthquakes, and there are ways, geochronological methods of measuring the dates of ancient earthquakes. They don't go back far, but ten earthquakes or eleven earthquakes is ten intervals of 135, so it's of the order of 1,000 years, 1,200, 1,300 years.

But these earthquakes are not regular clockwork events. In fact, most of the intervals are short. Very few of them are long. The shortest interval was 44 years, and the longest interval between earthquakes was over 300 years. The average is 135.

If earthquakes are not clockwork events, then it means that an earthquake fault is not an isolated object. There's something happening from the outside world, other neighboring faults, perhaps, that undoubtedly causes the times at which earthquakes are going to occur at one site to shift.

Essentially, this wristwatch ticks at a regular rate because basically it is insulated from interaction with the outside world. But if I were to take this object and shake it violently, perhaps at a frequency of one shake per second, which would be at the same rate— Or let's say at a shake per three quarters of a second, that action would change the rate of motion of this wristwatch, the rate of ticking of this wristwatch.

As a matter of fact, I gave to my undergraduates last winter the problem of whether there is a shift in the frequency of a wristwatch, a mechanical wristwatch, when at rest compared to being worn by somebody who is swinging their arms when walking the street. And there is a shift. It depends on the relative rate of swinging of the arm as a pendulum and the rate of rotation of the balance wheel in the wristwatch. And if these are far apart, there's very little influence.

Well, if you find a shift in the frequencies of earthquakes, and the earthquakes occur at all sorts of different times, it means that this region where the earthquake intervals were being measured is being influenced by earthquakes all over the rest of Southern California, and these are not taking place themselves at regular intervals but are taking place at irregular times. And so we know that the problem is one of interactions of faults. Earthquakes on one fault influence the times of earthquakes on other faults, and these problems are very poorly studied right now. I exaggerate. They are not even being studied right now. They're difficult, very difficult, very difficult questions. And I'm not sure that we know how to attack these problems.

But questions in geophysics in general on the horizon are questions of mantle convection, why continents drift, why and how they drift, and a major problem that people haven't come to grips with yet is what is the influence of a surficial structure due to continents or the uppermost parts of the oceans riding on a generalized convecting system. It's as though you were studying the self-stirring of a pot of soup on the stove and then someone put a little wafer or some floating obstacle or object in the middle of that soup.

What is that influence? Not only is that stuff going to be transported by the stuff underneath, but how does its presence influence the circulation? And if that object inserted on top of the kettle of soup has a varying thickness, such as we have found under the ancient shields (parts of the superstructure have keels underneath them), how does that influence the circulation? That's one area that I think is going to be strongly looked at in the future.

Issues of the origins of the Earth's magnetic field and its very interesting properties of reversals of polarization, these are just beginning to be explored. A very brilliant person doing this on our campus is Paul [H.] Roberts. He's in the Institute of Geophysics and Department of Mathematics. And these are intensely difficult problems.

You mentioned in the prologue before we switched on this tape that we were going to say something about the issues of computing, which we talked about a little bit in earlier conversation, the incompatibility between sizes of computers and the scales of the problems that we wanted to attack. In the cases of mantle convection and origins of the magnetic field, again we have to make— The people working in these fields—I'm not working in those fields—have to make certain assumptions and computations.

The computations that you get are the byproduct of your assumptions, and there is an ongoing danger if one attempts to draw generalities from a collection of computer outputs. It's really quite rare that broad generalizations can be obtained by studying large amounts of computer output. There is a great danger that the computer output that you get is a consequence of the specific assumptions that you build into constructing the computer program.

So, will refinement of computing power, the ever-escalating size of computing power, improve our ability to do science? Maybe. I'm sure it will, but not to the degree that the computing manufacturers would have you believe. There has to be a compatible or even a greater increase in insight in how to do key studies from which one can draw generalizations.

VAN BENSCHOTEN: So the old “garbage in, garbage out” problem.

KNOPOFF: Well, that’s an exaggeration. People, I think, are well motivated. I don’t think that they have— Most of the papers I see in the journals, I say “most of them” certainly, are generated with good intentions but with poor insights. I’ll change the word *poor* to *superficial*.

VAN BENSCHOTEN: All right. We’d also talked briefly about big science and the dangers of big science, and I was wondering if you could maybe elaborate a bit more on that, the dangers of it. I know for yourself, for instance, I think you were saying that you preferred to sort of work with a small group.

