HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION

Herbert Elias Klarman

Coordinated by
Center for Hospital and Healthcare Administration History
AHA Resource Center
American Hospital Association
One North Franklin
Chicago, Illinois 60606

HOSPITAL ADMINISTRATION ORAL HISTORY COLLECTION

HERBERT ELIAS KLARMAN

In First Person: An Oral History

Interviewed by Duncan Neuhauser, Ph.D.
1994

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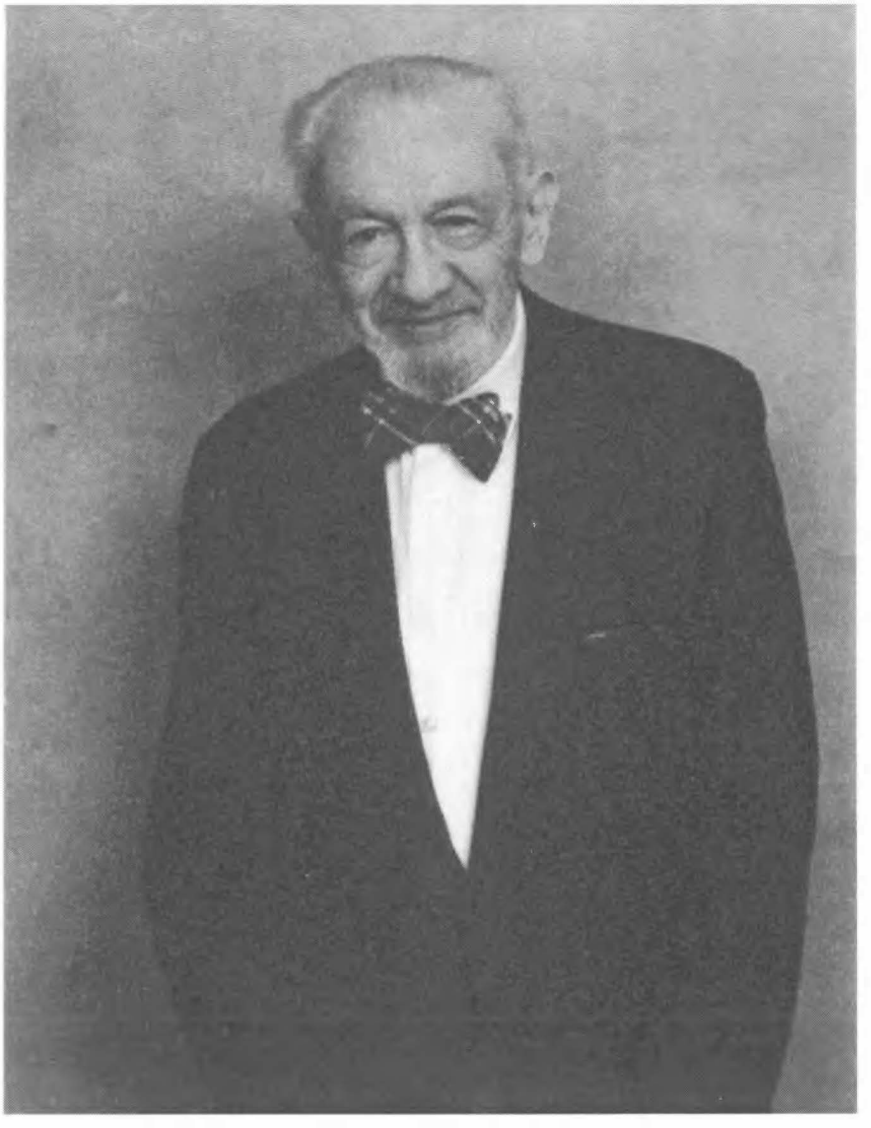
Chicago, Illinois



Herbert Elias Klarman

CHRONOLOGY

1916	Born December 21, Chmielnik, Poland
1929	Emigrated to New York City
1939	Columbia University, New York City, AB
1941	University of Wisconsin, Madison, MA
1941-1942	United States Treasury Department, Washington, DC, Division of Tax Research Assistant Economist
1942-1946	United States Army Private to Captain Assistant Director, Resource Analysis Division, Office of the Surgeon General of the Army
1946	University of Wisconsin, Madison, PhD, Public Finance
1946-1947	United States Department of Commerce, Washington, DC, National Income Division Economic Analyst
1947-1948	Brooklyn College, Brooklyn, NY, Department of Economics Assistant Professor
	Columbia University, New York City, Committee on Nursing Profession Consultant
1948-1949	Columbia University, New York City, New York State Hospital Study Assistant Director
1949-1962	Hospital Council of Greater New York, New York City Assistant Director, 1949-1951 Associate Director, 1952-1962
1951-1952	Executive Office of the President, Washington, DC, National Security Resources Board Medical Economist
1952-1954	Columbia University, New York City, National Manpower Council Consultant

CHRONOLOGY (continued)

1956-1957	United States Department of Defense, Washington, DC Consultant
1959-1960	Federation of Jewish Philanthropies, New York City Consultant
1961-1962	City of New York, Interdepartmental Health Council Consultant
1962-1969	Johns Hopkins University, Baltimore, MD, Departments of Public Health Administration and Political Economy Associate Professor and Professor
1966-1977	Social Security Administration, Washington, DC Consultant
1966	City University of New York, New York City, Urban Economics Research Project Consultant
1966-1968	World Health Organization, European Region, Copenhagen, Denmark Consultant
1967-1970	National Institute of Mental Health, Rockville, MD Consultant
1969	National Center for Health Services Research, Rockville, MD Consultant
1969-1970	State University of New York Downstate Medical Center, Brooklyn, Department of Environmental Medicine and Community Health Professor
1970	World Health Organization, Geneva, Switzerland Consultant
1970-	New York University, New York City, Graduate School of Public Administration Professor of Economics, 1970-1982 Professor Emeritus, 1982-

CHRONOLOGY (continued)

1971	Center for Communicable Diseases, Atlanta, GA Consultant
1972-1973	Robert Wood Johnson Foundation, Princeton, NJ Consultant
1972-1973	United States Bureau of the Census, Washington, DC, Historical Statistics Consultant
1973	Department of Health, Education, and Welfare, Washington, DC, Panel to Review Rand Health Insurance Experiment Consultant
1976	United States Congress, Washington, DC, Office of Technological Assessment Consultant
1976-1977	National Center for Health Services Research, Rockville, MD Senior Researcher
1982-	Johns Hopkins University, Baltimore, MD, School of Hygiene and Public Health Senior Associate

MEMBERSHIPS AND AFFILIATIONS

American Economic Association
Organizer, Session on Medical Economics at Annual Meeting, 1950
Member

American Hospital Association
Advisory Committee on Health, Member, 1976-1978

American Statistics Association
Member

Community Council of New York
Member, Interagency Research Committee, 1956-1960;
Chairman, Interagency Research Committee, 1960-1961

First Conference on Research in Health Economics
Member, Planning Committee, 1961-1963

Institute of Medicine
Member, Committee on Health Maintenance Organizations, 1973-1974

International Journal of Health Services
Editorial Board, Member, 1974-1980

Johns Hopkins University, School of Hygiene and Public Health, Society of Hygiene
President, 1966-1967

Milbank Memorial Fund
Chairman, Committee on Readings in Health Planning, 1976-1977

Milbank Memorial Fund Quarterly
Member, Editorial Board, 1972-1976

National Air Pollution Control Administration
Member, National Air Quality Criteria Advisory Committee, 1967-1970

National Center for Health Services Research and Development
Member, Publications Advisory Board, 1969-1971

National Institutes of Health
Member, Health Services Research Study Section, 1962-1966

MEMBERSHIPS AND AFFILIATIONS (continued)

National Research Council
Member, Committee on Health Care Resources in the Veterans Administration, 1973-1977

National Urban League
Member, Advisory Panel on Health, 1969-1973

New York Health Services Research and Policy Seminar
Member, Executive Committee, 1970-1973

Office of the Governor of New York State
Member, Health Advisory Council, 1975-1981

Royal Economic Society
Member

Second National Conference on Health Economics Research
Chairman, Planning Committee, 1967-1970

Third National Cancer Survey
Chairman, Medical Economics Advisory Committee, 1969-1972

United States Bureau of the Budget
Member, Committee on Chronic Kidney Disease, 1966-1967

United States Department of Health, Education, and Welfare
Member, Committee on Comprehensive Services for Children, 1966
Member, Advisory Committee on Medicare and Medicaid Estimates, 1970-1971

United States National Committee on Vital and Health Statistics
Member, 1967-1971

Veterans Administration
Member, Special Medical Advisory Group, 1977-1981

White House Task Force on Facilities for the Aged
Member, 1966

AWARDS AND HONORS

American Association for the Advancement of Science
Fellow

American Public Health Association
Fellow

Association for Health Services Research
Distinguished Career Award in Health Services Research, 1989

Columbia College
Pulitzer Scholar, 1935-1939
Greene Prize, 1939

Guggenheim Foundation
Fellow, 1976-1977

Institute of Medicine, National Academy of Sciences
Member, 1971

Norman Welch Prize in Medical Economics, 1965

Phi Beta Kappa, 1938

University of Wisconsin
University Scholar, 1939-1941

PUBLISHED WORKS *

Books and Monographs

✔ 1. Klarman, H. E., editor and contributor. *Empirical Studies in Health Economics*. Baltimore: Johns Hopkins Press, 1970.

2. Graduate Program in Hospital and Health Administration. *Medical Economics: Essays and Articles*. Iowa City: University of Iowa, 1969. (Informal collection of papers, published and unpublished.)

3. National Commission on Community Health Services. *Financing Community Health Services and Facilities. Report of the Task Force on Financing Community Health Services and Facilities*. Washington, DC: Public Affairs Press, 1967.

✔ 4. Klarman, H. E. *The Economics of Health*. New York City: Columbia University Press, 1965.

✔ 5. Klarman, H. E. *Hospital Care in New York City. The Roles of Voluntary and Municipal Hospitals*. New York City: Columbia University Press, 1963.

6. Klarman, H. E. *Background, Issues and Policies in Health Services for the Aged in New York City*. New York City: Interdepartmental Health Council of the City of New York, 1962.

7. Klarman, H. E. *Emergency Ambulance Service in New York City*. New York City: Hospital Council of Greater New York, 1950.

8. Klarman, H. E. *Income Taxation in the States*. Madison: University of Wisconsin, 1946. (Ph.D. dissertation.)

9. Numerous reports and articles, unsigned, for the Hospital Council of Greater New York.

* See p. 115 for explanation of check marks.

PUBLISHED WORKS (continued)

Articles and Chapters in Books

1. Klarman, H. E. Health insurance in the United States. *Lancet.* 2(8405):754, Sep. 29, 1984. (Letter.)

✔ 2. Klarman, H. E. Op-ed pieces in the Opinion/Commentary page of *The Baltimore Sun.* On social security reform, Jun. 28, 1985; on income tax reform, Sep. 26, 1986; on catastrophic health insurance, Sep. 5, 1989.

3. Klarman, H. E. Commentary on Rufus Rorem's writings on financing and planning. In: *Quest for Certainty.* Ann Arbor, MI: Health Administration Press, 1982.

✔ 4. Klarman, H. E. The road to cost-effectiveness analysis. *Milbank Memorial Fund Quarterly. Health and Society.* 60(4):585-603, Fall 1982.

✔ 5. Klarman, H. E. Training requirements for health services research: How many? At what skill level? From which disciplines? *Health Services Research.* 16(3):259-66, Fall 1981.

6. Klarman, H. E. Measuring economic effects of biomedical innovation. In: Roberts, E., Levy, R., Finkelstein, S., and others, editors. *Biomedical Innovation.* Cambridge, MA: Massachusetts Institute of Technology, 1981, pp. 219-54.

✔ 7. Klarman, H. E. Economics of health. In: Clark, D. W., and MacMahon, B. W., editors. *Preventive Medicine.* Second edition. Boston: Little Brown, 1981, pp. 603-15.

8. Klarman, H. E. Health care financing. In: Clark, D. W., and MacMahon, B. W., editors. *Preventive Medicine.* Second edition. Boston: Little Brown, 1981, pp. 617-33.

9. Klarman, H. E. Moderator's remarks: Panel No. 2 on reimbursement and financing. In: *Proceedings of the Conference on the Changing Needs of Nursing Home Care.* Philadelphia: American College of Physicians, 1980, pp. 95-97.

10. Klarman, H. E. Future directions and summary: research efforts applicable to state and regional planning in dentistry. In: Brown, L. J., and Winslow, J. E., editors. *Modeling Techniques and Applications in Dentistry.* Washington, DC: U.S. Government Printing Office, 1981, pp. 205-11.

11. Klarman, H. E. Observations on health services research and health policy analysis. *Milbank Memorial Fund Quarterly. Health and Society.* 58(2):201-16, Spring 1980.

PUBLISHED WORKS (continued)

12. Klarman, H. E. Some alternative approaches to health planning and regulation. *Health Communications and Informatics*. 5(5-6):339-50, 1979.

13. Klarman, H. E. Health economics and health economics research. *Milbank Memorial Fund Quarterly. Health and Society*. 57(3):371-79, Summer 1979.

14. Klarman, H. E. Observations on health care technology: measurement, analysis and policy. In: Altman, S., and Blendon, R., editors. *Medical Technology: The Culprit Behind Health Care Costs? Proceedings of the 1977 Sun Valley Forum on National Health.* Washington, DC: U.S. Government Printing Office, 1979, pp. 273-91.

15. Klarman, H. E. Transitional and long-range implications of reduction in house staff in New York City hospitals. In: *Proceedings of the Health Policy Forum on Foreign Medical Graduates in New York City*. New York City: United Hospital Fund, 1979, pp. 73-95.

16. Klarman, H. E. Review of Ronald Andersen, Joanna Lion, and Odin W. Anderson, *Two Decades of Health Services: Social Survey Trends in Use and Expenditure*. *Journal of Economic Literature*. 15(4):1388-90, Dec. 1977.

✔ 17. Klarman, H. E. Health planning: progress, prospects and issues. *Milbank Memorial Fund Quarterly. Health and Society*. 56(1):78-112, Winter 1978.

18. Klarman, H. E. Summary and follow-up action. In: *Selected Papers: Key Issues in Hospital Financing*. Albany, NY: Hospital Association of New York State, 1977, pp. 25-31.

✔ 19. Klarman, H. E. Planning for facilities. In: Ginzberg, E., editor. *Regionalization and Health Policy*. Washington, DC: U.S. Government Printing Office, 1977, pp. 25-36.

✔ 20. Klarman, H. E. The financing of health care. *Daedalus*. 106(1):215-34, Winter 1977.

20a. Klarman, H. E. The financing of health care. Reprinted in: Knowles, J. H., editor. *Doing Better and Feeling Worse. Health in the United States.* New York City: Norton, 1977, pp. 215-34.

21. Klarman, H. E. Comparison of health-care costs and expenditures for Western European and North American countries. In: Hu, T. W., editor. *International Health Costs and Expenditures*. Washington, DC: U.S. Government Printing Office, 1976, pp. 339-61.

✔ 22. Klarman, H. E., and Guzick, D. Economics of influenza. In: Selby, P., editor. *Influenza: Virus, Vaccines and Strategy*. New York City: Academic Press, 1976, pp. 255-68.

PUBLISHED WORKS (continued)

23. Klarman, H. E. Comment on Robert G. Evans, *Beyond the Medical Marketplace: Expenditure, Utilization and Pricing of Insured Health in Canada.* In: Rosett, R. N., editor. *The Role of Health Insurance in the Health Services Sector.* New York City: National Bureau of Economic Research, 1976, pp. 493-99.

24. Klarman, H. E. *Prospective vs. Retrospective Reimbursement of Nursing Homes.* Paper prepared for Second Conference on Long-Term Care Reimbursement sponsored by National Center for Health Services Research, Albany, NY, Nov. 24-25, 1975.

✔ 25. Klarman, H. E. *National Policy and Local Planning for Health Services.* William D. Bryant Lecture, University of Missouri-Columbia, Jan. 21, 1975. Published as pamphlet by W. D. Bryant Memorial Lecture Committee, 1975.

25a. Klarman, H. E. National policies and local planning for health services. *Milbank Memorial Fund Quarterly. Health and Society.* 54(1):1-28, Winter 1976. (Revised version.)

26. Klarman, H. E. Problems and issues in health care organization, delivery and financing. In: U.S. House of Representatives, Committee on Ways and Means, Subcommittee on Health. *Panel Discussions on National Health Insurance, July 24, 1975.* Washington, DC: U.S. Government Printing Office, 1975, pp. 263-67.

27. Klarman, H. E. *The Place of Proprietary, Public and Voluntary Institutions in the Provision of Care for the Elderly.* Statement prepared for a public hearing conducted by the New York State Moreland Act Commission on Nursing Homes and Residential Facilities, Mar. 24, 1975.

28. Klarman, H. E. The economic determinants of health care expenditures. In: Ehrlich, D. A., editor. *The Health Care Cost Explosion. Which Way Now?* Bern, Switzerland: Hans Huber, 1975, pp. 7-17.

29. Klarman, H. E. Foreward. In: Rafferty, J., editor. *Health Manpower and Productivity.* Lexington, MA: Lexington Books, 1974, pp. xiii-xix.

30. Klarman, H. E. Which health care system(s) for the United States? Panel discussion. *Transactions. Society of Actuaries.* 26(2):D10-14, 1974.

31. Klarman, H. E. What kind of health insurance should the United States choose? In: Morreale, J. C., editor. *The U.S. Medical Care Industry: The Economist's Point of View.* Ann Arbor, MI: University of Michigan, Graduate School of Business Administration, 1974, pp. 93-108.

PUBLISHED WORKS (continued)

✔ 32. Klarman, H. E. Major public initiatives in health care. *The Public Interest.* 34:106-23, Winter 1974.

33. Klarman, H. E. Economic aspects of mental health delivery service systems. In: Freedman, A. M., Kaplan, H. I., and Sadock, B. J., editors. *Comprehensive Textbook of Psychiatry, Volume 2.* Second edition. Baltimore: Williams and Wilkins, 1974, pp. 2506-11.

34. Klarman, H. E. *Three Proposals.* Statement prepared for hearings of the Subcommittee on Public Health and Environment of the House Committee on Interstate and Foreign Commerce, Dec. 12, 1973, pp. 220-23.

35. Klarman, H. E. Approaches to health manpower analysis, with special reference to physicians. *American Economist.* 17(2):137-42, Fall 1973.

✔ 36. Klarman, H. E. Application of cost-benefit analysis to health systems technology. In: Collen, M. F., editor. *Technology and Health Care Systems in the 1980's. Proceedings of a Conference, January 19-21, 1972, San Francisco, CA.* Washington, DC: U.S. Government Printing Office, 1973, pp. 225-50.

36a. Klarman, H. E. Application of cost-benefit analysis to health systems technology. *Journal of Occupational Medicine.* 16(13):172-86, Mar. 1974. (Reprint.)

36b. Klarman, H. E. Application of cost-benefit analysis to the health services and the special case of technologic innovation. *International Journal of Health Services.* 4(2):325-52, Spring 1974. (Revised version, most frequently cited.)

37. Klarman, H. E., Abel-Smith, B., Ekholm, L., and others. Can we reduce the cost of medical education? *WHO Chronicle.* 26(10):441-50, Oct. 1972.

✔ 38. Klarman, H. E. Analysis of the HMO proposal: its assumptions, implications, and prospects. In: *Thirteenth Annual Symposium on Hospital Affairs, Health Maintenance Organizations: A Reconfiguration of the Health Services System.* Chicago: University of Chicago, Center for Health Administration Studies, 1971, pp. 24-38.

✔ 39. Klarman, H. E. What school can teach about health services planning. *International Journal of Health Services.* 1(2):154-65, May 1971.

40. Klarman, H. E. Needs in health services resources and utilization data. In: *Report on the Twentieth Anniversary Conference of the United States National Committee on Vital and Health Statistics.* Published as: *Vital and Health Statistics.* Series 4(13):21-24, 1970.

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41. Klarman, H. E. Implications of the changing social and economic environment for the public hospital. In: International Health Advisory Council. *Symposium: The Organization, Administration, and Financing of Public Hospitals and Public Health Facilities*. New York City: Booz, Allen & Hamilton, 1970, pp. 41-44.

✔ 42. Klarman, H. E. Economic research in group medicine. In: Beamish, R. E., editor. *New Horizons in Health Care*. Winnipeg, Manitoba: Congress on Group Medicine, 1970, pp. 178-93.

✔ 43. Klarman, H. E., Rice, D. P., Cooper, B. S., and others. *Sources of Increase in Selected Medical Care Expenditures, 1929-69*. Washington, DC: Social Security Administration, Office of Research and Statistics, 1970. Staff Research Paper 4.

44. Klarman, H. E., Rice, D. P., Cooper, B. S., and others. Accounting for the rise in selected medical care expenditures, 1929-1969. *American Journal of Public Health.* 6(6):1023-39, Jun. 1970.

45. Klarman, H. E. Comment on Mark V. Pauly and David F. Drake, *Effects of Third Party Methods of Reimbursement on Hospital Performance.* In: Klarman, H. E., editor. *Empirical Studies in Health Economics*. Baltimore: Johns Hopkins Press, 1970, pp. 315-19.

✔ 46. Klarman, H. E. Trends and tendencies in health economics. In: Klarman, H. E., editor. *Empirical Studies in Health Economics*. Baltimore: Johns Hopkins Press, 1970, pp. 3-14.

47. Klarman, H. E. Increase in the cost of physician and hospital services. *Inquiry*. 7(1):22-36, Mar. 1970.

✔ 48. Klarman, H. E. Reimbursing the hospital: the difference the third party makes. *Journal of Risk and Insurance*. 36(5):553-66, Dec. 1969.

49. Klarman, H. E. Comments on papers 1 and 2. Canadian American Conference on Hospital Programs. *Medical Care*. 7(6):29-33, Nov.-Dec. 1969.

50. Klarman, H. E. Review of *Commitment to Welfare* by Richard M. Titmuss. *Journal of Economic Literature*. 7(2):481-83, Jun. 1969.

51. Klarman, H. E. Economic aspects of projecting requirements for health manpower. *Journal of Human Resources*. 4(3):360-76, Summer 1969.

✔ 52. Klarman, H. E. Approaches to moderating the increases in medical care costs. *Medical Care*. 7(3):175-90, May-Jun. 1969.

PUBLISHED WORKS (continued)

53. Klarman, H. E. Comment on Melvin W. Reder's *Some Problems in the Measurements of Productivity in the Medical Care Industry.* In: Fuchs, V. R., editor. *Production and Productivity in the Service Industries.* New York City: National Bureau of Economic Research, 1969, pp. 132-39.

54. Klarman, H. E. Economic aspects of mental health manpower. In: Arnhoff, F. N., Rubinstein, E. A., and Speisman, J. C., editors. *Manpower for Mental Health.* Chicago: Aldine Publishing, 1969, pp. 67-92.

55. Klarman, H. E. The contribution of health services to economic growth and well being. In: Joint Economic Committee, Congress of the United States. *Federal Programs for the Development of Human Resources. A Compendium of Papers. Volume 2.* Washington, DC: U.S. Government Printing Office, 1968, pp. 451-68.

✔ 56. Klarman, H. E., Francis, J. O'S., and Rosenthal, G. D. Cost effectiveness analysis applied to the treatment of chronic renal disease. *Medical Care.* 6(1):48-54, Jan.-Feb. 1968.

✔ 57. Klarman, H. E. Sources of mechanisms of financing patient care. In: *Report of the Committee on Chronic Kidney Disease.* Washington, DC: U.S. Bureau of the Budget, 1967, pp. 85-100.

58. Klarman, H. E. Occupancy is not right measure of hospital efficiency. *Modern Hospital.* 109(6):95-96, Dec. 1967.

59. Klarman, H. E. Present status of cost-benefit analysis in the health field. *American Journal of Public Health.* 57(11):1948-53, Nov. 1967.

60. Klarman, H. E. Economic factors in hospital planning in urban areas. *Public Health Reports.* 82(8):721-28, Aug. 1967.

61. Klarman, H. E. Discussion of papers on economics of health. *American Economic Review.* 57(2):151-55, May 1967.

62. Klarman, H. E. Financing health and medical care. In: Clark, D. W., and MacMahon, B., editors. *Preventive Medicine.* Boston: Little Brown and Co., 1967, pp. 741-79.

63. Klarman, H. E. What the health insurance pacesetters are doing after Medicare. In: *The 1966 Blue Shield Annual Program Conference.* Chicago: The Association, 1966, pp. 47-52.

PUBLISHED WORKS (continued)

64. Klarman, H. E., Clark, D. W., Hofstra, R., and others. Health services research in Scandinavia. *Milbank Memorial Fund Quarterly. Health and Society.* 44(4, Part 2): 229-61, Oct. 1966.

65. Klarman, H. E. Medicare and the future role of government. In: *The 1965 Blue Shield Annual Program Conference.* Chicago: The Association, 1965, pp. 69-76.

66. Klarman, H. E. Report on Conference on the Economics of Medical Research. In: *Report of President's Commission on Heart Disease, Cancer and Stroke, Volume 2.* Washington, DC: U.S. Government Printing Office, 1965, pp. 631-44.

67. Klarman, H. E. The case for public intervention in financing health and medical services. *Medical Care.* 3(1):59-62, Jan.-Feb. 1965.

68. Klarman, H. E. Socioeconomic impact of heart disease. In: Second National Conference on Cardiovascular Diseases. *The Heart and Circulation, Volume 2.* Washington, DC: U.S. Government Printing Office, 1965, pp. 631-44.

✔ 69. Klarman, H. E. Syphilis control programs. In: Dorfman, R., editor. *Measuring Benefits of Government Investments.* Washington, DC: Brookings Institution, 1965, pp. 367-410.

✔ 70. Klarman, H. E. Some technical problems in areawise planning for hospital care. *Journal of Chronic Disease.* 17(9):735-47, Sep. 1964. Reprinted in: White, K. L., editor. *Medical Care Research.* London: Pergamon Press, 1965, pp. 11-24.

✔ 71. Klarman, H. E. The increased cost of hospital care. In: Mushkin, S. J., editor. *The Economics of Health and Medical Care.* Ann Arbor, MI: University of Michigan, 1964, pp. 227-54.

72. Klarman, H. E. Discussion of papers on medical care spending and the demand for general hospital facilities. In: *1964 Social Statistics Proceedings.* Washington, DC: American Statistical Association, 1964, pp. 106-8.

73. Klarman, H. E. On hospitals. *New Republic, Supplement on Health.* 149(19, Issue 2555) :5-8, Nov. 9, 1963.

✔ 74. Klarman, H. E. Effect of prepaid group practice on hospital use. *Public Health Reports.* 78(11):955-65, Nov. 1963.

✔ 75. Klarman, H. E. The distinctive economic characteristics of health services. *Journal of Health and Human Behavior.* 3(1):44-49, Spring 1963.

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76. Klarman, H. E. Role of philanthropy in hospitals. *American Journal of Public Health.* 52(8):1227-37, Aug. 1962.

77. Klarman, H. E. Panel on methods suggested for controlling hospital use through organization of medical services. In: *Fifth Annual Symposium on Hospital Affairs: Where Is Hospital Use Headed?* Chicago: University of Chicago, Graduate Program in Hospital Administration, 1962, pp. 55-63.

78. Klarman, H. E. Health insurance for aged poses problems. *Modern Hospital.* 90(1):63-64, Jan. 1958.

79. Klarman, H. E. Transformation in medical bills. *Challenge.* 6(1):13-16, Oct. 1957.

80. Klarman, H. E. Medical care costs and voluntary health insurance. *Journal of Insurance.* 24(1):23-41, Sep. 1957.

81. Klarman, H. E. Review of Brian Abel-Smith and R.M. Titmuss, *The Cost of the National Health Service in England and Wales. Review of Economics and Statistics.* 39(3) :353-55, Aug. 1957.

82. Klarman, H. E. Review of J. Frederic Dewhurst, *America's Needs and Resources. Reporter.* 12(13):45-46, Jun. 30, 1955.

83. Klarman, H. E. Depreciation in hospital accounting. *Modern Hospital.* 82(1):85-87, Jan. 1954.

84. Klarman, H. E. Physicians. In: National Manpower Council. *A Policy for Scientific and Professional Manpower.* New York City: Columbia University Press, 1953, pp. 219-41.

85. Klarman, H. E. Economic problems of the voluntary general hospital. *Hospitals.* 26(10):56-59, Oct. 1952.

86. Klarman, H. E. The economics of hospital service. *Harvard Business Review.* 29(5):71-89, Sep. 1951.

✔ 87. Klarman, H. E. Requirements for physicians. *Papers and Proceedings. American Economic Review.* 41(2):633-45, May 1951.

✔ 88. Klarman, H. E. Economic aspects of hospital care. *Journal of Business.* 24(1):1-24, Jan. 1951.

PUBLISHED WORKS (continued)

89. Klarman, H. E. Suggestions for a rate policy for voluntary hospitals. *Modern Hospital.* 74(6):73-76, Jun. 1950.

✔ 90. Klarman, H. E. Do hospitals need cost accounting to solve efficiency and rate-making problems? *Journal of Accountancy.* 89(5):396-99, May 1950.

91. Klarman, H. E. Review of American Nurses' Association inventory of professional registered nurses, 1949. *American Journal of Nursing.* 50(3):38-39, Mar. 1950.

92. Klarman, H. E. Prerequisites for research in nursing. *American Journal of Nursing.* 49(12):780-82, Dec. 1949.

93. Klarman, H. E. Income tax deductibility: a rejoinder. *National Tax Journal.* 2(1):89-90, Mar. 1949.

94. Klarman, H. E. Income tax deductibility. *National Tax Journal.* 1(3):241-49, Sep. 1948.

95. Klarman, H. E., and Ginzberg, E. The paraplegic. *Bulletin of the U.S. Army Medical Department.* 7(10):892-97, Oct. 1947.

96. Klarman, H. E. A statistical study of income differences among communities. In: Conference on Research in Income and Wealth. *Studies in Income and Wealth,* Volume 6. New York City: National Bureau of Economic Research, 1943, pp. 206-26.

97. Klarman, H. E. Numerous *Bulletins* for the Hospital Council of Greater New York.

KLARMAN:

I have prepared an outline.

NEUHAUSER:

Excellent!

KLARMAN:

Maybe I'm over prepared.

NEUHAUSER:

I think this is wonderful.

KLARMAN:

Item number one: where I lived before coming to the United States and my education in this country. The second item is positions I've held, and the third item is my professional activities and consulting appointments. Fourth is selected publications. The fifth item is people I came in contact with in the course of the third item. Number six is reflections on the evolution of health economics. The final brief items are some policy views and some continuing technical concerns of mine. It is likely, as we proceed and as you intervene with questions, comments, and suggestions, that so formal a scheme will break down.

As my *Who's Who* entry notes, I was born in Poland in a small town, Chmielnik. It is located in what was Congress Poland, with close ties to the Tsar of Russia, as created by the Congress of Vienna in 1815. However, the Yiddish we spoke was close to Galicia, which belonged to the Austro-Hungarian empire. In 1916, during World War I, Chmielnik was occupied by the German army.

