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Current Opinion

Annals of Industry — Noncasualties of the Work Place

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The Editor of this journal has solicited its readers to respond to the letter by William E. Morton, M.D., Dr. PH (JOM 15:860-61, 1973). In addition to responding to that letter, I think it important that some response also be made to the series of five articles by Paul Brodeur which appeared in the *New Yorker* magazine (October 29, November 5, 12, 19 and 26, 1973).

The accusation is either made, or implied, in both the letter and the series of articles, that industrial medicine and industrial physicians lack adequate integrity, honesty, and morality. In Dr. Morton's letter, the accusation is made that industrial physicians and allied health workers, suppress publication of information and data which are needed by the rest of the medical profession to care adequately for their patients who are workers. Although undoubtedly this may be true in some instances, the suggestion that it is universally true simply demonstrates Dr. Morton's surprising lack of familiarity with the available literature and references in the field of industrial and occupational medicine and toxicology.

Perhaps the most used reference in the field, *Industrial Hygiene and Toxicology*, a respected two volume work, had as its original editor, Frank A. Patty, industrial hygienist for *General Motors*. When Patty became unable to continue his editorship because of disability, David W. Fassett, M.D. of *Eastman Kodak Co.* and Don D. Irish of *Dow Chemical Co.* became the editors of Volume 2 of the revised edition.

The textbook *Modern Occupational Medicine* was authored by A. J. Fleming, C. A. D'Alonzo, and J. A. Zapp of the *E. I. duPont de Nemours & Co.*

Toxicology and Biochemistry of Aromatic Hydrocarbons, perhaps the most definitive volume on this subject, was authored by H. W. Gerarde, M.D., Ph.D., while he was on the staff of the *Esso Research and Engineering Co.*

The most up-to-date, even though admittedly now out of date, book on occupational cancer is my own *Industrial Carcinogens*.

Incidentally the history of this small volume, which does contain a section on asbestos cancer, is an interesting one. Published in 1959, 2000 copies were initially printed, of which only 1000 were bound. After about ten years, the publishers wrote to me and indicated that the 1000 unbound copies were still in their warehouse and offered me the author's option to buy some of these before they were destroyed. With less than 1000 volumes of this book purchased in over ten years, this is one indication of the interest that the rest of the medical profession has in the field of industrial medicine. It is not that industrial health workers do not write for the education and edification of the medical profession; it is that the medical profession doesn't often read their published articles. As a member of the editorial board of the *Archives of Environmental Health*, I know that its subscriptions number only slightly more than 9,000 per year. This again is an indication of a general lack of interest in this field amongst the medical profession.

Turning to the literature, the editor of the *Journal of Occupational Medicine* gave some refutation to Dr. Morton. He might also have added that Smyth, Carpenter and associates, of the *Union Carbide Co.* Fellowship at the Mellon Institute, published a large series of papers on range-finding toxicity tests performed on a great variety of chemicals. These appeared in various issues of the *Archives of Environmental Health* or its predecessor journals or the *American Industrial Hygiene Association Journal*. The original description of acro-osteolysis among polyvinyl chloride reactor cleaners in this country appeared in the *Journal of the AMA* with Wilson and associates of *B. F. Goodrich Co.* as authors. The *Medical Bulletin*, published for years by the *Exxon Corp.*, formerly the *Standard Oil Co. (N. J.)*, annually has one issue containing a statistical breakdown of the health of the workers in its affiliates. D. L. Block, M.D. of *Ford Motor Co.*, with an associate, has published on the incidence and natural progression of bronchitis among his workers. Richard Stewart, M.D., then of *Dow Chemical Co.*, has published articles on breath analysis as a means of monitoring worker exposure to chlorinated solvents. It would appear that Dr. Morton's criticism results to great extent from a lack of familiarity with the literature of our field or from incomplete knowledge. He makes three quotes in his letter, "Of course, we couldn't publish that"; "Management refused permission to publish that study," and "We don't dare report the frequency of that reaction in our plant," which will be dealt with later on in this article.