KNOPOFF: Well, that’s my style. That’s my style. But the dangers of big science are that— I’ll use a word that I’ve just used in another context, and that is that there have to be insights into the selection of problems. There are a large number of problems in the universe, thank goodness, that should be attacked, far more than we have time for and far more than we have resources for. As the problems get more difficult and more intense, it’s been found in recent years that we should be attacking these things as teams. And in so doing, funding goes toward funding large programs, and programs submitted by individual investigators generally are pretty slightly funded, and individual investigators are disappointed. That’s hard on young people trying to get started. They are discouraged from applying for research support unless they join in on a big project, and then they become part of a large team, and the establishment of their identity within that large team becomes difficult.

But occasionally, a large program of research goes bust; it was ill-conceived or didn't work out for one reason or another. In geophysics, the example of—how many decades ago?—the Mohole [Mohorovicic Discontinuity] Project, which involved drilling through the crust and into the mantle, and since the crust is thinner in the oceans, it was decided to do the drilling off the coast of Baja California, using an ocean drilling rig onboard ship.

It didn't work out. The program was supported for a while and ultimately cancelled. And then, of course, after such an event, there's always hindsight. We could have used the money in other ways. Of course, hindsight is a bad thing to apply, but we're doing it all the time. We're doing it in our international political situation these days with great glee in seeing how— In regard to U.S. performance abroad.

But there is danger, and the reason there is a danger is that very often the spokespeople for these large programs, which have to be funded at the federal level, these spokespeople are not always the greatest people to undertake the leadership of the program or the people with the greatest insights into the science. So there's a real problem there.

I've also been concerned with the people who do program direction in the government. I'm a snob and I believe that the most creative intellects are here in research university environments, and that the people who direct the programs aren't the most qualified with understanding or insight into the meaning of the science, and as a consequence, they select referees of proposals who do not necessarily themselves have the greatest insights.

And so the programs generally at the federal level tend to be normative. They tend to have a regression toward the mainstream, and I think that a lot of imaginative ideas that are floated out there could develop a large amount of creativity, could be the products of great creativity, but aren't being funded. And since these programs are being paid for by the citizens and the citizens have a right to have some oversight, people don't know how to create this kind of oversight when somebody is sitting back and saying, "I'm thinking about how to solve this problem, and just trust me it's going to turn out all right." Well, that isn't enough for the overseers.

I'm reminded of an item in the newspapers a number of years ago when a famous mathematician was returning from Europe, from a conference in Europe or a stay in Europe, and submitted an expense voucher for his travel, but he traveled across the Atlantic by ship, and the auditors questioned this. And he said, "Well, I was thinking mathematics the entire time of the cruise," and it was true. We don't stop thinking once we slam the door here and go home.

VAN BENSCHOTEN: Yes, exactly.

Let me flip this over.

TAPE NUMBER: VIII, SIDE TWO

September 3, 2003

VAN BENSCHOTEN: Let me just do a short intro. This is tape eight, side B.

KNOPOFF: So the balance between accountability and imaginative enterprise is a difficult one. The popes had it right when they hired Michelangelo. They just said, "Here, just do it."

VAN BENSCHOTEN: I know that, for instance, in the biomedical sciences, the Pew [Scholars Program in the Biomedical Sciences], for instance, gives out money, a certain lump sum, and the people can use this, the researchers can use this fund however they wish. Of course, that would foster, I would think, a bit more maybe imaginative or cutting-edge research, risky research. Is there anything like that in your own field, or would that perhaps be something that would benefit?

KNOPOFF: I think all fields would benefit from that kind of program, but no, not really. There's no big foundation support of— I don't know about the Pew program in the medical sciences, but, again, you're up against the overwhelming weight of the NIH [National Institutes of Health], which understands how to, or thinks it understands how to, organize big programs with large numbers of sub-areas to study as parts of a big program. I don't think the Pew can compete with the NIH, can it?

VAN BENSCHOTEN: No, that's for sure. [mutual laughter]

We'll begin talking about general questions of science, and one of them is competition, competition getting a journal article out, competition getting funds and funding. Is competition, in your mind, generally good for the doing of science, good science?

KNOPOFF: Well, sure, the answer is definitely yes. *Competition* isn't quite the word I would like to use, but I think we all use the ideas of others as springboards for our own creativity, and that's always valuable. If I go to a meeting and hear somebody make a presentation, I'm usually aware enough, if I'm interested in the problem, to either see how to do it better, if I want to, or I can generate some ideas for new problems that I might want to study, and that's always good. So the fact that there are other people working in the field, I have enough self-confidence that I think I can identify what problems are good and what problems aren't so good to attack. By that I mean what problems are worth my time, and my time is very valuable these days.