Now something occurs to me that I've never thought of before, namely, how did one get medical care in Chmielnik? There was no doctor; there was a feldsher. I did have some medical

problems, and for them my mother and I traveled to Kielce and even to Warsaw. My eye doctor was in Warsaw. One of the odd, fortuitous coincidences, it turned out, was that the eye doctor I saw in Warsaw was also the eye consultant to the American Embassy. This was most fortunate when the time came for me to emigrate to the United States and join my father, for I had a severe case of chronic conjunctivitis and our worry was that the condition was trachoma. That would mean you were not to be admitted; it could have been a most serious matter. This doctor's prior diagnosis of conjunctivitis was a very lucky break for me and my family.

We arrived in New York City at the end of May 1929. I was then twelve and a half years old. I did not know any English, and really didn't start learning it until that fall when school began. We spent the summer on the lower East Side of Manhattan, where my father had an apartment at 66 Avenue D. He had emigrated to the United States in 1923; this was his third try. It took five years minimum for him to become a citizen, and he acted with dispatch for we arrived here within five and a half years; this was unusually fast. By September, when I entered school, we had moved to Brownsville, in Brooklyn.

I attended Junior High School 109, also known as the Ida and Isadore Straus Junior High School. The Strauses, members of the family who owned Macy's, went down on the Titanic. My sister and I were assigned to a special class for foreigners, which I attended for 12 weeks. It was very interesting; all kinds of languages were spoken by the students. The teacher knew only English, and how she managed to teach us I don't know, but the class went well. After the midterm, I was transferred to class 6B, and there, I still maintain, I was treated with generosity. I remember getting both A for achievement and A for effort, and asking the teacher, how come? I didn't think I was up to snuff on achievement, and she said, it's alright; you'll earn it next term. After that, I was assigned only to rapid advancement classes, so that the seventh and eighth

grades were each completed in one semester.

Some events stand out. In the fall of my second year, I participated in a current events contest broadcast on the radio, sponsored by *The Brooklyn Eagle*; I lost out on some sports questions. I promised myself never to let this happen again. That's when I started to read the sports pages. My second time in this competition I finished second in the borough of Brooklyn, which wasn't too bad considering that the first prize winner was in high school.

I did very well in school, graduating with several medals. I went on to high school in Brooklyn for one term, and then we moved to the Bronx, where I was assigned to the James Monroe High School.

James Monroe was not one of the leading high schools in New York City in 1932. It certainly wasn't bad, although recently it was on the city's list for closing. I would say, in retrospect, that I got an excellent education at James Monroe High School, plus lots of extracurricular opportunities. My program comprised four years of English including a one-term course in journalism, four years math, American history, European history, almost four years French, three years German, chemistry, physics, even a semester of economics. In fact, in my last year, teachers were competing for me, and so I had to skip the lunch hour. Also, there had been some difficulty with the school newspaper's staffing. A person would be appointed editor, and then he would flunk a couple of courses. So I became a foolproof candidate for editor-in-chief of the paper, editor-in-chief of the yearbook, president of the French club—you name it. I graduated with 9 or 10 medals, but barely passed swimming. Also, the speech instructor insisted that my accent was still heavy.

It was a very good time altogether. Teachers were extremely helpful. A few of them showed us around the city; I remember particularly a trip to the top of the Empire State Building

and one to *The New York Times* printing plant. [On the newspaper, *The Monroe Mirror*, I gained several longtime friends.]

Somewhere along the line, from the sister of a classmate in junior high school, I had learned about the Pulitzer Scholarship. Today, the name Pulitzer is associated with all those distinguished prizes, but the Pulitzer Scholarship had nothing to do with the literary and journalism prizes. The Pulitzer Scholarship was awarded annually to 9 or 10 graduates of the public high schools in New York City. It carried free tuition at Columbia College, then $400 a year, plus $250 annually in cash. In the 1930s, Columbia College ranked high among Ivy League colleges. I didn't go there directly; I stopped off at City College for one semester because I couldn't count on winning the Pulitzer. It required high grades, plus extracurricular activities. In addition, one had to take college entrance exams, which was unusual at that time. Otherwise, we just took the New York State Regents' exams. The Regents gave me a prize of $100 in cash a year. Between 1935 and 1939, I was living at home in the Bronx and earning $350 a year in cash, while attending college full-time on scholarships.

NEUHAUSER:

That was a lot of money in the 1930s.

KLARMAN:

Yes. I was doing very well; no question about it. I found Columbia College somewhat strange, but also congenial. I liked it better than City College, which I found loud and almost confrontational; students were always competing in class. That was not done at Columbia. Also the Columbia College registrar, my adviser, afforded me wide latitude. Sure, Columbia had some prescribed courses, but one could always opt out by taking and passing exams. So I opted out of quite a few required courses, and was allowed to follow my own bent. For example, I took

two years of accounting at the Columbia Business School, which at the time was an undergraduate school. That came in handy later on. I took a large amount of economics, both undergraduate and graduate courses. I took statistics. I took philosophy. I took literature and chemistry, but not biology, I'm sorry to say. Teachers were very helpful, supportive in many ways. Several of them advised me, in my senior year, to try to get away from the New York area because my foreign accent was still heavy and sometimes hard to understand. That's how I picked Madison, Wisconsin, for graduate school in economics. Financially, I would have been better off at Cornell in terms of fellowship money and free tuition. In retrospect, I think Wisconsin helped me a lot; that's where I met some of my lifelong friends.

To my surprise, one of the items omitted from my curriculum vitae was that, upon graduation from Columbia College, I won the Greene Prize, first in my class. I didn't expect it. It so happened that my parents in the candy store learned about it before I did, when they saw it in *The New York Times* that morning. I learned about it at the graduation ceremony.

I was a junior year Phi Beta Kappa. That provided an interesting experience, because junior Phi Beta Kappas participated in the election of senior Phi Beta Kappas. That was the first time I was involved in such an activity jointly with faculty. I remember that one student had transferred from Princeton to Columbia after two years, and the faculty was strongly opposed to giving that person a Phi Beta Kappa key, despite high grades because, instead of moving on and taking junior and senior year courses at Columbia, he had repeated freshman and sophomore courses. The faculty won that issue. This was a most interesting and novel experience for me.

NEUHAUSER:

That was the golden age of high school education in New York. There were probably notable people in your class who may have become famous in other areas.

KLARMAN:

I don't remember anybody from my high school class who became famous. To me, what was notable in the high school system at that time in New York City was the quality of teaching and the instructors that the city was able to attract. Due to the Depression, private sector jobs were scarce. So, some of my teachers were excellent. In fact, the quality of my French teachers in high school was so much better than at Columbia College that, after taking it for one term in college, I dropped it. In high school, we translated from English to French, while in college, it was from French to English, which is much easier. In high school, I read the great dramatic classics in French.

An interesting aspect of the faculty in college was how many of them were part-timers, and even among the full-timers some were only instructors, that is, not on the tenure track. This was particularly true of the Jewish faculty members. I can't pinpoint just when after World War II academia opened its doors to Jews, but I believe it began in the late 1940s and early 1950s. As far as I could tell, in the 1930s, each university that I knew anything about had one Jew per department. We'll get to more on this later.

In fact, if not for my foreign accent, my ambition at that time would have been to teach in a New York City high school. But my foreign accent disqualified me; the Board of Education would not hire anyone with an accent. So I went to Wisconsin, where it turned out that some of my best friends were from New York. An interesting coincidence was that, in the year 1939, when I moved to Madison, Wisconsin, the young Milton Friedman also did. I had had him for a course in economic theory at Columbia.

NEUHAUSER:

Was he an instructor at Columbia?

KLARMAN:

Only part-time. He was principally at the National Bureau of Economic Research, founded by Wesley Claire Mitchell of Columbia and then established in New York. I don't know whether he was working on his dissertation at that time or whether he had already finished it; it was published as the study of professional incomes, coauthored by Simon Kuznets, also a future Nobel prize winner in economics.

NEUHAUSER:

I think 1938 was the date he finished it. But as you know, people work on their dissertations for years.

KLARMAN:

Friedman couldn't have been working on it for years because he was still a young man.

At Wisconsin, I majored in public finance, which at that time was still mainly about taxation, rather than expenditures or fiscal policy. My second field was statistics. That was a fairly weak area, since at Columbia I'd had a year with Frederick Mills and a couple of years with Harold Hotelling before he moved to North Carolina. One of the people I met in a Hotelling class, believe it or not, was Abraham Wald, then learning English; he had just arrived from Vienna. Another statistician you may know, Jacob Mosak, was in that class. I would judge the statistics classes at Wisconsin as inferior. In general, I think there was perhaps a letdown in economics at Wisconsin after their accomplishments in the 1920s and the early 1930s; John R. Commons was no longer active.

I also took the required minor sequence. I chose political science—a year each in the history of political thought and in constitutional law. That brought me into contact with the Law School, which was a very nice experience. Having taken theory with Friedman at Columbia, I

didn't take the general theory course at Wisconsin. Instead, I took a microeconomics course with Jim Earley; it wasn't as good as Friedman's. At Wisconsin, Friedman taught a course in income and wealth, which I took. In fact, I wrote a term paper, which led to publication in the National Bureau's series on income and wealth. Friedman had devised his own adaptation of the analysis of variance employing ranks. My paper applied this method. I don't know if you've come across that method in some other context.

NEUHAUSER:

No, I have not.

KLARMAN:

For that time, Friedman was an able, top-notch mathematician, in addition to his command of economics. It was a severe disappointment to many of us students at Wisconsin that Friedman was denied appointment as assistant professor. Now, as then, one cannot ascribe the reason or reasons with certainty: was it Friedman's relative youth, his laissez-faire policy views, maintenance of a Jewish quota, the charge of trickery in bringing him to Madison in the first place on a Rockefeller Foundation fellowship as technical adviser to the Wisconsin income study, the incessant factional squabbling among the economics faculty, or some combination of factors?

One of the things I remember best about Wisconsin was getting rather close to some members of the faculty, including Ed Witte in economics, who wrote the Social Security Act, and John Gaus in political science, before he moved to Harvard.

Another prominent person was Walter Heller, who was somewhat of a classmate, but not quite. He was a couple years ahead of me in graduate school. When I arrived in Madison, he was away for the year on a fellowship traveling among the states to study state income taxes. He

came back in my second year, and everybody knew him. He was married—not many of us were—and his was the house where we graduate students played poker. At the end of my second year, I spent the summer in Madison instead of going home, and took a couple of courses, one of which was a course in public finance taught by Heller. My outstanding recollection of the course was that, after the midterm exam, Walter read my exam paper to the class as an example of clear and terse answers, precisely to the point. That had never happened to me before. This gave me the feeling that maybe I was becoming competent in English, able to express myself clearly in writing, if not yet orally.

One of the skills I had acquired going back to college was taking complete notes in class. This became very handy later on in my career, but even at Columbia my notes were good enough to help a friend, who had to work and didn't always get to class, pass the course in public finance.

Another memorable item from my school years is some of the people I met at Columbia and at Wisconsin. I remember attending lunch with a small Columbia faculty group and the visiting Harold Lasky. I remember attending another small lunch with the historian Richard Hofstadter, then young and not so famous as he became. I must have heard Norman Thomas speak at Columbia numerous times; he was a superb public speaker.

At Wisconsin, a frequent visitor who gave seminars on the economics of socialism was Oskar Lange, then a professor at Chicago. We enjoyed many a beer at the Rathskeller. Unfortunately, he made the mistake after the war of first serving as ambassador to Washington from the new Polish government and then returning home to Warsaw, where he was given a hard time and isolated.

By the way, when we moved to the Bronx in the summer of 1932, my father quit his

factory job because his coworkers had lost a strike and he was too proud to go back. My parents bought a candy store. It just consumed their time and energies totally. It also meant that, even though I never seemed to do enough at the store, I had to go and fetch the Sunday papers Saturday night from a central point, and Sunday morning I delivered the papers to some customers. This gave me exposure to the front pages of the comics, news, and sports sections.

NEUHAUSER:

Have you continued to read the sports pages?

KLARMAN:

I still do.

NEUHAUSER:

I expect running a candy store was a full-time occupation. One practically lives there.

KLARMAN:

Yes. Indeed, my parents tried to outdo everybody, so instead of opening the candy store when other people did, they had to open at 6:00 or 5:00 a.m. Instead of closing the store at 11:00 or 12:00 p.m., they closed at 1:00 a.m. One of them always had to be in the store. I have an older sister, Yetta, who worked in the store. I'm not at all sure that my parents appreciated her services.

As previously mentioned, I made several lifelong friends at Wisconsin. One is Walter Heller. You will recognize the name of Joseph Pechman, later at the Brookings Institution and head of its economics programs and a leader in taxation, my closest lifelong friend.

NEUHAUSER:

Yes.

KLARMAN:

Richard Goode, who later worked at the Bureau of the Budget, Brookings, and the International Monetary Fund. Jesse Burkhead, who became a professor of economics at Syracuse. Maury Liebenberg, who worked at the U.S. Department of Commerce.

Having passed the Ph.D. comprehensives in the fall of 1941, I decided to take a job in Washington. I moved there in November 1941, one month before Pearl Harbor. I went to the Treasury Department because Harold Groves, my major professor, was conducting a study at the Treasury Department and offered me a job. I remember hearing about the attack on Pearl Harbor after I got off the bus in Arlington, Virginia. I was visiting an old friend from high school, Harold Kocin. At that time, working at the Treasury were my friends Milton Friedman and Walter Heller.

I was assigned to prepare a memorandum on income tax deductibility. You deduct state income tax liability on the federal tax return, and you may or may not be allowed to deduct the federal tax liability on the state tax return. I did write the memorandum, but as often happened in my career, its completion was delayed. I didn't get it done as promptly as Groves would have liked. In March 1942, I was invited by my draft board in the Bronx to appear. I did, and asked for postponement of my induction, which they were glad to grant. I joined the Army in August 1942, with the long memo completed.

Let me turn now to my military career. I may have been too long in describing my pre-occupational background, but mine was not the usual background, even in New York, and certainly not in the Midwest, which is where Mary, my wife, comes from. I think spending some time in Wisconsin was good for me, preparing me for life in the Army. It certainly did help attenuate my accent.

In the Army, I was lucky at just about every point. I entered the military, classified limited service, as a private. Limited service was due to faulty eyesight and flat feet. I showed up at the Reception Center at Camp Upton, which later became the site of Brookhaven Laboratory. Who is the person to interview me but Jim Rice, who had just married Dorothy Pechman. It's a name you know, Dorothy P. Rice. So he wrote me up. I remember his telling me later that his sergeant could hardly believe such a paragon existed.

From Camp Upton, I moved for basic training to Camp Lee in Virginia, a Quartermaster Corps post. How did I get there? What happened was that, while still at the Treasury Department, just before reporting to the Army, one Saturday morning—this was a time when we worked five and a half days—I decided on a hunch to call the Pentagon on the off chance that I might possibly be allowed to influence my service arm assignment. I told them who I was and gave my credentials. That's how I got to be assigned to the Quartermaster Corps. It wasn't the Reception Center at Camp Upton that assigned me; it was the Quartermaster Corps in the Pentagon.

I moved to Camp Lee, and there I ran into an interviewer who happened to be from the Bronx. He enjoyed the idea of breaking in somebody from the Bronx. What was the best school at Camp Lee? It was the warehousing school, and that's where I wound up. I never did get to see a warehouse. My regular Army sergeant and corporals in base camp couldn't have been nicer. They were very supportive, always looking out for my interests, and what they could do for me.

From there, I went to Officers Candidate School at the University of Florida in Gainesville for officer training. In Gainesville, one of my instructors was somebody I had met as an instructor in an accounting class at Columbia. He was a wise guy, and I was pretty much onto

him. When he asked a question, what you had to do was think of a sensible answer and then say the opposite. That's how I played him. I did very well, finished at the top of my class and was asked near graduation time where I would like to be assigned. Naturally, I said I'd love to move as close to home, New York City, as possible. So I was sent to Camp Upton, Long Island.

Here I was at Camp Upton, a second lieutenant in the Reception Center at Headquarters Company. That proved to be a mixed bag. The commanding officer, a captain, was not a pleasant man, but the other officers were nice. One had a car, so we could ride to New York City on weekends. One of the perks at Camp Upton was escorting troops to training centers in the south, maybe every month or every six weeks. You didn't get paid on the way down, traveling on the troop trains, but on the way back, you got eight cents a mile. Also, all trains passed through Washington, where you could stop off for a day or so. I had friends there. On one of the trips north, I was on the steps of the Treasury building, going to see if any of my friends were still there, and I ran into an old friend from Columbia, Isaiah Frank. "Herb, what are you doing these days? What are you up to?" he asked. I told him I was moving troops. "You ought to be doing something useful. Why don't you come and see me next time you're in Washington?" Within a month or so, I was in Washington again, and went to see him at the Office of Strategic Services, predecessor to the Central Intelligence Agency. Isaiah was in a large room with several people I knew. Harry Schwartz was there from my college days, also a Pulitzer scholar. Harry later became the Moscow correspondent on the Hudson for *The New York Times*, a member of its editorial board, and he also wrote on medicine. We became pretty much lifelong friends. I remember Moses Abromoviz was there, whom I had met as a guest lecturer at Columbia. The guys started talking, "What would be a good job for Herb?" And somebody said, "I know of a new job. Why doesn't he go and see Eli Ginzberg?" Eli had just established a new division at

the Office of the Surgeon General of the Army. Eli didn't remember me, but I remembered him because, as the gatekeeper to Wesley Claire Mitchell's classes, he had turned me down as a Columbia College undergraduate. "Why don't you go and take Milton Friedman's course in economic theory," he had advised me. This I did.

And who was then on Ginzberg's staff? Bill Schweitzer, a fellow student from Junior High School 109. It didn't require a long conversation for Eli to offer me a job. I went back to Camp Upton and waited for the transfer order. Maybe six weeks later, Eli calls up. "Where are you? Why aren't you in Washington?" I said, "I'm in the Army and need orders to move." Eli said, "Orders, shmorders." Anyway, he went ahead and did get the necessary orders. That's how I moved to Washington and entered the medical field professionally, without any prior preparation or training.

NEUHAUSER:

You have quite a story to tell of all of these connections and branch points in your life that seemed to have worked out.

KLARMAN:

Everybody—almost everybody—was so helpful.

NEUHAUSER:

You have quite a connection with memorable names.

KLARMAN:

Yes, there'll be more.

NEUHAUSER:

While in Chicago, I sat in on just a couple of classes taught by Milton Friedman. I was so impressed with him being a superb teacher, but that had been after a lifetime of honing his skills.

KLARMAN:

He had the skills as a young man. In the late 1930s, Friedman, George Stigler, and Allen Wallis were widely known as a triumvirate while they were still graduate students in economics.

NEUHAUSER:

They were graduate students at Columbia?

KLARMAN:

No, Chicago. They were well known, just as Paul Samuelson was known as a student at Harvard. Samuelson was rumored to have said to Professor Hansen in class, "That's wrong."

So, here I am in Eli Ginzberg's Division of Resources Analysis. We were first located at 1818 "H" Street Northwest, which was, believe it or not, a building destined for the Social Security Administration, but never occupied by it, and eventually taken over by the International Monetary Fund. Subsequently, we moved to the Pentagon.

Eli had a small staff. The Resources Analysis division had the assignment to help plan the Army's medical care system—essentially, its hospital care system in the United States, not for casualties treated abroad, but for casualties brought home. The Army operated a number of general hospitals in the United States.

Even then, Eli showed a remarkable ability to function at the highest levels. I still remember that one day, Dr. Eli Ginzberg, a civilian, escorted Major General Kirk, the Surgeon General of the Army, to see his superior, Lieutenant General Summerville, the commanding general of the Army Service Forces. Eli provided the link between them. The immediate superior over Eli was Colonel Albert Schwichtenberg. Under him was a Lieutenant Colonel McGiboney.

NEUHAUSER:

You mean the expert on hospital management?

KLARMAN:

Exactly, later in the Hill-Burton program. When you talk about making contacts: above the Colonel was Brigadier General Raymond Bliss, who later became Surgeon General, and further above him was Major General George Lull who was the Deputy Surgeon General. You know what General Lull's job was after the war?

NEUHAUSER:

No.

KLARMAN:

Executive vice-president of the AMA. He gave me easy entry into the AMA.

At the Resources Analysis division, Eli Ginzberg's unit, we hired several civil servants, and in every case, it was my belief, my assumption, that I would be working for them. It turned out that they would work for me, but that's not the way I expected it to be. One man who proved to be very capable—just a great staff person—was Isaac Cogan. He was one of those anonymous bureaucrats. He had a bit of a problem later on because, in his youth in Milwaukee, he had joined the Communist Party, as many others did in the 1930s.

I was lucky to have avoided that misstep. There were several reasons I can think of. One reason, which did not dawn on me then, was that, when I was still in Poland, I attended the synagogue with my grandfather every Friday night and Saturday morning, since my father was in America. After the synagogue service, I had dinner at my grandfather's. At my grandfather's table sat my grandmother and their nine children. One of the sons was an active Socialist Zionist, one was a Communist, and one son was too young. My grandfather himself was a

Mizrachi, a religious Zionist, but not a fundamentalist and certainly not a Chasid. The conversation around the dinner table was spirited and substantive. My socialist uncle happened to be a great favorite of mine, so I leaned in that direction.

Later on, at Columbia, I skipped the junior seminar in economics and took the senior seminar in which we read a book a week. We also talked a good deal outside the assigned books. One of the discussions I remember was about the sort of pseudotrials that John Dewey conducted on the crimes of Stalin against Trotsky. Thus, I was never drawn toward the Communist party. Isaac Cogan, who lived in Milwaukee in the 1930s, apparently did join. He was so warm a person and a good worker that, when the time came to rescue him from the McCarthyites in the early 1950s, both Eli Ginzberg and General Bliss stood up for him, and he kept his job.

A couple of other senior people we hired didn't work out well; that's the way it goes.

I recall that one of the secretaries in the division objected to doing personal work for me in filling out the monthly voucher for my Army paycheck. I told her, "You either fill it out or you can go elsewhere. I cannot fill it out because I can't type." Why can't I type? Because in high school I was not allowed to take typing. That was reserved for the commercial course, not the academic course.

I remember being loyally supported in that office, in a number of respects. We worked hard and we worked late hours. One day, inadvertently, I left the office safe opened; that was considered a serious offense. But I was protected by my superiors on the ground that I must have been very tired at the end of the day. "We'll just have to overlook it."

From time to time, I was sent on short "inspection" trips. These excursions took me to Newport, Rhode Island, Atlantic City, Virginia Beach, Daytona Beach—you get the idea. The purpose was to give me a break, to allow me to relax. When I reported at these Army posts, I

was treated like royalty, coming as I did from the Pentagon.

In 1945, I was given the opportunity to work on war plans for the invasion of Japan, after the war ended in Europe. That was a huge exercise.

Sometime between VE Day and VJ Day, the top brass, including Eli Ginzberg, took off to the Pacific, so I was left with General Lull to run the office. I remember that General Lull once gave me some fatherly advice. He said, "In making staffing recommendations to General Kirk when he comes back, remember that he was an orthopedist when he started. You will find a shortage of orthopedists and a shortage of orthopedic mechanics. On other personnel categories, you may be objective." That was how I got to know General Lull. This was, of course, very useful advice.

Also, I learned about supervising a staff. Eli didn't have too much to do with that. I still remember that one of the people we hired out of Cornell was a woman who later married Eli, Ruth Szold, of the family of Szolds, if you know that name; Henrietta Szold is the founder of Hadassah. I recall that on one occasion I looked at Ruth's work sheet and said, "That's wrong." And she said, "You have a hell of a nerve. I worked for days on this, and you just look at it for a couple of minutes and pronounce it wrong." Of course, the only reason I knew something was wrong was that the columns didn't jibe. I learned how you deal with people and whom you want to encourage and whom you don't mind losing. I don't know that I ever fired anybody, but I certainly made it clear one way or another that I didn't mind that person's leaving.

Toward the end of the war—I don't remember now just how it happened—I ran into Walter Heller. He suggested, "Why don't you submit the long memorandum that you prepared for Harold Groves at the Treasury as your doctoral dissertation? You've been in the military for almost four years. I think the Economics Department at Wisconsin may well accept it."

NEUHAUSER:

This is the one on taxation and state and federal income tax deductibility?

KLARMAN:

Yes. On federal-state relations in individual income taxation. I took Heller's suggestion, submitted the memorandum, and the Department accepted it. It was then a matter of traveling to Madison for the final oral exam in May 1946, which we all treated pretty much as a weeklong celebration. Nobody took the exam seriously. I ran into old friends like Kay Frederick, later a dean, who bought me a couple of beers. I remember that, after the oral exam, we adjourned to the Union to have some beers; it was all very collegial.

There really wasn't much to do then in the military, so I spent time looking for a civilian job. I tried getting a teaching job. Eli was very helpful, as always; he got me an offer at Bowdoin College in Maine. I was not ready to go that far away from home. I went to see a man by the name of Bach at Carnegie in Pittsburgh; that was before it became Carnegie-Mellon. He had written a textbook which yielded handsome royalties. He offered me a job at a very low salary.

I finally settled on staying in government for the time being. The choice was between the Commerce and Treasury departments. Treasury moved very slowly, so I went to Commerce—in the National Income Division. I did not like it there because much of the intellectual foundation had already been laid. That was the period—we're talking about 1946-47—when the Department of Commerce was preparing the first set of national income figures, with 1929 as the base year. The foundation had been laid by Bob Nathan and by Simon Kuznets, but this was the definitive work done by official Washington. I shared an office with Maury Liebenberg and Selma Goldsmith, a very charming woman. She was married to Martin Goldsmith, then a well-

known name in economics. The leaders of the division were George Jaszi and Ed Denison, who later moved on to Brookings, and Charlie Schwartz, all working for Milton Gilbert who was, I would say, an easy supervisor. All I was expected to do was to come up with numbers for a certain sector, particularly government corporations. I didn't find that interesting work.

After a year, I was glad to leave, and moved to Brooklyn College. There Eli was again helpful since he knew the president, Harry Gideonse . The chairman of the department, by the name of Steiner, was glad to bring in some fresh blood. I found it very hard work at Brooklyn College. I lived in a furnished room and taught 15 hours a week. This meant five classes, with three separate preparations each semester. Also, my appointment didn't sit well with the senior colleagues. I remember that the chairman had appointed me to a very important committee—the most important committee in the department. After three months, it turned out that the committee was scheduled to meet precisely when I had class. It was a clear signal to convey what my colleagues thought of my presence. Also that year, Eli persuaded me to work for him part-time, on weekends, on a nursing study.

NEUHAUSER:

Was Eli back in New York by then?

KLARMAN:

He was back at Columbia. He was still only an instructor. Eli had never left Columbia. He had been there as an undergraduate, graduate student, and on the faculty, but never higher than an instructor until the late 1940s, when suddenly the promotions began and he moved quickly.

The nursing study was conducted out of Columbia University's Teacher's College. In my view then and now, the Teacher's College representatives on the committee conducting the

study were not impressive. But that's when I really mastered the skills to take thorough notes and produce minutes of meetings. Eli was able to base a book on those minutes.

I think what was successful about my minutes was that, without distorting what had been said, I made everybody look good, talking in complete sentences. If I added anything to fill in the context, I put it in parentheses. The insert wasn't so much editorial; it was more background information. There are always participants at a meeting who know and assume more than they say.

About that time, Eli began campaigning that I join him full-time for a year's study of the hospitals in New York State, financed by a Hill-Burton research grant. In addition to construction money, the Hill-Burton program provided funds for research administered by the states. Eli had the knack very early in his career of latching onto grants. I've never really understood how he did it. Even in the 1930s, he had obtained grants from the U.S. Department of Labor. I remember he had done a study of the unemployed. He had done a study of union leadership. One day, I remember, he and I were going to the Washington YMCA, and I asked him, "What did you learn from these studies?" He told me something that has stuck in my mind ever since. The essential ingredient to be a successful labor leader is *sitzfleish*. Do you know what that means?

NEUHAUSER:

No.

KLARMAN:

Well *sitz* means sit and *fleish* means flesh. The ability to sit and negotiate all night, if necessary. It has to do with stamina—a strong physical constitution. As I reflect on the people I've known well who achieved great success, I would say that this is one of their dominant traits.

I saw it in Walter Heller; I saw it in Eli; I saw it in Joe Pechman and in Martin Cherkasky, administrator of Montefiore Hospital. I recognized early I didn't have it.

Eli managed to exert a great deal of outside pressure on me. I wasn't all that eager to join him because, even though the work at Brooklyn College was hard, the fact is that I was learning things in preparing to teach three different courses, something I'd never done before. But at that time, General Eisenhower was president of Columbia and Tom Dewey was governor in Albany. They brought pressure on the Brooklyn College president Harry Gideonse, who was glad to comply with their wishes to release me for a year. That's how I wound up working on the Hill-Burton study of hospital care in New York State.

NEUHAUSER:

That must have been Eli's doing, making the connection with Thomas Dewey.

KLARMAN:

No question about it, who else? Maybe he got Eisenhower to call Dewey; I don't know. Eli had his own connections. With the state government, in fact, one of the best connections he had, and one that I made too, was with Herman Hilleboe, the Health Care Commissioner, who was slated to go to Washington with Dewey. Hilleboe never made it to Washington; he and I became friendly colleagues. In fact, one of the things I learned during the Hill-Burton study was that Hilleboe had assembled a highly competent senior staff. That impressed me greatly, just as I was impressed later by the staff that Martin Cherkasky assembled. Do you know that name?

NEUHAUSER:

Yes. Montefiore Hospital. Community medicine.

KLARMAN:

All his subordinates moved to lots of places. When they found it wasn't quite like

Montefiore, some of them came back; it always had a job for them.

NEUHAUSER:

Well, I think John Thompson, for example, was one of those people.

KLARMAN:

Not quite. In my time, John Thompson was only an intern or resident.

NEUHAUSER:

In nursing.

KLARMAN:

Yes. I got to know John. He wasn't at or near the top of the Montefiore administration. That came later. He went on to Yale, where he got into operations research.

NEUHAUSER:

I'm thinking of people like George Silver.

KLARMAN:

Who I think is one of the smartest people I've met. In fact, later on, just before I left New York for Hopkins in 1962, I met with a group of people like George Silver, Paul Densen, and Milton Terris for lunch. It was at the Faculty Club at Columbia, and most of the participants didn't seem to grasp what I was talking about, from the findings of my book on New York City hospitals. George Silver translated. Later on, he was at Yale. He had me come up to New Haven for a series of sessions with the residents. Indeed, one of the few things I wrote in the 1980s was a letter to *Lancet* in September 1984, commenting on an article of his. As usual, George graciously said in reply that I was probably right; that is George.

To go back to the Hill-Burton study; I have detoured quite a bit.

NEUHAUSER:

These are wonderful detours.

KLARMAN:

They remind me of what I came to appreciate even more later on. It tells you something about the big boss, when the subordinates are very capable. And it tells you something else when they're not.

NEUHAUSER:

It's perhaps the most useful thing, one of the few useful things in Machiavelli's *The Prince*. The prince is judged by the competence of the people he draws around him.

KLARMAN:

I didn't know that and I thank you. This is certainly my own observation over the years.