With reference to the series of five articles in the *New Yorker* by Mr. Brodeur, this represents a detailed and at times tediously repetitive case history primarily concerned with one material, asbestos, and with emphasis on one plant, namely the Tyler, Texas, plant of the Pittsburgh Corning Corporation. As important as the subject of that focus is, there cannot remain unchallenged the concept that the story is representative even if it is proven to be accurately reported. Initially it is tempting to point out errors in Mr. Brodeur's knowledge and to

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question the credibility of his reporting on the basis of factual errors such as his suggestion that the Occupational Health Institute (OHI) was formed recently and that Clayton and Associates prepared a standard for benzene. Actually, OHI was formed more than 20 years ago. The $71,000 plus contract, mentioned by Mr. Brodeur, was given to OHI to develop a valid accrediting program for occupational health services, a project that was supported by representatives of organized labor. The major portion of the money is being spent by a subcontractor, the Kettering Laboratory of the University of Cincinnati Medical School, which is actually developing and testing such criteria. The only monies being spent by OHI for itself and the Accreditation Commission are travel monies for its staff and the commissioners to meet with and review the criteria as they are developed by Kettering. Before the criteria are finalized, an open meeting which will include labor, management and the professions, will be held so that their inputs can be gained. It is the hope that these criteria and accreditation programs will serve to upgrade occupational health programs much as the Joint Accreditation Commission on Hospitals, and its accreditation program, has served to upgrade hospitals.

The fact that such accreditation programs may fall short of their aspirations is well recognized, but that they do much to improve things cannot be denied. When firmly established such an accreditation system should do much to accomplish some of the things suggested by Dr. Morton since accreditation can be rescinded at any time. Strong, enthusiastic, vigorous support from labor, management and the professions will be needed if these hopes are to be achieved.

In regard to the document prepared by Clayton and Associates, this was not a standard but a criteria document, with an included *suggested* standard. The review of it held in Pittsburgh was an initial *informal* review. A final, *formal* review of it was still scheduled before it was to be turned over to OSHA. This formal review committee included Jeanne Stellman, Ph.D., Louis Beliczky (both of whom were mentioned and identified by Mr. Brodeur), Carl Nau, M.D., of the University of Oklahoma Medical School, Richard Stewart, M.D., of the Medical College of Wisconsin, and myself. This review committee met in Washington in early 1973. The meeting was amicable and on a scientific plane, accomplishing a lot in terms of sharpening up the document. We were not always initially in agreement but were able to reach agreement after discussion and hearing each other's viewpoints. Why the document has not yet issued I do not know. It is a tough, down-to-earth, practical document which should adequately protect workers.

However, pointing out such shortcomings in Mr. Brodeur's article still does not answer the criticisms he has made of how the case reports developed and were handled. I am concerned that Mr. Brodeur leaves the impression that all industrial or occupational medicine acts only in the economic interest of industry and not in the interest of the health of the workers for whom they are responsible. I am also concerned by his rather glib and frequent use of the terms "medical-industrial complex" and "the independent medical and scientific community." These terms, as used in Mr. Brodeur's articles, and also implied in Dr. Morton's letter, leave the impression that industrial physicians as a whole form a great complex with industry which is in direct conflict with, and opposed to, the interests of workers and of the rest of the medical and scientific community. Having been in the field of industrial or occupational medicine for over 25 years, I find this thought extremely distressing and in distinct contrast to my own experience. In those years, have I done nothing to help save the lives and suffering of workers? Certainly this was my goal and objective in entering the field.

To answer this question, I would like to present a case study much as Mr. Brodeur did, this time a case study of the life of one industrial or occupational physician — me. Perhaps this can develop a different perspective of industrial medicine. My own experiences are used as an example simply because it is with them that I have the greatest familiarity. From talking with hundreds of other industrial physicians, I am sure other industrial physicians could cite similar examples. I hope my experiences may serve to help prevent the discouragement of younger men who are thinking of entering the field. God knows we need competent, able, eager young men by the thousands in this field. Since 1955, when it first started doing so, the American Board of Preventive Medicine has certified only between 500 and 600 men as specialists in occupational medicine, and anyone who has studied the Occupational Safety and Health Act and the regulations promulgated knows we need thousands more such specialists. This is not to say that certification in occupational medicine is the only measure of a man's ability in the field, but it is an important measure of efforts to achieve high standards of performance. Not one resident physician is in training in an approved residency this year. Further, allied occupational health personnel (industrial hygienists, industrial nurses and toxicologists) are needed in far greater profusion if we are to fulfill adequately our obligations under the Act.

