

NEW YORK UNIVERSITY-BELLEVUE MEDICAL CENTER
OF NEW YORK UNIVERSITY
INSTITUTE OF INDUSTRIAL MEDICINE

PLAINTIFF'S EXHIBIT

MET-569

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Dear Doctor Armstrong :

As you may know, Professor Lanza advised me that my appointment in the University would be terminated, and this action is due to take place on June 30. Events have suggested to me that this and certain prior actions are related to the fact that my studies involved disclosure of occupational cancer hazards affecting employees of industrial organizations that, I understand, retained Professor Lanza as a consultant. I am advised that obstructions have been placed in the way of studies of environmental cancer hazards elsewhere, when these have touched upon economic interests. I believe it is in the public interest to explore this matter. For this reason, I am now writing to request a hearing.

I am confident that the University would not knowingly endorse events of the nature indicated, which are inconsistent with the "right to seek the truth" defined by Chancellor Heald as our most priceless asset, according to the Alumni Bulletin for November, 1954. I believe, therefore, that the University has a very real interest in ascertaining the facts in this matter, and I wish to submit the following account of events as they have appeared to me.

In 1949, I accepted Professor Lanza's invitation to join the faculty of New York University and to take responsibility for cancer research in the Institute of Industrial Medicine. Prior to that time, I had devoted four years to cancer research at the Rockefeller Institute, two years at the Sloan-Kettering Institute, and had published extensively in this field. Since 1949, I have published twenty three articles in this field (references attached). Two issues of the A.M.A. Archives of Industrial Hygiene and Occupational Medicine have appeared, which were devoted to environmental, particularly occupational, cancer studies either conducted by me or which I was instrumental in bringing to publication from other institutions here and abroad in a manner to the credit of New York University. During the past seven years at the University, I have worked with a series of five part-time assistants of doctorate grade and have collaborated in research with five other members of the faculty, in each instance tangible results ensuing, as evidenced by the attached list of publications. In 1950, I conducted, and subsequently published, a survey of occupational tumor studies

Great Britain. In 1962, I served as an American delegate to the Symposium on Lung Cancer held in Lourvain by the International Union Against Cancer. In 1964, I served as chairman of the Symposium on Cancer Prevention at the Sixth International Cancer Congress in Sao Paulo. I mention these activities as evidence of energetic and creditable work in the field in which I was invited to serve the University.

When I joined the faculty of the new Institute of Industrial Medicine in 1949, I emphasized the desirability of developing studies on environmental factors in lung cancer. My suggestions for such an effort in the space then available to me were declined by Professor Lanza on grounds that other than current studies should first be brought to some conclusion. This was done, as recorded in publications referred to above, notably those conducted by me in collaboration with petroleum companies, and which constituted the first major scientific contribution of the Institute.

In the fall of 1950, conversations on lung cancer studies were resumed with a biochemist in the Institute, Professor Horton Nelson, who had not previously engaged in cancer research. I assisted Prof. Nelson in development of proposals for such studies, which were agreed to be a collaborative enterprise. I looked forward with pleasure to the opportunity of pursuing studies in this field with Professor Nelson. He stated that the proposals were of a tentative nature and were to be sent to the dean for consideration. My subsequent inquiries as to progress of these proposals met with replies that they were under review by a committee of the medical center. Finally, I learned that the fact was that they had been submitted without my knowledge as a request for a substantial grant from the Damon Runyon Fund with Professor Nelson named as the investigator, and that this grant had been made. After the grant was received, my inquiries on the subject met with evasive replies.

I was, however, requested by Professor Nelson to prepare a review of literature on experimental studies of lung cancer. I prepared and published such a review (Ref.No.32). I also prepared and published a review of occupational factors in lung cancer in man, at the request of the International Union Against Cancer (Ref. No. 35). I elsewhere published my method for experimental production of lung cancer, which provided a technique for experimental production of lung cancers of the types common in human cases of this disease (Refs.Nos.21&22). I had previously published studies on the types of lung tumors common in mice (Ref.No.20). I mention these studies to document my interest and activity in lung cancer, noting that Professor Nelson had not engaged in research in this or other fields of cancer work.