VAN BENSCHOTEN: All right. Here's a question, and I'll throw this out. It's a question about— First of all, I start with the assumption that in many cases, both in science and other places, mistakes— Our mistakes are sometimes more fruitful than our successes in at least teaching us about ourselves and the problem that we're studying. This question was partly suggested by [Vladimir] Keilis-Borok when I spoke with him, and he and I would be, I think, curious to understand. Could you maybe choose a mistake that you have made, or a couple, or whatever, and then maybe analyze what did you learn from it.

KNOPOFF: Should I admit that I made mistakes? [laughs]

VAN BENSCHOTEN: It's a dangerous question. I guess I'm asking you to take a chance here.

KNOPOFF: I think that— You catch me off guard, but I can think of several examples in which I have published errors in analysis. I think what I've learned from these was that in the cases that come to mind, all of the cases were done by jumping to conclusions too rapidly. I made assumptions, and in my eagerness to work out the conclusions, I didn't look carefully at the assumptions that I made.

In one case, I think that I missed the boat on a big area of research if I had thought the problem through before I published, and I had to reverse my position, but I didn't spend any— I decided at that point, after having reversed my position and come out in print with that reversal, I decided that I wasn't going to proceed with research in that area any longer.

And in another instance, I accepted some experimental results and developed the model based on someone else's experimental results, and I hadn't looked very carefully into the experimental results and drew some conclusions that were rational, but were quite irrelevant. Fortunately, the matter dropped, and I realized that and went off in other directions.

But I think as I've gotten older, I've become more able to discriminate. I've been able to learn the lessons of Dave [David T.] Griggs, and that is to be able to criticize myself much more carefully and with reason than I was able to in past years.

VAN BENSCHOTEN: Another question about maybe the history of science. You're writing a history, if I understand, about the Institute, then. Is that what I saw?

KNOPOFF: No, no, no. I was asked— And here I admit to a great amount of embarrassment. After Louis Slichter's death, I was asked by the National Academy of

Sciences to write a memorial to Louis, and I've put it off for a long time. Part of it was emotional. After that phase, there was a long interval of just trying to gear myself up to get started on this, because there were a lot of other problems I've wanted to attack. But I've finally done it. And embedded in that is a history of the difficult times that Louis had in getting the Institute started. I'm not writing a history of the Institute, no, but I'm writing a brief biography of Louis Slichter, which is essentially completed now, thank goodness.

VAN BENSCHOTEN: The Academy will be happy.

KNOPOFF: The Academy will be happy, but I will be happy, too.

VAN BENSCHOTEN: I came across, too, I think, a tribute—I don't know if that's the right word for it—for [Sir] Harold Jeffreys and for Beno Gutenberg as well.

KNOPOFF: Yes. Well, one is asked frequently to write memorials, and both were great, great geophysicists, the two great seismologists of the twentieth century. I was asked by the Academy to write a memorial for Gutenberg. I was not the first to be asked, but I received the assignment after a number of others had tried and failed. I finally did it, and the memorial was well received, especially by Gutenberg's family, so I felt very good about that. And he was a great man.

And of Harold Jeffreys, who was also a great, great man. That one was requested— Jeffreys was not only a great geophysicist, he was a great statistician. A statistics journal, I think it was, that wanted a series of memorials to Harold, and I think there were four parts to that. I wrote one part focusing on his geophysics, and there were other parts focusing on his statistics and his other accomplishments.

VAN BENSCHOTEN: The reason I bring up these two is partly because you're leaving behind a historical record of the field, partly, again through these tributes to these men, also of Slichter. I was curious about how important do you believe knowing the history of science, a particular field, geophysics, for instance would be. Would that be of any benefit, do you feel, to students who are coming up or to scientists in general?

KNOPOFF: Well, my own feeling about these memorials is that the students today—Science moves so rapidly that students today just don't have an understanding of what scientists forty, fifty years ago were doing. I think history of science has fallen into some sort of a quiet period in terms of the science of the twentieth century. I think we know there are people writing histories of science. There's a recent book about [Isaac] Newton, and biographies of famous scientists of the past are perhaps a little bit more visible for personalities of the past than they are for personalities of the present or the recent past.