When I first joined the Medical Department of the then Standard Oil Co. (N.J.), one of our main projects was an epidemiological study of scrotal cancer among wax pressmen in one of our refineries. This was coupled with an experimental study of the carcinogenicity of various waxes being performed for us by Dr. K. Sugiura of the Sloan-Kettering Institute of Memorial Hospital. We learned that out of a very small group of men who had worked on our wax presses for ten years or more, about 10% had developed scrotal cancer, an unusually high percentage most indicative of an occupational origin. When the studies were complete, we proposed to publish them in the medical literature. However, a company lawyer was strongly opposed to publication of these studies at that time. This view prevailed, and the studies were not published. I was then faced with my first dilemma. Should I resign in protest or shouldn't I? In evaluating the situation, I made a mental balance sheet. On the positive side of the ledger were the following:

1. First, the union leaders, then the men themselves, had been called together and the situation (and hazard) had been fully explained to them.
2. Men who had developed scrotal cancer were being well treated medically and compensated.
3. A protective program had been developed which we felt confident would prevent further cancers from developing except in those men who had already been exposed long enough so that they would develop scrotal cancer even if they worked at something else.

4. A program of regular skin examinations of the workers had been begun to pick up any cancers early when they were still curable.

5. Dr. Wilhelm Hueper, director of the carcinogenesis branch of the National Cancer Institute had been called in, all data had been reviewed with him, and his complete concurrence with our protective program had been obtained.

6. All member companies of the American Petroleum Institute had been alerted through the Medical Advisory Committee of the Institute of our experience and what we had done about it.

7. And most important, scrotal cancer in wax pressmen had already been reported in the medical literature.

The question, therefore, became: "What will publication accomplish?" Obviously, our egos would be fed. But was that what we were in this business for, or was it to protect workers? Besides, our data had already been presented verbally at several medical meetings, and was freely available to anyone who inquired of us. In this light, the matter of publication seemed less significant. I decided that I could probably accomplish just as much, perhaps more, from within the company as from outside it, so I decided to remain. Subsequently a change in technology (solvent dewaxing) made these presses obsolete and they were sold to one of our overseas affiliates. The overseas affiliate, however, was alerted to the problem and instituted our protective program from the first day of use of the presses. In more than 20 years of operation, there has been no incidence of cancer in their pressmen. This is a case of noncasualty in the work place. Later, we did publish our studies in the *Archives of Industrial Health.*[1,2]

A second major problem being studied at the time I began my career in the company was the carcinogenicity to mice of certain fractions of fluid-bed catalytically cracked oil. The fluid-bed catalytic cracking process had been developed during World War II to provide our aircraft with the high octane gasoline they needed. However, the high boiling residues from the operation were found to be highly carcinogenic to the skin of mice. In this case, publication was agreed to fully since this new process had never before been reported in the literature. The data were published[3] after oral presentation at the American Industrial Health Conference in Atlantic City in April 1951. Before the oral presentation in Atlantic City was made, however, we had done the following:

1. Called first the union leaders, then the men actually involved, together and explained fully our animal results, and how, although we had no cancers in men, we would fully expect them to develop if exposures to the oil were not reduced to a minimum.

2. Instituted a protective program to prevent skin contamination and inhalation.

3. Begun a program of regular skin examinations for those potentially exposed.

4. Brought W. Hueper, M.D. in as our consultant-advisor to make sure we had not overlooked anything.

5. Begun a program to minimize any potential danger to our customers.

Again, after more than 20 years, no cancers have occurred in these employees. This is a further case of noncasualty in the work place. Technology has now eliminated this problem because with a combination unit, these carcinogenic fractions are cracked to extinction.

A few years after I joined the company, one of our physicians became alarmed when several men in a small area of the refinery had to undergo disfiguring facial operations. An immediate epidemiological study was started and in our isopropyl alcohol manufacturing operation, we found two cases of nasal sinus cancer and two cases of intrinsic larynx cancer. By themselves, they were not statistically significant but in inquiring among other makers of isopropyl alcohol, we found that Union Carbide was encountering a similar problem. A third isopropyl alcohol manufacturer, with whom we met, reported no problem but this company lacked a real industrial medical program and it could have been that they had a problem of which they were unaware. Union Carbide published their results.[4] We did not, but this time it was the medical department's decision not to publish. It was our thinking that since our results were statistically nonsignificant, and since Union Carbide was publishing significant data, publication of our case reports would add nothing to the literature. However, again, the following positive action was taken within our plant.

1. The union leaders first, and then the men involved, were called together and told just what our studies had shown (what the hazard was).

2. All men potentially exposed were carefully examined. Men with cancer were well treated medically and compensated.

3. Robert Kehoe, M.D. of the University of Cincinnati Medical School and Dr. Hueper were both called in as our consultants.