During development of the lung cancer program in the Institute, the manuscript of the review which I had been requested to prepare for it and which set forth goals for study, was repeatedly borrowed by Professor Nelson; but I was excluded from planning or participation in the program. I brought these facts to the attention of Professor Lanza, to no avail, in the fall of 1961. All personnel for this work were engaged without my knowledge until their appearance in the laboratory. Meanwhile (1961 and 62), laboratory facilities, aside

from animal quarters, were withheld from me, and Professor Lanza declined my request for a single full-time assistant of doctorate grade when the chairman of the Center's cancer committee invited me to apply for such a man under the institutional grant from the American Cancer Society.

In the spring of 1962, Dr. G. P. Rhoads invited me to speak at Memorial Hospital to representatives of cigarette and cigarette paper industries, who wished to learn of techniques available for estimating potential carcinogens in cigarette smoke. I described this meeting to Professors Lanza and Nelson. A few weeks later, I was invited at noon to attend a meeting at two o'clock in Prof. Lanza's office at which Prof. Nelson presented, to some of the same individuals I had met at Memorial, a typed proposal, prepared without my knowledge, for study of their questions with recommendation that they support such studies through contributions to the Damon Runyon Fund. Professor Nelson pointed out that, since both he and Prof. Lanza were on the board of that fund, they were in a position to assure that such contributions would be funneled into the lung cancer grant, of which Professor Nelson had complete charge. Since several individuals at this meeting had come from as far away as North Carolina, I could only conclude that the meeting had been planned substantially in advance of the two hours notice given me.

At the end of this meeting, Professor Nelson informed me that he did not have experience to equip him for this work, and was therefore detaching my only assistant to assist him.

In May 1963, I was requested by Professor Nelson to prepare material for a press release intended to raise funds for the Institute. On May 14, 1963, this material appeared in the New York Times in an article emanating from a press conference held by Professors Lanza and Nelson. This article dealt largely with cancer studies I had conducted in collaboration with petroleum companies, and credited the work wholly to Professor Nelson. He later told me that the Times article crediting him with my work was a reporter's error. I find this difficult to believe, since he was similarly credited in an article in the New York Post on the same date. I have no wish for public notice, but it would seem that Professor Nelson, in addition to seizing control of the future cancer program of the Institute, acquired for himself credit for such work as I had done for the Institute in the past.

When effort was begun to identify potential carcinogens in cigarette smoke condensates, it became apparent that techniques utilized for such studies with petroleum samples were unsuitable by reason of requiring large volumes of material for repeated applications to animals for periods approximating their life spans. I thereupon developed a rapid screening test which has permitted detection of carcinogens within four days (Refs. 36 & 39). In the early spring of 1963, I suggested to Professor Nelson that this rapid screening test put us in an advantageous position to trace carcinogens in urban air and engine exhausts as well as in cigarette smoke. I suggested that we develop studies in this direction. Dr. Nelson said that he would like to think this over. He shortly advised that he had, without further consultation with me, submitted proposals for study of air pollution problems to the New York Port Authority. His action in this matter thus paralleled his initial action in the development of the lung cancer program.

The usefulness of the rapid test, mentioned above, is evidenced by our successful application of it in tracing carcinogens in oils, tars and cigarette smoke, and by the fact that it has been taken up in many university and industrial laboratories. In order to develop my discovery of this test, I requested, in the spring of 1964, participation in the institutional grant then being sought by the Center from the American Cancer Society. Within a few days, Professor Nelson informed me that he had found it necessary to cancel my application in favor of an application for funds for his own work.

I would have a sterile view indeed did I not welcome anyone who wishes to devote time and effort to the cancer problem. I do not, however, believe that a newcomer to a field, such as Professor Nelson, should utilize his administrative responsibilities in the Institute and his position as an officer of a Fund to exclude from the field a man with a record of thirteen years productive work in it.