On the Hill-Burton study, we hired a staff and, as always, some people turned out better than others. Some people who performed ably made more of a contribution to the final product because people who are capable empirical researchers go off on their own and pursue leads; there wasn't a very useful contribution from the theoretically inclined. I'm reminded of a name you may or may not know, Jerome Rothenberg, who later joined the Massachusetts Institute of Technology as a professor of economics. He published a paper on welfare economics applied to health care in 1951, which preceded Kenneth Arrow's paper by a decade. Not only didn't I learn to use him well on the study, but his one long memo contained data that just didn't look right to me. I decided not to incorporate his findings in my draft of a chapter for the final report. This is the only time I can recall when I suppressed data. It was years later, when I was working on something else and looking through reports at the United Hospital Fund, that I learned

Rothenberg had erroneously copied the adjacent column. I felt relieved; my judgment was warranted this time.

NEUHAUSER:

I can see the value of your two years learning accounting. That seems to come through as an important skill that you've brought to all this.

KLARMAN:

I haven't even thought of it; my accounting stuff comes in later. But you're right. I think some staff members needed more attention and supervision than I recognized at the time.

For the Hill-Burton study, we traveled across the state. Under the Hill-Burton program, New York State was divided into seven regions, of which New York City was one. Long Island was another, as were the Rochester, Syracuse, and Buffalo regions. I got to know the leadership in each region. At that time, the Rochester region was the most impressive. You may remember the name Paul Lembcke.

NEUHAUSER:

Yes, he's the one who did the quality of care studies.

KLARMAN:

Correct. There was also a man by the name of A. D. Kaiser there. And there was also Chuck (Charles) Royle. One got to know these people. They had you as guest in their homes; it was all very friendly. I also remember one occasion when Eli tried to convince me that I should travel less, since he felt two Jews traveling the state were perhaps one too many. He wanted to take General Snyder with him. Eli had hired General Snyder, Howard McC. Snyder, as medical consultant to the staff. General Snyder was a very nice, gentle man. Do you know who he was? He happened to be General Eisenhower's personal physician, and Eli had hired him some time

before Eisenhower arrived at Columbia as its president. In fact, I remember Eli on the telephone, as early as 1944, working on Eisenhower's appointment at Columbia.

Eli has always attracted top-notch advisory committees. That's how I met Jack Masur, who was at Montefiore at the time and then moved to a job in Washington; he had the third rank in the Public Health Service under Scheele. The committee included Dean Clark, who was the founding medical director of Health Insurance Plan of Greater New York; Lester Evans, president of the Commonwealth Fund; and Dr. Stanhope Bayne-Jones. I remember John Pastore, who became very important in my life; he was the executive director of the Hospital Council of Greater New York. Again, one of the ways I earned my keep was by writing long minutes of the committee's meetings.

The only sector I was kept out of was the psychiatric system. Eli relied heavily on his conversations with a cousin, Sol Ginsburg, a psychiatrist. Ginzberg is spelled differently; Eli's spelling is unique. Ginsburg is the more usual spelling. Other than that, I was also in charge of the staff and just about every other area. Drawing heavily on the minutes, Eli proceeded to write the book, which he dictated. I would say he published pretty close to the first draft, as edited by his wife, Ruth. Eli composed with great facility.

NEUHAUSER:

That explains his productivity and the number of books he wrote.

KLARMAN:

I've always said Eli has published more than 100 books. I may or may not be too high when I say over 100; I don't really know.

I remember raising with him once the question of granting me coauthorship. In effect, he answered, if you want coauthorship, you'll have to do still another book or a collection of essays.

That was the main reason I decided to leave, lest I never get my name on anything. I looked around for a job.

NEUHAUSER:

I think very rarely does Eli have a coauthor for his books.

KLARMAN:

No. This did change, some time in the 1960s. He often writes with a coauthor now. What brought about the change I do not know. But I knew that I couldn't get coauthorship with Eli in the late 1940s. In fact, Isaiah Frank, whom I mentioned earlier, had worked for Eli and could not get coauthorship.

So, I looked around for a job. Of course, I could have returned to Brooklyn College, and I did speak to the chairman, Professor Steiner. Could I perhaps get a reduction in the 15-hour weekly teaching load? I had produced two articles during my Brooklyn College stay, culled from my doctoral dissertation. I could see no way to publish anything else while teaching 15 hours per week with three different preparations each semester. This meant spending 60 hours a week just teaching and preparing for class. Professor Steiner said no. I decided to go to the Hospital Council of Greater New York. Sometime during the last few meetings of the advisory committee to the Hill-Burton study, there was something about what John Pastore was saying that troubled me. I recall talking to Eli about it, and he confirmed my perception. Even so, I didn't see any way not to turn to the Hospital Council.

I must say this for Eli: over the years, even though I left him, we've remained on good terms; he was always available when I sought job advice. Indeed, he was very good at giving me advice on jobs.

I was just about to join the Hospital Council of Greater New York, my first full-

time nonmilitary job in health care. We are now in the fall of 1949; the curriculum vitae is misleading in the claim of coauthorship of the nursing study. There's no doubt in my mind that I earned it, but in fact I didn't get it.

You did tell me that you wanted a good deal about personalities, and I'm certainly giving it to you. Most of the individuals I mention are vivid in my mind.

Let me go back a moment before discussing the Council. When the report on the Hill-Burton study was delivered to New York State, a formal luncheon was held at the Columbia Faculty Club. Present were, in addition to Eli, myself, and General Snyder on behalf of the study, Herman Hilleboe, Thomas Dewey, and Dwight David Eisenhower, president of Columbia. It was a most interesting experience, as you can imagine. While waiting for the elevator to ascend to the dining room, Ike introduced himself to me as General Eisenhower, not as university president. I was tempted to say, "I'm Captain Klarman," but refrained.

NEUHAUSER:

Then I'll have to ask you about your perceptions of Dewey and Eisenhower.

KLARMAN:

I shall do that, but allow me to relate briefly how I happened to attain the rank of captain. The leadership at the Pentagon was embarrassed to have a second lieutenant on the staff. I was promoted to first lieutenant almost immediately upon arrival and then to captain within six months.

Governor Dewey, I would say, was reasonably well informed on hospital matters, and rather knowledgeable about faculty members at Columbia. Eisenhower was simply out of it. I don't know whether he just didn't know anything about his faculty, but there was a Professor Counts, who headed the Liberal party in the state. Eisenhower had no inkling as to who he was;

Dewey knew precisely who he was. Eisenhower did not impress me at the luncheon. And yet, let's not forget that Eisenhower finished at the top of his class at the Leavenworth general staff school. Anybody who finishes number one there is very bright. In fact, it would have been one of my ambitions to attend Leavenworth had I remained in the Army. In any case, Eisenhower's stay at Columbia was cut short when he was appointed Commanding General of NATO on leave from Columbia University.

Speaking of Ike reminds me of meeting his younger brother Milton in 1966. I happened to be serving as sort of secretary—the entire staff—for a committee that Dean Stebbins of the School of Hygiene assembled. Indeed, one of my books was written as staff director for that committee. I remember Milton Eisenhower well. He was a member of the advisory committee, but he attended only one meeting. He was president of Hopkins at the time and had also served as a close adviser to his older brother. I've never met anybody who listened with such close attention and intensity.

We're way ahead of ourselves. Let's go back to my joining the staff of the Hospital Council of Greater New York. It must have been the day after Labor Day in 1949, and I had a long-standing luncheon date with Jack Bourke. He was the Hill-Burton program director for the State of New York. I got to know him well—a very nice man. He wasn't all that knowledgeable technically, but he hired able people, like Hildegarde Wagner, who knew their stuff. We liked one another, so we had arranged lunch. The Hospital Council was one of the regional recipients of Hill-Burton planning grants. In this capacity, it was one of seven agencies in New York State that handed out Hill-Burton construction grants.

NEUHAUSER:

The Hospital Council was the planning organization for metropolitan New York?

KLARMAN:

Not for the metropolitan area, but only for the city. It was rather unusual for an existing voluntary nonprofit organization to play that role, but there it was. I still remember coming back from lunch; John Pastore asked me, "With whom did you have lunch?" I said, "Jack Bourke." And Pastore said, "Why didn't you tell me about it? Don't you ever have lunch again without telling me whom you're having lunch with." That only confirmed my fear my new boss was a control freak, which characteristic I had vaguely recognized. Unfortunately, I had not seen too many attractive job alternatives at the time.

On the staff of the Hospital Council was a woman by the name of Francesca Thomas. She was graduated from college in the early 1930s, from Smith, and, in effect, was hired as a secretary and worked herself up to indispensability. I don't think she had much authority as associate director, and she certainly didn't get paid very much. I got to know her and she was very helpful to me. She taught me how to design a table, how to compose a table heading, how to connect text and table—these are very useful skills not taught in school.

NEUHAUSER:

A large number of people don't have that talent to this day.

KLARMAN:

Mrs. Thomas taught me. I visited her house quite often. She came to like me because she loved to answer questions. I may have been subdued at the office, but in her home I'd discuss this and ask about that, and she was always helpful.

One of my first assignments at the Hospital Council was to work on an ambulance study for the city and another was to prepare a master plan for hospital care on Long Island, analogous to the master plan for New York City prepared in 1944 by John Pastore. To me, Dr. Pastore

seemed to know the answers before he started a study, and he just knew that the proper location for emergency ambulances was in the fire department. The important point was to change the existing system.

He would never repeat exactly what somebody else was recommending; thus, his master plan formula for general hospital bed requirements was based on the number of deaths in New York City. I don't know whether you know the Bachmeyer study for the American Hospital Association—that bed formula was based on the percentage of all deaths occurring in hospitals. John Pastore simply had to devise some new and different wrinkle. If you don't test your formula over time, any formula is as good as any other because they're all derived from the data at a given time.

In connection with the ambulance study, I traveled to Chicago where I talked to a Captain McCarthy in the fire department. He thought that basing ambulances in the fire department was sensible, but not essential, and certainly not a fundamental principle. There was no doubt that my assignment was to write the report à la Pastore and not to raise too many questions. I don't know whether you have noticed this: *The New York Times* reports that one of the proposals by the Giuliani administration is to turn the ambulances over to the fire department. You could certainly make a case that the one place the emergency ambulance service doesn't belong is with the Municipal Hospital Corporation, because its hospitals face a conflict of interest: they want to fill their own beds. Still, why locate the emergency ambulances at the fire department?

NEUHAUSER:

Neighborhood health centers and community health services have gone through many cycles of this sort in the past century.

KLARMAN:

When I had been at the Hospital Council for three or four months, I was approached by Kenneth Williamson. Do you remember him?

NEUHAUSER:

The American Hospital Association's person in Washington, yes.

KLARMAN:

Before that, he was vice president of the Health Information Foundation. Admiral Blandy was the founding president. Williamson offered me a job with the new organization. Believe it or not, I turned it down, even though I did not like my job at the Hospital Council. My concern was that I'd get a bad reputation for too frequent job turnover. Was this a wise decision?

NEUHAUSER:

That was before Odin Anderson and George Bugbee joined the Health Information Foundation?

KLARMAN:

Yes, the organization changed after Blandy died and Bugbee became its president. When George got this job, I had already left the Council for Washington. After two years, I decided to seek another job. I looked first in New York City. One hospital administrator told me, "If you want to go to work for a hospital, I'll hire you. But you'll have to take a cut in pay." My salary at the time was $7,500, and I didn't want to go down to $5,000. With a lot of help from my good friend Joe Pechman in Washington, I got a job with the National Security Resources Board as a "medical economist." At that time, the field was still called medical economics. Harold Clark was a very good boss—he gave you lots of room. He hired good people and let them work. I found myself chairing an interdepartmental committee. I traveled around the country, offering

research grant money to people like Ken Arrow, whom I had met years earlier when he was a graduate student at Columbia the year I spent there on the Hill-Burton study. At that time, I was dating his sister Anita. You know the name—the younger brother of Paul Samuelson took the name of Summers. Her name now is Anita Summers. And you know the big man, Lawrence Summers, from Harvard, who is now with the Treasury Department in the Clinton Administration? He is the only person whose paternal and maternal uncles have Nobel Prizes in economics—Paul Samuelson and Ken Arrow. Ken refused, feeling committed to other projects for two years. I offered a research grant to Lloyd Ullman, whom I knew from Columbia College and Wisconsin, then at Minnesota. He, too, said no. On that visit to Minneapolis, hosted for me by Walter Heller, I spent a couple of hours chatting with Andreas Papandreou, the future leader of the Socialist Party of Greece and prime minister, and his wife Margaret. At the time, his reputation was that of a brilliant economic theorist, and he showed no signs of an interest in politics, following his father. I spent even more time with the other Regents Professor in the department, Leonid Hurwicz, a distinguished mathematical economist. He seemed so down to earth, so interested in learning about health care, that years later I recommended him as my successor on the Health Services Research Study Section, and he was appointed.

I was quite unsuccessful at disbursing grant money. Shortly thereafter, the world collapsed on me. I lost my job when the National Security Resources Board was abolished without prior notice to the staff.

This was a government agency, housed in the building occupied by the Bureau of the Budget; it was part of the Executive Office of the President. It was founded in the 1930s to give government thinking some long-term depth. The organization's name was somewhat misleading; in my unit, we weren't dealing with national security, but with human resources.

Manpower was Harold Clark's specialty. The agency had enough status so that when we convened an interdepartmental committee, representatives attended, but by then we had no influence. I don't know whether we ever had any.

Suddenly, in July of 1952, I was jobless—the only time that has happened to me. I went back to New York; I didn't know what to do. I even collected unemployment insurance for a couple of weeks. This is where Francesca Thomas was most helpful. She wanted me back at the Hospital Council. One of the events I have neglected to mention is that when I was leaving the Council in the spring of 1951, John Pastore, having just left his doctor—let's say it was after an annual physical—who told him he was in great shape, collapsed on the street and died. The effect was that I didn't get to Washington for another couple of months because I stayed on to help with the transition. By the time Francesca said she'd help me get a job at the Council, it had already hired a new executive director, Tony Rourke. It's a name you know. He became a prominent hospital consultant.

NEUHAUSER:

Anthony J. J. Rourke.

KLARMAN:

You got it. How you do this is beyond me. Do you know everything and everybody? Anyway, I was about to say Anthony J. J. Rourke.

NEUHAUSER:

He always had the J. J.s in the middle.

KLARMAN:

It was more than that. Originally, he was O'Rourke, but he removed that O! Here's a lesson I learned from Rourke, which was to be repeated often in my experience. How did

Anthony Rourke happen to join the Hospital Council? He had been elected president of the American Hospital Association for a year. That was an honorary volunteer job. Today the AHA executive—what was formally the executive director—is called the president. Rourke was sort of chairman of the AHA board.

NEUHAUSER:

He would have been an elected person for the year, usually someone who runs a hospital at the same time.

KLARMAN:

Exactly. Rourke ran the Stanford University Hospital. And because he was on the road so much as president of the American Hospital Association, his board of directors at Stanford was displeased. He wasn't paying enough attention to the hospital that paid his salary, so he lost his hospital job. Whether they fired him or they arrived at a mutual settlement, I don't know. The Hospital Council had a new boss. I should say that, while Rourke and I functioned at different levels, we got along well. He showed me how to inspect a hospital with white gloves. One of the things he enjoyed was serving as a hospital consultant, even then. So he took on a consulting job in Philadelphia, and I worked for him part-time, Saturdays and Sundays, with pay. He felt entitled to keep the money he earned as a consultant, but Norman Goetz, president of the Council, didn't approve of that. Norman Goetz was one of those top-notch New York civic volunteers—very pure. A senior partner in Proskauer, Rose, Goetz, Mendelsohn—you know the name Proskauer?

NEUHAUSER:

The notable Judge William Proskauer in New York. I don't spell very well, so I'm glad your spelling is so good.

KLARMAN:

I learned the importance of English spelling in that special class for foreigners. We had a daily spelling quiz—10 words—and a Friday quiz, usually 50 words. Throughout my stay in that class, I didn't get a single word wrong.

The disagreement between Rourke and Goetz about outside earnings led to job termination for Rourke; he wouldn't budge, and Goetz adhered to his position. At that time, 1954, there was a small movement in the country toward limiting the earnings of full-time faculty to their salary. The University of Chicago was at the head of that movement; it didn't spread far.

When Rourke lost his job at the Hospital Council, he went into consulting. This meant then that he had no job. If you looked at the address on his letterhead, it was his home address; he didn't have an office. It turned out that he became very successful; I'm glad for him. He was a different person when we were doing that Philadelphia study than he was at the Hospital Council. In the former, he was interested, he was knowledgeable. He showed me things that I was unaware of. I can see why a hospital would want such a consultant. He did not make a lasting impression on the Hospital Council. He did a few studies. I don't remember what they were. He was forced out; I stayed on. At one point, Rourke had hired a doctor by the name of Clement Clay from Columbia as associate director.

NEUHAUSER:

Clay returned eventually to Columbia.

KLARMAN:

Not eventually, very shortly. The reason was that he found the Hospital Council work uninteresting. I remember once he was sitting around with nothing to do, and he did what we

called a hospital study as an exercise. I read it and said, "Clem, this is it; it is the way we do it." He was shocked; he didn't like it. So in a few months, he went back to Columbia. I don't know what else happened; there may have been other factors involved. Then the board of directors wanted to send in Dr. Morris Hinenburg to serve as acting director. I spoke to Francesca about it, and we agreed that this was nonsense. In fact, I was running the organization. I was not going to work for somebody else temporarily. I had had enough of this. Although I never got the title of acting director of the Hospital Council, for all practical purposes I was, and outsiders who dealt with the Hospital Council knew it and acted accordingly.

During that interregnum, I met with Mr. Goetz once a week at his home for breakfast. That was just before his masseur came. He was pleased that I told him things, both about work at the Council and what was happening on the national scene. He was eager to learn. Yet one of the conventions at the Council had been that the staff tells the board as little as possible; they'll only misuse it. So here was I telling him a good deal.

One day, Mr. Goetz pulls out a letter and says to me, "Read it please." I read it. He said, "What do you make of it?" I said, "The man says no." Mr. Goetz said, "You're wrong. The man has accepted the job." That was Hayden Nicholson, then dean at the University of Arkansas Medical School in the heyday of the Winthrop Rockefeller governorship.

A lesson I learned then, which has stuck with me: do not hire a man who is reluctant to take the job. Here was a bright man, a very effective medical school dean. Maybe he would have been successful at a New York medical school. But he was not a success at the Hospital Council. Either he wasn't interested in its work, just wasn't up to it, or didn't know enough about New York and its politics. In any event, he allowed me to run the place.

This is also interesting: Dr. Nicholson likes me and we get along well, but there is also a

bit of resentment on his part. How do I know this? At one point, I had a disc operation followed by a convalescent period. I must have been in the hospital about two weeks, and stayed home another week. When I returned to work, I learned that everything that had been going on previously was changed. It took me about six months to get back to where we had been. For that time, Dr. Nicholson was well paid; his salary was $25,000 and the Council put another $25,000 a year into a retirement or pension fund for him.

Nicholson also did some hiring while I was out. As happens with all hiring, some of it works out well and some doesn't. One thing I learned about dealing with senior staff; you learn to draw on each one for his special skills or knowledge. Most people do have something to offer. One man wrote very well; he was not a profound thinker or statistician. Another man was very good on hospital structure and equipment, but couldn't articulate his findings and couldn't connect things. He reminded me of most self-taught individuals, of whom I was one. We tend to do things the hard way; we're not facile or fluent; we're not confident about what we think or do. I remember once he tried to compare construction figures—how much was spent on hospital construction over time. He didn't know whether to divide or to multiply actual expenditures by a hospital construction cost index.

Over the years, we introduced a few changes in our work. Even individual hospital studies came to include some of the features of our areawide studies. In other words, take a look at the geographic area that the hospital actually serves rather than pronouncing, arbitrarily, that the hospital is situated here and you look at its immediate surrounding neighborhood. How did we know whom the hospital serves? We drew a sample of patients from its admissions book. How do you draw a sample? I never learned that from Hotelling's courses. Sampling practice was developed by Hansen and Horwitz at the U.S. Department of Agriculture. I had hired a man

who had just gotten a Ph.D. at Harvard under Seymour Harris. He had written a dissertation on the economics of cancer. Later on, he went to work for Hughes. Mary would also remember his name, Howard Laitin, since we had lunch with him and his wife in Los Angeles a few years ago. He told me that when you wish to draw a representative sample, you draw a number, say 1,000 or 1,500. When the U.S. Department of Commerce drew samples for its occupational income surveys in the 1930s and 1940s—they have done many—they drew a percentage of the total. So, what did I do? I went to Washington to see my old friend, if not colleague, Jerome Cornfield. Remember that name?

NEUHAUSER:

The statistician. Yes.

KLARMAN:

I had met him when he was at the Bureau of Labor Statistics. By 1955, he had moved to, I think, the National Institutes of Health. He was glad to see me. I asked him about sampling, and he confirmed that indeed proper sampling size is a number, not a percentage. When I came back and reported that to the Master Plan Committee, the chairman, Arthur Jones, was extremely annoyed. He said, "Why do you bother us with such details?" I thought the committee was supposed to be interested in technical details.

Anyway, we got to do area studies, even when we were merely requested to do a single hospital study. We learned not to draw concentric circles around the hospital, but to ascertain its actual service area, where its patients came from.

Another activity I undertook at that time was to go around town regularly to interview hospital administrators I had met who impressed me as well informed and sensible. Usually, they were glad to talk. We would schedule an hour, but often the session lasted two or three

hours. I would ask short questions, and they would talk to me at length about current problems and what they saw as the emerging problems in hospitals. That is how I learned a good deal about what was going on in New York City. I also learned something else that confirmed what I had learned from Tony Rourke's experience: when a man talked to me more about the national scene than about his own hospital, I said to myself that he would be losing his job soon. This happened several times to successful administrators I knew. Also, regarding the process of learning things: we were doing a study of the Lower East Side, which included Bellevue Hospital. I went to see the several medical school deans. Staffing of Bellevue was then divided among four medical schools: Cornell, Columbia, New York University undergraduate, and New York University postgraduate. One of the lessons I am sorry to say I have carried with me from that experience—by the way, one of the deans was Dr. Thomas, who later became a famous essayist.

NEUHAUSER:

Norman Thomas?

KLARMAN:

No. Norman Thomas is the socialist.

NEUHAUSER:

Lewis Thomas.

KLARMAN:

Yes. I'm sorry to say that I learned not to trust him. I didn't believe what the deans were telling me. It didn't jibe with what I'd learned from other sources, such as data we had compiled. This reminds me somewhat of an experience later on in the late 1970s when I served on the Special Medical Advisory Group to the Veterans Administration. At one meeting, questions

arose about the health effects of Agent Orange. Most of the committee members were either deans of medical schools or medical vice presidents of universities, and since the Administration didn't want to get involved with Agent Orange, the deans simply supported the Administration. There was no evidence offered, no nothing. They didn't know anything about the subject, but they knew where their money came from. I kept my mouth shut because I didn't know anything about it, but I felt uneasy about the discussion and its outcome.

Something happened around 1960 that gave me an opening. At no point previously had the Hospital Council dealt with questions of cost or revenues—anything to do with finance. Its work dealt with determining the need for hospital care. Rates of occupancy were pretty much fixed: eighty percent was optimum, never mind what determines the actual level, and you could always fall back on the Poisson distribution. That's discussed in the Bachmeyer book. You do remember that the Poisson is based on the number of deaths due to horse kicks in Frederick the Great's Prussian army? It's in the index of R. A. Fisher's book.

NEUHAUSER:

Was it the number of horse kicks that soldiers got by regiments?

KLARMAN:

Yes. I think R. A. Fisher must have been intrigued by that.

NEUHAUSER:

Wonderful.

KLARMAN:

The things I do wander into.

NEUHAUSER:

Yes. It's the kind of thing I enjoy.

KLARMAN:

Something had happened in hospital use in New York City. The load in the tuberculosis units of general hospitals and in special tuberculosis hospitals had been going down, treatment was very effective, and the city was closing its own hospitals and its own units. Consequently, the city also closed a small unit at Montefiore Hospital—maybe no larger than 12 beds. Martin Cherkasky, the administrator, was furious. "How dare they do that to me? They didn't consult me. They didn't talk to me. They just did it." And this is where I had an inspiration. It's the only time I totally initiated a project at the Hospital Council. In part, it was about the appropriate relationships between voluntary and municipal hospitals. What recourse does a voluntary hospital have when the city unilaterally closes down one of its units? The city continued to use its own hospital. Is there any recourse in this relationship? Out of this incident grew the study that eventually was published on hospital care in New York City.

This is where George Bugbee became so important. Bugbee agreed to chair the special committee. I wanted this study out of the hands of the established Master Plan Committee.

Before I continue, let me go back briefly to the Master Plan Committee. Traditionally, this committee consisted of hospital administrators, plus a staff person from the Community Council or a welfare agency. I had enough influence at that time with Hayden Nicholson to propose that we add some people who knew and did research. So I had put Neva Deardorff on the committee, who was the first director of research at the Health Insurance Plan of Greater New York. When the Health Insurance Plan was organized, its founders established a research department: Deardorff was the predecessor of Paul Densen and later Sam Shapiro. Do you know the name Deardorff?

NEUHAUSER:

No.

KLARMAN:

Like many people in the 1930s, she had done some work for the Work Projects Administration—that's how she got paid. I added an expert hospital accountant, Charles Roswell of the United Hospital Fund. He was the author of the first book on hospital accounting, a pioneer in that area. Also added was a sociologist, Jack Elinson of Columbia University, who had come to New York from the Hunterdon County study done under Ray Trussell.

NEUHAUSER:

The Huntington . . .

KLARMAN:

Hunterdon. Hunterdon County in New Jersey. Is it volume four in the Commission on Chronic Illness Studies? I'm not certain.

NEUHAUSER:

I think that the Commission conducted at least a couple of local studies. Maybe the one I'm thinking of was the one describing the medical group practice, showing it as a potential model for the way the world might be.

KLARMAN:

Jack Elinson had worked with Trussell. When Trussell moved to the Columbia School of Public Health, Elinson went with him. In fact, when Trussell moved to Columbia, he also had offers from Downstate Medical Center and from the Hospital Council—I don't know from whom else. Dr. Trussell was very good at using his academic base to influence public policy and regulations. Did you get to know Ray Trussell?

NEUHAUSER:

No. He is another famous name.

KLARMAN:

He took no prisoners; he was a tough guy. As I remember, he wanted to effect a change in the top leadership of the local Blue Cross plan, and he got it done by New York State, just like that. He also had other connections: he sat on our board, and the other directors had to bend over and pay attention to his comments uttered in a low voice. Have you ever seen that? It's quite a trick.

NEUHAUSER:

Jack Elinson was a longtime faculty member in public health at Columbia.

KLARMAN:

Yes. Ray Trussell brought him there.

NEUHAUSER:

Elinson was interested in health status measures and was a very close friend of Jack Feldman, as I remember.

KLARMAN:

You are so right, probably more right than you realize. A couple of weeks ago, there was a celebration at Hopkins of Sam Shapiro's 80th birthday. Until February 28, Sam was acting chairman of Karen Davis's department at the Johns Hopkins School of Hygiene. It took almost two years to fill the job, and Sam was doing it temporarily. So, who came to the birthday together?

NEUHAUSER:

Jack Elinson and Jack Feldman?

KLARMAN:

Jack Elinson was visiting in Washington, and Jack Feldman drove him to Baltimore. You made a perfect connection. I have not followed Sam Shapiro's work since he left the Health Insurance Plan of Greater New York; however, I have noticed his great influence—the mammogram evaluation study derives from him. I still remember his study on perinatal mortality at the Health Insurance Plan, which impressed me much more at the time than the preceding studies of hospital use by Health Insurance Plan members. This was the first time, as I recall, that Sam was senior author of a Health Insurance Plan study. You may recall that Feldman moved from Harvard, when denied tenure, to the National Center for Health Statistics as associate director. I am reminded of a man who moved from hospital administration to academia, that is, in the opposite direction, Cecil Sheps. My first meeting with Sheps was in Boston, where he was serving as executive director of the Beth Israel Hospital. I had asked to visit its home care program, one of the first and most prominent in the country. What struck me most at the time was that approximately 30 people sat around the table, discussing one case for about two hours. My reaction then was that Sheps was bound to lose his job as hospital administrator; I wasn't mistaken. Sheps, despite his lack of formal credentials, was a perfect academic—objective, bright, informed, curious, and a wonderful colleague. I saw him later on, serving as chairman of a committee, and I derived even greater pleasure when I was chairman of a committee and he was an active, supportive member.

Let us return briefly to the three people I had invited to serve on the Master Plan Committee. It turned out I had not chosen well. By then, Deardorff had lost interest; she kept silent. Roswell was a fine accountant, but either he didn't know any more than accounting or didn't want to talk about that either. At a Hospital Council accountants' meeting, he had nothing

to say to the accountants. I never did understand Elinson's role. He didn't seem interested, other than in talking about his own agenda. He didn't address the study that the staff was working on and submitting for consideration. There may have been a good reason; perhaps we were asking the wrong questions. This experience taught me to be more circumspect about recommending committee members.

The formal name given to the committee chaired by George Bugbee was the Committee to Study the Relations Between the Municipal and Voluntary Hospitals. George had been a member of the Hospital Council's board of directors, but now was the time I got to know him well. I had met Odin Anderson earlier; indeed, I remember an early lunch with Odin. He had hired Monroe Lerner from Metropolitan Life; Odin wanted my opinion of Monroe's method of deriving the sources of increase in the costs of medical care. I told him that Lerner was employing the then accepted procedure, but I was not happy with it. At the time, I used the same method, which I was able to improve later on by borrowing from Ed Denison. Ed reduced index numbers for an interval to annual rates of change. This served to cut down the size of the interaction term considerably, thereby yielding a clear numerical answer. Another advantage was that, instead of multiplying and dividing, one did all calculations by addition and subtraction. However, I didn't know about this method in the 1950s.

You know where the new, improved method came from? It came out of the study of Soviet economics in this country. American economists, including Herbert Levine, from the University of Pennsylvania, could not rely on the numbers presented in Soviet Union publications on national income. They resorted to using index numbers. They came up with this device of reducing everything to annual geometric rates of change. This procedure was later employed by Ed Denison in his volumes on the U.S. economy's sources of growth. As you will

note, one has to spread a fairly wide net for methods, as well as ideas.

In the study of voluntary and municipal hospitals, I did something I had never done before and have not done since. I prepared an elaborate eight-page outline. It comprised four parts, plus an appendix: Part I, Purposes of the study; Part II, Questions, immediate and long-range; Part III, Areas (or subjects) of study; Part IV, Evaluation of findings in terms of explicit criteria. The appendix dealt with the types of data required, existing or newly collected. From the outset, it was recognized that some data might be pertinent to answer more than one question.

As is always the case in empirical research, you win some and you lose some. I lost badly on the quality-of-care side—I could not think of relevant data, nor did I get any help from the staff. Promises were made by members of the staff, but not always kept. By the way, we produced this report with the regular Hospital Council staff and a secretary, with no additional hiring.

I remember a man, Peter Ruderman, who spoke at a meeting, was strongly impressed by the low staffing. By the way, if you can find the review of the report in *Hospitals*, it was highly laudatory. My reaction at the time was that my mother couldn't have written a kinder review. Yet, I don't think this book has received proper attention in the profession.

