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Reviews and Commentary

PRESENTATION OF THE FIRST JOHN SNOW AWARD TO ABRAHAM M. LILIENFELD

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The John Snow Award was established by the Epidemiology Section of the American Public Health Association at the Annual Meeting in 1974. It is perhaps unique in that it was funded entirely by contributions from 140 members of the Association, almost all of whom are members of the Epidemiology Section.

The John Snow Award is given "for distinguished service to the health of the public through outstanding contributions to epidemiology." In establishing the Award, the Section stipulated that it "is to be given to individuals of the highest caliber. The citation is not to be interpreted in any narrow sense. Nor are there to be any parochial or national boundaries involved in the choice."

The Award was established in recognition of the advent of a new epoch in the history of epidemiology and public health. For a hundred years, starting with the middle of the nineteenth century when Snow carried out his classic studies of

cholera, epidemiology provided the scientific basis for the remarkable victories of the public health movement over the infectious diseases.

During the latter part of this period, attention began to be focused on other diseases of a noninfectious nature, such as the nutritional diseases, the occupational diseases and cancer. In a scientific sense, one can perhaps say that Goldberger's classic work on pellagra ushered in the second epidemiologic revolution—the era of the epidemiology and prevention of the noninfectious diseases—just as Snow's work three-quarters of a century before had signaled the beginning of the first.

In each instance the maturation of the movement took time; there was a long incubation period. Not until a quarter of a century later did the work of Pasteur and Koch confirm the epidemiologic evidence for the germ theory of disease transmission which Snow had marshalled so brilliantly. Leaders of the public health movement such as John Simon and William Farr did not accept Snow's findings but adhered instead to the effluvia theory of transmission.

In our own era, that of the second epidemiologic revolution, the incubation period has likewise been long. Perhaps we may say that the definitive onset of the movement

¹ Dr. Terris made this presentation as Chairman of the Awards Committee of the Epidemiology Section, American Public Health Association, on November 18, 1975. Members of the Committee were: Henry Blackburn, John Cassel, Jerome Cornfield, George Hutchison and Neal Nathanson.

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occurred in the early forties, dramatically symbolized by the resignation of John Ryle, the Regius Professor of Physic at Cambridge, to become the first Professor of Social Medicine at Oxford, heading a department concerned almost exclusively with the epidemiology of the noninfectious diseases. Indeed the British movement toward social medicine—led by such keen investigators as Richard Doll, Tom McKewon, Jeremy Morris and Alice Stewart—was primarily a movement toward the second epidemiologic revolution.

It is fitting that our Section has given John Snow's name to the Award for two reasons. One is the many brilliant contributions which our British colleagues have made to our discipline. The other is that the analogy between John Snow's work and ours is closer than appears at first sight. Snow came to remarkably accurate conclusions on the etiology and prevention of cholera by the use of "merely statistical" evidence, which is the layman's and clinician's term of disparagement of epidemiology, of the study of disease in human population groups. Only later was there confirmation by so-called "real science," namely, the study of disease in rats, mice and guinea pigs. Snow's natural experiment in the real world of London had to be validated a quarter of a century afterwards by contrived experiments in the laboratory.

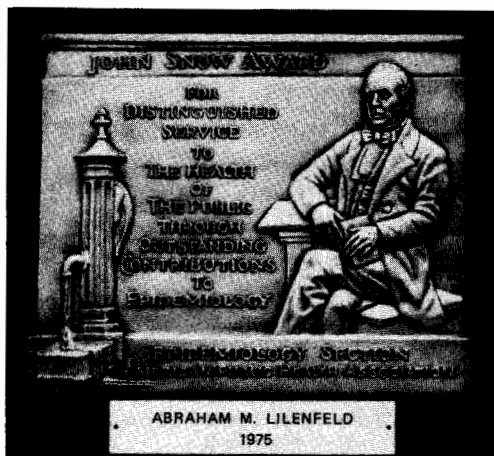
And so it is today. We epidemiologists have come to remarkably accurate conclusions on the etiology and prevention of a host of diseases, such as lung cancer, ischemic heart disease and cerebrovascular disease, long before the laboratory scientists will succeed in discovering the precise pathophysiologic mechanisms that are involved. Because, as in John Snow's time, the latter information is not yet available, we have difficulty in obtaining acceptance of our scientific conclusions and our recommendations for preventive action. The lung cancer controversy is perhaps the most dramatic example of the struggles we have

waged and will continue to wage in the interest of the health of the public.

Abraham Lilienfeld, the first recipient of the John Snow Award, has been and still is in the center of these struggles. From almost the very beginning, he has personified the new epidemiology and led in its development.