4. Primarily as a result of our consultants' recommendations, the old, outmoded production facility was torn down and production was transferred to a modern completely enclosed operation in a different refinery. No cancers have developed in any of the men working on this newer unit in over 20 years of operation. Here again is a case of noncasualty in the work place once the problem had been discovered. Its discovery was entirely unanticipated, and I am sure not even the most expert carcinogenesis or occupational health-trained man would have told us to look there for such a problem. Even to this day we are not positive of the identity of the causal agent. It may have been a sultone similar to propane sultone shown by Preussman to be carcinogenic to the olfactory nerve of rodents, or a sulfate similar to dimethyl sulfate also shown to be carcinogenic to rodents by Preussman. The process has changed so radically over the years that we have been able to isolate neither of these compounds even though we have searched for them.

One day a research chemist from our Research Company dropped into my office to report work on a new and promising catalyst. It was beryllium oxide! My shudders could probably be felt throughout the building. Rather than tell him he couldn't use it, I suggested that I have my industrial hygienist visit him to tell him what he would have to do to control beryllium to the TLV. A few days later he called to tell me they had abandoned their research on beryllium and were concentrating on another metal. Even though this other metal was not as efficient as beryllium, the cost of controlling beryllium completely changed the economic picture and put beryllium out of the running. Another case of noncasualty in the work

place because this one never got out of the research lab.

About 10 years after I joined the company, the Medical Director of one of our affiliates called to tell me they were to begin the operation of a benzene plant, and that they wanted to check out their proposed control program with me before they began. This is what they described:

1. The area would be monitored at multiple points with a sequential sample analyzer with a built in alarm if any sample showed more than 25 ppm (the then accepted TLV, even by ANSI, for benzene).

2. Men would be monitored for urinary sulfate ratios regularly and if an abnormality were found the reason for it would be investigated. (That was then the accepted means of monitoring men, although today urinary phenol determinations have been substituted.)

3. Men would be examined and have regular blood counts. Any changes in the latter would be cause for immediate thorough investigation.

4. Protective (impervious) clothing would be provided where splashes or other skin contact might occur.

5. Self-contained respirators would be liberally distributed throughout the unit in case of accidents and would be required to be worn in any doubtful situation.

6. The men would be informed of the hazards and how to protect themselves (i.e. if you can smell it, it's too much).

7. Ventilation (local exhaust) would be provided around sampling ports.

8. Special precautions and special lids would be used around tank car or tank truck filling. It seemed to me they had thought of everything. That they had is attested to by the fact they have had no cases whatsoever of benzene poisoning. Contrast this to the response I got from a Medical Director in a foreign country, who, in response to my question about precautions in his benzene plant responded, "But, Doctor, it's against the law to have benzene exposures in our country." Needless to say we made available to him the program developed by our domestic affiliate.

A year after I had been consulted by our domestic affiliate, I received a message from Carl Nau, M.D. of the University of Oklahoma Medical School. He had been called in as a consultant by a small chemical company. They had no industrial medical or industrial hygiene program, and were, therefore, unaware of the great hazard of benzene. They had already had a few cases of benzene poisoning. Naturally we referred him to the Medical Department of our domestic affiliate for assistance in developing a practical protective program for this company.

In the early days of our operations, our industrial hygienists used to find a meter repair shop in almost every refinery. Invariably, mercury was liberally spilled and potential exposures were present. People couldn't believe mercury was volatile. So a simple demonstration was developed. Some mercury was put in a crucible, a fluorescent screen mounted behind it and a U.V. lamp in front of it. Dramatically, mercury vapors could be seen rising off the dish like steam off a boiling pan of water. It was then a simple matter to get them to keep mercury underwater whenever possible, clean up spills immediately, and change the floor to smooth surfaces which could be regularly cleaned with sodium polysulfide. Again, noncasualty in the work place. Today most meters don't contain mercury and their use is discouraged insofar as possible. Advancing technology has essentially eliminated the need for mercury-containing meters, and, therefore, this potential hazard of mercury exposure.

It came to our attention that in one of our processes, nickel was used as a catalyst and carbon monoxide (CO) as a reaction chemical. The potential for nickel carbonyl formation was obviously present. This is a highly toxic and possibly carcinogenic compound. The alert went out to our operating people. The result — no cases of nickel carbonyl intoxication as long as I have been with the company. Yet another company had 32 cases with two deaths! Our Medical Research Division developed a portable, instantaneous field kit which would let people on the job know immediately that a unit was free of nickel carbonyl and, therefore, safe to enter. Again a case of noncasualty on the job.