While I lost completely on the question of quality, I did very well on finances—on revenues, costs, and deficits or surpluses. How did I accomplish this? Initially, I worked with the published financial reports issued by the New York Blue Cross Plan. I wrote a draft of a chapter and sent it to one of its vice presidents, Harry Sesan. Harry wrote back saying, "Herb, you don't know what you're talking about." "Fine," I said, "I'm trying to learn. How about giving me the right stuff?" This he proceeded to do—all good unpublished stuff. I'd never seen these data before.

And I learned something important from Rufus Rorem, a wise man. I had met Rufus many years earlier on my first visit to Philadelphia, about 1949. One didn't visit Philadelphia without seeing Rufus. He was a gracious gentleman as well as learned. He had me home for dinner that first night. Did you know the man?

NEUHAUSER:

I know of him.

KLARMAN:

Well, there are some people who just swear by Rorem, like Bob Sigmond and Dave Willis, former subordinates of his. A perfect man. I think in a way, that's too bad, because I believe everybody should be questioned. Anyway, Rufus gave me a piece of advice that I followed. It worked out perfectly. I wanted to compare hospital financing in New York with financing elsewhere. There were hospital councils in metropolitan areas all across the country, among which the most prominent at the time was Cleveland.

Have you ever met Guy Clark? Or were you too late for that?

NEUHAUSER:

Guy Clark in Cleveland? No. The person I met in Cleveland was John Mannix.

KLARMAN:

Yes, I got to know Mannix, who was his successor.

Rufus's advice was this: if you want to obtain information from people, you give them some information first. What I proceeded to do was design a questionnaire on which I recorded the relevant information for New York City. This was sent out along with the blank questionnaire. Because I was trying to get information correctly and appropriately, the questionnaire passed through several drafts. It was improved tremendously by the effort to

answer it for New York City. I must have gotten 100 percent response rate from the hospital councils, all due to Rufus's sage advice.

The time came to submit pieces of the report to the Bugbee committee, and then there was a complete document. I fully expected it to go out as a Hospital Council report, following tradition; however, George Bugbee insisted that my name go on it as author. I owe that to him entirely. Being in New York, we took the manuscript to the Columbia University Press. I remember during that period of editing, the publisher said he wasn't surprised when he was informed that I was then looking for another job; in his experience, that was often the case after a person had completed a book.

NEUHAUSER:

I can see the extraordinary effort you had to go through to fill in gaps in the data and to make projections to the whole area. It is probably more easy now to collect, with Medicare cost reports. This was long before the time of easy laptop computers and things like that. I imagine the amount of work that you had to go through generating all this was extraordinary.

KLARMAN:

One person who was exceedingly helpful was Francesca Thomas. She was winding up her career at the Hospital Council in the late 1950s. I had been instrumental in getting her a salary raise; I thought she was badly underpaid. In effect, she willingly functioned as my chief statistical clerk. I don't know that any other professional would have done this. Other staff people helped with this or that item, but also, as I have said, there were sharp disappointments with respect to quality of care. To this day I'm not comfortable with quality-of-care data, especially on outpatients. I would like to hear you talk about this.

NEUHAUSER:

Was this about the time when Mildred Morehead was beginning to do studies about quality of care for the labor union plans?

KLARMAN:

Yes. She was then married to Ray Trussell.

NEUHAUSER:

I think her work was generally viewed as a real pioneering effort.

KLARMAN:

It could be, but I think there is a difference between doing pioneer work and producing something that other people can use in making evaluations and making policy judgments. In fact, there's even a question of when the time is ripe to make a judgment.

NEUHAUSER:

Osler Peterson had a long-standing interest in quality measures, and he would point to Mildred Morehead as a person who had done very thoughtful work measuring quality.

KLARMAN:

The person who has impressed me most in the last decade or two in quality work is Bob Brook. We served together on a committee of the National Research Council, and he always talked sensibly. Nowadays, he's doing a lot of work, but I'm not familiar with it. I don't know whether I would know how to apply his findings. Maybe physicians can in clinical practice.

NEUHAUSER:

Well, I think the more applicable work is now being done by his wife, Jackie Kosikoff, in terms of developing consensus about what are the appropriate criteria to do a procedure, and then looking to see how often the procedure is actually done based on the suggestions that the

experts made.

KLARMAN:

Even before that, I think there is the question of how the patient feels about the doctor. The relationship with the physician has a lot to do with whether the patient will follow the prescribed regimen.

NEUHAUSER:

I think one of the most thoughtful people there was Eliot Freidson.

KLARMAN:

Freidson always spoke in complete paragraphs at the Health Services Research Study Section. One of the things that struck me was that he was funded by the Health Information Foundation at one time. He did a study at Montefiore of the Health Insurance Plan of Greater New York medical group, which was considered one of its best medical groups. He reported there was no such thing as peer review in actuality.

NEUHAUSER:

An astonishing finding.

KLARMAN:

Astonishing, yet probably true. Probably, for most of us don't do it. Either we don't care to judge our colleagues, or we realize that, if we do, it won't be appreciated. This raises some further questions—some basic questions—but we're not up to that.

NEUHAUSER:

No. The reason I asked was that sometimes the sociologists are over here and the economists are over there. In the academic setting it's easy to have notable people, but if they're in a different branch or department you scarcely hear of them.

KLARMAN:

One of the things I learned on that study section, plus my other committee exposures, was to appreciate what you can learn from other disciplines. Indeed, I have written some remarks on half a dozen or so disciplines and what I learned from them. Let me give you a small sample. This is not at all formal sociology, but it is sociological. When I was in the Army—first as an enlisted man—I had my usual dental problems. The officer dentist made it clear to me that I received better treatment than other soldiers because, evidently, I had been seeing a dentist regularly. That statement was very striking to me. I remember, too, we had done a lot of walking in basic training, and I reported to the doctor in first aid. "What's the problem?" he asked. "An abrasion of the heel." "Where'd you learn such language?" Abrasion? I wasn't supposed to be able to talk to him in precise terms. Let me add that, in general, from my experience in the military, I had much higher regard for the regular Army personnel than for the reservists. I remember particularly one doctor, a reserve officer. We passed one another and he shouted at me, "Why didn't you salute me?" A regular Army officer would never do this. The regulars were the people I got to know, and they were bright and able. They were good soldiers, and they were intellectuals as well. If they weren't intellectuals, they appreciated people who knew things. If you want to learn about class differences, I would say the Army experience is telling. By the time I became an officer, there was no doubt that a dentist would take good care of me.

I have written up some notes on several disciplines: statistics, accounting, sociology, and others.

NEUHAUSER:

I hope you'll send me a copy.

KLARMAN:

I didn't do much preparation for this interview, but I did look at a few things. When I left New York in 1982, I was given three dinners and I made a short speech at each. I have looked at them. In 1989, the Health Services Improvement Fund, which is part of the Blue Cross plan in New York, invited me to give a talk. I did, drawing heavily on my 1982 remarks. I don't know why I didn't try for publication. It is likely that I sort of ran out of gas. Moreover, who needs another publication? I did publish a few articles in the 1980s, but they're really short op-ed page advocacy pieces. I'll talk about them later. I think only one of them is important, the article on catastrophic health insurance in 1989. I'm often tempted to write a letter to the editor, but usually I don't get around to doing it.

I'm back in late 1961, early 1962. I started looking for a job, an acadmic job. The big study was finished, the book at the publisher. I talked seriously with Brandeis, the University of Connecticut, and I think I may have talked at that time with Herman Hilleboe, who was then at Columbia. My question to Herman was, "Are you going to be here five years from now?" He said, "I can't promise you that." I said, "In that case, the answer is no to a job offer."

One of the things that came up repeatedly, although not always initially—I sort of wised up to it in the course of continuing interviews or running into people at meetings—was that I was expected to raise my own money. I had a strong distaste for that. I'd had a different experience in government and at the Hospital Council. I could concentrate on the work at hand and not spend time writing research grant proposals and progress reports. Somehow, my feeling still is, if I had had to raise my own money, I would have preferred to go into business or management. Still, it's also clear to me that some people prosper and do well in the research grant process and do good work. You may have some views on that.

NEUHAUSER:

Oh yes. I think it has some clear drawbacks.

KLARMAN:

At Hopkins, no such requirement was imposed on me. I didn't have to raise money. Indeed, toward the end of my stay at Hopkins, I was given a program grant by the National Institutes of Health, which I didn't even request in a formal application. The staff at the National Institutes of Health didn't trust me to write it up; they were aware of my distaste.

That's how I moved to Hopkins rather than to any of the other schools. At Brandeis, it was made clear early in the interview with the dean, Charles Schottland, and the man I had talked to most, Robert Morris, that I was expected to be financially self-supporting.

NEUHAUSER:

I was thinking of Irving Zola, a sociologist.

KLARMAN:

Yes. I've met Zola, and got to know him quite well. Didn't he go off to Egypt or some such place? I don't remember in what context I met him.

NEUHAUSER:

He was at Brandeis, and I can't remember whether he's still there now. A notable man in sociology and aging. I think he even wrote a semiautobiographical book about growing up with a physical limitation. And Howard Freeman was also at Brandeis, also a sociologist.

KLARMAN:

Howard Freeman was on the Health Services Research study section. That's where I got to know him, before he moved to California.

NEUHAUSER:

Yes. A delightful man.

KLARMAN:

Absolutely! That's the right word. There were two other members on the study section who were delightful. One was Sol Levine, a social psychologist who later joined Hopkins, and Ozzie Simmonds, an anthropologist who died prematurely. A very nice man, smart and witty.

NEUHAUSER:

They were both involved in that study about quality of life under drug treatment, published in *The New England Journal of Medicine*.

KLARMAN:

Could be. I started to read *The New England Journal of Medicine* regularly when I retired. We subscribe to it now.

Sol Levine left Johns Hopkins to return to Boston. And then, somewhere along the line, he also connected with the Kaiser Foundation. I think he has gone back to a university. Sol was the total charmer; I have never met anybody who was so charming.

When I left Hopkins in 1969, I thought Sol was going to remain there the rest of his life; he was treated so well. He must have gotten an offer he couldn't turn down, and he was able to take his colleague, Norman Scotch, along with him.

I don't know that I've done full justice to the book on hospital care in New York City. It displays everything I knew at the time, everything I had learned. It was certainly not in the Hospital Council tradition, given the emphasis on finances, on hospital organization and ownership, on physician staff appointments, and stuff like that.

Out of the economist's kit of tools, I brought to bear the Lorenz curve. People were

shocked when they looked at the distribution of the roster of physicians by the number of patient admissions to the hospital. It is such a simple graphic, but health services researchers have not used it. Unfortunately, I didn't publish any articles on the subject.

My effort, at that time, to determine the sources of increase in hospital costs was rather lame. But there was a new emphasis on institutional arrangements, as I call them: that there is such a thing as a hospital staff; that there is such a rule as only physicians can admit patients; and that physicians are expected to provide some service to the hospital in return. The Hospital Council never dealt with such matters and neither do economists. Maybe sociologists do, but it's never brought together. Milton Friedman would—and did—translate this fact directly into a monopolistic income advantage for physicians. Yes, there is that aspect, but the picture goes way beyond that in complexity.

NEUHAUSER:

I think that's an issue for a lot of medical economists. Mark Pauly, for example. Just how does one conceptualize this peculiar organization in economic terms?

KLARMAN:

With Mark, it doesn't seem to matter what he is studying. The approach is the same; hence, so are the answers. I prefer to read authors whose conclusions are not so predictable.

NEUHAUSER:

I had come to know the University of Chicago health economist Reuben Kessel.

KLARMAN:

He drew heavily on the Friedman-Kuznets book. I got to know him. I remember sitting next to him at meetings, enjoying his comments. He died much too early, and never really found himself. Do you agree with that?

NEUHAUSER:

I think that's right. Perhaps to his disadvantage, he lived under the shadow of an extraordinary group of economists at the University of Chicago at the time. We have spent some time in a summer home in Maine, and, for several years, he also rented a house nearby, so we would get together. I gave him a copy of Richard Titmuss's book, *The Gift Relationship*, which I had just read and he hadn't, and I said, "Now here's someone who's worthy of your merit to take on, because he's really challenged the Chicago school." Reuben took it up and wrote an essay on it. I think that became one of his major enterprises, thinking about the economics of the blood supply. I did like him.

KLARMAN:

We had a friendly relationship, but not very close. Certainly Kessel was obscured by strong personalities like Friedman, Stigler, Wallis, and Frank Knight. At that time, Jacob Viner was still at Chicago, before moving to Princeton.

NEUHAUSER:

I can remember being a doctoral student at Chicago. It was not always easy.

KLARMAN:

I think I would have loved it, but I can't be sure. For some period, I was close to Milton Friedman, as I previously related. I was never close to Stigler, but we did have dinner a few times at the Liebenbergs, Maury and Bea, in 1944-45. You remember I mentioned Maurice Liebenberg. Stigler was then working for the Office of Price Administration. He was not enough of a mathematician to work for the wartime government, the way Friedman did on the proximity fuse. Friedman was a top-notch mathematician-statistician, as well as economist.

NEUHAUSER:

Friedman was working on the proximity fuse? I imagined that being the thing engineers do.

KLARMAN:

Yes, but you needed statisticians, too, mathematical statisticians. Stigler wasn't quite up to that level. Stigler wasn't unpleasant, but had a rather sharp tongue. He really bit, intending to do so. I remember attending the annual meeting of the National Bureau's conference on income and wealth in the late 1940s at which the young Bob Solow got up and said, "Well, I don't know anything about the subject, but they put me up because they think I can handle Stigler."

That's Bob Solow for you. I also met Solow later on, in another connection. I wrote an article for the special 1974 issue of *Public Interest*, jointly edited by Ginzberg and Solow. I enjoy reading Solow in *The New York Times Book Review*. He makes good sense to me at least, and I invoke his words frequently.

Let me finish my recollections of Friedman. I met him as an instructor at Columbia. He was not a faculty member in the regular university; he was an adjunct, a part-time teacher, at the School of General Studies. In 1939, we happened to move to Wisconsin at the same time. I got to know him there. Whenever I visited Chicago for some years after the war, I would visit his home. I still remember, with my eyes closed, Milton playing with his young children on the floor. I got to know his wife, Rose Director; her brother was Aaron Director, of the Chicago law school. Both Aaron and Rose struck me as much more ideological than Milton. Milton was always outgoing, friendly, and he would go along with you and say, "Alright, I'll concede a couple of points, and then we can argue." I remember Walter Heller telling me once that he used to appear regularly in debates with Milton at conventions. Walter said, "It didn't matter how

many points he gave me. I always lost." Friedman's ideology came across mostly in his writings. I don't quite know when the ideology took hold, but he knew my own policy leanings and yet, I would say, we were friends. In fact, in 1946, he offered me a postdoctoral fellowship at Chicago. I thought the time had come for me to go to work; I'd had enough of schooling so I declined. In our last contact, I wrote him after he was awarded the Nobel Prize and told him that it was late in coming; I thought the delay was unfair. He wrote back thanking me; that's the way he felt about it, too.

NEUHAUSER:

Well, yes, I do remember some rather strong words from Gunnar Myrdal about Friedman's winning the Nobel Prize.

KLARMAN:

If you want to talk even pure economic theory, Friedman had done solid work, and his monetary history with Anna Jacobson is still the standard work in the field. My main point, I suppose, is that he's one of the few people of that caliber I can claim as a friend.

To change the subject, you probably don't know what Adam Smith, the founder of modern economics, said about physicians, but he said something specific.

NEUHAUSER:

I can't remember.

KLARMAN:

Well, he said that physicians ought to have the level of income that's appropriate to their station in life as people whom we trust. Isn't that interesting?

NEUHAUSER:

When I read Adam Smith, I was truly astonished. This could be a basic textbook for

today.

KLARMAN:

But it's not quite that way. A lot of economics since then is about issues that certainly didn't arise in the economic conditions of Adam Smith's time. Adam Smith was a man of the world, his world; this made a difference. I think it may also make a difference among economists whether or not they have experienced health problems personally. Those who are most successful tend to be very healthy. They have the energy, the stamina, and see physicians differently from those who have had a lot to do with doctors in their lifetime.

NEUHAUSER:

The economist from England, Dennis Lees? I think he may have had polio.

KLARMAN:

Well, he had the experience, just not a good one.

I had the same internist in New York for more than 30 years. His name was Steven Yohalem. Unfortunately, he died before me. This is another lesson I learned: at some point, you must pick a physician who is younger than you. We became social friends. He was bright, one of the brightest people I have met. You know Friedman is bright, so is Martin Feldstein, and so was Yohalem. He would kid me about being a hypochondriac, but he was always there when I needed him, and he would listen to my complaints. I tried any number of orthopedists for my bad back, but if I told him we're not getting anywhere, he would refer me to a different one. I remember one night, Mary, Seth, and I were in a restaurant; I fainted and was taken by ambulance to St. Vincent's Hospital where I stayed for a few days. Steve did not have a staff appointment there; therefore, he couldn't charge for his visit. He came and saw me anyway. In addition to the M.D. degree, he had a Ph.D. in chemistry, I think. He was interested in

everything, and related many stories about his wartime service in the Pacific. He could still do minor surgery. He was a gourmet cook. He would have loved being a professor, but there was just no way. He was always complaining to me that the professors of medicine are not clinicians, but people who work mainly in the lab.

NEUHAUSER:

That certainly was the case. I think it's going to change again.

KLARMAN:

You think it'll go back to clinicians?

NEUHAUSER:

Yes, I think so for two reasons. One is the molecular biology revolution—the research is such that the people who do it, do it full-time now. That someone could be a clinician half-time and in a laboratory half-time has come to an end. The second reason is the demanding nature of the business end of a large clinical service. Clinical chiefs will be managers of large organizations within organizations. I think clinical research will increasingly be about how care can be made better. This will include research on quality of life. The research that Bob Brook does will be seen as a very acceptable research background for someone who might become the chairman of a department of internal medicine.

KLARMAN:

Unfortunately, as I've told you, I'm not on top of what Bob Brook is now doing. I don't know how his research applies to people like me with chronic, long-term complaints. That is what interests me as a patient. How do I make the judgment that I'm getting good care? When you talk about the way people have conducted research part-time in the past, I recall Saul Farber. He's the medical school dean at New York University. When I knew him, he had been chairman

of the department of internal medicine for many, many years.

NEUHAUSER:

I was mixing him up with Sidney Farber, the cancer specialist.

KLARMAN:

I know of Sidney Farber; he worked in Boston. As for Saul Farber, I met him when we served on the Veterans Administration committee at the National Research Council. We became fairly close later, and he got me onto the New York State Health Advisory Council. I remember his telling me once that he had a lab; he went there once a week on Friday, maybe for half a day; his staff would do the research. I found that style of leading research hard to grasp.

When I was still on the faculty at New York University, and after Mary rejoined the faculty at Johns Hopkins with my strong endorsement, I found myself commuting between New York and Baltimore. It was wearing. One night, I had the fainting spell I mentioned and wound up by emergency ambulance at St. Vincent's Hospital. It was a stressful period altogether because the director of the health program had been removed. For a while, the position was held by the associate dean of the school, and finally I was called upon. I tried hard to do the job and concentrated on helping our adjunct teachers. I reasoned that, by my taking an interest in them, they'd take a stronger interest in their teaching. Dave Willis, one of the adjunct teachers in the program, was critical of the quality of teaching by his fellow adjuncts. Knowing Dave, I trusted his judgment. He believed that the students were simply not motivated to study.

I was running around a lot by subway, covering too much ground in a short period of time. Looking back, it would have been better if I had been given the job 10 years earlier, in terms of my physical stamina. After the fainting incident, I was out for maybe a month, but came back full blast.

I wasn't myself and I did not fully appreciate the self-seeking behavior of some colleagues, who often coupled criticism of an adjunct with the suggestion to take over the course. So, I was eager to leave the chairmanship. That's when we hired Tony Kovner.

At some point later that year, Dick Netzer decided to leave the deanship. By the way, Dick and I had an excellent personal relationship. I remember asking him once, "Dick, when I ask for something, I get it. Other people ask for almost the same thing and don't get it. Why?" He replied, "Because you respond promptly and to the point. I ask you something and you give me an answer. If people are going to take six months to answer, I say no."

A faculty committee was elected to search for a new dean. In effect, I would say, I was blackballed from membership on it. I was surprised and disappointed. I thought of myself as a good colleague. For instance, when a colleague presented a seminar, I would make oral comments and then I would submit my major comments in writing, assuming that the colleague was going to publish. But this was too naive on my part. My comments were meant to be helpful: I don't try to write other people's papers; it's their questions, their issues, their problems, their style. All I'm trying to do is to clarify and maybe reorganize the order of presentation a little. I've come to realize that I must have generated resentment. But it is something I had not sensed at the time. I did not draw the obvious conclusion from an earlier incident at New York University.

There was a colleague who assumed that nobody knew he was gay, although we all knew it. He was angry at the world when the story of his outing broke. He expected us all to revile him, and we didn't. He told me angrily that he didn't like my dotting his i's and crossing his t's when he circulated a manuscript. He asked for my response. "Why do you ask me to read it then? If you don't want my comments, don't bother to show me the paper." Perhaps it wouldn't

have happened that way if English hadn't been my third language learned the hard way—Yiddish first, then Polish, and then English. Whatever the reason may be, there's no question that I've become a stickler for English grammar and spelling and the nuances of expression. I don't think that's such a terrible fault. As I said then, you don't have to adopt my suggestions.

Altogether, there was much too much politicking in the school for my taste. I never engaged in, always avoided, local academic politics. I didn't approve of it. How much of that have you experienced?

NEUHAUSER:

I think it goes on in some degree everywhere, but I think that's the right answer—just walk away from it.

KLARMAN:

Well, we hadn't had all that much politicking at Hopkins, or if we did, I had enough other outlets to disregard it, both on the outside and inside of the department. I had especially close relations with John Hume, the chairman, and Stanley Mayers, a member.

When the suggestion was made at New York University in 1981 that maybe I ought to take early retirement, I didn't resist it. Not only was the health program insulated from the rest of the Graduate School of Public Administration, I also felt personally alone. In the negotiations that followed, I dealt mostly with the new associate dean, Roy Sparrow. Roy had not been a productive researcher and did not have any great notions about himself as a teacher. He turned out to be a marvelous administrator to deal with. He was generous; he always took my side on timing and on benefits. He got me as good a retirement package as New York University had at the time. It wasn't nearly as generous as the financial package Mary got later from Hopkins, but I was pleased that New York University treated me as well as it did and without acrimony. At

New York University, it's a distinct honor to be retired with the rank of Professor Emeritus.

It was Mary who found this apartment in Baltimore; I had nothing to do with it. I okayed it when I saw it.

Which explains why I left New York University when I did. I haven't been back to New York very often since my sister moved to California. We used to go in to see her as well as to attend the ballet.

Let me point out one thing about my publications: few of them were self-initiated. Nearly all were requested by somebody else. The hospital book that I have discussed at length was self-initiated, the major exception. My accounting article was self-initiated, coming out of my thinking about costs. This doesn't mean that people asked me a specific question. Sometimes they just gave me an area, or invited me to serve on a panel to discuss a certain topic. "Why don't you talk or write about health manpower or health planning or health care spending?"

There is something else about my publications; it's something I dwell on at some length later. I believe it's important to revisit questions one has studied. For one reason, you may have made a mistake. Two, better methods are introduced. Three, the institution is likely to have changed. All kinds of things happen over time. I have no doubt that if you study something once and then forget about it, you're not doing it justice. This is one of the objections I have to grant- or contract-financed research, under which researchers have to move on, not in accordance with whatever revised thinking they may have on a particular subject, or with the opportunity to revisit it a few years later and learn what has happened, as I used to do in New York when I went around town talking to well-informed people. I've witnessed poor presentations of papers by very able people when they're asked to talk about something they wrote two years ago. They have forgotten it and haven't gone back to reread it. I remember once I introduced a

distinguished man whose name I will not mention here. I knew more about his paper than he did, because I had read it before the meeting; he hadn't.

In my 1951 paper in the *American Economic Review*, I did the best I could at the time, given the scarcity of relevant literature. I believe that the unpublished version, which is somewhat longer, is superior to the published version. We ought to be able to do something about that. I know the journals are properly concerned about space, but sometimes you delete good stuff regarding context. Still, to this day, I don't know what the proper solution is to that problem.

I was able to pose the question for my paper at the American Economic Association meeting because I organized the session. How did that happen? Milton Friedman asked me to do so. How come Friedman asked me? He knew me, and knew I was working in the health field. But that wasn't the only reason. It's the president-elect of the American Economic Association who organizes the program for the annual meeting. Milton was acting on behalf of Frank Knight, who was an old man then. Was Knight still alive when you were there at Chicago?

NEUHAUSER:

I think he may have been alive, but not active.

KLARMAN:

He did not live in this world, Frank Knight; that's my impression of him. And Milton was glad to do this for his mentor. I convened the session, and I assigned myself a paper. I dealt with the question of the need for physicians: to this day, I'm not satisfied with how to measure that. I do believe that I know more now than I knew then. I tried this way and I tried that way. Ultimately and essentially, the paper was based on prepaid group practice, because I was

favorably impressed by the people who launched the Health Insurance Plan in New York City. They were capable, they were kind, they intended to do good. I got to know George Baehr. He was more than a founding physician; he was the founding president. George Baehr was also Fiorello La Guardia's favored successor as mayor. Once, at an American Public Health Association meeting in Kansas City, over a beer, I asked George, "Is it true what I hear about La Guardia's strong preference?" He said, "Oh yes."

One learns about people over time. The first time I met George Baehr, I was working on the Hill-Burton study. I went to see him to talk about the current scene. Apparently, he wanted to impress me: how would he do that? He talked at length about social welfare in New York State in the year 1810. Very articulate and slick. That's the way these personalities show off. From what friends tell me (I never had him as a doctor), he was a very able, kind physician. He was also such a do-gooder that he never could see a possible conflict of interest in the ways he functioned since he never accepted money. I recall, once, his appearing before the Hospital Council's Master Plan Committee, on which he served. He was testifying on behalf of the city's Board of Hospitals, of which he was also a member. He couldn't see any possible conflict between the two positions. In his view, conflict of interest had to do only with money, not ideas, or causes, or institutions.

NEUHAUSER:

I should know more than I do about the origins of the Health Insurance Plan of Greater New York.

KLARMAN:

Mayor La Guardia was a strong force behind it. I think the person still to talk to is Sam Shapiro, who was there almost from the beginning. I think Sam was forced out, but he has not

said that to me; I don't know what actually happened.

Sam was one of the people I would go and talk to in New York. I came back to New York in 1969 and must have gone and talked to him in 1970. "What are you doing?" He tells me he's looking for a job. He has several offers. Hopkins is a possibility; Sam was surprised that I advised him to go to Hopkins. He said, "But you just left Hopkins." I answered, "What has my leaving got to do with what I think is a very good job for you?" Of course, it's the best thing that ever happened in his career. His wife has never been happy about Baltimore; she's always talked it down. I think it was an excellent academic job for Sam Shapiro, with only a B.A. degree.

That's one of the good deeds I did for Hopkins. Maybe Sam would have gone there anyway, but my advice to him was strong.

NEUHAUSER:

Did you have much more contact with Nora Piore in New York?

KLARMAN:

Yes, a lot. We also became social friends. The Piores had wonderful New Year parties. I met Professor Rabi, the Nobel physicist there. I remember meeting Jeff Greenfield there. You would know him from the Ted Koppel program; he was then a political adviser, a consultant. I met Bob Marshak, a physicist, then president of City College, formerly a student at James Monroe High School and Columbia College.

I was in New York at the last session of the Health Services Improvement Fund, held in October. They keep on inviting me. I don't attend every year, but this time it was going to be about health care reform and the speakers were Karen Davis and Harold Luft. I've known Karen well for many years. So I went. In my opinion, the speeches were not worth the trip. Nora sat in

the row in front of me. That was nice, like old times. She told me they had just moved downtown. Mary and I are going to New York for a birthday party on the 19th. A former student of mine, Paul Thompson, is having a 50th birthday, and his wife is organizing a party for him. It's not often you are that close to a former student. He tells me he has invited the Piores, Nora and Manny, but hasn't yet heard from them. My former doctor's widow, Alice Yohalem, will be there.

NEUHAUSER:

Did you come across Margaret Olinsky?

KLARMAN:

No.

NEUHAUSER:

I think Nora Piore was involved in writing a book on health and poverty in New York City.

KLARMAN:

Well, Nora's starting job in health was as a speech writer in the Health Department. And then she moved on and upward. We sat on a number of committees together. She was a very good organizer of meetings. I came to feel that she was not so tough as I. I remember on one committee, Nora and I were on the same subcommittee and the subcommittee delivered a report. I assumed that this subcommittee report would then be considered by the whole committee. That wasn't the way the chairman of the committee felt and decided. Because I had served on that subcommittee, I was told I couldn't participate in the deliberations on reports by any other subcommittee or by the committee as a whole. Have you ever experienced that? It's never happened to me before or since.

NEUHAUSER:

It seems like an odd thing to do.

KLARMAN:

I still don't understand why you work hard, you try to do as good a job as you know how, and then you find yourself out in left field. I don't know whether it bothered Nora. We've never talked about it, but she did not react as strongly as I. I resigned from the committee.

NEUHAUSER:

How long have you known David Willis?

KLARMAN:

I don't remember just when I met him. I may have met him through Bob Sigmond—the Pittsburgh-Rufus Rorem connection.

NEUHAUSER:

With Cecil Sheps?

KLARMAN:

No, that was different—the School of Public Health. Rufus organized the Hospital Council in Pittsburgh.

NEUHAUSER:

That council was famous in its day.

KLARMAN:

Yes, deservedly so, if only for the hospital-physician relationship. Nevertheless, let me tell you something that I have long believed. I used to tell it to Bob Sigmond; I'm not sure he agreed. I told him that what you learn in cities like Pittsburgh, Rochester, Detroit, or Birmingham, Alabama, about technical methods for planning is likely to apply elsewhere, but

what you are able to accomplish substantively by persuasion in these cities is not transferable to other parts of the country. Why not? The cities listed above contain a few big, influential corporations and strong labor unions, which dominate the local voluntary nonprofit sector. What they say goes. Rorem and Sigmond were disappointed when they moved to Philadelphia and tried, but failed, to implement what they had learned and accomplished in Pittsburgh. I knew New York City, which perhaps wasn't the most difficult, but it wasn't as easy as Pittsburgh or Rochester, where a few people made the decisions.

I remember once, when Jack Halderman had been recruited to head the Hospital Council of Greater New York, a dinner meeting was held in Washington to say good-bye. I sat next to Halderman and tried to brief him on the ethnic and religious sensitivities and the political situation in New York. He wasn't interested; he thought such factors didn't make any difference. I didn't and still don't understand his reaction.

NEUHAUSER:

Each city is different and distinctive.

KLARMAN:

Why Bob Sigmond didn't think this was important I don't know. He's a very intelligent and astute man.

NEUHAUSER:

Yes.

KLARMAN:

I borrowed your oral history interviews with Bob Sigmond and Walter McNerney. I had no idea that Bob was still so active. He's certainly one of the smartest people in health planning. Not only smart, but also wise. He could see relationships few others saw. He was the one who

brought to my attention the whole problem associated with duplicate or multiple hospital staff appointments for physicians. That's when you really have hospitals compete with goodies for physicians. I'd never thought of that. It's a terrible deprivation for a doctor to lack a hospital staff appointment: so what's wrong with having two rather than one? He told me that they didn't do much analysis of this relationship in their individual hospital studies.