When I've shown my memorial to Gutenberg to others who know the name, they're really quite surprised at the wealth of things he did, and, "Oh, was he the one who first discovered the low-velocity channel?", "Oh, was he the one who first did an analysis of the density distribution in the Earth?". Everyone knows that Gutenberg first measured the radius of the core of the Earth, but it doesn't go much beyond that, but he did a huge number of things.

So I think it's useful to remind people of what the accomplishments of these people were. I'm not a historian. I think that when the histories are written of geophysics in the future, perhaps these memorial documents—they're not biographies by any stretch of the imagination—these memorial documents that highlight the important accomplishments of important geophysicists will allow the historian to know what it is that the historian should be looking at and looking for. What are the important contributions? And then by some digging or other, I'm sure the historian will find out what the insights, the personality, and so on were. So I think I see these memorials first and foremost as being a publication for the record to make sure that a summary of the contributions and a picture of the personality as known to immediate colleagues isn't lost.

VAN BENSCHOTEN: Let's turn to university service. I bring this up because I think it should be part of the public record. It shows up on your CV and in other places as well. Of the many things, you're on the Joint Chancellor-Senate Committee on Seismic Risk, I've mentioned that you were on the Budget Committee, you were, of course, Associate Director of the Institute. You were chair of the University of California Systemwide Committee on Seismic Risk between '85 and '89. Also you endow a chair, with your wife, I believe, for the basic sciences.

KNOPOFF: No, it's not in the basic sciences. It's an endowed chair in— Well, two things are wrong with that statement. One is that it's a chair to be jointly held in the Institute of Geophysics and the Department of Physics, not broadly in the basic sciences. We felt strongly that I've had such a wonderful experience at UCLA, encouraged by both the Physics Department and the Institute of Geophysics, I think this ought to be passed on to someone else to have the same privilege I did.

We didn't endow a chair, a professorial chair. Professorial chairs are endowed at a level twice what we did, and what we did was we endowed a— We made available an opportunity for a junior faculty person to hold a "career development" position jointly in both the Institute and the Department. These endowments in the University of California, you understand, do not pay salaries, salaries in the university, whereas in private institutions, the endowments pay academic salaries. Faculty salaries flow from the state budget. And so the income from the endowment is the equivalent of a research grant. We haven't yet paid off the mortgage. We're still paying the— We're trying to pay off our obligations under this agreement on the installment plan, so we're still going.

VAN BENSCHOTEN: That was some of the things I read of your university service. We've already mentioned some of your service to various commissions again, outside, U.S. Atomic Energy Commission [AEC], for instance, Secretary-General of the International Upper Mantle Project.

KNOPOFF: Oh, forget that Atomic Energy Commission.

VAN BENSCHOTEN: We'll cross that out.

KNOPOFF: Well, that was a disaster. The AEC at the time had a scientific panel, and it was basically designed, in my opinion, to be a rubber stamp of their activities. At one point the AEC exploded a large nuclear device underground—

[interruption]

KNOPOFF: That was a small part of my— I haven't even thought about it in years. But at one point they wanted to detonate a large megaton, several-megaton nuclear device under one of the Aleutian Islands, and it was at the site just a short time earlier of a very large earthquake, the Rat Islands earthquake.

I was interviewed, and I said that I didn't know if the explosion would be within the zone where the rupture had taken place or just outside. As a matter of fact, it was quite uncertain from the evidence. If the explosion had taken place within the zone where the earlier earthquake rupture had taken place, then the stresses in the Earth would have been relieved by the earthquake, and there was no danger that an explosion would have triggered something else. Now we know at this point, and have known for a number of years, that explosions aren't likely to trigger major earthquakes, but at that time the situation was uncertain.

Well, that wasn't quite what the AEC wanted to hear, and that uncertainty was picked up by a number of people in the community that were anti-nuclear anything. And so I got very quickly dropped by the AEC as a member of their panel.

VAN BENSCHOTEN: I'm glad you clarified that. The reason I was reading down this list, though, is I do want to emphasize and get on record some of the service that you've done for the field. You're also on many editorial boards as well and have been.

KNOPOFF: Well, I was. I've tried to drop most of these things by now. They are time-consuming, if you take them on with a seriousness of purpose. But I've always felt that one should serve the community, pay one's dues, as is said, and I think that my two major contributions were serving on the Upper Mantle Committee and being the director of this Institute. And the rest of it, I'm content that there are bits and pieces here and there, but I think those are the two major things.