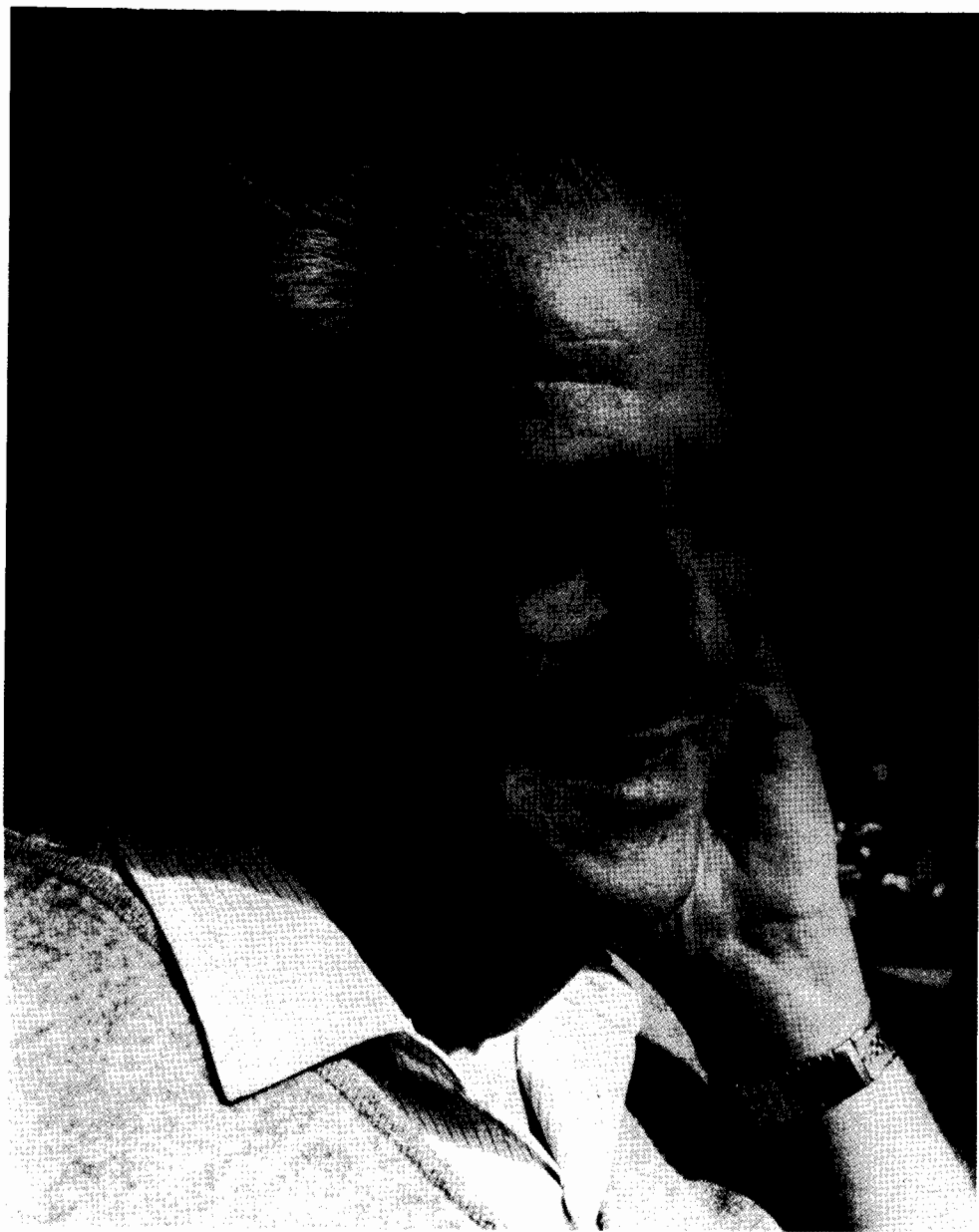
Abe began as a resident in obstetrics and gynecology who never went on to practice his specialty. Instead he became interested in public health and, after working in the tuberculosis control division of the US Public Health Service, joined the staff of the New York State Department of Health. He then became a district health officer in the Baltimore City Health Department, following which he went full-time into epidemiology, first at The Johns Hopkins University School of Hygiene and Public Health and then for four years as chief of epidemiology at the Roswell Park Memorial Institute in Buffalo, returning in 1958 to Johns Hopkins to head first the Department of Chronic Diseases and then the Department of Epidemiology.

Abe's contributions to epidemiologic research have been remarkable for their breadth and originality. It was he who originated the concept of "the continuum of reproductive casualty" which he and Benjamin Pasamanick later applied in a



Plaque presented on the occasion of the first John Snow Award.

photo by Danny Gordis



ABRAHAM M. LILIENFELD

series of outstanding investigations that provided the scientific basis for the massive prospective study by the National Institute of Neurological Diseases and Blindness.