IT IS MY CONTENTION THAT, WHEN AN ADMINISTRATOR AND FUND OFFICER WHO HAS NEVER ENGAGED IN RESEARCH IN A GIVEN FIELD ATTEMPTS TO DRIVE AN EXPERIENCED INVESTIGATOR OUT OF THAT FIELD AND TO ACQUIRE CONTROL FOR HIMSELF, THERE EXISTS A SITUATION PREJUDICIAL TO THE REPUTATION OF THE INSTITUTION CONDONING IT AND TO ANY FUND THAT KNOWINGLY SUPPORTS IT, A DANGEROUS PRECEDENT FOR SCIENCE IN AMERICA AND AN AFFRONT TO SCHOLARS EVERYWHERE.

I wish to state very clearly that I have no monopolistic delusions or ambitions, and no wish to exclude anyone from engaging in cancer research. I have at all times stood ready to cooperate with any individuals in the Institute interested in cancer problems. I have had no quarrel with Professor Nelson's responsibilities as director of research for the Institute. Within that broad framework, which involved many types of problems, I believe that my subordinate responsibilities in the special field of cancer research should have been accorded the simple courtesies which would so easily have made for effective and honorable team work. I have repeatedly indicated this to Professor Nelson, to no avail.

It has seemed exceedingly curious that these simple courtesies have been so notably lacking. In attempting to comprehend these events, I recall that, according to my understanding, Professor Lanza served since about 1949 as consultant for a large asbestos company, which I shall define for the present purpose as Company X. In the spring of 1960, medical officers of this and other asbestos companies visited the Institute to discuss a report published by the Chief Inspector of Factories in Great Britain indicating association of lung cancer with asbestosis. I stated to them that I expected to make a survey of certain other occupational tumor studies in Great Britain during the ensuing summer, and could attempt to learn something about the cases on which this report was based.

During my work in England, I traced 21 of the 31 then reported cases of co-existing asbestosis and lung cancer, and learned that all but 1 of the 31 had been employed in the industry prior to 1932, the year in which exhaust ventilation devices had been installed in the plants yielding these cases. The Senior Medical Inspector of Factories, the pathologist and the plant medical officer advised that they felt asbestosis did predispose to lung cancer, but that the number of cases of asbestosis and the fulminating course of individual cases had greatly subsided since installation of ventilating equipment.

I transmitted their views, especially their opinion that here was a triumph of industrial hygiene, in my report (Ref.No.33) presented at a meeting of the Cancer Prevention Committee in New York on Oct.18,1950. This meeting was attended by medical officers of asbestos companies and by their consultant, Dr. Lanza. I had not learned at that time that the practice of industrial medicine, in some quarters, calls for obfuscating hazards for as many years as possible. My mention at this meeting of what seemed an opportunity for preventive medicine evidently disqualified me for research in lung cancer, for shortly thereafter Dr. Lanza advised that he did not feel it feasible for me to go ahead with studies previously formulated by him in a letter of August 25, 1950 for experimental assay of the potential carcinogenicity of asbestos dust, and endorsed transfer of all lung cancer studies to Professor Nelson, who had never worked in this or any other field of cancer research. Since that time, major decisions for the cancer program of the Institute, which I had been brought here to develop, were made behind my back, without my knowledge, and in a manner to exclude my participation.

In the following year, Professor Lanza spoke to me about field studies of occupational cancer problems then being conducted by the National Cancer Institute of the U.S. Public Health Service. These included studies of lung cancer among chromate workers. Professor Lanza stated that he had been retained by a group of chromate companies as a medical consultant, and that in this capacity he had called upon the surgeon general and had made objections to these studies. Professor Lanza told me that his objections had been successful, and that all field studies of industrial cancer risks by the National Cancer Institute would be terminated. I was frankly shocked by his apparent gratification at this disruption of the work of a responsible government agency at a time when it was developing data of profound importance to the public health. I had the very distinct impression that he communicated this information to me as a threat that he would similarly terminate my work if I took continued interest in problems bearing on occupational factors in lung cancer.