VAN BENSCHOTEN: I want to talk a little bit about your teaching. You've already mentioned some of this, but what has been the place of teaching, I guess, in your long and distinguished career? Because you have won the Distinguished Teaching Award here at UCLA.

KNOPOFF: Of the Physics Department. Not of UCLA. Several times.

I enjoy teaching. It's a different kind of activity than— Well, there are two kinds of teaching. One is in a classroom, and if you're in a classroom with a number of students, especially in the world of physics, you're sometimes reviewing physics of the nineteenth century. It's an enjoyable period in physics for me to discuss, which is a preparatory stage for students who want to go into modern physics. I enjoy the classical physics part of it.

The material is codified, it's difficult for the students, and I enjoy trying to give them insights so that they can become familiar and comfortable with the material themselves. To a certain extent, I have to try and give them my enthusiasm for it, and I think that's my best— That's something I can do. I enjoy waving my arms and jumping up and down in front of the class and trying to stimulate them by showing that the stuff is fun.

Teaching on the individual level, which is teaching not the undergraduates in the classroom, but teaching graduate students where one is in a one-to-one interaction, I find that much more challenging and much more interesting, because I'm learning at the same time that the students are learning. Here I can learn and apply my reasoning ability to the solving of puzzles and try and develop that skill in the—what we've talked about a number of times—the ability to develop that kind of skill in the student and develop the ability to be critical. That's the key issue. And along the way, perhaps inspire the student to develop some creativity as well. That's really a lot of fun.

The classroom is also a discovery for me, but teaching these materials that were developed in the nineteenth century, perhaps teaching it now with twentieth century tools, I start to see subtleties. I can see how to be creative in the development of how a student might be learning to attack and understand already well-understood material. But the feedback for me is seeing the spark of understanding in the students, but not the spark of developing new ideas, because the ideas are already there. But there the fun is developing an enthusiasm for physics in these students.

It's for that reason I most enjoy teaching third-year students in the Physics Department, and the reason is that this is the first time that the students emerge from the large-population classes that are inhabited by students, especially from Engineering, who are overwhelmed, have to take physics, introductory physics, and these students, I don't think, are as strongly motivated towards physics as the physics majors are. The physics majors emerging in the third year are suddenly the focus of all attention, and the problem is to create a flame out of that spark. So that's fun.

VAN BENSCHOTEN: I just have a few loose ends, a few questions at the end here. One of these was suggested by, again, Keilis-Borok, which I thought was a good question. It's sort of a speculative question. But what would you do if you could have a second, a parallel life? How would you spend it?

KNOPOFF: I have no idea. [laughs] Parallel life in terms of it going on at the same time?

VAN BENSCHOTEN: Yes. Let's say that you could do the physics. I think what he was trying to get at was, you know, you could do, of course, the thing that you love, geophysics, but is there some other interest that sort of piqued your curiosity that you might have taken another path perhaps?

KNOPOFF: Oh, I don't know.

VAN BENSCHOTEN: I know biomedical researchers, some of whom are very interested in more sort of the business of science and working with companies and developing products rather than doing research, and that's sometimes their parallel life, I think.

KNOPOFF: Oh, I think if I had a parallel life, I would— This comes out of the blue here, so I'm not sure I'm—

VAN BENSCHOTEN: Blue is sometimes good.

KNOPOFF: I think I would try to spend more time working on solving the puzzles of other kinds of self-organizing complex systems. I'd spend more time on trying to understand music than I have. I might even try my hand at understanding, trying to come up with a model, which I have toyed with from time to time, for economics. I don't think I've ever been highly motivated to make a big fortune. The university has been good to me, I live very comfortably, and I have no need for going into private enterprise.

I think that I would enlarge my vistas beyond geophysics by doing things in other areas of complex systems analysis. I'd try to understand how we use language, how would one go about quantifying art. Music plays an important part in my life, and I'd like to spend more time on it than I have.

VAN BENSCHOTEN: All right. My last question. What would you add? What would you add to the record that we haven't already covered or that perhaps may need clarification?

KNOPOFF: Well, at this stage of the game, it makes it sound like it's about to become the final oration of Caesar before—

VAN BENSCHOTEN: This is a provisional summary, I guess.

KNOPOFF: Well, do I have to do it that way?

VAN BENSCHOTEN: No.

KNOPOFF: Can I give you an anecdote, a remark?