In the case of two other hazardous situations, we developed impressive demonstrations to drive home our points. My former associate, Horace W. Gerarde, M.D., would make periodic visits to the boilermaking shop of our refineries. With him he would bring a menagerie of rats and various jars and chemical equipment. With all workers assembled, into one jar he would place a rat and then generate hydrogen sulfide within the jar. Within a few seconds the rat would show signs of agitation and appear as though looking for a way to get out. Within a few more seconds the rat would suddenly collapse and stop breathing. Dr. Gerarde would then calmly remove the lid from the jar, place the unconscious animal on the table top and give him artificial respiration by gently squeezing and releasing his rib cage. In a short time, the animal would start breathing on his own again and soon was running around the table top as good as new. What did this dramatic demonstration say to the worker?

1. Hydrogen sulfide in high concentration is extremely toxic. It can knock me out in a few seconds. I *must* be careful if I can smell it (it smells like rotten eggs).

2. If my buddy gets knocked out by hydrogen sulfide, I won't try to rescue him unless I have on a self-contained breathing apparatus, or unless I can get in and get him and myself out while I'm holding my breath.

3. If someone is knocked out by hydrogen sulfide, even though their breathing has stopped, their heart is still beating so as soon as I get them out to fresh air, I must begin artificial respiration. Once they breath off some of the hydrogen sulfide, their respiratory center will recover and they'll start breathing for themselves again. But time is limited because the brain can only do without oxygen for 4 to 8 minutes, and a person not breathing, or being breathed for (artificial respiration) isn't getting any oxygen to his brain.

4. Recovery from hydrogen sulfide poisoning is complete if our action is begun immediately.

To emphasize that toxic gases are not necessary, Dr. Gerarde then would remove the oxygen from another jar and drop a rat in it. The same thing would happen to the rat as in the case of hydrogen sulfide. The point of this demonstration was to teach employees that if they were entering a closed space or vessel and there were any question about whether or not there were enough oxygen in it (i.e., if it had been purged with nitrogen), they should test the atmosphere for its oxygen content and be sure of its adequacy before entering. People often forget that lack of oxygen can be as damaging as a poison gas, and as quickly. This demonstration was so dramatic, so well received,

and so well remembered by the workmen who saw it that when Dr. Gerarde left Esso Research and Engineering Company to go with Becton-Dickenson Company and Fairleigh Dickinson University it was video-taped and now can be shown on a regular basis to the workmen. One more case of noncasualty in the work place.

Are we infallible, and do we never err? The answer to this must, of course, be an emphatic no. No one is infallible, and occasionally disasters will happen. Once we had a man enter a tank that had been purged with nitrogen. He collapsed and was without oxygen so long that he suffered permanent brain damage. His case led to the second demonstration by Dr. Gerarde described above. None have occurred since we began use of his demonstration. We have had men die from hydrogen sulfide exposure. We have had occupational cancer in the past. These only serve to keep our senses and sensors sharp. Every time something goes amiss, we consider it a personal failure and redouble our efforts to see that it doesn't happen again. Through our mistakes and errors we learn. But so it is with all of medicine, not just the "medical-industrial complex."

I remember well when I was Chief of Medicine at Fort Meade Regional Station Hospital. We had a young serviceman in one of the wards who was in deep coma but we did not know his diagnosis. During the weekend while I was off duty, he died. Autopsy showed he had a sub-dural hematoma (bleeding under the skull). On Monday when I returned, I kept asking myself whether the diagnosis would have become self-evident and by remaining at the hospital might have saved his life? In any event, about a year later, when I had returned to civilian life for another year of residency in medicine, we had a woman patient enter the ward in deep coma. The head of the neurology service concluded she had an encephalitis. Nonetheless, something about that woman kept haunting me. Finally it came to me. She looked like the young serviceman. I proposed that we put bilateral burr holes in her skull to see if we could find blood. The chief of neurology looked at me as though I had lost my senses. She subsequently died, and she, too, at autopsy had a subdural hematoma. My chief's comment was: "You were right but for all the wrong reasons." I quote this example simply to demonstrate that in any type of medicine, no one is infallible. Unfortunately a mistake in the medical field may have disastrous consequences.

Medicine in any setting carries a heavy responsibility. It is no less in industrial or occupational settings, perhaps more because not only do you have to convince yourself, but you have to convince a lay manager. Often he may play golf on the weekends with a physician who knows nothing of occupational medicine. In the course of their casual conversation recommendations may be tested out on the physician golf partner. Monday morning you are confronted by your lay manager with the statement that you know not whereof you speak because Dr. John Doe says so, and after all Dr. Doe is a very nice guy and an obviously successful practitioner.