Also in 1952, the director of a well-known scientific institution, a physician who had spent years in the study of asbestosis and other forms of pneumoconiosis, announced his important discovery that an industrial dust brought about cancer in the lungs of animals inhaling it. Within a year, Professor Lanza stated to me that he had utilized his position on the board of that institution to force this man's resignation, and that he had subsequently prevented this man from getting a job in a medical school in California by going personally to call upon the dean of that school. Again, I had the very clear impression that this information was conveyed as a threat to discourage me from studies in

this field. I wish to inquire whether Dr. Lanza's trip to California was paid for by the company referred to above as Company X. I understand he was then serving as a consultant to that company in connection with a problem of another dust disease (diatomaceous earth pneumoconiosis) affecting workers in their California plant, a matter I will comment upon further in this letter.

Meanwhile, progress in evaluation of the question of lung cancer in relation to asbestosis had been awaiting age-adjusted statistics. These were forwarded to me in February of 1964 by the medical officer of a plant in England after review by a statistician assigned by the Medical Research Council with their opinion that a lung cancer risk existed in this situation. Knowing Professor Lanza's interest in this subject and his service with asbestos Company X over here, I showed these data to him. He responded with what I can only describe as a vehement denunciation of cancer investigators as "trouble makers", and within a few months notified me that my appointment in New York University would be terminated.

I believe this matter involves a fundamental problem of freedom of academic research. I further believe it is pertinent to recall that Dr. Herbert K. Abrams, formerly chief of the Bureau of Adult Health, State of California, published, in the May 1964 issue of the American Journal of Public Health, an account of the diatomaceous earth pneumoconiosis problem mentioned above as affecting a plant operated by Company X in California. Dr. Abrams' article indicates that, although many workers in California had died from this disease, the matter was allowed to drift along for twenty years until the International Chemical Workers Union won a strike to gain recognition for the presence of a hazard. Workers of the Canadian affiliate of this same company also found it necessary to strike for better industrial hygiene conditions in asbestos operations. These men were also fighting for their health and their very lives against a preventable disease, but, according to advice given me, these workers were beaten up by police, priests who supported their cause were transferred elsewhere, and the editorial staff of the Jesuit journal that had spoken out for them was broken up. To this list is about to be added the firing of a professor at New York University by Dr. Lanza, who served as a consultant to the above company in connection with the strikes in California and Canada.

I recall that when I began research in industrial cancer hazards a distinguished scientist predicted that pretexts would be found to block my work as well as to wreck the program of the National Cancer Institute in the same field. Both events have occurred.

Many cancer investigators now believe* that the most practical direction open today for prevention of many forms of this disease depends upon identification and control of environmental sources of exposure to cancer-inciting agents, many of which are now known. I appreciate the need to conduct studies of potential industrial and environmental cancer hazards in a manner avoiding undue public alarm or unjustifiable claims for workmen's compensation. In my work

* Cf. Resolutions, Acta Union Internat. contre le Cancer, II, No.1, 72-78, 1955.

with petroleum companies, I have had the opportunity to learn how problems in this field can be solved through sincere study and effort. They cannot be solved by driving scientists out of their laboratories.

At the recent Sixth International Cancer Congress, it was observed that some of the most active scientists in this field had been driven out of their laboratories soon after describing findings bearing on cancer hazards of industrial origin. In the hearing requested in this present letter, our University has the opportunity to add to this roster of obstructions to advance of knowledge, or to take a stand for better things. Much good can be anticipated from clarifying this matter. American industry leads the world in support of scientific research. I firmly believe that leaders of industry, if assured of the opportunities open to them for control of environmental cancer hazards, will foster efforts in this field with renewed vigor.

Very sincerely yours,

William E. Smith, M.D.

Associate Professor
of
Industrial Medicine

cc: The Chancellor