Much depends on who is the planning agency's boss. A person like Rufus Rorem didn't have many answers in advance; he had ideas, but was always unsure, uncertain, and posed lots of questions. We never had that kind of intellectual and questioning leadership at the New York Hospital Council. In my time, there was John Pastore, the control freak; Tony Rourke, who was tossed out for being a successful consultant to individual hospitals; and Hayden Nicholson, who seemed to be uninterested in planning.

Nicholson left the Hospital Council about a month after I announced my imminent departure. I stayed around to help clean up. Jack Halderman turned out to be a poor choice as successor for reasons I'd rather not discuss. Talk to George Bugbee, who was on the search committee. When George told me what he knew at the time of the appointment, I told him he shouldn't have made it.

I don't know whether Rufus Rorem could run a staff without a Bob Sigmond as deputy.

NEUHAUSER:

George Bugbee and Odin Anderson were quite a combination. George would be the first to say he wasn't a researcher, but he was an awfully good marketer of research ideas.

KLARMAN:

Much more than that. He was a man you could talk with. He was an intellectual. He talked about ideas.

You brought up Jack Feldman earlier. The first time I encountered the name was in the book authored by Odin Anderson on the national health insurance survey, with the assistance of Jacob Feldman. I was told later it was the young Jack Feldman who designed the survey.

NEUHAUSER:

I think that survey was done by the National Opinion Research Center.

KLARMAN:

That's what Paul Sheatsley told me. I got to know Paul quite well.

NEUHAUSER:

I think that's right. Jack would organize the data, and then Odin would analyze the data and put it together in a book.

KLARMAN:

Jack went on to Harvard, but was denied tenure. That's why he moved to the National Center for Health Statistics. The Center couldn't give him a high enough salary without making him titular head of something. That's a real problem in a bureaucracy. In this instance, it was a mistake. He should function alone, be given a long rope. He is very smart, but I don't think he relates easily to people.

NEUHAUSER:

I had the feeling he was a deputy head at the National Center for Health Statistics.

KLARMAN:

I'm not sure how far one should go beyond discussing technical questions with Jack. The year I spent on the Guggenheim fellowship, Mary took leave from the New York Medical College and spent the year at the National Center for Health Statistics under an intergovernmental personnel agreement. My old friend Dorothy Rice was the director. The four

of us met once to talk about health planning. I observed that beyond technical statistical questions, Jack Feldman had little to offer on public policies.

NEUHAUSER:

Jack is a thoughtful person on research questions, no doubt.

KLARMAN:

More than thoughtful. I should say, expert.

I rank him very high. At one time I thought of him as a boy genius. I was pleased when he went to Harvard. Why he didn't get tenure at Harvard I do not know. Perhaps—this is a wild guess—he didn't teach well or didn't publish enough.

NEUHAUSER:

I don't know. A person that you may have come across is Al Yerby.

KLARMAN:

I knew him and saw him quite a bit when he worked in New York. He wound up at the Armed Services Medical School.

My recollection of black professionals in general is shaped by my own experience and subsequent reflections. I had a student, a black man, who later arranged to put me on a committee of the National Urban League. I think my credentials on blacks are good, so I feel that I may talk freely. The good black students are identified early in the black community. They don't have the chance to mature and grow. Too much is expected of them too soon. Instead of climbing the professional ladder gradually, step by step, they are appointed heads of departments. My student missed too many classes; he wasn't around when I gave exams. He made it up, but didn't get a solid enough education. It wasn't his fault by any means. How can you refuse all the invitations to lead, to make up for past discrimination? I think senior people

should exercise some restraint and allow good prospects to grow and mature slowly and earn their keep. That's been my experience as a teacher. I've had only good black students, and they didn't stay long. They moved up and out much too fast.

NEUHAUSER:

I think Al Yerby died two weeks ago after a long illness.

KLARMAN:

I didn't know that. I must have missed the obituary.

NEUHAUSER:

There was a fairly lengthy obituary about him in *The New York Times*. This reminds me of Rashi Fein. He was originally from Baltimore.

KLARMAN:

Let me tell you how I first met Rashi. He's originally from Canada, Manitoba or Saskatchewan. His father was a rabbi. You know the name Rashi, author of a famous commentary on the Bible?

NEUHAUSER:

On the Talmud?

KLARMAN:

I think it's on the Old Testament, but maybe on the Talmud, too. I don't remember although I studied both as a youngster in Chmielnik. The Rashi script is quite unusual.

I first met Rashi Fein in Washington in 1951-52 at the National Security Resources Board. His major professor at Hopkins, Evsey Domar, who moved to the Massachusetts Institute of Technology later, brought him to talk to me, a live health economist. Subsequently, Rashi spent several years at Brookings. His problem was the same as everybody else's with Joe

Pechman. If you didn't produce a book a year, you weren't good enough. He was forced out. I had a lot of sympathy for Fein, although he moved on to the Harvard Medical School, which is not a bad post. Joe and I were close friends, but he was a tough taskmaster with very high standards of performance. Burt Weisbrod, who was at Brookings for a couple of years, was also forced out.

I got to know Rashi; we ran into one another frequently at meetings. At the Commonwealth Fund dinner I was given at retirement in New York, he was the main speaker. He has ceased to be a major public player in health economics for obvious reasons. He's so clearly a partisan advocate of single payer national health insurance.

If you read *The New York Times* today, who is quoted most? Uwe Reinhardt and Stuart Altman. Occasionally, an article cites Mark Pauly.

NEUHAUSER:

Well, Uwe is a master of the short comment that grabs your attention. He creates a superb sound bite.

KLARMAN:

More than that. Uwe loves to play the role of a Socrates, this very witty and clever common scold.

NEUHAUSER:

Well, Uwe certainly has the talent of filling a room.

KLARMAN:

It has been a long, slow climb for Uwe to gain recognition. Many years ago, I recall, he complained to me about lack of acceptance by colleagues. Nowadays, of course, Uwe makes many speeches for pay.

I don't know how many speeches he makes a year, but let me tell you a secret I learned from Walter Heller after his service as chairman of the Council of Economic Advisors. Walter would spend two weeks a month teaching and two weeks a month on the road, where he would make basically the same speech—let's say six speeches over two weeks with a little local color thrown in. This is something I've never done, so I wouldn't necessarily understand. A fact that isn't widely known is that Uwe is a member of the Physician Payments Review Commission. He has inside access to current research. He knows what's going on in Medicare and Medicaid, just as Karen Davis does. Why Karen has chosen not to get too involved publicly, I don't know.

This commission was formerly headed by Philip Lee, now the assistant secretary of the Department of Health and Human Services. I have high regard for Phil Lee, whom I saw a good deal in the late 1960s at the Sun Valley Forum. Phil is a marvelous chairman, a good moderator, and a nice man. His father was Russell Lee; I don't know if you know that name. He founded the Russell Lee Clinic in Palo Alto and was a forceful advocate of group practice. Phil has all the requisite diplomatic skills and is thoughtful.

Stuart Altman, whom I first met in the Nixon Administration, had previously worked for the Johnson Administration. He is a member—chairman, perhaps—of the Hospital Payments Commission. He, too, has access to solid inside information. Uwe and Stuart are perhaps bureaucrats in a different guise.

NEUHAUSER:

I think Stuart Altman and Phil Lee are similar examples.

KLARMAN:

No. Not so much Phil Lee. I'm talking about the health economists, Stuart and Uwe.

Henry Aaron is very much in the Brookings Institution tradition, as established by Joe

Pechman. You've got to be an activist. This is what the role of senior fellow is. As long as your research is straightforward, be as outspoken as you care to be since that's what Joe did. Henry Aaron is a sweet guy, a lovely person. I called him up once, it must have been more than a year ago, asking for some report I sought. He sent me a whole bunch of his articles. I read his stuff and wrote him a long letter. He came back apologizing because he didn't answer it promptly. In the meantime, his mother had died, but he didn't mention it. I've seen something in his performance that I wish he didn't do. I can't tell it to Henry. When Clinton had that famous economic summit, which I saw on C-SPAN, Henry spoke up and was quite negative. He sounded irritated, annoyed. Recently, he wrote an op-ed page piece for the *Times*, which you may have seen, on the Cooper bill on managed competition. I thought it was unnecessarily harsh. One can make the same points without being harsh. Maybe he did this because he's not an insider in the Clinton administration, and he would like to get closer to it. I don't know. If he were a close friend like Joe Pechman, I would offer him advice. Still, Henry manages pretty well without it.

I see these guys operating and think about it. There's no question that when you sit on these high-level committees, you enjoy an advantage, having access to information that nonmembers don't get.

When you retire, you do lose the benefits of your information networks. When Dorothy Rice was in Washington—you know her connection with Joe—she would send me all kinds of things, including internal memos. These weren't secret, but, normally, I wouldn't have seen them. I was on her mailing list. Her technical people, the data producers, came to love me. I was the outside guy who was interested in their work, who would also share his information and findings with them.

Once, I served briefly as a technical consultant to review the U.S. Census. I was pleasantly surprised at how much its people knew about health care. They publish a volume every 10 years. They certainly didn't need me as a consultant. I had no idea that staff were that knowledgeable about health care in the Bureau of the Census.

For several years, I served on what was initially called the Governor's Health Advisory Council in New York State. Over the years, it became less and less important, particularly after its top members dropped out or moved to positions in Albany. What was most useful was having all of the senior officials of the state come and testify. You do learn things.

We're talking about the late 1970s. I was shocked to learn that the New York State mental hospital budget hadn't gone down. The patient load was way down. Consequently, there was no money to move to community mental health centers, which had been promised. The officials gave us reasons, but why those facts were not published I don't understand. Still, I don't think it was my job as a Council member to publicize them.

Officials with considerable reputations testified. I don't know where some got their reputations. Governors are entitled to make mistakes, I suppose, when they appoint commissioners; maybe all they want is a commissioner who doesn't get them into trouble.

NEUHAUSER:

I think there's a lot to that.

KLARMAN:

I haven't held a top executive position. It's hard for me to know just what I would do. When I worked for Hayden Nicholson, I would frequently say to him, when we were ready to decide on our recommendations, my personal preference would be to do this over that. However, as the head of this organization, I don't think you ought to do this. It may seem unlikely, but I

spoke to him in this manner frequently.

NEUHAUSER:

Can we talk about Johns Hopkins now? You were right over here in the old campus. [Note: The Klarman apartment overlooks the Johns Hopkins Homewood campus.]

KLARMAN:

No. I worked at the medical campus, at the School of Hygiene and Public Health. While the Homewood campus was glad to give me a joint appointment in political economy and let me teach local public finance, it didn't pay any part of my salary. It got a freebie. I felt welcomed by some Homewood faculty—Carl Christ, Ed Mills, Lou Stettler.

NEUHAUSER:

Who were some of the faculty in the School of Hygiene and Public Health then?

KLARMAN:

At that time, in 1962, John Hume was chairman of the Department of Public Health Administration. Members of the Department were Tim Baker, Stanley Mayers, Charlie Flagle, John Young, Murray Wylie, and Carl Taylor. Later on, we were joined by Kerr White, Phil Bonnet (Phil was also a member of the Study Section), Bob Kohn, Monroe Lerner, Sol Levine, and Norman Scotch. The Public Health Administration Department broke up in 1970, and its divisions were converted into departments when John Hume became the School's dean.

The first day I reported at Hopkins, two letters were waiting for me. One was a formal invitation to join the Health Services Research Study Section of the National Institutes of Health. This was the first time I learned that I was engaged in health services research. I was reminded of the famous line in Moliere's play, *Le Bourgeois Gentilhomme*, when Monsieur Jourdain learned that he'd been talking prose all his life. Perhaps, that's not quite fair, since I was

expected to function as the economist member of the Study Section.

The other letter was from my nephew, Howard Machtinger, telling me that Columbia College had turned him down for admission. At the time, fortuitously, a classmate of mine, John Alexander, was acting dean of the College. I called up John, a friend, and asked, "Can you tell me why?" Howard thought his grades were very good. John said, "No, I can't really answer you, but I think the application was taken up when I was out of the city. Let me get back to you." In a couple of weeks, I got a letter informing me that Howard had been admitted; moreover, he was awarded a Pulitzer Scholarship. That's what happens if you happen to know the acting dean.

By the way, when I was at the Hospital Council, I had similar experiences writing letters on behalf of relatives. One was about a young relative, a patient in a state psychiatric hospital; his parents were getting old and hated to travel to Long Island to visit him. Could life be made a little more comfortable for them, I asked. Shortly afterward, he was transferred to Manhattan State. All I had done was ask politely; however, I did use Hospital Council stationery. Another such experience was helping a cousin enter an occupational therapy school.

NEUHAUSER:

Great.

KLARMAN:

Cecil Sheps chaired the first meeting of the Study Section that I attended. He turned over the chairmanship to Kerr White.

NEUHAUSER:

This was the Health Services Research Study Section.

KLARMAN:

Yes. Kerr and I became good friends, and we still are—social friends. He and Isabelle live in Charlottesville, where my son, Michael, teaches in the law school. In fact, this arrived yesterday—I thought you might like to see it. [Shows a University of Virginia *Law Review* paper published by his son, Michael.]

NEUHAUSER:

Oh, this is one article.

KLARMAN:

It has over 600 footnotes.

NEUHAUSER:

If I remember correctly, the University of Virginia has one of the leading law schools.

KLARMAN:

Yes. It's become a better school than the one he joined. He has tenure now, is very highly regarded, and he earns more money than I ever did. I attend Michael's classes when we visit Charlottesville; he is an excellent teacher.

It was Kerr who lent me his copy of the George Bugbee autobiography. I owe this interview to him; it doesn't surprise me, given the wide-ranging contacts that he continues to maintain.

NEUHAUSER:

George Bugbee's autobiography, as far as I know, is the only published autobiography of someone who was essentially a hospital administrator. The closest comparison is a book of appreciation for the first administrator at the Johns Hopkins Hospital, Henry Hurd, published about 1910.

KLARMAN:

I don't know that one.

NEUHAUSER:

I have to divert you once more. It's my fault. Did you overlap at all with Sigismond Goldwater at Mt. Sinai Medical Center in New York?

KLARMAN:

No, he was a big name, but his career was in the 1930s. I was still in school. I had no idea then that I would be in this field.

NEUHAUSER:

Who else was on the Study Section?

KLARMAN:

I have mentioned Phil Bonnet and Sol Levine, also Duncan Clark, Eliot Freidson, Ozzie Simmonds, Howard Freeman, Bob Haggerty, Paul Sanazaro, Len Rosenfeld, George James at some point, and Ray Trussell at some point. Membership rotation was constantly going on. I think Matt McNulty, too, but I'm not sure because I think I served with him on two different committees. This gives you a feel for the wide range of disciplines represented.

NEUHAUSER:

Yes, a notable group of people.

KLARMAN:

Very notable, and it was a most interesting and important experience for me. The discussions at meetings, the reports on applications, the conversations at meals were stimulating. Paul Sanazaro was appointed the first head of the National Center for Health Services Research. I regarded it as a great appointment; it turned out to be a disaster. This is where certain personal

qualities come in. I wasn't there; I don't know just what went wrong at the Center.

One of the things I brought with me to Hopkins, which wasn't formally a part of my job, but it became so, was that I had accepted an invitation from Victor Fuchs to write a paper on the empirical aspects of health economics. Let me now go back a moment. Fuchs had been in a not very good job as instructor at the School of General Studies at Columbia University. He decided to join the staff of the Ford Foundation; he also decided to be there for a definite and short period, maybe two or three years. What would he try to accomplish during that period? He decided to commission three sets of paired papers on the U.S. Department of Health, Education, and Welfare. One person would write an empirical paper and the other person a theoretical paper—a well-known distinction in economics. Let me tell you who was selected. Ken Arrow, theorist, and Herb Klarman would write on health. In fact, I was not Victor's first choice. Fuchs had tried Rufus Rorem and got some progress notes, which he found disappointing. For welfare, the pair was Bob Dorfman on theory and Margaret Gordon, professor at Berkeley, to do the empirical paper. For education, the pair was Carl Kaysen of Harvard and Ted Schultz of Chicago. By the time this scheme became operational, Kaysen had joined the Kennedy White House. He was no longer available; Schultz took over the whole field of education. Dorfman had been visiting at Berkeley; when he returned to Harvard, he felt he did not have the necessary time. That's how Margaret Gordon came to do all of welfare. In health, it remained the two of us.

I am not able to think or write about empirical aspects separately from theory. I had recently had a similar experience with a paired relationship with Franz Goldmann on aging in New York City.

NEUHAUSER:

Franz Goldmann. Public health in New York City?

KLARMAN:

He wasn't in New York. I don't remember whether he was at Yale or Harvard at the time. I think the American Hospital Association could be very helpful on some of this, especially on the spelling of names. I have to rely on that. Tell the Association it owes it to both of us.

NEUHAUSER:

You've got so many names.

KLARMAN:

By 1962, I realized that I had been far removed from economics and its literature for 20 years or so. I had to learn economics all over again, which I proceeded to do gradually. It was hard work; it wasn't easy to find materials that were reasonably relevant to health care. It was then a slim field. Among the economists who had published, most were part-timers in health. They had done a narrow piece of work. The only person then working full-time as a health economist was Selma Mushkin. I had met her first in 1944, when she was at the Social Security Administration sharing an office with Byron Johnson, a Wisconsin classmate of mine. Later, I got to know her in the health field. She had joined the Public Health Service. It was a special division, Public Health Methods, headed by a man by the name of Perrott. Was it George St. John Perrott? I still remember visiting Selma's office once and I saw the way she was teaching or guiding her medical colleagues. I remember Dr. Stewart, who was greatly influenced by Mushkin. I met Bill Kissick there. I thought that, surely, he was going to be governor of Pennsylvania in a short time; it didn't work out that way.

I found it difficult to come up with materials I could lay my hands on and use. The parts that I had worked on, the hospital stuff, I was reasonably comfortable with, based on my New York experience. On physicians, I drew heavily on Frank Dickinson of the American Medical Association. On cost-benefit analysis, I came to rely on Mushkin. It so happened that, simultaneously, I was working on a paper for a conference at the Brookings Institution. Bob Dorfman had been asked by Joe Pechman to convene and organize a conference on public expenditures. Both Pechman and Dorfman made it clear to me that my paper had to present numbers, not just concepts. Accordingly, I went and spoke with John Hume. I asked him, "What would be a good area in health for me to work on and get numbers on?" He said the only field he knew was venereal disease, and he'd be glad to help me. "I'm available," he said. That's how I happened to write on syphilis. The part on cost-benefit analysis drew heavily on Mushkin's papers.

NEUHAUSER:

Yours is the first book on cost-benefit analysis in health care?

KLARMAN:

Not quite. Monographs by Rashi Fein and Burt Weisbrod preceded mine, but my book received a lot more attention since it is broader in scope, aiming to be a comprehensive summary of the field. It has been used as a textbook.

NEUHAUSER:

I was struck with the relative sparseness of the footnoted references in this book.

KLARMAN:

This goes beyond the scarcity of materials I've discussed. The reason is that I distinguish between footnotes and references. I decided to have no footnotes. If something is important

enough, it goes into the text, even if only in parentheses. It's a style of writing I developed in preparing minutes of meetings. Hence, all my footnotes are solely references. I recall that Joe Pechman thought there were just too many references; he was among the individuals who reviewed the final draft for me. I think I did this largely as a reminder to me of where I acquired a fact, a phrase, or an idea. I strongly believe in that style of writing to this day: no footnotes, only bibliographic references.

NEUHAUSER:

You and your son, Michael, would have something to talk about in that area. You said his paper had 600 footnotes.

KLARMAN:

I don't try to influence his writing style. In reviewing his drafts, I try to make sure of two things. One, is the text clear? Second, is the organization logical? Is a paragraph placed where it belongs? Is an argument of his as well developed as can be? Never mind whether I agree with his position. I don't think he'd be coming back to me all these years for review and editing if he didn't think that I was serving his purpose. I wouldn't dream of trying to influence the way he writes. That's none of my business.

NEUHAUSER:

I'm one of those people who rush to see the citations at the end. I was looking with some interest and reflecting on names of people who were active in the field at the time.

KLARMAN:

It certainly was a very, very slim list.

At the Hospital Council, in addition to other duties, I was assigned early on to write the monthly bulletin. When I came there, the four-page bulletin consisted of maybe three, four, or

five articles, plus an editorial which often was not related to any of the articles. I found that too demanding of my time. Within a short period, I turned the bulletin into one factual article, plus a commentary as the editorial. I still remember a man, a hospital planner, Jack Steinle, who called me one day and asked why I said this or that in the bulletin. I asked him, "How do you know it is I who said it?" He replied, "Your style is unmistakable."

By the way, there is one event I neglected to mention when I was in the process of leaving the Hospital Council. My future boss, Harold Clark, called me from Washington, and said he wanted to talk about my work with my present employer. I put John Pastore on line but something led me to do something I never do. I stayed on the telephone; it's the only time I've ever done this. Pastore said all kinds of nasty things about me, none of them true. I don't think Clark would have hired me with that kind of job history. I went to see General Stanhope Bayne-Jones, asking for help. You know the name?

NEUHAUSER:

Was there a Bayne-Jones report on medical education?

KLARMAN:

I wouldn't be surprised. How did I know him? From the Army, the Surgeon General's office. By the way, I don't think I've mentioned that I also got to know Mike DeBakey in the Army, then merely a major, and Will Meninger, a Colonel.

NEUHAUSER:

No, you didn't.

KLARMAN:

I told General Bayne-Jones what had happened on the phone. Dr. Bayne-Jones was a member of our board, and Pastore had worked at the New York Hospital before he moved to the

Hospital Council as executive director. Bayne-Jones simply said, "Let me take care of this." He wasn't the least bit surprised by what I told him, and he did take care of it. Pastore had to take it all back.

NEUHAUSER:

Is this the right time to talk about Kerr White, since we're in the middle of your Johns Hopkins years?

KLARMAN:

Kerr was chairman of the Study Section in 1962-66, for as long as I was a member. I don't know whether he continued after 1966, when Bob Haggerty took over as chairman. I have found Kerr to be a very effective chairman of a committee. He listened, encouraged discussion, yet kept things moving.

Some years later, I had the experience of serving on the membership committee of the Institute of Medicine. Due to the chairman, when we finished a meeting, I didn't know what we had settled, if anything. Then Kerr took over that committee, and the change was immediate, as from night to day. He was tolerant; the discussion was always open, yet the work got done.

In addition to the usual job of screening applications for grants and rating them, Kerr undertook some additional work for the Study Section. We held a conference on health services research and got those papers published in two volumes of the *Milbank Memorial Fund Quarterly*. That was entirely Kerr's initiative and influence. Study Section members were assigned specific areas and recommended likely authors of papers.

NEUHAUSER:

That was the volume that also had information about health services research in Scandinavia.

KLARMAN:

Yes. I was on that trip. One of the things Kerr was very good at was how to get PL-480 money—U.S. government money for use on projects in countries like Yugoslavia. I got a couple of trips to Yugoslavia off that, to talk to economists.

I don't remember just how the Scandinavian trip was arranged. Bob Haggerty led it; Duncan Clark was on it, and a couple of staff people whom I didn't know well. They were not the regular secretaries of the Study Section.

Kerr was also good at picking sites for travel. I remember once, he held a Study Section meeting in Puerto Rico. I remember, too, joining Ray Trussell on a site visit to Canada—I don't recall whether it was up in Toronto or in Montreal. I vividly remember joining Kerr on a site visit to Rochester; it turned out to be unusual, rather peculiar. We went to see Marion Folsom at Eastman Kodak. He was the former under secretary of the U.S. Department of Health, Education, and Welfare; this applicant couldn't imagine that anybody would question his deserving the grant. Folsom asked, "What the hell is going on here?" But he wouldn't provide any concrete details about the proposed project. Well, we didn't make much of it at the meeting, but the Study Section rejected the proposal. One lesson I learned: people do sometimes get too big for their britches.

I believe I was somewhat influential with John Hume in attracting Kerr to Hopkins. I have already told you about John's great help on the syphilis paper. He listened to what I said, he recently reminded me. When I strongly objected to a proposed departmental rule that we submit papers for prior review by the department before submitting them for publication, the proposal was dropped. I remember one occasion, when the building of the new city of Columbia, Maryland, was under consideration at Hopkins, there was a lot of pressure to establish

there a prepaid group practice unit, sponsored by Hopkins. John called me in and said, "I have a meeting at 2:00; here's a bunch of material that I'm to read and respond to. Would you please let me know before the meeting what you think?" I read it promptly and reported to him. I said, "A lot depends on how rich the university is. If you think it can afford to lose about half a million dollars a year, then by all means have Hopkins do it. Otherwise, get somebody else to do it. The problem with the draft proposal is that the planning has been done on data from Kaiser Permanente by an assistant administrator at the hospital. He has not given thought to a transition phase. You're investing capital now, and you're hiring staff. But clientele builds gradually. For a time you lose money. Current Kaiser data do not reflect that phase." I'm sure John Hume killed the Hopkins proposal at his meeting.

Kerr had this way about him; he kept track of everybody everywhere, no matter how young. He knew about Martin Feldstein as a graduate student. When Martin was about to finish at Oxford, Kerr invited Martin to lunch and had Carl Christ, author of the first textbook on econometrics, join us. What I witnessed, in effect, was a doctoral examination in econometrics, a model examination, for Carl is a great teacher. Martin, of course, handled himself brilliantly. As it turned out, Hopkins didn't get Feldstein. How come? I didn't learn the reason until years later. Hopkins never had a chance. Why not? On a site visit in Boston, I was taxiing from one institution to another. That evening, I was joined at dinner by John Dunlop who was sitting next to me. You know who John Dunlop is? A Harvard labor economist and politician, a former Secretary of Labor. He told me he had obtained a grant with which to hire Martin. That's how Harvard started its program in the Department of Economics, which, within a couple of years, produced health economists Jerry Rosenthal, Dave Salkever, Joe Newhouse, Frank Sloan, and Bob Evans. I don't remember all the graduates' names, but I've listed enough. Martin had a lot

to do with the success of this program.

I have run into Martin Feldstein now and then. I chaired the committee for the empirical study of health economics at the second conference on health economics held at Hopkins. We produced a volume, a collection of papers. One of the papers commissioned was by Martin Feldstein. We wrote to him what was almost a routine letter, saying his paper was a little on the long side; would he please cut it back? He came back with a greatly shortened paper. In effect, he had gutted it; he saved the introduction and the conclusion, but sent the middle section to a journal. The committee turned the revised paper down. I never heard a peep from Martin; he understood, I think.

I had run into the Feldstein name several years before that. On the Scandinavian trip that Kerr White financed, we started in Copenhagen for a briefing by the European Regional Office of the World Health Organization on what to look for and whom to see. It occurred to me then that the officials were showing undue interest in me personally, more so than in the other visitors. I couldn't imagine why until on the way back, I'm in Copenhagen again, and I'm invited to visit headquarters. Would I please come to a meeting they're running? The economist they expected to attend had turned them down. It was Martin Feldstein, then a graduate student at Oxford. Why did he turn them down? They didn't pay a fee for his paper. All they gave him to participate was a daily expense allowance; Feldstein wasn't about to accept that. He didn't take into account the fact that the daily expense stipends were quite generous. Indeed, they sufficed to pay for a lot of the presents I brought back. At the World Health Organization headquarters, they advised where visitors should shop. Anyway, that's when I first learned about Martin Feldstein and the confident way he dealt with people. He wasn't about to be anybody's easy mark.

Early in 1971, Martin invited me to a session held at the Resources for the Future, which he chaired. He introduced Paul Ellwood to us; that's when I first heard about the "health maintenance organization" (HMO). Earlier, I had learned quite a bit about the Health Insurance Plan of Greater New York and Kaiser Permanente. As Ellwood spoke, I soon realized that the HMO was the unlikely combination of prepaid group practice and the independent practice association, which was the community medical foundation in California. The two were arch-rivals there. It was quite a trick for Ellwood to combine them, to merge them under one banner, the HMO.

Ellwood struck me then as a most charming person. This was confirmed for me later, when Mary was teaching at a summer session of the University of Minnesota: Ellwood took us out on his boat for an afternoon. I came to think he could sell almost anything to anybody. When he was introduced to us, we were informed that he was then advising both Senator Ted Kennedy and the Nixon Administration. I was impressed.

About this time, I ran into Martin again. He and his wife Kate wrote a column for *The Wall Street Journal*, and we had lunch in New York . The last time I saw him in person was at a small ad hoc committee meeting—it lasted two days—that Stuart Altman convened when he was working in the Nixon Administration. Stuart was then at the U.S. Department of Health, Education, and Welfare. We were asked to examine a proposal by the Rand Corporation to conduct a health insurance experiment. I don't remember who else attended. Martin was the most articulate member; essentially, I agreed with his position, but not because I had done any hard thinking on the subject. In fact, I had always felt that Milton Roemer was probably right about Roemer's Law, even though I hadn't seen any rigorous proof of it. The available evidence was rather anecdotal. I could see that when you build a city hospital in the borough of Queens

that has no business being filled up, it tends to be occupied.

Martin objected to the proposal, and I supported him, but Stuart did something quite unusual. Stuart Altman is a very effective guy, a skilled politician; I don't mean that characterization unkindly. Instead of having the group vote or issue a consensus opinion, he had each of us write a letter to him afterward. I later collected the letters Stuart had received, for teaching purposes. The majority opinion of the group was no.

Martin had persuaded most of us; his argument was that, in the proposed designed experiment, the numbers of persons and of utilization in an area would be so small that one couldn't possibly detect a supply effect, if there were one. To me, that argument was persuasive.

I remember Joe Newhouse responding to the negative recommendation. He wasn't really addressing Martin's argument. For Newhouse, the major issue was this: if the Office of Economic Opportunity didn't give Rand the grant to conduct this project, the money would revert to the U.S. Treasury. This would be a terrible loss to health services research for no good reason. I think Stuart Altman agreed with his committee, but somewhere at a higher level in the Department the project was approved. That's how the Rand Health Insurance Experiment was funded.

Many people, I think, would argue that, while the project may not have been good for the particular problem it studied, it did yield a lot of solid, interesting, analytical papers. From what you have said, you must know Chuck Phelps; I don't. I wonder under what circumstances he left Rand. There may have been a split within the Rand community, or was it simply that the project had come to an end, so Newhouse moved on to Harvard and Phelps to Rochester. I don't know.

Recently, in *The New England Journal of Medicine*, Phelps published a paper that attracted a lot of comment on how you do this kind of evaluation. Many people justify the

project even if they share Martin's criticism.

One recent night, I was watching C-SPAN. On the screen, unexpectedly, was Feldstein talking; it was a speech he had delivered to the American Enterprise Institute, in which he was discussing the federal budget process, the deficit problem. He argued that the deficit would be much worse than estimated by the Clinton Administration for reasons he gave, namely, that the Rand estimates provided the basis for the Clinton projection of health care spending. Although the Rand figures allow for a demand response, they do not allow for an offsetting supply effect. The result is that you project excessive savings due to greater reductions in use than you will get. This speech reminded me of that committee meeting long ago. Except for one thing: Martin Feldstein was now a national figure in public finance and economic policy, as well as in research.