Abe has carried out numerous studies in the epidemiology of cancer, including cancer of the breast, urinary bladder, lung, cervix, stomach, ovary, prostate, and leukemia. He has, furthermore, refused to limit himself to cancer, and has made important contributions in cardiovascular and cerebrovascular disease, mongolism, ulcerative colitis and others. Much concerned with the problems of screening and early detection, he has led a number of national studies to evaluate radiologic and cytologic screening for lung cancer, and thermography in the detection of breast cancer.

Throughout the years, Abe has had a consuming interest in problems of methodology. He was an active and leading participant in the methodological controversies on lung cancer and cigarette smoking. He carried out a series of studies on measurement and diagnostic problems; his first publication, for example, was a report in 1949 on a study of variations in the interpretation of x-ray pelvimetry. He went on to investigate the accuracy of supplemental medical information on birth certificates, the reproducibility of muscle testing, variability in the interpretation of chest x-rays in the detection of lung cancer, the quality of death certificate diagnoses of arteriosclerotic heart disease, and the inaccuracy of interviews in estimating patient reliability in taking medication. The methodology of genetic epidemiology, the effects of errors in classification, the methodologic problems in noninfectious as compared with infectious diseases—the last being the subject of the first Wade Hampton Frost Lecture which he gave in 1972—all of these concerned and intrigued him.

There are three of his methodological studies that I have always cherished for

their originality, simplicity, and what, for lack of a more accurate word, I shall have to call charm. Who but Abe Lilienfeld could have thought up a study to demonstrate that by using a binomial test of the genetic hypothesis, one could show that attendance at medical school is inherited as a recessive gene? Or a simple demonstration that men's reports on their circumcision status were confirmed by examination in only two-thirds of the cases? Or an analysis of the social class differences in arteriosclerotic heart disease in England and Wales which showed that the differences were not in the social classes but in their physicians?

I first came to know Abe personally when he arrived in Buffalo in 1954. We not only served together in the Department of Preventive Medicine at the University of Buffalo, but were members of a rather unusual collection of individuals known as the Western New York Epidemiological Society, which should probably be memorialized as the most influential small-town epidemiological society in the history of the discipline. Our members included such outstanding epidemiologists and public health workers as Wendell Ames, Saxon Graham, Rema Lapouse, Mary Monk, William Mosher and Warren Winkelstein. Abe was our genius and in every sense the leader of our informal, close-knit and remarkably productive group. Those years began friendships that have remained strong despite the years of dispersion and separation.

I want to speak now about Abe as a person, and about the Lilienfeld style of work. I shall mix these up, because the two are inseparable.

The most important thing about Abe is his selfless devotion to epidemiology. This is his life, and he gives himself to it. Nobody works harder or with more dedication.

Abe gives of himself without stint, to the federal and other agencies who want his

advice and consultation, to his colleagues, and to his students. He never holds back on his ideas in the miserly way in which so many of us protect the few gems of originality that we come up with. Uninterested in his own glory, Abe never hesitates to offer a wealth of research ideas, suggestions, methodological pointers and encouragement to others. The key here is his selflessness; one never feels that Abe is in it to push Abe forward, but rather to push science, knowledge, epidemiology forward. And his concern with epidemiology is not that of a mere technician. On the contrary, Abe did not go into public health to play games with computers. He started out as a public health administrator, and his basic commitment is to public health, to the prevention of human misery. His approach to epidemiology is an idealistic one, for he rightly sees this as the key discipline to obtain the knowledge we need for effective public health action.

The other remarkable thing about Abe is his rationality. Always he explains and explains, patiently, without anger. I watched him once at an after-dinner discussion with one of the most prominent critics of the work linking cigarette smoking and lung

cancer. This was, considering the subject and the diametrically opposite views of the discussants, a highly inflammatory situation. It never came to that because coolly and rationally and patiently, Abe Lilienteld explained his point of view.

This is one of the keys to his great success as a teacher. He is a master of the Socratic approach, drawing out students, encouraging them to think. I can speak with authority on this score, having sat in on his courses at the Graduate Summer Session in Minneapolis and observed his sensitive treatment of students and the superbly dialectic nature of his teaching. Here, as in his research, he never fails to emphasize the continuity and essential unity of infectious and noninfectious disease epidemiology, and the fundamentally biological nature of our discipline.

Abe Lilienteld is one of the great epidemiologists of our time. I do not limit the statement by place, for his influence and his reputation are international in scope. Here is a person who has had a profound effect on the development of our discipline and the forward thrust of public health. It is an honor to present the first John Snow Award to Abraham M. Lilienteld.