VAN BENSCHOTEN: Definitely, anything that you would like.

KNOPOFF: Well, I've been blessed with a wonderful wife and wonderful children [Joanne Van Cleef Knopoff (wife), Katherine (Katie) Alexandra Knopoff*, Rachel Anne Knopoff, Michael Van Cleef Knopoff]. Our marriage started off at a very rocky point for me because I missed the first scheduled time of our wedding with illness.

Oh, let me remind myself of an anecdote about that. When I went off to Jordan on this archaeological dig, the expedition leader had got financial support from the BBC and the *London Sunday Times*, Middle East Airlines, and Batchelor's Dehydrated Foods. When we arrived on the site, I was the designated cook for the six westerners, because I had mountaineering experience in the High Sierras and knew how to cook up dehydrated foods. So, the first night, I got over the campfire and stove and whipped up the dehydrated foods.

Apparently, the Bedouins who were working for us became very upset, because they had been contracted to do all the labor, the task-related activities of the expedition, and so they had come with a cook, whose job I had just taken away. But it turned out that they were baking their bread and burying it in the ashes of the sand where they'd built a campfire, and that was their mode of cooking, and they didn't understand reconstructing dehydrated foods.

So the next night it was forcefully brought home that I was to supervise the Bedouin cook, but I spoke no Arabic and he spoke no English. So I quickly inquired, and I learned the word for water, and I would indicate "add," by a stroke of my arm, "add water." And then I'd learned the word for "enough," so I said "enough" in Arabic, and that meant he had to stop. Then I'd motion with my hand around, a circulatory motion, indicating "stir," and he would stir. And so it went, and it wasn't bad.

The BBC came along, and these men were not interested in following the day-to-day activities, so they spent most of their time in the big city having a good time, I guess, and one day they came out and visited our camp, and we were photographed.

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[*] As of 2005, Katherine (Katie) Alexandra Knopoff Wadley.

I came down with hepatitis in Israel, as I remarked, and went back to England. The Israelis wanted me to stay there because they said, "We've got excellent physicians," which they did, "and we could treat you." It hadn't been identified yet. But I said no, I had to start moving westward so I could reach Los Angeles to get married a few weeks later.

I reached Cambridge, it was bitter cold, and I came down with— I was taken to the hospital two days before I was scheduled to return to L.A., for the following weekend was going to be my wedding, and I was in the hospital for three weeks and, of course, the weekend. The marriage was postponed. I returned to L.A., and my bride-to-be, Joanne, just took wonderful care of me on a daily basis. It was one of the wonderful times when we were close together for long times.

We were married and we returned to Cambridge, essentially on a honeymoon trip, and shortly thereafter I went into the Department of Geodesy and Geophysics at Cambridge, to try to pick up a thread or two. The people there said, "Oh, we've just seen you last night (or a few nights ago) on the BBC."

And I'd missed the BBC program of the show showing the archaeological dig in Jordan. And so with great interest as a budding television star, I inquired about my role in the show, and they said, "Well, you didn't appear in any of the scenes, except in one instance where you are shown supervising the cooking of the evening meal. And the voiceover, appropriately to your recent marriage, said, 'He is as fussy over his cooking as a new bride.'" [mutual laughter]

VAN BENSCHOTEN: That's funny.

KNOPOFF: So, of course, they did it with great wisdom.

VAN BENSCHOTEN: Okay. Did you want to add anything?

KNOPOFF: No, I think I'll stop there.

VAN BENSCHOTEN: I do have one more question. I'll get it in.

KNOPOFF: Sure.

VAN BENSCHOTEN: How do you spend your time now? How would you describe a typical day for you? What is that like?

KNOPOFF: Well, I meet with my graduate student for a number of hours. We try to work together probably about six or so hours per week. And I meet with a man who is now retired. He was my Ph.D. student many years ago, and he drives up from Del Mar, Julio Landoni. And we work for about three hours once a week together. He wants to keep his oar in, and we work on— He's a wonderful mathematician, applied mathematician, and we work together. So I spend a lot of time working with these two students, student and colleague.

Then the rest of the time, I really try and attack this enormous backlog of papers that I'm trying to write, as well as carry out a little bit of research that this kind of work suggests. But mainly it's writing and rewriting and rewriting, and as I've gotten older, I spend so much time rewriting and polishing that it's something that my colleagues think is overkill. I don't think I spent this much time when I was younger. But I spend a huge amount of time writing, and I've got a huge number of papers to get out, as I have a long shopping list of work that's been completed and a smaller shopping list of problems still under study.