I wouldn't want you to think that I spent all my time after joining the Hopkins faculty attending meetings, but Hopkins does make one a national figure. Also, Washington is nearby and readily accessible by car; you're invited to look at all kinds of things and you accept. To me, all this was my postgraduate education.

Let me note that at Hopkins I did a good deal of teaching. I taught a course at Homewood, and I taught a course each semester at the School of Hygiene. Indeed, Vicente Navarro was one of my students, so was Bob Blendon, and Cliff Gaus, a classroom star. I also lectured more frequently in the public health administration course. When I arrived, every faculty member delivered one lecture. I protested; I didn't see any continuity in these presentations. Accordingly, we agreed to organize blocks of lectures. I don't remember whether I was assigned three or four lectures. I did more teaching than other faculty members; still, teaching was not at the top of anybody's agenda.

The emphasis was not only on research, but also on participation in outside activities. The

danger was that one might neglect the home base. In my opinion, that became Kerr White's problem. I regret that I didn't stay at Hopkins long enough to caution him on this, to warn him. Some years later, Kerr and I were at an Institute of Medicine meeting and I did say to him, "Kerr, why have you done this?" I don't think he understood that he had neglected his main job. I was really drawing on the examples of Tony Rourke and Pete Terenzio, the administrator of Roosevelt Hospital. It wasn't altogether Kerr's fault because the dean and the associate dean lionized him.

Kerr came to Hopkins from North Carolina, and before that he had been at Vermont. He felt strongly that a public health school properly belongs as a department at a medical school. It shouldn't be independent. Dean Stebbins and John Hume liked the idea that Kerr had this dissenting opinion, which he expressed freely and clearly.

I remember the first year Kerr was at Hopkins. There was an annual meeting of the schools of public health, one I have never attended. Kerr was sent as a representative of the School of Hygiene. He must have felt secure in his Hopkins position. This was unfortunate, because he knocked around quite a bit after leaving Hopkins. The Rockefeller Foundation managed to come up with a decent job for him as associate director. I found it sad when he retired from the Rockefeller Foundation. I remember that only Monroe Lerner, who had been in his department, attended the dinner. There was nobody else from Hopkins; I was no longer there. Kerr and I have remained good friends. Whenever I bring up our relationship, the reaction at Hopkins is that this may be one of my peculiarities.

NEUHAUSER:

Well, I can easily imagine how Kerr White was spending his time being a citizen of the world. Comment about that extraordinary book that he produced of the summary of his surveys

of health usage in eight countries, published by Oxford University Press. It's huge.

KLARMAN:

I have not read it. Robert Kohn (coauthor of the book) was one of his appointments. I have never understood why Kerr did not attract a top-notch group of scholars. Even when his appointees were individuals I should have been happy to appoint, some didn't work out. For instance, he appointed Dave Rabin, who later went on to George Washington or to Georgetown. Dave was an able guy, but he didn't have the entrepreneurial energy to go places.

Even though I think I lacked it, too, I must have had something that got me into so many outside activities. One appointment led to another. A clear example: I was on a study committee of the National Research Council on which the medical director of the Veterans Administration also served. The next thing I knew, when this committee adjourned after finishing its work, who is appointed to the Special Medical Advisory Group to the Veterans Administration? I'm the only National Research Council committee member he drew on. He liked my approach, always questioning, drawing out accomplishments as well as shortcomings.

NEUHAUSER:

What was the National Research Council's Veterans Administration project?

KLARMAN:

What should the Veterans Administration be doing in health care? This is a perennial problem. Should it cut back; should it do this or that? In fact, I remember one young man on the committee who wasn't prominent yet, Paul Starr, then at Harvard. He felt that there was no room for the Veterans Administration because it gave services to veterans that the rest of the population didn't get. What about nonveterans who have the same needs? By contrast, my view is you start with where you are, with what you have. [I don't know Latin; Mary teaches me

Latin; I teach her French.] But there is no such thing as a tabula rasa in public policy analysis. You begin with ascertaining the facts about where you are. And then I invoke the maxim by Florence Nightingale: above all, do no harm. If you combine these two sayings, keeping the Veterans Administration makes sense, even if it's not the best possible system.

I remember an uncle of Mary's in Milwaukee, a retired high school principal. Where did he go for his medical care? To the Veterans Administration hospitals. That's something one would not do in New York City when I was there because we had all the other leading medical centers.

I joined the Veterans Administration's advisory group, which gave me ready access to the Veterans Administration staff. This is always useful; they tell one what is actually going on. The one incident I remember clearly is the one I related earlier about Agent Orange.

Let me turn now to a very satisfying experience. When I was appointed to the committee to advise the U.S. Bureau of the Budget on chronic kidney disease in 1967, Charlie Schultze was its director. I was going to Europe that summer, and expressed a lack of interest in, as well as total ignorance of, the subject matter. But they insisted, so I joined the committee a few months late. It had already been meeting for three months or longer. The other economist member was Jerry Rosenthal; I don't know how well you know Jerry—he's a sweet person, very bright, articulate, and a poor administrator.

NEUHAUSER:

Yes. The last time I saw him was at one of those yearly cocktail parties at the Institute of Medicine. I think he came in a serape and sandals, when everybody else was dressed in a suit.

KLARMAN:

He'd been in Africa, I suppose.

NEUHAUSER:

He had been in Mexico City. I actually came across him there.

KLARMAN:

When I received my award from the Association for Health Services Research, Jerry presented it; he was both flattering and focused. He's lovable in every way, but having spent a year at the National Center for Health Services Research when he was its administrator and I was on the Guggenheim Fellowship, I have to say the agency wasn't being run at all. It wasn't just poor administration on his part; he believed strongly that members of his staff were grown-ups and all would try to do the right thing. But that's not the way it works. At least some people do require some supervision and some coordination as to the agency's mission.

Let me return to the Committee on Chronic Kidney Disease, with Carl Gottschalk as chairman. He was a professor of medicine at the University of North Carolina. I had never met him; he proved to be an effective leader. Other members were prominent, distinguished physicians. I remember the leading surgeon from Minnesota—John Najarian. The committee worked well as a group.

We met often and talked a lot. The physicians certainly taught me a lot about kidney disease. Experts came to our meetings to present current research findings; I remember visitors from France and England who talked to us about clinical programs in their countries. Somewhere along the line I got the idea—I must have talked to Dorothy Rice first to ask who would be a good person to inform us on what health insurance in the United States was doing in financing long-term care for kidney disease patients. Does it pay for transplantation? For dialysis? Dorothy recommended a colleague, Louis Reed, from the 1920s-30s Committee on the Costs of Medical Care; he did a splendid job. The man was lost in the federal bureaucracy. He

had written a good book on Blue Cross, which was mimeographed and never attracted much attention. The other staff members of Committee on the Costs of Medical Care were much more successful, being more assertive. Within a couple of months, Louis came and gave an exhaustive presentation, which concluded that health insurance plans in the United States weren't doing anything and weren't planning to do more. That cleared the ground on possible sources of financing.

There were two questions before the Committee on Chronic Kidney Disease. One was, what are the costs of care? Second, what about outcomes?

The physician members talked freely. Initially, they saw costs as the fees charged by academics, which were, say, $1,000 for a transplantation. I don't remember just how, but the figure rose to $3,000. I thought that was no way to derive realistic cost figures. I learned that there was a separate program in kidney disease at the Medical College of Virginia in Richmond. I went there and talked to the director. In that conversation, I found myself drawing heavily on my career as an economist, plus some knowledge of accounting. This was a separate unit with its own separate sources of financing. What does it cost to operate this unit for a year? How many of these procedures do you perform? Let's try it now in two different ways: how many procedures and how many patients, since some patients are repeaters? I remember vividly standing at the blackboard and presenting to the committee the numbers I was given and my unit cost derivations from them. Nobody had a word to add. The numbers are shown in an article (number 56 in Published Works)—$13,000 was the cost of a kidney transplantation and $14,000 was the annual cost of dialysis at a hospital.

As for the second question, what we get for the spending, Bernie Greenberg, a distinguished statistician at the University of North Carolina, joined our deliberations. I don't

remember whether he was a full member or whether Gottschalk drew him in as an expert consultant. He produced life-expectancy tables to yield estimates of life years gained. Bernie was very able, no question about that.

So we had both cost figures and life-expectancy figures to answer the two questions.

Gottschalk spoke to Charlie Schultze. Initially, Charlie said, "This isn't a persuasive enough report. I want a cost-benefit analysis; otherwise, it's not economics today." I had met Charlie often at Brookings, where Joe Pechman, my close friend, was the director of economic studies. I always found Charlie a splendid listener interested in health policy. I don't know whether you've heard of the Sun Valley forums organized by Margaret Mahoney of the Commonwealth Fund. Charlie attended them from time to time, and we got to know one another.

There was no choice but to try to perform cost-benefit analysis. Jerry Rosenthal, the committee's other economist, and I started talking; we talked intensively for weeks. What he thought was a simple thing to do, I said couldn't be done. I was relying heavily on a 1966 paper by Tom Schelling prepared for the Second Brookings Conference on Government Expenditures; his paper expounded the willingness to pay approach to valuing benefits.

NEUHAUSER:

I think that's a wonderful article.

KLARMAN:

I agree. That's Tom Schelling. The most innovative, most creative person I've known. I met him when he was a young analyst at the Bureau of the Budget; he married David Saposs's daughter, Corinne. David Saposs, a labor economist out of Wisconsin, provided a gathering place for its graduates. I dated Tom's sister, Nancy for a while; she married Bob Dorfman, the

Harvard economist. Washington was a wonderful place to be, in wartime and afterward. True, I was in the military, but I lived in Washington like a civilian. When we went out to dinner, one never knew who or how many would show up.

Jerry and I talked and talked. Finally, I did persuade him that cost-benefit analysis was not feasible. With Jerry, there are never any hard feelings after the debate is done.

You read my article, which I have regretted since then, because I sent off a first draft. You should never do that. Nevertheless, it does make clear why the committee used a cost-effectiveness analysis, and explains the method employed.

While the committee's recommendations reflected this cost-effective analysis, social and political factors were also involved. This is what other countries were doing, and we were hardly in a position to say no. In the back of my mind, I was aware that we may face similar problems in the future with other organ systems, but I felt that we'll deal with them when we get there; who can foretell the future and the actual scenarios? That was my unspoken reasoning. Most of the article is devoted to the cost-effectiveness analysis. I doubt that this is the first such article in health care, but I think it's a good one, owing to the detailed methodological exposition. It is shorter than Milton Weinstein's book on hypertension, which is a terrific monograph.

NEUHAUSER:

There's one on hypertension that Weinstein did and there's one on decision analysis.

KLARMAN:

The former is the book I mention.

There was something else in the back of my mind. A Congressman had introduced a bill—I don't know to this day who it was—to make care of patients with end-stage kidney disease part of Medicare, to create part C. Lots of people advocated such a part C, and the

Gottschalk committee wisely latched on to it. It was simple, so we thought it was doable. Indeed, it was doable; it was done. All this has led me over the years to pay close attention to anything pertaining to care of kidney disease. A major development, not anticipated, was the entry of for-profit firms into dialysis. It had another effect later. I have mentioned three op-ed page pieces I wrote in the 1980s. I didn't feel like getting involved with *The New York Times* or *The Washington Post*. I published them in *The Baltimore Sun* and, therefore, attracted no national attention.

One example of these writings: much of what people say today about the catastrophic health insurance fiasco is simply not so. There is widespread misunderstanding of what this legislation was about. Some people who opposed it did so for the wrong reasons; however, I believe that the elderly were correct in opposing the bill and the statute as enacted. The main reason is that Medicare isn't only for the elderly; it's also for the disabled, and it is for patients with end-stage kidney disease. I happened at the time to talk often to an analyst at the Congressional Budget Office; by the way, it's a very good source of data, often unpublished. I also draw on studies by the General Accounting Office and lately, though rarely, on the Congressional Research Service. The latter is harder to get because it gives access only through your congressman.

At any rate, what I learned was that kidney disease patients were 0.4 percent of the Medicare clientele, but drew 14 to 18 percent of all the new benefits. In the cause of budget neutrality, revenue had to equal expenditures for the new program. The decision was made that the revenues and expenditures would be neutral with respect to the entire Medicare population, but not for the country's population as a whole. The elderly were asked to pay for the benefits offered to the other Medicare beneficiaries.

There was another complicating development in the 1980s. I'm skipping some years now. More and more health care legislation is enacted in the course of the budget reconciliation process. It doesn't pass through the expert House and Senate committees on health care, whether authorization or appropriation committees. This means that, often, the Congress didn't understand what it was doing. I don't mean to suggest they were always wrong, but they were not well informed when voting. One of the changes enacted in the 1980s was that, if you were employed or if your spouse was employed, Medicare became the secondary payer. Once again, elderly people are complaining, "What have I paid for? I'm not getting Medicare, although I pay the premiums."

I often feel that I should write a letter to the editor explaining this kind of reaction in *The New York Times*. Catastrophic health insurance wasn't a fiasco because the Congress got scared of the elderly lobby. There were sound reasons why Congress reversed itself after the elderly came to oppose it. But that's not the way the matter appears in the press.

The way I'm describing events may surprise you. Does it?

NEUHAUSER:

Yes.

KLARMAN:

Catastrophic health insurance is one of the few bills I followed closely, and it reflects my own role on that committee. I had a wonderful experience with a chairman like Carl Gottschalk. I remember about a year or so later, when Mary and I happened to be in Chapel Hill, we were treated to a wonderful party by the Gottschalks.

Serving on the Gottschalk committee was one of my most satisfying consulting experiences. I don't distinguish between membership on committees and consulting, because I

didn't get paid as a consultant. The one or two times when some money was paid, I didn't keep it. The money went to the agency paying my salary. For instance, near the end of my stay at the Hospital Council, the New York City Health Department, George James in particular, then the deputy commissioner who later became the first dean of the medical school at Mt. Sinai Hospital, asked me to do a report on care for the aged. As I mentioned earlier, Franz Goldmann was going to do the theory and I was going to do the empirical part. Goldmann did prepare a memorandum, but I didn't find it helpful. I had to do some theorizing or thinking on my own. I remember going to the Community Council, where I obtained some data. I hired my then City College nephew, Leonard Machtinger, for the summer to compile data on homes for the aged. I had lots of data, but wasn't approaching a resolution. What to do?

I remember this: at some meeting in Southampton, Long Island, Eli Ginzberg and I talked a good deal; as always he was a splendid adviser. One day on the beach, he said, "Herb, I don't care what you say and I don't care what your conclusions are; you've got to finish the report." That was his style of operating. It makes good sense, but it's easier to give the advice than for me to follow it. At some point, you've got to decide things have to come to a halt. So I completed that job for the city and submitted my report. The city paid, but it paid the Hospital Council, not me. I never felt that I was imposing on the council's staff. Rather, I saw it as an opportunity for the Hospital Council to learn and know about care for the aged. I paid special attention to home care.

NEUHAUSER:

This was a project done when you were at the Hospital Council?

KLARMAN:

Yes. Formally, I was a consultant to the New York City Health Department, but I don't

distinguish it as a separate activity because I didn't take the money. The city paid whatever we billed. I don't remember whether it was at a daily rate; it wasn't much. In fact, I got good vibrations out of it because several years later, when Leona Baumgartner had left New York City and moved to Boston—she was married then—I remember she asked me to come up there to help a former council intern, Bob Murphy, to advise her on comprehensive health planning.

My impression remains that, for reasons I don't quite understand, when I worked on a committee or did a consulting job, people liked the performance or product. They felt I had done my best, and regarded me as an honest broker of both facts and competing viewpoints.

NEUHAUSER:

Leona Baumgartner. Her husband was Alexander Langmuir, the epidemiologist?

KLARMAN:

You got it. I had not expected her to request my services in Boston.

One of the developments during my years at the Council that pleased me no end was getting to know Nora Piore. She called or came to see me frequently; she was then an adviser, a consultant to Dr. Baumgartner on media, and a speech writer. The proprietary hospital people called me with their concerns. It was generally understood that I was trustworthy on facts and objective in presenting diverse viewpoints.

I had little inclination to talk to news reporters. My experience with reporters was, on the whole, not pleasant. You remember the name Lucy Freeman? After working for *The New York Times*, she became sort of a social worker, a therapist; she wrote books. At a big dinner, like the Greater New York Hospital Association's, she would sit next to me so I could translate what was going on and explain to her what it meant. That was certainly flattering, and Lucy was appreciative. Other times, when I asked *Times* reporters to read a bulletin of ours for elaboration,

the answer was they had no time to read it. I didn't see why I should bother to talk to people if they had no time to read four pages. To me, this meant they were lazy. And I wasn't interested in the publicity, in being cited.

Perhaps my attitude went back some years to my high school graduation. I have mentioned that I'd gotten a bunch of medals. Once, I was written up in *The Jewish Daily Forward.* It was then still written in Yiddish. They had gotten a picture from somebody in Paris who had seen my name on something. Then, there was a paper called *The Bronx Home News.* They wanted to interview me after high school graduation; I said, "No, I don't want to be interviewed. What am I going to say? Am I going to be humble or shall I brag? What should I say? The awards speak for themselves."

Of course, not all outside activities were educational or enjoyable. I was a member of the U.S. National Committee on Vital and Health Statistics in the mid-1960s. The problem was that the chairman was on leave in Europe for much of my term; he came back for the occasional meeting. The committee hadn't done anything, and the chairman wasn't engaged. That assignment was a waste.

I sat on an HMO committee in the early 1970s at the Institute of Medicine. I don't remember all its members, but the person who was outstanding to me at the time was its secretary, Karl Yordy. You know the name?

NEUHAUSER:

Oh, yes.

KLARMAN:

Karl has recently left the Institute of Medicine. I have been meaning to write to him, but I haven't done so. There occurred a natural experiment involving Karl. As the secretary to the

committee, Karl wrote the minutes and helped to move things along. Then, he was away for maybe six weeks; I don't know whether it was due to illness, or vacation, or performing another assignment. During his absence, another staffer substituted. I thought he did poor work; the minutes didn't reflect what was said. Then, Karl returned, and the committee straightened out. It's not often that you witness this kind of experiment—a break and resumption. Karl Yordy was as competent a bureaucrat as I've come across. He was a pleasure to work with. I'm sorry he has left the Institute of Medicine. He was my link there; if I wanted to know what the Institute of Medicine was doing or studying on a subject, I called Karl. He would talk to me; it was usually a long conversation. For instance, he was the first to mention to me the name of Judy Capers as somebody who belonged as a member in the Institute of Medicine.

Let me talk about the Institute of Medicine. The Institute was founded in 1970 by a committee—I don't know whom they represented. Rashi Fein was a member of that group, and he would know who the other members were. The first news I heard was that Victor Fuchs, Martin Feldstein, and Herb Klarman were elected—see the 1971 class in the listing. I think economists were given more recognition and higher regard than we had yet earned. In 1972, Dorothy Rice was elected a member. In subsequent years, it was fairly easy for economists to be elected. I doubt it was that easy for most other membership categories. In some cases, especially among clinicians, some incumbents simply exercised a veto power. Who was I to fight them? What are the chances that I'd know the work of a particular clinician? Indeed, how should a practicing physician be judged? I saw no point in my fussing about it at meetings of the membership committee.

NEUHAUSER:

I think economists are still held in high regard.

KLARMAN:

Often, when you read the newspapers—I read *The New York Times* regularly and I did get *The Wall Street Journal* for several years—health economists are cited; I don't know who some of these individuals are. I recall that some years ago the point was made that the American Sociological Association comprised two categories—members and associate members. The associate members, I gather, were people without the necessary graduate school credentials. Quite properly, in my opinion, economists don't draw such a distinction. Still, I would like to know more about the economists being cited.

When I left Hopkins, I went to the Downstate Medical Center of the State University of New York and stayed one year. I decided within about three months or so that I had made a mistake. My boss was Duncan Clark, for whom I had previously taught as an unpaid adjunct. Have I mentioned that I did some teaching in New York in the 1950s? I taught a course in medical economics at the Columbia School of General Studies, and I gave several lectures a year to medical students at Downstate. I also knew Duncan well from the National Institutes of Health Study Section. I thought we were good friends and had high regard for each other. Several people who had been in his department had warned me that he was an arbitrary boss who made all the decisions. I didn't believe his usual behavior would apply to me. I was mistaken. So, I decided to look around for a job. Mary, who also went there, stayed on somewhat longer. I wound up at the Graduate School of Public Administration at New York University. Again, Joe Pechman was very helpful because Dick Netzer, the dean of the Graduate School, had written a monograph for Brookings. Dick paid attention to what Joe told him about me.

Looking back, on balance, this job may have been a mistake. If so, a good question arises: why did I leave Hopkins? Next, how was the Graduate School of Public Administration

perhaps a mistake? I left Hopkins for one immediate reason. Mary had some long-term reasons. Mary and I were married in December 1967; she had always felt discriminated against as a woman in both salary and rank. Basically, she was right. Fortunately, that was more than made up to her when she returned to Hopkins in 1981. I left because John Hume was going to be dean. A search committee was looking for a successor at the department. The person representing the department who served on the search committee had already retired—she was not an active colleague. I didn't care whether I was on the committee, but I thought they should talk to me about candidates. It was unlikely that they would consider an individual whom I didn't know; however, I was told by one distinguished member of the search committee, "If you want to have influence, go to the economics department at Homewood. This is the School of Hygiene, and you're not an M.D. or nurse." I resented this. Moreover, the first candidates I learned about through the grapevine were poor choices in my opinion. Had I known it was going to be Art Bushell, I probably would have acknowledged it as a fine choice, having known Art in New York. He would sometimes attend the Hospital Council's board meetings as a high-level substitute for the Health Commissioner. I knew him as a very nice man, a bright man. I don't know how much technical skill or scholarship he possessed, but to me these are not important attributes for a chairman.

As you know, on a medical campus, the chairman is much more powerful than in the liberal arts setting. In the liberal arts setting, serving as chairman is considered a temporary burden, a pain in the neck. The job is not sought. In medical settings, chairmanships are actively pursued.

My departure from Hopkins meant, to some degree, that I also left neighboring Washington. I was still called back for Institute of Medicine committee meetings, but not so

often by government agencies.

I returned to New York in 1969, but didn't really reestablish my roots as I thought I would. I was getting older, with less energy to conduct my former rounds of interviews. The demands of teaching were much heavier at the Graduate School of Public Administration than at Hopkins, although Dean Netzer did give me a below-average teaching load—two and two courses rather than three and two. No longer did I attend meetings at the Community Council or at the Citizens' Committee for Children.

I'm reminded of an incident at a meeting of the Committee for Children. In the 1950s, its executive director was Trude Lash, Joe Lash's wife. She was a German immigrant who still spoke English with a marked accent. The Lashes, you may or may not know, were close friends of Eleanor Roosevelt. At one meeting attended by Mrs. Roosevelt, she fell asleep. She didn't seem to have much interest in the subject under discussion, but she attended as an act of friendship.

NEUHAUSER:

This was at New York University?

KLARMAN:

Yes. Evidently, Netzer thought I was quite a catch for the school, which had a short history of full-time faculty. Dick was the son of a physician, had some interest in health economics, and knew of my work. He made sure that, during our job interview, I didn't meet anybody else. In that respect, he was probably right, although I don't know what job alternatives I had. As long as Mary worked in New York, what options did I have? Two job offers in the same city were rare. I took my responsibilities seriously, teaching four courses a year—health economics, health planning, public expenditure analysis, and a course in cost-benefit analysis. I

made a particular effort not to duplicate materials between courses, related as they might be. It was hard work to develop appropriate reading materials. In my teaching, the focus was on the reading assignments, since my lecturing took off from the readings. If a student hadn't read the assignment, he or she was out of it. Those who had read the assignment could participate in class.

My teaching, as it turned out, was very time-consuming. There were frequent faculty meetings at the school. There were 10 to 12 full-time faculty members. At these meetings, colleagues would make all kinds of proposals on curriculum; presumably, teachers would implement them, when approved. I would work hard to implement my part. Some of the others didn't; they changed nothing.

I thought that courses were supposed to build on one another. At the Graduate School of Public Administration they didn't. My notion of good teaching came out of my Columbia College experience. It means hard work. Consequently, I didn't publish nearly as much in the 1970s as I had in the 1960s; however, I realized that I did publish more than I thought I had when I recently looked at the curriculum vitae. Some papers were perhaps a carryover or extension of past work. George Bugbee would occasionally commission a paper from me. One such paper was prepared for the second conference that I addressed for him. I have read the paper recently and was pleased with the contents.

NEUHAUSER:

These were the University of Chicago forums?

KLARMAN:

Yes, Joel May would introduce me. I would ask myself questions and go on to examine the evidence bearing on them. Let me tell you this incident with George. The first time he asked

me to examine hospital use under prepaid group practice was long before the emergence of the HMO. I told him, "George, I don't know anything about this subject." And he said, "That's why I'm asking you to deal with it; I trust you." At the time, prepaid group practice was a partisan, controversial issue, and he was looking for an objective presentation of the evidence. He believed I was the person to do it. Exactly how I got so fine a reputation with George, I do not know. To this day, I'm proud of it and grateful, for George was a keen critic.

I could talk about other committees I joined. Some met only once or twice. Sometimes we did the task we were assigned, and that was it.

Some of the things I learned from delivering speeches or from committee meetings were interesting. I remember once leaving the podium after my talk; it may have been my prepaid group practice paper on hospital use. Near the exit, Sam Shapiro was standing, and he said to me, "Herb, you did so much more with the data than we did."

NEUHAUSER:

That's a high compliment from Sam Shapiro.

KLARMAN:

I should say so; and I took it as such.

Another time, I spoke at a meeting in California. The man who was the founding medical director of Kaiser Permanente, Sidney Garfield, had published a statement criticizing prepaid group practice clientele. These people were all hypochondriacs; they were seeing the doctor much too often, Garfield wrote. The staff at the Kaiser headquarters resented his statement. They didn't know what to do with the man, a founder. They didn't think they could fire him, but he certainly didn't reflect their views. I remember having spoken and Milton Roemer, who was sitting on the platform, whispered to me, "Herb, you don't know the half of it. Of course, Kaiser

runs low hospital use. One reason is they build few beds—a low ratio to membership. The other reason is they give priority of admission to non-Kaiser patients. The public doesn't know this. Of course, you get low hospital use by your own subscribers."

My point is professional colleagues were very supportive, on the whole. I don't remember—maybe I choose not to remember—any bad parts. Why would I want to remember them?

NEUHAUSER:

Why did you choose to retire to Baltimore?

KLARMAN:

In 1981, Mary got an offer from Leon Gordis, then chairman of the Epidemiology Department at Johns Hopkins. She did not know whether to accept it. I encouraged her to move. One, the New York Medical College professorship wasn't a great job; she wasn't really doing much there; she wasn't accomplishing anything. Mary was almost losing interest in her work, which is most unusual for her. I thought we could handle it by commuting between New York and Baltimore for as long as necessary. At various points, she and I had tried to relocate simultaneously with two jobs in the same city. This proved to be virtually impossible. I would get an offer or she'd get an offer, but never both of us.

It was probably my urging that persuaded her to take the Hopkins job. This proved to be fortunate in several ways. One, she became interested in her work again. Two, there occurred a bit of an internal brouhaha at one point within the department; I believe my advice helped her to stay clear of it. I think this was instrumental subsequently—not that I expected it—in her being appointed deputy chairman; she was a helpful presence in healing the department. Three, Mary always felt she would not wait to retire at 65. She wanted to retire at age 62 or so. The university

treated her as if she had been asked to retire early. The university was very fair about financial arrangements. Indeed, the university offered her a generous benefits package in pension and health insurance.

You have asked me to try to identify what I consider my important publications. Let me tell you what I've done to speed things up. In perusing my curriculum vitae, I have entered checks to denote extra importance (see pages xi-xx). Under books and monographs, I've checked numbers 4 and 5, and, with less certainty, number 1. Under articles and chapters in books, checks appear at numbers 2, 4, 5, 7, 17, 19, 20, 22, 25, 32, 36, 38, 39, 42, 43, 46, 48, 52, 56, 57, 69, 70, 71, 74, 75, 87, 88, and 90. By way of explanation, where an article is essentially a revision of an earlier one, my list usually doesn't assign it a separate number. Instead, I add a letter of the alphabet: number 25 is followed by 25a. Since the early 1970s, I've been consistent about this practice. I saw no point to padding the list. Numbers 36, 36a, and 36b are essentially the same article; still, 36b is the most frequently cited version.

Number 39 is my unusual attempt to discuss teaching by a professor. When I gave that lecture at the first of my seminars at Johns Hopkins about six months after leaving, some people in the audience expressed shock that anybody would come and talk about teaching.

Number 66 raises, in a direct way, the question of word usage—what words mean to economists and what they mean to people like Dr. Michael DeBakey. Abe Lillienfeld was the staff director for the President's Commission on Heart Disease, Cancer, and Stroke (Michael DeBakey, chairman) and he wanted to suppress my article. I protested strongly and won. Abe didn't see the point of talking about such things; I thought it was important.

Numbers 86 and 88 are essentially the same, but this was a very interesting occurrence. I had published an article in the *Journal of Business*, and *Harvard Business Review* wrote to me

with a request, could they republish it? I revised it a little, but the duplication was absolutely at their own initiative, since they'd never published anything on hospitals before. Number 87 is from the American Economic Association session I organized for Milton Friedman; I have come to believe that the longer, unpublished version is probably superior, more complete.

I don't know whether I've said everything I should say about the people who asked me to prepare papers, such as George Bugbee, Milton Friedman, Eli Ginzberg. Nevertheless, I've talked quite a bit about each of them in other contexts.

Let me move on and talk about economics, health economics, and health services research. First, please note that my early publications are about medical care or medical economics. They're not about health care or health economics. I don't quite know when the word health was substituted. I don't want to claim undue credit, but probably *The Economics of Health* book had something to do with the change in adjectives. As I have told you, that in turn derived from Victor Fuchs's decision at the Ford Foundation to commission papers on health, education, and welfare, as in the name of that department.

Economics, as we all know, has to do with the fundamental notion of the scarcity of resources, the fact that resources have alternate uses, and that there exists in this world a diversity in wants, tastes, preferences, call them what you will. Indeed, I got this from page 56 of the Spring 1994 issue of *Health Affairs*. This volume has produced an effect on me. It tells one who are the economists involved in the current policy debate. I was going to finish this interview by talking about suggestions for health care reform, but I think I'll refrain from that. Let me stick to what I was intending to say.

To most of us, I think, economics is, more than anything else, a big set of concepts turned into tools, such as supply and demand, an upward sloping supply curve, a downward sloping

demand curve, the possibility of shifts in demand, unit costs, and average versus marginal cost. These are all concepts in traditional or Marshallian economics, today's microeconomics. When you look for material on a multi-product firm, the only solid chapter in a textbook I've seen is in George Stigler's *The Theory of Price*. Nobody else deals with it directly. Stigler had it in the original edition, but must have had second thoughts. I don't know why, but I think it is very important. There are also index numbers, which are very useful in dealing with increases in spending, various isoquant curves, and cost-benefit analysis. I would put cost-effectiveness analysis right under the last heading. Neither gets involved in the question of distribution. It's a total or average, never mind who gets what.

Then there is reliance on competition in the market versus government intervention through regulation, subsidization, or ownership. There is the quantification of tax expenditures, which are really a subsidy through the tax system, along with various objects that escape taxation. Increasingly, we deal with macroeconomics—national income or gross national product or what has now more recently become the "gross domestic product." The difference between them is not all that important for our purposes. However, when you measure only what enters the marketplace, activities carried out at home vanish. There's always the possibility in something like long-term care that you see an increase in spending, not because anything has really changed in the quantity of services rendered, but because you're now monetizing things that were not counted before. I learned this from reading Kuznets in the 1940s.

None of this may be obvious, but I'm aware of the nuances. I think that much of my awareness comes more from teaching than from research. Also, as I mentioned in connection with doing that hurry-up reading job for John Hume, there may be a question of the initial investment in an enterprise and what I call the cost of reaching full capacity. Here is enough

about the accepted set of concepts and tools.

In June 1989, I employed a celebratory occasion to talk about research that is useful for policy formulation. Borrowing heavily from Bob Solow's review in *The New York Times Book Review* of *The New Palgrave: A Dictionary of Economics*, I defined—and adopted—his three categories of judgments. First, recognition of value judgments: economists will differ on the contents of equity—who gets what—and the proper roles of the several levels of government. Second, there are factual judgments: the future takes off from the real world of the present, not from a blank slate. Third, and perhaps most important, there are analytical judgments: distilling the essential, really important features of the institution or industry under examination. In this connection, I cited a couple of basic principles about health care that have been—perhaps formerly were—widely accepted in this country, even if imperfectly applied in practice. One, nobody is to be denied needed health care for lack of ability to pay for it. Two, since the physician always knows more about medicine than the patient, society expects the physician to act responsibly, to behave in a fiduciary capacity.

I have an extended quote here from something I wrote about value judgments. Economists differ in value judgments, such as the trade-off between efficiency and equity, the proper role of government, where government functions are to be located—is it at the federal level, the state level, or the local level? When you decide to regulate, do you set standards or do you rely on incentives or subsidies to get people to do what you want them to do? What importance is attached to having nonprofit organizations render a service, whether or not supported by philanthropy? What is the proper role of quasi-government corporations, and their accountability is to whom? In the health field itself, there is the point nicely raised by Arrow. What is the sense of talking about competition in health care, with sovereign consumers, when

the consumer always knows so much less than the provider?

In health care, there is the notion, at least it was a widely accepted notion in the 1940s when I examined the question in my paper for the American Economic Association, that in this country, society aims to make medical care of good (or adequate) quality available to all people, when they need it, regardless of their ability or willingness to pay for it. In my unpublished version, I offer additional supportive citations. Unfortunately, I eliminated some of them from the published paper, not because I regarded them as unimportant; I had to shorten the paper. What are the policy implications for a society that chooses to be guided by this standard? They are powerful, but also multiple.

Let me note—perhaps I should have done this earlier—that among the individuals who have made contributions to medical or health economics, most have come out of the various other subbranches of economics—out of theory, out of systems, out of insurance, out of labor, out of public finance, or out of social insurance.

Let me turn now to some of my quantitative work or, more precisely, calculations of the size or dollar amount of subjects I've studied, as well as the factors involved. Throughout, I've tried to convey the methods employed.

First, I mention again the sources of increase in expenditures for health care. Much of that work I did jointly with Dorothy Rice and a man who was then on the faculty at the Homewood Campus of Johns Hopkins in the economics department, Louis Stettler. I've already elaborated on the cost-effectiveness analysis applied to the treatment of end-stage kidney disease. I did several papers of equal sophistication, particularly in my second paper to the University of Chicago business school forum, using various sources and my own analysis of data, published and unpublished, leading to the conclusion that prepaid group practice does yield a saving in

hospital use.

What I couldn't determine to my own satisfaction was how to explain that the savings are so similar under such different institutional arrangements—the Health Insurance Plan of Greater New York, and Kaiser Permanente, with its own hospitals. Here, Milton Roemer's whispered comment to me concerning Kaiser (see pages 113-114) is revealing. Today, of course, additional and more diverse data sources may yield additional explanatory power.

To return briefly to the sources of increase in hospital spending, one gets different results by counting only the salaries of hospital personnel than when fringe benefits are included. The convention has been, when some amount is left over, to attribute it to changes in technology. Obviously, when you include fringe benefits, such as contributions to pensions, technology becomes less important. I realize as I talk to you that some of these considerations have struck me rather late in the game.

Looking at the role of philanthropy, one sees a major change. It started out as a way to finance hospital care for the poor, but ultimately—in New York City certainly—it proved to be a way to improve the quality of hospital care, whether through the appointment of full-time chiefs of clinical departments, or, later on, through providing seed money for conducting research.

In both my hospital book and when advising Eli Ginzberg and Peter Rogatz in the study of the Jewish Federation hospitals, I found, through the judicious use of scatter diagrams, that I was able to estimate the percentage composition of hospital patients by pay status or by residence, and, at one point, even the proportion of Jewish patients in hospitals. Later on, after I had left New York, other researchers found on the basis of much more complete data sets, that my estimates were reasonably accurate. I was both surprised and pleased. I had done much of this work with some trepidation, but felt it was necessary.

In the hospital book, I was even able to reconcile the apparently large differences in patient day cost between the voluntary and municipal hospitals in New York City by introducing certain pertinent factors, such as the value of tax exemptions. I also learned that every hospital, no matter how big and how prominent, serves a large local clientele. The number of patients coming in from the suburbs may be sizable, but the clientele is predominantly local.

The relative importance of health insurance in the finances of voluntary hospitals in New York depended on the variables chosen for the study—the percentage of all patients, the percentage of all patient days, or the percentage of patient revenues. This is a striking finding, since researchers seize upon and apply whatever data set is available to them. Then they proclaim that's what the situation is, but if you look at each variable at the same time, you find that the answers are quite different.

Finally, empirical findings from a local area such as New York City may apply nationwide, as, for instance, in the length of graduate physician education—residency training—or may not apply nationwide, as in the proportion of patients financed by tax funds.

What is it that economists tend to miss? One feature they have missed or neglected is that the hospital is a multiple-product firm. The results you get from cost accounting will depend on the assumptions made in allocating overhead. This is true even if you do everything consistently and systematically. What's the significance of this? In New York, at least, since you were looking for funding by philanthropy or the city, the tendency—or temptation—was to make the cost of an outpatient department visit as high as possible. On the other hand, when you wanted higher reimbursement rates from Blue Cross, you raised the cost of the room-and-board component of inpatient care. This feature of cost accounting became of central importance when independent ambulatory care units were established. Hospitals had to learn to compete in cost.

For hospital outpatient departments, a competitive price figure emerged, consistent with the cost of the independent ambulatory care unit.

In my view, academic economists just weren't there when third-party financing gained ground in this country. The important question is not only what happens in the behavior of consumers when the out-of-pocket price is reduced, but what is the effect of employing a particular method to pay providers? Under Medicare, we simply decided at the outset that cost reimbursement to hospitals was the fair approach. It happened to repeat the federal government's experience during World War II when it bought small amounts of services from outside hospitals. I was told by Agnes Brewster that this is where Medicare staff got their reimbursement method.

The method adopted under Medicare and Medicaid to pay physicians separated government finance from government production. A public hospital combines finance and production, but now we separated them. I think one reason for making physician reimbursement what it was initially was the thought that, having beaten organized medicine, let us not rub it in. How about getting its cooperation? That was the main reason for adopting the customary, usual, and prevailing fee schedule. When you deal with a frequency distribution of the target fees for all physicians in an area, it just meant that every physician had an incentive to raise his fee, if for that reason alone. It was a cooperative effort, it seems to me.

From my own New York experience, I learned that existing institutional arrangements are very important, whatever their historical origins may be.

For somebody who was brought up on the nonprofit ownership of hospitals, consider the writings of Arnold Relman in *The New England Journal of Medicine*. Dr. Relman is very suspicious of what used to be called proprietary and are now called for-profit facilities. He

doesn't trust them. Legally, there was a case decided by the New York State Court of Appeals, in which Doctor's Hospital was sued to deprive it of its nonprofit status, since the hospital didn't provide free care to the poor. Doctor's Hospital prevailed on the ground that, even in the absence of free care, a compelling case can still be made for the voluntary nonprofit form of organization.

One has to recognize the important shift in the role of government from joint financing and production to financing alone; I pointed out in another context that there remains the Veterans Administration system as an exception. As Bob Reischauer made it clear in his testimony to the Senate, major changes in public policy tend to exert very big and diverse effects, some of which are unintended and unexpected. Let's consider a few such effects. From the interviews I conducted—my interviews were not uniform or systematic—I learned that negotiated fringe benefits, which made health insurance a nontaxable benefit, were introduced during World War II as analogous to retirement pensions and, therefore, noninflationary. Let me explain. Pensions were giving people a way to save; they won't be able to cash in on them until much later, after the war. We regarded health insurance the same way. I would suggest that, in the years after World War II, if labor unions were able to negotiate better and larger fringe benefits, that success tended to undermine any movement toward national health insurance. Why do you need national health insurance if you can get its advantages without it? Of course, unions were more potent at that time than they have since become.

Another totally unexpected development was in our attitude toward prepaid group practice; it was originally viewed as virtually socialist and therefore strongly opposed by organized medicine. Later, it became subsumed under the benign HMO label. That, in turn, has become the precursor of the pro-competition movement in health care. In fact, when people talk about managed care today, the major instrument is the HMO. This is what is brought up, not

utilization review or second opinions, etc.

Another thing—a nondevelopment—has surprised me. In the original Medicare legislation, there's a lot of talk about the extended care facility; this reflected an earlier time, when we still had convalescent homes. There's little mention of the use of extended care after 1965. I recall doing a study of a convalescent home in the Bronx and finding no clear-cut role for it. The best service it provided was respite service to the family caring for a patient with a long-term illness. That just wasn't a strong enough reason for keeping the facility.

I drew heavily on these remarks in a letter I wrote to Victor Fuchs in September 1985. He asked certain colleagues for their contributions to the field since he was preparing the article on health economics for *The New Palgrave: A Dictionary of Economics*. My impression is that he didn't use much of what I furnished, as was his privilege.

It occurs to me often nowadays that we may be missing the boat in one area. That applies not only to economists, but to all of us. It's not simple to determine, or measure, the quality of care rendered by physicians in hospitals. Still, it has been done. I remain skeptical about our ability to measure the quality of care in the ambulatory setting. Patient satisfaction is one prong, but hardly suffices. Even in group practice, physicians tend to refrain from commenting on the performances of colleagues, as Eliot Freidson found.

I think it is much easier for lay people to measure the quality of care in nursing homes. One can look at whether patients develop bed sores, does the place smell, are calls answered promptly, are residents eating well, getting appropriate exercise, etc. I think we have missed the opportunity to do research that can be done. I realized this from reading reports by visitors from the United Hospital Fund. Mostly, these visitors visited hospitals. In retrospect, I think their work would have been more useful had they visited nursing homes.

Let me talk now about some unresolved problems in health economics. One is the proposition expounded by William Baumol, which some people have called the cost disease. It has to do with the notion that a Mozart quartet still has to be played by four people. Essentially, medical care is the same! A doctor has to make the diagnosis after taking the history and performing the physical examination, and do the prescribing. As we achieve productivity gains in various sectors of the economy, especially in manufacturing, greater than in service industries, and especially greater than in the health care sector, the cost of health care will inevitably go up. All the talk and concern about its going up is, according to Baumol, beside the point.

The answer to Baumol has been that health care itself, the product, has changed quite a bit. Some delegation is certainly possible from the doctor to the nurse or his technical assistant. Nevertheless, I think there is some merit to Baumol's proposition. The question is how much merit. I don't think this question has been resolved, and the technologies of prevention, diagnosis, and treatment do change.

Another unresolved question, to my mind, is something that I've already noted, the basic difference between Joseph Newhouse and Martin Feldstein about the effect of changes in coinsurance rates. Feldstein said you have to consider not only the demand side, but also the supply response. This repeats the controversy over Roemer's Law, which was criticized by Bob Sigmond. I would say, at a technical level, this issue remains unresolved. We all have our individual views, but I don't think we can say that one has been established conclusively over the other.

On the sources of increase in spending, the portion that is often ascribed to changes in technology is a function of how fully the other measured factors are captured. In any case, annual rates are a vast improvement over rates for the interval studied. As I point out elsewhere,

even the effect of the aging of a population can be a problem. I found in one study that I did much better when I divided the 65-and-over age group into 65 to 74, and 75 and older. While hospitals didn't show much of a difference between the two approaches, the nursing homes surely did. What level of aggregation do you examine? It's necessarily a matter of trial and error. One learns to try to understand the type of institution under study, its clientele, and the services it renders.

We are still undecided between cost-benefit analysis and cost-effectiveness analysis. We usually say it's cost-benefit analysis, but as a practical matter, it's almost, to my mind, impossible to measure and value the benefits of particular health care services. Moreover, as a person gets older, his valuation of benefits is likely to change. However, I should add that even in cost-effectiveness analysis, the unit of outcome is not easy to determine and measure. It depends on what you're studying.

I should also call attention to the arduous work I had to perform when I sought to ascertain the costs of care on behalf of the Gottschalk committee. My point is, even when you achieve success in a study, it doesn't necessarily carry over to other studies.

NEUHAUSER:

Yes. Measuring opportunity cost is no easy thing to do.

KLARMAN:

Let me note still another unresolved problem, perhaps at a higher theoretical level; the difference between Milton Friedman and Adam Smith on what is the proper level of income for a physician, and, consequently, what lessons about the number of physicians required or the existing scarcity or surplus of physicians do you draw from physicians' income data. Clearly, Friedman is inclined to emphasize the profession's monopolistic practices; Adam Smith was

talking about the high regard we have for physicians and our trust in them, and, therefore, their proper, presumably above-average, financial rewards.

I don't think the controversy between Kenneth Arrow and Mark Pauly has been, or can be, resolved. Pauly answered Arrow's paper within a few months in the *American Economic Review*, focusing on the desirability of market competition over public intervention. My own view is that one's general political orientation may govern.

Likewise, I think the question of nonprofit versus for-profit sponsorship of facilities is unresolved in logic, if not in practice.

There is an associated problem that may perhaps be easier to deal with, namely, the question of the way physicians refer patients to facilities in which they have invested. Perhaps we trust the physician in his own office, but don't trust him outside.

What do economists borrow from other disciplines, and what can we borrow? We can obviously borrow from accounting. I have discussed the pricing of services by a multi-product firm. I can recall a specific example: at one point, the United Hospital Fund simply lowered the cost of hospital ward service, because that's not where a hospital's money came from. This event gave me the idea for the first time that these figures are not rock solid.

For any institution, one must be concerned about its cash flow versus its profit-and-loss statement, because it has to stay afloat.

Another item I would borrow from accounting is the definition of surplus. It's revenue minus expenditures and applies to a nonprofit firm as well as a for-profit firm. The leadership has to know whether the institution is operating in the black or in the red.

Another concept and tool economists have borrowed is life expectancy. This comes from demography and insurance. In turn, the notion of work-life expectancy arose. I don't know who

introduced it, but I applied it fairly early.

Premiums for health insurance can be broken down between the pure premium and what was formerly called the retention rate. The latter includes administrative costs, selling costs, and profits when the insurance is commercial. Today, some of us seem to believe it's all administration or waste. I still prefer the term retention rate taken from life and property insurance.

Actuaries have always made projections of future utilization and cost, in order to advise on premiums. The implicit assumptions are that past experience will continue and that the adverse selection of risks will be avoided.

Another discipline I regard as highly important is epidemiology. I don't recall who it was who called it the queen of the health sciences. These are things that I have had to learn ad hoc over time. I didn't know anything when I started about the distinction between incidence rates and prevalence rates; it's obviously of vital importance in planning for long-term care. Much to my surprise, I once met a distinguished epidemiologist who had worked on long-term care and didn't apply this distinction. It's one thing to know about a concept, to refer to it, and it's another thing to apply it to a specific problem.

There is the question of experimental design—epidemiologists rely on it heavily. How practical is it for policy purposes? I don't know. I liked it a lot when we had natural laboratories in the states. Unfortunately, we don't have those as much as we used to in the progressive era. Almost everything that was incorporated in the Social Security Act in the 1930s by Ed Witte and colleagues came out of experience in Wisconsin.

Epidemiologists write about outcome measure. One of the questions I have is how refined should they be? How refined can they be and still be useful? Let's not forget that

outcome measures have independent status as denominators in cost-effectiveness analysis. Without outcome measures, cost-effectiveness analysis cannot be performed.

I've already referred to some policy decisions shaped by history. I think it is profitable to study and learn from the past without ignoring the present, and without seeming to claim that we possess certain knowledge of the future. I think this is feasible.

One lesson I have learned from history is that it teaches humility. On the one hand, we do stand on the shoulders of giants, as Professor Merton has taught us. On the other hand, whether or not our solutions are immediately effective and deemed successful, they may and are likely to lead to new unintended and unanticipated problems, so that the solutions introduced need to be modified over time.

Let me cite a couple of examples of what I have learned from history. I was astonished to learn that in the 1930s, house staff in municipal hospitals in New York City did not receive any pay. Their wives or parents must have supported them.

I have certainly learned that the function of philanthropy changed. Money for free care for the poor became a base for teaching and research.

My major conclusion is that I'd be cautious about forecasting even with the most sophisticated models, especially when the changes enacted are large. Certainly that's what Bob Reischauer was trying to tell the Senate in his testimony. Do you get *Congressional Budget Office Reports*?

NEUHAUSER:

No, I do not.

KLARMAN:

Well, I find them enlightening. I seem to be on a duplicate list, so I may have an extra

copy.

Another discipline economists have borrowed from is operations research. This has surprised me in many ways. Here is a name I would like to mention: I first ran into Charlie Flagle at a meeting of the local chapter of the American Statistical Association in New York City. At one point, he got up and said something, and I thought here's a man who knows his stuff and talks good sense. When I came to Hopkins, he was a member of the department. Not only had I read him, but I had seen him.

The major contribution to health care that I could discern from operations research is the distinction between random arrivals and scheduled arrivals. With scheduled arrivals, you could have two admission clerks. One handles finances and one takes the clinical history. But if arrivals are random, it is better to have one clerk who performs both functions and therefore earns more. For somebody brought up on Adam Smith's division of labor and economies of scale, this finding was quite a shock. Nevertheless, it made sense.

The same idea was later applied by John Young, who was, I believe, Charlie's major student; unfortunately, he died young. John applied it to the staffing of a nursing unit. You can substitute aides and certain licensed practical nurses for registered nurses up to a point, but not where you have random arrivals.

I don't think most health economists know about these studies. I doubt that I would have followed them if I hadn't been involved in the nursing study with Eli.

Let me reflect now on health planning. You'll be surprised by what follows because you're a relative youngster. In the 1940s, the number four was virtually regarded as a universal constant.

NEUHAUSER:

Four beds per thousand.

KLARMAN:

Four beds per thousand in the military. Four beds per thousand in civilian life. Four percent of the gross national product spent on medical care. I remember an article by Frank Dickinson of the American Medical Association. He said the number applied everywhere; it's a universal constant, and it'll probably last forever. Dickinson had been a professor of insurance at Indiana, who moved to the staff of the American Medical Association.

In planning, we learned something about isoquants. I've mentioned them earlier. It's a notion about travel time by patients. It was turned later into travel costs, and even the opportunity costs of time, by Jan Acton. I think he did that study for the Rand Corporation. It also served as his Harvard Ph.D. dissertation.

What about physician staff appointments in hospitals? They are very important for physicians, where they care for their patients. Staff appointments are also very important for continuing education. If a doctor doesn't have an appointment, he misses out on those two aspects of his career, medical practice and education. However, as we learned from Bob Sigmond, if a doctor has more than a single hospital appointment, the hospitals compete for his referrals. That's not necessarily good for the system as a whole.

In this connection, let me go back a moment. I have talked about cities like Pittsburgh, Rochester, and so forth. Let me repeat that, while we cannot learn from these cities with dominant corporations and labor unions about the feasibility of applying their lessons to other areas, we can learn from them about technical procedures. Does a particular technique work elsewhere? Do we know how to apply it?

It may be that I used to make much more of the distinction between diagnostic and treatment facilities than I would today. If you specialize and the diagnostic facility is located at one point, then patients do the traveling, and the referring physician may feel that he has lost a degree of control. Somebody else is doing the diagnosing, and he has to accept it. Planners felt more comfortable with respect to treatment facilities, since at that time, these were located mostly in the hospital, which was subject to a certificate of need process. I suspect that much of this has changed. I don't know whether anybody has studied this.

Let me now note some rather simple lessons I learned in planning. What's the effect on a hospital's occupancy rate when it has large wards, semiprivate rooms, and single private rooms? The effect has to be large when you practice isolation by gender. A good planner in the hospital field needs to know about hospital design and layout, something I knew nothing about when I joined the Hospital Council.

The planner has to make some policy judgments when diverse objectives conflict. Let me cite an example. At one time, Roosevelt Hospital applied to the Hospital Council seeking to add an obstetrics unit "in order to round out the hospital," the term then used. They also, I believe, wanted to retain their best gynecological specialists because the practice of gynecology is more attractive at a hospital where an obstetrics unit feeds it. Manhattan was then overbedded in maternity units, which were physically separated from all other beds. I would say that the Hospital Council was partly at fault for this situation because, when Mt. Sinai Hospital had applied years earlier to create a new obstetrics facility, the Hospital Council said nothing about its size. Mt. Sinai built a very large one. The net result was the Hospital Council wrote a report that recommended that Roosevelt Hospital should not add such a unit, but the report also indicated that it was alright if Roosevelt disregarded the recommendation. I think some people,

particularly members of the board of directors, felt that was a terrible thing to suggest since, according to our basic principles, Manhattan was clearly overbedded. What about the fact that the Hospital Council had contributed to the situation?

There are a few remarks I'd like to make about population projections. We found that net population changes were not useful. Let me cite an example: one of our findings was that the Puerto Rican group in New York City remained poor. When we asked around, the answer was simple. Immigrants who were successful in New York went back to Puerto Rico and lived there very well; the unsuccessful ones remained. Clearly one needed figures on gross migration changes.

Another lesson I learned was that if you don't have to project population, don't do it. When we did a study of Staten Island, where there was ample room for future development, we finally decided that we couldn't come up with a projection that other experts would accept and go by. Instead, we said, be flexible in the size of hospitals you build so that the hospitals can be expanded; we recognize that such flexibility in construction is likely to be more costly. We suggested, continue to concentrate on what you will do when you reach certain population milestones; the first milestone will be when you reach 300,000. This was new thinking at the Hospital Council. I felt confident that the suggestion was the best attainable, since we had employed an expert on population projections who had worked for the New York City Planning Commission, and he couldn't come up with a projection that he felt he could stand by. The notion of flexibility in construction was the safer option.

In general, it turned out, I was more confident of what I was doing when dealing with numbers rather than with rates or percentage changes, because rates or percentage changes don't convey the size of the problem the way numbers do.

Let me turn to what we did, and could, learn from sociologists. Obviously, one of the contributions by sociologists was survey research; one could learn from their findings even when one didn't explicitly apply them. Another well-known contribution is the difference in access by social class and the resulting differences in utilization. I've already discussed my military experience, which taught me sociology informally. Another contribution from formal sociology, which I've already mentioned, was Eliot Freidson's finding that peer review was not taking place even in a highly regarded prepaid group practice, the Health Insurance Plan of Greater New York group at Montefiore Hospital. Finally, one has to be aware of the difference in views regarding the aims of the physician. Many sociologists tend to talk about professional dominance. Many economists are more likely to talk about the physician's fiduciary duty, the responsibilities of the physician in caring for the patient who possesses less information, the patient who has to trust the physician, believing that he's not financially greedy and pursues the patient's interests.

In connection with survey research, let me elaborate on interviewing. I had no formal training in interviewing, but Mary, my wife, is an expert. When I told her about my interviewing approach, she said I did it just about right. I would start an interview by setting forth my major concerns and questions, and then let the interviewee talk. I wouldn't intrude until very close to the end, when I might ask pointed questions. By then, I trusted that the answers I got reflected that person's beliefs and thinking. I remember being shocked at one interview. This was a joint interview with a colleague, who asked probing questions like a prosecutor. After that event, I tried hard not to send this man on interviews.

I turn now to the last discipline I listed, statistics. I've already noted the size of samples, and I've referred to natural and designed experiments. The major designed experiment in health care was, of course, the Rand experiment in health insurance. I've discussed that somewhat

thoroughly and at some length.

I learned from a statistics book, authored by Wallis and Roberts, the implications of screening the population at large, with the high proportion of false positives obtained when the prevalence rate is low. I have this example noted down: when the prevalence rate for a disease is as low as 5 per 1,000, and you set 95 percent reliability and 95 percent sensitivity, only 1 of 11 or 12 persons with positive reactions has that disease. I doubt that most people are aware of this computation.

I've already referred to the Poisson distribution, which pertains to the occupancy rate. With all admissions random, not scheduled, and with mainly single-bed rooms, you still have to consider the length of stay. Why? With the shorter average length of stay, what happens to the interval between admissions? I've not seen a study of this specific problem.

Finally, there is the question about whether social workers can find a place to send the patient to. Most of us go home, but not all of us do.

NEUHAUSER:

I'm curious you didn't add law to that list you might have learned from.

KLARMAN:

Formerly, we didn't litigate as much as we do today. You're quite right; however, I plead guilty. Maybe I'm drawing too heavily on my own experience at a time when people did not sue a voluntary nonprofit institution.

There are problems of communicating among economists, doctors, planners, and so forth. I witnessed personally one such example. Walter Heller was chairman of the Council of Economic Advisors at the time; this is the early 1960s and there was the DeBakey President's Commission. I knew both men. As previously noted, I had met Mike DeBakey as a major in

the surgical consultant's Office of the Surgeon General of the Army. Even as a major, Mike DeBakey acted just like a general—he was full of confidence. Hence, his later prominence did not surprise me. This is the incident I want to relate. The DeBakey President's Commission staff director, Abe Lillienfeld, wanted to know how you decide on the allocation of research funds, because the Commission started out as an inquiry into research. For some reason, which I never learned, the Commission later turned to planning, and it came up with the Regional Medical Program. But that's not where it started. There was a switch somewhere in midstream.

At Walter Heller's request, I convened a panel of economists. The process was rather unsatisfactory; the economists just wouldn't even try to answer the Commission's questions. They said: one, they didn't know enough about research on heart disease, stroke, and cancer; and two, many of the questions had to do with equity, who gets what. Economists don't know the answers to these questions in research funding; they are matters of opinion. As economists, they cannot answer them. At one point, DeBakey spoke up and said, "What do you mean that you can't answer questions of equity? The government has put up the money for research. It has equity in research, you know, an investment." So, here is the word equity. Surely, 100 percent of all economists know that equity has to do with distribution. None has thought that it referred to the sum you put down to buy a house.

I remember another incident on wording. There was the presidential commission having to do with aging or long-term care—it's listed as one of the committees I served on. Unfortunately, while this was a presidential commission, the President, Lyndon Johnson, never appeared except the one time I was teaching and missed the meeting. I regret not seeing the President functioning. At another meeting, I said something about inflation in the health care sector, and I remember a Harvard professor, who was then a member of the Council of Economic

Advisers, correcting me and pointing out that the word inflation applies only to the economy as a whole. It has nothing to do with rising prices in one sector of the economy. You and I know that; I was guilty.

I think people have great difficulty with the economist's notion of the discount rate. For economists, the discount rate is meant to reflect time preference or the value of time preference; the problem is how you measure it. Most economists have had difficulty with determining it. One way to get around the problem is to try out and present two different discount rates, as Burt Weisbrod did. Most consumers, of course, when they talk about discounts, don't mean that; they're talking about reductions in the price of a purchased good.

Today, the issue of health care rationing is looming. Economists simply assume that consumer purchases are always rationed by price and by income. The general public thinks of rationing as receiving vouchers to buy necessities in wartime; probably, we don't want to turn to that in medical care.

One source of confusion may have been introduced by the economists themselves, as in Selma Mushkin's treatment of cost-benefit analysis. You have the cost of a disease on one hand, and the cost of operating a program to treat it on the other hand. The costs of a disease, of course, are the potential benefits. If you don't watch out here, you will be confused. I know I was more than once.

There is also this general tendency to want to appear up to date and smart. People don't say any more that something is cheaper; they say it's cost-effective. It seems to me that cheaper has to do with the price of an input. Cost-effectiveness pertains to outcome. One shouldn't use the terms interchangeably.

That's enough on wording and communication, don't you think?

NEUHAUSER:

You give very good examples of a large problem.

KLARMAN:

A very, very large problem. It's up to our profession to be aware of it and to try to avoid confusion as much as possible. Certainly the profession should not add to it.

We've already discussed the active public roles of Stuart Altman, Uwe Reinhardt, and Henry Aaron. The Spring 1994 issue of *Health Affairs* contains articles by other people whom one would recognize as health economists. Listed are Victor Fuchs, Joseph Newhouse, and Mark Pauly. There is a piece co-authored by a man you mentioned yesterday in connection with Aaron—William Schwartz, a physician. Another man you mentioned yesterday, Donald Cohodes, has a small article.

I was going to talk a little about other economists. Consider Karen Davis. My guess is she has not participated in public debate as head of a nonpartisan foundation. I do remember that she has favored a major extension of Medicare. Perhaps when Representative Pete Stark writes his plan, she will become active. I've mentioned Jerry Rosenthal; he seems to be outside the domestic scene. I don't see Frank Sloan or Dave Salkever. In Salkever's case, I know he doesn't care to get involved in public policy debates. He has an active teaching schedule, and he has a disabled daughter. Both he and his wife have been active in studying and working in this problem area. They run a program for disabled people, with heavy reliance on the use of computers. I'm pleased to say that we contribute to this facility.

NEUHAUSER:

Where is that?

KLARMAN:

In Baltimore. It's an interesting operation. Dave took a sabbatical last year, and he worked mainly on disability. He prepared a course and developed requests for grants. I don't really know about Frank Sloan; in many ways he impresses me as being similar to Salkever; he focuses mainly on research. I see that Michael Grossman is not involved; he is the major product of Fuchs's teaching in New York. I'd almost forgotten that when Victor left the Ford Foundation, he joined the City University before moving to Palo Alto as head of the new West Coast branch of the National Bureau of Economic Research. Grossman works mainly on the effects of medical care on health status. In the current debate on health care reform, nobody seems to be interested in the effects on health status. It's taken for granted that reform is good. If more preventive services are provided, we can eventually save a lot of money. Given what Grossman is interested in, he may not have anything pertinent to contribute to the current debate that major participants will pay attention to.

NEUHAUSER:

Well, he's not an articulate speaker.

KLARMAN:

True. That's a nice way to put it; he has a stammer. I think for someone with a stammer he's done marvelously well, and his speaking is much better now than it was when he started.

NEUHAUSER:

Yes. So I understand.

KLARMAN:

He has made tremendous progress; I'm impressed.

NEUHAUSER:

How about Jack Hadley?

KLARMAN:

You're quite right. The only reason I didn't mention him is he's always worked for some agency or firm, he's not an academic. I've always liked his stuff, but I haven't read any lately.

Am I perhaps overly prepared today?

NEUHAUSER:

I think this is very good, very thoughtful. It creates a nice structure to your comments.