So my days are busy, my days are full. I try to get here around ten in the morning and leave around six in the evening, and these are full days.

VAN BENSCHOTEN: I know you talked a little bit about this off tape before we began, but what do you do to relax? What do you do? Do you have hobbies or things that you do now to get away from everything?

KNOPOFF: Oh, weekends, I work in the garden a little bit, and evenings sometimes I relax with a murder mystery. The university library is such a marvelous storehouse of murder mysteries that I don't have to reread any of them.

Music is an important part of my life. My wife and I go to concerts.

VAN BENSCHOTEN: You recently went to the Hollywood Bowl, too, didn't you?

KNOPOFF: Yes, we have a season ticket to the L.A. Philharmonic and have one— The Hollywood Bowl is not— I don't consider it to be serious music. It's more of you go there for all of the associations of the music rather than the music itself. But the acoustics are not optimal for hearing good music, as they might be if you were in a concert hall.

And we travel a lot. We travel a lot.

VAN BENSCHOTEN: You're due to go on a trip fairly soon.

KNOPOFF: Yes, we leave in under three weeks now for a month-long trip to Europe.

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VAN BENSCHOTEN: This is tape nine, side A.

We were talking about some activities you do to relax. You were talking about music and travel.

KNOPOFF: Well, we also do a lot of traveling. Usually the traveling has some science associated with it, a conference or something like that. In recent years, it's been rare that I've gone off by myself to a conference. The two of us have frequently gone together. This upcoming trip, our children will be joining us for part of it. In earlier years, I did a lot of traveling on my own, and I've been in some wonderful places.

Together we spent sabbatical years in Cambridge [England] on three different years. The first one, we were together for the second half of it, but I was there for the first half by myself. We were married during the middle of it. We spent six months in Venice. We spent six months in Karlsruhe. We spent a northern hemisphere summer in Santiago and were there in the events leading up to—just weeks before—the overthrow of [Salvador] Allende and were there during some of the shooting, with our kids. It was quite exciting.

I've been privileged to go to Tibet, and that was a wonderful, wonderful experience. Keilis-Borok, through his agency I went up— I was in Tajikistan. And do we have time for another anecdote?

VAN BENSCHOTEN: We do.

KNOPOFF: I was a mountaineer here in the Sierra and enjoyed wonderful times going to the mountains. I haven't been in the mountains in recent years. In Tajikistan one day I said, "I'd like to explore the back country," and a Russian colleague, Vladek Pisarenko, joined me, and we went on a day hike up this long canyon, which was, in fact, I was told, the caravan route between Tajikistan and Kirghiz, Kirghizia, Kirghizstan. Some miles up the trail, we came upon a group of shepherds off the trail, five of them, and there's a photo over there. Did I tell you the story of the shepherds?

VAN BENSCHOTEN: Yes, but it was off tape so it was—so it would be good to get it on tape.

KNOPOFF: And they waved us over, had a little campfire going, and we were talking. At one point, I said that I'd like to take their photo. There was a large amount of conversation because these men were Muslim. In the Soviet Union, which was nonreligious, they still had some Muslim upbringing, I'm sure. And there was some discussion.

We had been talking a while, and one question they'd asked is, "Why is it that you wear boots like that?", pointing to my mountain boots, which had Vibram soles on them.

And I said, "Oh, alpinists wear that." The Russian word for a mountain climber is an alpinist. That I said through Vladek as a translator, because they spoke Tajik and a little Russian, and he spoke only Russian, and I couldn't speak either one. Then they said, "Oh, you are an American." They had heard on the radio all about

Americans. “Following you will come an army of tourists all carrying cameras and portable radios.” I was the first ever alpinist in this canyon.

Then I asked to take their photo. Or no, before that, I’d asked. Then they became very curious, and they wanted to know whether there were pigs in America, because pork is forbidden for Muslims. And I said there are pigs in America. They were quite disappointed.

And then they asked how many children I had, and I said three, and at this point, they became extremely excited. They were slapping one another on the shoulders and slapping their knees and saying, “What a stout fellow.” I could recognize that much Russian. *Kakoye molodets, Kakoye molodets.*.

I reacted with some puzzlement, and I said, “What is so exciting about having three children? Only a few days ago I was in your village at the mouth of this canyon, and I saw a hoard of children all lined up in their best dress for a village wedding.”