KLARMAN:

We may want to put this under the heading of planning, which is one of the subdisciplines we could draw on. I remember attending a meeting once; I think it may have been at Brookings. It was a small group—maybe 10 people or so—and the moderator asked me to talk about health planning. I said, among other things, something negative about health planning and planners, particularly as it applied to the borough of the Bronx. The man became angry with me. Since I had worked in health planning, I was expected to favor the process and the product under all circumstances. This attitude is foreign to me, as an academic. In fact, a good friend, a former colleague whom we haven't mentioned yet, Joe Peters, once observed that I was an academic before I held an academic post. He said that I treated the Hospital Council's staff as if we were in academia. In fact, he became fully aware of the change in intellectual climate after I left; it had become a different work place.

I enjoy the idea of giving Joe a plug. As you know, he's been a successful hospital consultant. Whether he's a hospital consultant or a planning consultant, I don't know. He has a degree in hospital administration from Columbia, and he did run a hospital, Beekman, for a few

years. It happens to be a hospital of which I had made a study before he joined the Hospital Council. For Manhattan, it was a small hospital, well run, with a large ambulance service.

We proceed now to a new area. What am I concentrating on these days? I've been thinking hard about how to prepare, educate, and train health economists and policy analysts. I regret that I do not have a definite plan in mind. I do know this: I hold no brief for learning on the job, being virtually self-taught, as I was. The reasons for this are evident: the tendency to do things the hard way; the gaps in one's ability to do a job; the dependence on luck in one's colleagues and chance variation in opportunities to serve on committees and to consult. On the whole, I have been fortunate in that I have often found colleagues and peers who were willing to bear with me, to teach me, and fill my gaps. I've credited Francesca Thomas for her invaluable help at the Hospital Council, and I learned a lot on the Health Services Research Study Section. In a different setting, with a policy orientation, I learned a lot on the Gottschalk committee.

The question is how do we educate or train a person as a health economist, policy analyst, or an adviser in these areas? At a minimum, we want to add to the basic disciplines of economics and statistics, some epidemiology—I don't know how much—and some demography, especially life tables. I have no idea what we should—or could—learn from clinical medicine ahead of time, before facing a particular problem or issue, or from formal organization theory.

My own education was in economic theory as a core. Even so, it wasn't the standard economic theory prevalent at the time. At Columbia College, we were essentially in institutional economics à la Veblen. At Wisconsin, we were institutional à la John Commons. Neither has remained a central figure in the economics mainstream. In that sense, I was almost miseducated. In the late 1930s, my major field, public finance, dealt largely with taxation. We hadn't reached the Keynesian revolution, which put equal stress, or maybe even greater stress, on government

spending.

Within economics, statistics was a minor. In retrospect, I think it was taught better at Columbia than at Wisconsin. At Columbia, I had as teachers both Harold Hotelling and Frederick Mills, whom you may remember from his textbook. They were, of course, worlds apart. There was little carryover in Wisconsin. As I've mentioned, I designed my own political science secondary field—the history of political thought and constitutional law. Both parts had intellectual interest for me, and I was lucky in the quality of the former.

The question is, was my formal education good enough? Perhaps it was for an economist, but certainly not for a future health economist.

The role of policy analyst may be different. One thing I would insist upon is that a person be thoroughly grounded in one of the relevant disciplines. I'm not sure that I know which one to choose, but in preparation for performing empirical research and analysis in health care, one should have to display mastery of at least one of them—its subject matter, theory, and techniques. Later on, the worker will acquire analytical judgment (Solow's term) from experience.

What about the disciplines one did not study? Here, it is necessary to learn which representatives of the other disciplines to trust and what to trust them for. Can this type of judgment be taught? I doubt it. Do you value most in others theoretical knowledge, technical skills, or institutional grasp? Personally, I can identify individuals whom I learned to trust like Francesca Thomas, John Hume, Stan Mayers, and members of the Gottschalk committee. I think this is hard to teach, if it can be taught at all.

In summary, these are the major requirements: have thorough command of a discipline, and then learn how to work with people from other disciplines.

One facet of economics has changed fundamentally over the years: the emphasis on econometrics. This has gone way beyond what used to be mathematical statistics. Econometrics has to do with all kinds of special devices, formulas, and computer bits that are beyond me. I think econometrics has replaced a foreign language in the requirements of some Ph.D. programs in economics. It strikes me as so complex to learn and apply that I don't know how much room is left for anything else. Still, don't we owe it to our graduates to enable them to get and hold jobs? Conversely, with the emphasis on econometrics and the manipulation of existing data sets, when do students learn about data collection or how to make judgments about the usefulness of an existing data set that becomes available? Does knowledge of experimental design become obsolete?

One problem that has grown over the years, I think, is that researchers tend to be more secretive about their data and the way it was analyzed. I believe the tradition used to be freer; if you did a statistical study and somebody wanted to look at the materials, you were glad to share them.

Another problem I see is the absence of interviewing by the researchers themselves. I've mentioned that I placed heavy reliance on meeting certain people periodically when I was in New York, especially in the 1950s, and learning from them what was going on in the real world of institutions and agencies. I don't know any substitute for this approach.

Another thing I have learned, both when Fuchs paired me with Arrow, and when George James paired me with Franz Goldmann, is that I was unable to separate the empirical work I was assigned from any attention to theory. There's no question that Ken Arrow is a superior theorist to me, by huge quantities. But I cannot simply do my work without some reference to underlying theory, a set of beliefs, or bundle of approaches—all of which I may modify in the

course of work. What problems are you trying to solve? What questions are pertinent? What facts bear on them?

As you know, by now, having mentioned it repeatedly, I came to attach much importance to outside contacts, committees, or consulting assignments. I learned that I had to work hard. Consequently, when I joined the Gottschalk committee, I obtained a commitment from him, an understanding, that I could publish any work I did for the committee. I don't see how else an academic can afford the investment of time and effort. The converse of this is that I don't see the point of joining an activity unless I'm prepared to do some solid work.

From our conversation, you know that the Gottschalk committee and Kerr White's Study Section represent the highlights of my outside work. Moreover, even when I was hired as a consultant, as I have mentioned, by the city of New York, I did not pocket the money. I didn't want to feel that I had to do that work on my own time, in evenings, and on weekends, and I wanted to feel free to consult anybody on the Hospital Council staff. How could I do that if I was making money and they were not? Perhaps this was shortsighted of me; I never earned much money. One year, when I was at Downstate Medical Center, a drug company asked me to join an advisory committee, and they offered me several thousand dollars a year. I said no. Perhaps I didn't trust myself to remain an objective scholar; although I had never studied drugs, I didn't know that I wouldn't in the future. So I wasn't about to accept the lucrative offer.

Let me turn again to our nearly total reliance for financing on project grants or contracts today. In the 1960s, when I was at Hopkins, there was such a thing as a program grant. These have been eliminated, I think. With a program grant, one could do some research, as well as teaching. Indeed, you could supplement the program grant money with a grant for a particular research project. To my mind, this was a superior arrangement. At any rate, I never learned how

John Humes financed my salary of $18,000. The question never came up between us. Having seen John a few times in the past few years, I wouldn't be surprised if he could almost read my mind; he is so sensitive.

You will recall my opposition to having the department censor my papers before submitting them for publication. Why my vehement outburst? What were my specific arguments? I felt that I talked freely to my colleagues about my work and was always open to comments and suggestions. I saw no point to formal internal scrutiny; approval of my work has to come from my peers in the outside world. Some colleagues in the department wouldn't even understand my language as an economist or the relative importance of the topic reported on. With John, my arguments carried readily.

As I have sometimes said clearly and more often implied, the practice of financing research by project grants is bound to hurt teaching. With one exception, perhaps; I've seen it in Dave Salkever who devotes a great deal of time to supervising Ph.D. dissertations. It's almost a one-on-one tutorial, as at Cambridge or Oxford University. But most students don't get that much from the faculty. There is also the inherent problem in project grant funding, that one must spend time applying, writing proposals; in that sense, one almost has to know ahead of time quite a bit about what he proposes to study. This means that you have to spend some time thinking about this or that research proposal before you're ready to apply. When your current project runs out—or preferably, is about to run out—you had better rush to finish whatever you're doing, ready or not. A major objection of mine relates to the researcher's lack of continuity of effort over time, namely my idea that one should revisit, reexamine an area from time to time. Even if you have a desire to revisit, you won't if other, newer things are financed.

Again, why have I come to think it's so important to revisit problems or areas? How did

I happen to do so much revisiting? One answer is I was asked to present a paper on some panel, and even though the question wasn't defined precisely, it meant I would go back to my earlier work in that area and ask myself what, if anything, may have changed. What have I learned from the literature or otherwise since my previous effort? A good example: I had worked on the sources of the increase in hospital costs for my hospital book, but I was not satisfied. Even though I told Odin Anderson that Monroe Lerner was doing his work correctly at that time, his method was pretty much like mine, and I didn't really like it. The residual term was so large. So, in later articles of mine, you will see applications of the method of annual rates of increase that came out of the monitoring of the Soviet economy by outsiders. The reason American economists had to resort to re-analyzing the Soviet five-year plan data is that Soviet publications did not present numbers; they showed index numbers. The Americans had to engage in a lot of data manipulation. The method of annual rates of change was later introduced to the study of the U.S. economy by Ed Denison.

Another lesson that emerged from my efforts to revisit problems was that I became increasingly aware of the difficulty, perhaps impossibility, of defining the need for health care and, therefore, the need for health care personnel. I had deliberately introduced a more neutral term; instead of need, I said requirements. My concern with this problem starts with the paper I did for the American Economic Association proceedings published in 1951. I drew heavily, at that time, on data from the Health Insurance Plan of Greater New York; I did the best I could, but I don't think I felt comfortable doing this. To this day I don't know how to measure physician requirements.

When I worked on my syphilis paper and was writing *The Economics of Health* at the same time, I applied the Mushkin method of cost-benefit analysis, but I was not comfortable

doing it. I did introduce a few small changes, but ultimately I was led to conclude, when I worked for the Gottschalk committee, that the appropriate method is cost-effectiveness analysis. Indeed, repeated revisiting led to my last published paper, "The Road to Cost-Effectiveness Analysis," published in the *Milbank Memorial Fund Quarterly*. Dave Willis, the journal editor, was glad to have it, but it was perhaps a peculiar, meandering paper. What happened was that a group was convened to prepare a memorial volume in honor of Selma Mushkin. The project didn't pan out, but I had written my contribution and was then able to obtain separate publication for it.

Certainly, I didn't learn on my first day at the Hospital Council the difference between incidence and prevalence. How you apply incidence when you look at the acute care general hospital, which was then our major interest, and how you have to move to prevalence, which we didn't do much with, when you look at long-term care. That distinction, I think, really came through for me when I worked on the assignment for New York City's Interdepartmental Health Council. I was really doing it for the Health Department, which served as intermediary.

When I first started looking at technology in hospitals, much to my surprise, I found that it was low-tech, like x-ray and routine lab services, that had a greater effect on hospital spending in the 1950s, an era preceding the introduction of much high-tech. I have no feel for it today. Would low-tech or high-tech dominate today? It's important to go not just by what's commonly believed and said today, but to look at things historically.

As I mentioned earlier in connection with health care spending, four percent of the gross national product spent on medical care was the conventional standard. Yet, I was always unwilling, and I still am, to do straight-line projections of where we're heading. Certainly, the 14 percent figure we have reached would have shocked me several decades ago. Indeed, I

thought 10 percent would be a shocker. At any rate, I don't trust straight-line extrapolation in the effort to understand what has been going on. Maybe more sophisticated models capture the factors involved. I don't know; I doubt it. The case is evident for occasionally revisiting techniques, methods of doing things, and the findings they yielded.

Let me stress, too, the importance of revisiting institutions and organizational arrangements. Again, I'd like to note a few, for I think most of them, maybe even all of them, have been discussed in different contexts. One: as we all know, widespread and deep deinstitutionalization has gone on in state mental hospital systems. I assumed, maybe all of us assumed, that spending would decline accordingly. I learned as a member of the State Health Advisory Council—I did not review the state budget routinely—that, although the patient load in some hospitals was down to as low as 50 or 100 from 3,000, the institution was kept going at substantial cost, due to local pressures, whether from employee unions, the local citizenry, or their representatives in the state legislature. Of course, this fact accounts, to a considerable extent, for the failure of large amounts of money to flow into community mental health centers, as promised. It wasn't merely that the public didn't approve of such centers or didn't feel they were necessary; the money was lacking. I'm sure this failure has contributed appreciably to the homeless problem.

In my writing, I prefer to use the word appreciable when referring to magnitude, reserving the word "significant" for its statistical sense only. What do you think of this idea of leaving significance to statistics only?

NEUHAUSER:

I agree.

KLARMAN:

Good. And you like appreciable as an adjective?

NEUHAUSER:

That's good.

KLARMAN:

I use the word sizeable, when the change is bigger.

I have talked at length about what has happened to prepaid group practice. This form of clinical organization gained prominence from the Committee on the Costs of Medical Care, with its strong staff, such as Michael Davis and Ig Falk. Let me take a short detour on Ig Falk. I first met him when he was in government in the Social Security Administration. I don't think he had a substantial job then, but he was there. He was certainly, even aggressively, trying to impress me with his knowledge. Intellectual intimidation would perhaps be too strong a phrase for his attitude, but he tried to show me that he knew everything that was going on, including the Health Insurance Plan of Greater New York, in New York City. True, I was supposed to be the expert on New York, but he knew more about it than I did. That was firmly my first impression.

My second meeting was sad. During the McCarthy era, he lost his federal job. Subsequently, he was acting as a consultant to some outfit, I think in Panama, which collapsed. In effect, Falk came to talk to me at the Hospital Council, looking for a job. I didn't have any, and, frankly, I didn't know how we could use a man of his intellect, abilities, and convictions. I do have the view that some individuals don't have to fit in, if they're geniuses. But I would be inclined to limit their employment to academia; I don't know how to use them in an organization that is trying to be responsive to various segments of the community. I felt bad that I didn't have a job for him.

This incident had an effect on me, for years later, when I was on the Study Section, an application arrived from Ig Falk. He had moved to Yale in the interim. He didn't state in his application what he intended to work on, but he wanted some money, perhaps to supplement his pension. The main reviewer, whose name I will not mention, said no, the man isn't telling us what he intends to work on. My reaction was suppose all he does is hire a secretary to take his memoirs of a long and distinguished career in research; what would be so terrible? Of course, I lost the argument. A project proposal had to be just so, and Falk didn't meet the standard.

Let me return to and focus again on prepaid group practice. This obviously includes what was called "socialized medicine." There was a clinic in Oklahoma that was ostracized by the medical profession.

NEUHAUSER:

Elk City, Oklahoma?

KLARMAN:

Yes, you got it. The Health Insurance Plan of Greater New York was established after World War II in New York City under Mayor Fiorello La Guardia, with strong medical leadership, including Dr. George Baehr. The Health Insurance Plan of Greater New York was going to be different. In one way, it was definitely different because it established a research department at the outset. Initially, Neva Deardorff was its director. Then, Paul Densen came and he hired Sam Shapiro, who was, in effect, his deputy. For years after that, all the data one saw on prepaid group practice came from the Health Insurance Plan of Greater New York. Maybe there was one exception, Puget Sound.

At the same time, Kaiser Permanente was growing in California. It was a direct transfer from the shipyard in the state of Washington by this very active and successful entrepreneur,

Henry Kaiser. He was very smart; the health plan benefits he offered helped him attract people to work for him. To this observer, the only difference between Kaiser and the Health Insurance Plan of Greater New York in the 1940s and 1950s was that Kaiser didn't wear its heart on its sleeve, it had no research department, and it issued no data. At the time, a distinction was drawn—I think it was a proper distinction—between health insurance and prepayment, because some of the commercial plans wouldn't insure obstetrics on the ground that childbirth was the result of a voluntary act. The prepaid group practice advocates were arguing for people to pay for health care while they were healthy, not when they were sick; that way it wouldn't be a financial burden. I thought that was an important distinction, and I thought an appropriate term would be third-party payment. Of course, you don't know whether a term that made sense then is also useful 30 or 40 years later.

I've not really studied the history of prepaid plans. I have not even read Harold Luft's book, I'm sorry to say. He sent me a draft of his doctoral dissertation and asked me to review it. I wrote back and said I'm sorry, I can't afford the time.

The next step was the HMO, and I've already told you how I met Paul Ellwood at a meeting convened and chaired by Martin Feldstein. There's no doubt in my mind of Ellwood's ability to sell an idea, even when he was merging these two arch rivals in California—prepaid group practice and independent practice associations. In an independent practice association, the participating physician simply retains his own office and practices as he always did, except that for this category of patients he is on a monthly retainer rather than getting paid separately for each service rendered.

One gets into the bad habit of using all these terms, like managed care. I do not understand how today, when you read it in the *Times*, the HMO is the very model of managed

care. I don't see it as managed care particularly. What has happened to second opinions, utilization review, and prior approval of various services, including admission to the hospital?

In the 1980s, I haven't done any sustained and systematic work on anything other than those few op-ed page articles I've mentioned. Without the sense of evolution, change, and what's unexpected and unanticipated, it is too easy to think that what is now is what has always been. Granted that you have to start any inquiry with what is the situation now, but you don't have to start with the way people think about it now. You have to start with the actual institutional arrangements; these may be different from the way they are viewed.

Let me turn now to another change that has come about in employment-based health insurance. There are people who think the time has come to get rid of it. I am persuaded by Henry Aaron's argument that the equity problems—the redistributional problems—would be so great that one probably couldn't accomplish anything; therefore, stay away from it as a practical matter. Even so, I'm troubled. In the absence of employer contributions, how do you mandate individual health insurance without imposing an individual tax? At any rate, we do remember that employer-based and paid insurance was introduced in wartime as a fringe benefit analogous to pensions. It was a powerful tool to prevent inflation in wartime, one of the major concerns of government. After the war, this fringe benefit became one way for a union leader to demonstrate to his members an ability to achieve benefits for all of them. Suppose you negotiate unemployment insurance benefits; that applies only to those who become unemployed. But every member gets health insurance; this is democratic, isn't it? It is also a vehicle for employers to attract desirable employees. Michael Harrington, who wrote about poverty, chose to teach at a college that furnished him with health insurance. He had been an independent organizer, a and consultant, a writer, but he needed now to have health insurance; he was getting into his late 40s

and had a family.

We have also learned that in the course of time, health insurance benefits have become a source of job lock for workers. They would stay in a job because they didn't dare make a change; health insurance is not portable. There is also the problem of preexisting conditions.

I think that employer-based insurance in large volume led to the ability of employers to exercise influence over insurers. Probably, it was influential in Blue Cross's moving away from its early practice of community rating to experience rating. I remember there was a man by the name of Stewart in Cincinnati. He ran the local Blue Cross plan, and he didn't want to abandon community rating, which he regarded as the basic concept, the root beneath Blue Cross. Finally, he had no choice if he were to continue to compete successfully with commercial insurers.

Today, we must recognize that employment-based health insurance is the only way to mandate health insurance without raising taxes. I don't know of any obvious alternatives to this easy way to collect premiums.

Let me now consider the changes that have occurred in the hospital. I first learned about civilian hospitals in the late 1940s. My military experience doesn't count. My civilian experience started with the statewide study under the Hill-Burton program in 1948-49. The general, acute care hospital had become the center for good patient care. I have bought the Rosemary Stevens history of the American hospital, but haven't read it yet, so, what follows is my own best recollection. At the time, a reputable voluntary nonprofit hospital in New York City also had an outpatient department for the poor; it did not have an ambulatory facility for middle-class patients. The emergency room was largely for people who were improvident, as well as for patients arriving by emergency ambulance. The attending physicians were really not involved in providing the ambulatory services.

In the course of time, hospital length of stay declined substantially. We learned early, in wartime, from Howard Rusk—his is another name I want to mention—that, by early ambulation, the Air Force hospitals were successful in reducing length of stay considerably; however, application of this knowledge in civilian hospitals took time. I got to know Howard Rusk because there was a group that met at the executive offices building. It must have been 1951-52, when I was working in Washington. Government executives came to make their budget presentations to this group. Rusk and I would listen to them, even though formally he belonged at the Pentagon. In fact, he was also chairman of a committee that advised the president. I remember once, when Pennsylvania Avenue was still open—you could walk on the side of Pennsylvania Avenue that was adjacent to the White House—I ran into Howard and we exchanged greetings. He said, I've just left the President, he's taking a shower, and it was time for me to leave. Rusk must have known Truman, since he was from St. Louis, Missouri. Later on, in one of my rounds in New York City, I interviewed him. Mostly he talked about his high-level contacts in Washington.

NEUHAUSER:

He was the one who developed the Rehabilitation Institute.

KLARMAN:

Yes. He got strong backing from an influential man who always talked more than he actually did, a fine talker. You, I'm sure, know the name, Bernard Baruch.

NEUHAUSER:

Oh, yes.

KLARMAN:

Baruch's support, I believe, played a big part in getting Rusk to move to New York and

to join New York University. At that time, rehabilitation was not considered a high-level service in leading hospitals like New York Hospital, Presbyterian, or Mount Sinai. They wouldn't establish one.

Hospital length of stay did decline over time, but it moved glacially. Also, length of stay was markedly shorter in California than in New York. It was a big difference—a 30, 40, 50 percent difference in days if you look at the gross statistics. I remember with births, due to the large Jewish population in New York City, obstetrical stay was longer because a boy stayed for seven days so he could have the *bris* (circumcision) in the hospital. That did change later, but I don't know when the change took place or what precipitated it.

Another thing happened: certificates of need were for hospitals, not for independent ambulatory facilities or for any equipment in physicians' offices. It doesn't really matter whether the certificate of need was effective or not; perhaps it was too effective, as I think some economists would argue. The fact is that the effort was made to control hospital construction and that some appreciable results were obtained. There was no attempt to control the construction or establishment of other facilities. I suspect there was a carryover from not doing anything in physicians' offices to not regulating other ambulatory facilities. Recognition of the implications of any of this for rate setting was slow. As I've already noted, the overhead costs could no longer be allocated according to ability to pay on the demand side, which was the Roswell method at the United Hospital Fund. Instead, the hospital had to be competitive with the independent clinic, which meant that inpatient rates had to rise. Let me add, however, that I have not been close to this development for several decades, not just since my retirement.

I didn't recognize the importance of the Hill-Burton program when I joined the Hospital Council in 1949, and even after I had worked there for several years. The main reason we didn't

recognize its importance is that the rest of the staff, other than Mrs. Thomas, didn't work on it. This program gave the Hospital Council considerable authority and influence that it would not have enjoyed otherwise if it were merely a voluntary do-good organization devoted to planning. I still remember John Thompson coming down from Montefiore Hospital and asking for a grant for long-term care beds at Montefiore. They didn't ask for much; all they sought was $100,000. Why? This would give them extra prestige and standing in raising money from philanthropists. Altogether, New York State was getting about $3 million a year for hospital construction grants, and the Council got about $1 million of that sum. I believe the contribution to the Council's prestige and authority was much larger. It occurred to me that this is an important organization.

I would like to mention now, although I'm not confident that it will mean much, that one way out of the dilemma of financing research through project grants to the disadvantage of teaching, is to give recognition and financing to scholarship. Treat it as equally valuable as research. What do I mean here by scholarship? It's pulling together all that is known in an area or on a problem—all the research that's been done to date—and analyzing it and trying to reconcile discrepancies in findings among studies. This is virtually the sort of effort I made in the paper on hospital bed use under prepaid group practice. I would also like to see a return to greater emphasis on and reward for publishing monographs over journal articles. One of the problems with articles arises from the project research grant and contract atmosphere. Authors break up a study in order to list as many publications as possible. As a result, the reader may have a hard time figuring out the history that led the authors to take up this problem area, what was done with sampling, what they did on this question or that. The truncated article doesn't tell us. Monographs don't have to be long, but they afford space and can make a bigger allowance for some failures in the research process, which are inevitable. As I have mentioned previously,

in the outline I prepared for the voluntary-municipal hospital study, there were some things like quality of care that I wasn't able to deliver on.

By the way, I don't think I worked much more than one year on this study. In the course of doing this study, I was able to consider some things I hadn't even thought of when I was preparing the outline. But I was aware of the possibility that some data could be used in several places, to illuminate different problems.

I trust that I haven't overprepared because it sort of minimizes your role today.

NEUHAUSER:

Not in the least.

KLARMAN:

This does give you some idea of the way I tend to work, whether it's to prepare for teaching or to write a paper or to submit a report to a committee.

In this last section, allow me to list some of the important lessons I've learned from experience. You will not hear anything new here. It's a shortened summary of what I've said; it may be useful.

I learned one lesson when I talked to Norman Goetz and he showed me the letter he had received from Hayden Nicholson. I said it meant no and Goetz said kindly, "You don't know how to read. He has just said yes." The lesson I draw now from this conversation is that you do not hire someone who is so obviously reluctant to come.

Another practical lesson I learned is that it's dangerous for administrators or professors to get overly involved in outside activities, neglecting their own institution's work. Martin Cherkasky is a strong example of a guy who stayed home and was very influential, very effective.

A third lesson is how important the chairman of a committee is, not only for a committee with a substantive agenda, but also for a committee like the Study Section, which, in a sense, doesn't have a clearly defined mission. I've seen too many chairmen who let things go on at meetings without closure.

I've also heard differences in the summaries that chairmen offer. Some don't convey what was really said; they tell what they'd like to have heard, so that they may pursue their own agenda.

I have learned that personal charm, such as in theatrical presence à la Lunt and Fontaine, and nonthreatening articulateness can be persuasive and influential. Paul Ellwood is a model for this. I doubt that many other people could have put across the HMO concept.

I have noticed that colleagues who are not close friends can be very supportive. I offered the example of Sam Shapiro of the Health Insurance Plan of Greater New York telling me privately at the close of a meeting that I had done more with their data than he did.

I learned that going to conventions can be helpful and interesting, not just by attending meetings, but by the meals and drinks one shares. I told you about my conversation with George Baehr, asking him was it was true that Mayor La Guardia wanted him as his successor.

I learned over and over that learning everything on the job, which happened to me, is to do things the hard way, the complicated way. There ought to be ways to prepare people to work in health care policy, planning, regulation, etc. I gave some indications how. I'm not sure that I know how much of the process belongs in education, how much in training, and how much derives from the sheer maturation of an individual over time.

I've noted, in the course of my observation of colleagues who achieved successful and prominent careers compared with others who were, I think, similarly capable and promising, but

were not so successful, the importance of physical energy and stamina. As examples, I have referred to Joe Pechman, Eli Ginzberg, Walter Heller, and let me add Joe's sister, Dorothy Rice.

A lesson I learned from Rufus Rorem: if you want good information from people, give them something as a down payment in exchange for taking the trouble to complete your questionnaire. There is an additional benefit Rufus did not mention: in the process of filling in your own questionnaire, you improve it. In retrospect, why did I write only to 9 or 10 agencies? Why didn't I write to 20? Well, I didn't think the response rate would be so high.

I am unable to separate empirical work from some relationship to theory. Indeed, I protected myself from the influence of Arrow's paper by not reading it until I had written my first draft. It was only then that I incorporated his ideas. It would have been an unfriendly act not to include him in the bibliography. I have mentioned the separation of functions that George James established a decade later between Franz Goldmann and me on the study of the aged in New York City. Goldmann came up with several principles, almost proverbial sayings, but I couldn't use them.

One of the pleasures of working at the Hospital Council was that people inside and outside came to trust me. All sides on an issue would call me when they sought impartial information. It was nice to be asked by the deputy health commissioner, who attended our board meetings, to do a study for the city; that was a high accolade. Years later, I felt honored when, as a member of that Veterans Administration committee of the National Research Council, I was singled out and invited to join the Veterans Administration's own advisory committee by the medical director.

Let me turn now to some negative reactions. I have mentioned in the discussion of planning that I was disappointed and felt rebuked for saying that planners are not always wise

and may make mistakes.

I was astonished when I learned about what was going on in the state mental hospital system—patient load going down substantially, but the budget staying the same.

I was disappointed when I interviewed the four medical school deans involved in Bellevue Hospital and got self-centered versions of the facts. This reaction of mine was confirmed later in connection with the Agent Orange question when the deans and medical vice presidents readily agreed with the Veterans Administration that there was no evidence that Agent Orange caused illness, even though none of them had studied it.

I was surprised by the poor performance by a distinguished academic author when he presented a paper at one of the meetings that Nora Piore organized so well. No doubt, I, too, would have invited him if that were my responsibility. Yet, he had little recollection of the findings of his own study. As I mentioned, I knew more than he did about his paper. Why wasn't he interested in preparing for the meeting? Was it a certain elitism on his part or had he moved on to another project?

I was certainly disappointed by the fact that the three appointees to the Master Plan Committee I had recommended did not contribute. Perhaps some people like to join outside committees as a release from the constraints of their regular jobs.

There are many things I don't understand. Why has the Health Insurance Plan of Greater New York stumbled along in this age of the HMO? They're the same size as they were 40 years ago. In fact, I never did understand the legal battles between several Health Insurance Plan medical groups and central headquarters. The former included what was probably the best medical group outside Montefiore, the Brooklyn group under Bob Rothenberg. Nor do I know why Sam Shapiro left the Health Insurance Plan. In his later years at the Health Insurance Plan,

Sam was not only director of research, he was also a vice president. There may have been conflicts in goals that may be quite legitimate.

I don't know the relative importance of technical skills and people skills when a person is recruited for a management or administrative position. I don't think we pay enough attention to this question.

What do you do when researchers tell you their results, but you don't really understand the methods by which they were obtained? I'm not referring to something like Roemer's Law where we are told the methods, but are not persuaded. I'm thinking about the way I feel about the Maryland Hospital Cost Commission, which is generally deemed a great success. I went to see the staff when I arrived at Hopkins in 1962. I didn't understand their exposition of how they worked. I remember mentioning it to several colleagues at Hopkins, whose response was that they didn't understand it either. Still, nobody criticized it.

I don't know whether this can happen today, but I remember with relish reading about an order of Sisters in Philadelphia. They withdrew their hospital from the local Blue Cross plan because they didn't understand its method of calculating reimbursement rates, and Blue Cross didn't clarify it for them.

NEUHAUSER:

I think one of the themes of your life can be called scholarship. That includes your work in government.

KLARMAN:

Let me say, having worked in government, I didn't meet the kind of bureaucrats people talk about. My experience in government, on the whole, was that people worked hard and meant to do the right thing.

Let me tell you an anecdote that Eli Ginzberg told me about scholarship. It was about John Maurice Clark, the son of John Bates Clark. J. M. Clark was a great economist at Columbia; he couldn't teach a lick, he mumbled, but it was a pleasure even so to listen to him. He was so original and profound. I asked Eli, how does this man write such good books? Eli answered, that, at the end of May or early in June, Clark loaded his van with all the journals that he had accumulated during the academic year and all the books on a given subject, took them to his country home, and read them, thought about them, and wrote the book. As I see it, it took steady application and industry, with final closure by Labor Day, in addition to the essential ingredients of critical thinking and analysis, plus writing with style.

It remains to be seen whether I possess any of these characteristics. At any rate, Duncan, as we arrive at the end of this interview, let me thank you. Your presence and participation, your comments and questions, have made this journey over time a most pleasurable experience for me.

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