And they said, “We didn’t ask how many children you had. We asked how many wives you had.” And so they were disappointed to learn the truth.

So at that point, I asked if I could take their photo, and finally after some debate, they agreed, and you see the result there. But I reached into my backpack to pull out a pencil because I’d agreed, as part of the bait for taking the photo, that I would send them a copy of the photo, a print. So I reached in my backpack for a pencil to write down the name and the address on my little pad of paper, and by chance the point on the pencil was broken. So I reached into my back pocket and pulled out my pocketknife to sharpen the pencil point, and their eyes bugged out, just became extremely amazed. And as it turned out, they had never seen a folding knife before, this ordinary clasp knife, pocketknife that we're all familiar with. So I gave them the knife as a gift, and this created an extraordinary bond of friendship.

And their response was, "We would like to give you a gift in return. How about a sheep?" Now here I was, ten miles up the trail from the nearest automobile, nearest cross-country vehicle, and I was a further fifty miles or so from Dushanbe in a very flimsy airplane, and then I would have to fly to Moscow and then fly from Moscow to the United States carrying a sheep. [mutual laughter] So, well, after some bargaining, we compromised—they had a fire going and some mutton soup going—and we compromised on some of their bread and mutton soup, and that was the end of the sheep.

But you can see in the photograph, there is this one patriarch with this marvelous beard and the turban and the three [other shepherds]. One is a relatively young boy. But they're all, the rest of them— They're in the high pasturage. This is the summer pasturage for the sheep, and you can see it's pretty desert-y territory, but they seem to be— The [fifth] man in the white shirt there is a cousin, apparently, who had been working in the big city, Dushanbe, I think, and had walked up during his summer vacation to visit.

VAN BENSCHOTEN: [inaudible]

KNOPOFF: This whole area had been devastated in a very great earthquake in 1949 with a large number of deaths, and that was the reason why the Russians had what they called an expedition not far away, some tens of miles away from this particular spot where you see the photo. And that expedition was, in fact, a series of permanent buildings with— It was a seismic observatory, and we'd been taken there because the people in that observatory had developed a method that they claimed would predict earthquakes. It was later shown that it was not a justifiable assertion, but they were very enthusiastic about it.

VAN BENSCHOTEN: I wanted to thank you for allowing us to sit down with you and tell us your life. You've been very open and you've given me a lot of research material to go through, and I just appreciate your help with this.

KNOPOFF: Oh, it's been my pleasure, and I thank you for spending the time, and I thank Gold Shield as well for supporting this.

VAN BENSCHOTEN: Thank you.

[End of September 3, 2003 interview]

Guide to Proper Names

Aitken, M.J.
Allegro, John Marco
Aroni, Samuel L.

Beloussov, Vladimir V.
Berman, Jack
Bjerknes, Jacob A.B.
Bridgman, Percy W.
Bullard, Edward "Teddy"
Burridge, Robert
Byerly, Perry

California Institute of Technology
(Caltech)
Carlson, Jean
Carnesale, Albert
Carter, Adeltha E.
Cranston, Alan

Edwards, Ray L.
Epstein, Paul S.

Fyfe, William

Gardner, David P.
Gilluly, James
Gilbert, J. Freeman
Ghil, Michael
Goorberg, Wilhemina van de
Griggs, David T.
Gutenberg, Beno

Hart, Pembroke J.
Holmboe, Jørgen
Holzer, Robert E.
Hood, Alexander
Hood, Mantle
Hudson, John
Hutchinson, William "Bill"

Institute of Ethnomusicology, UCLA
Institute of Geophysics, UCLA

Jeffreys, Harold

Kaplan, Joseph

Keilis-Borok, Vladimir "Volodya"
Kennedy, George
Kennel, Charles
Knopoff, Isaac
Knopoff, Joanne Van Cleef (wife)
Knopoff, Max (father)
Knopoff, Ray (mother)
Knopoff, Regina
Knopoff, Usher
Knudsen, Vern
Kremenliev, Elva
Kremenliev, Boris

Landoni, Julio
Langer, James
Lawrence, Steven
Leonard, Robert
Libby, William F.
Los Angeles City College

Mason, Maxwell
MacDonald, Gordon J. F.
Miami University
Millikan, Robert A.
Murphy, Franklin D.

Olwin, Keith

Palmer, Clarence E.
Perrine, Gertrude
Pickering, William H.
Press, Frank

Reichenbach, Hans