You win some of the battles, and some of the wars, but you also lose some of the battles and some of the wars. On balance, however, from the few illustrations that I have recorded, occupational medicine has been a worthwhile and rewarding career. We *have* saved lives and prevented suffering, and in the long pull have probably been as successful in this endeavor as are those who treat individual ill patients. To the young contemplating the field, I say come on in. We need you and need you badly. You will not make as much money as you will in private practice, but money should not be a primary motivation for any type of medical practice. We are not outcasts from the "independent medical and scientific community." We work hand in hand with them for the same objectives. Publications are not necessarily the measure of our success. What is quietly accomplished within our operations to save lives and prevent suffering is.

I would like to say that when I use the terms I and we, as in the above quoted illustrative examples, I am referring to the dozens of qualified industrial physicians, industrial hygienists, toxicologists, industrial nurses, chemists and managers who have worked with me over the years within and outside the company to achieve the objectives illustrated. It has truly been a team effort, and I want to give credit to all those who have worked so closely and cooperatively with me over the years to achieve these objectives. Many more illustrations could be given but I think I have given enough to make my point.

Another distressing feature of Mr. Brodeur's article is his implication that anyone who has worked with or consulted with industry has had his thinking done for him by the industry or has had his thinking influenced by payments from the industry. For instance, in the case of George Wright, M.D., he implies that because Dr. Wright recommended the 5 fiber standard he had not arrived at this decision independently but only under pressure from the industry. I have known Dr. Wright ever since he was one of my instructors in physiology while I was a medical student at Case Western Reserve University Medical School. Several things I came to admire about him during this time. One of these was his capacity for independent thinking, his inclination to question the "known" and to design experiments which would add new knowledge to our limited supply. Another, and one which often made much more work for myself, was his insistence that when you perform an experiment, perform it correctly. Many was the time he would come to me and my fellow students' physiology work bench, look over what we were about and the results we were getting, and then tell us to begin all over again because we were doing something wrong. It is difficult for me to believe that he could be influenced by anything that he did not himself sincerely believe to be the truth. If he supported the 5 fiber standard, I am sure that he did so out of the conviction that it would be protective of the workers' health, and that the 2 fiber standard would not represent a significant degree of added protection. I am sure he would listen carefully to the arguments of those who supported the 2 fiber standard and if these were convincing, would be willing to change his own thinking. Even Mr. Brodeur admits freely in his articles that the experts are not sure that the 2 fiber standard is the appropriate one. Perhaps it should be less, as Mr. Brodeur implies. Perhaps it should be more, as Dr. Wright and others have suggested. Who is to know at this stage of our knowledge? Even Dr. Selikoff, whom I know well and hold also in high regard and respect, would probably agree that the numerical standards being proposed for asbestos today are highly tenuous best guestimates. Certainly we need more information than we have available at the present time. I feel certain that the differences of opinion between Dr. Wright and Dr. Selikoff are honest differences of opinion, recognized by them both as such, due to our relative ignorance at this time. Hopefully

present and future research will provide the necessary information to allow these differences to be resolved.

Finally, Mr. Brodeur implies that labor unions have always had a great interest in the occupational health of their workers. Actually, with very few exceptions, their increased interest has been a most recent one. The American Occupational Medical Association (originally called the Association of Industrial physicians and Surgeons, then called the Industrial Medical Association) was founded in 1916 and has been in continuous operation since. It has concerned itself with the health of the American worker, attempting to upgrade occupational health programs wherever they exist. For many years it stood as the sole voice speaking up for preventive health programs for the work force. It has struggled with the problem of occupational health programs for small industries (under 500 employees) pointing out that it is often in such industries that the more serious occupational health hazards exist. It has through its journal, originally the Journal of Industrial Medicine and Surgery, more recently the *Journal of Occupational Medicine*, tried to bring recent and modern advances in its field to its approximately 4000 members. It annually conducts meetings in various areas of the United States to spread to its members and others in the area the gospel of occupational health. It has been joined in later years by other organizations, the Council on Occupational Health of the American Medical Association, the American Academy of Occupational Medicine, the American Association of Industrial Nurses, and the American Industrial Hygiene Association. It protested to the AMA when the Council on Occupational Health was abolished, and, with some of the other professional societies mentioned above, successfully convinced the AMA Board of Trustees to reverse its decision to discontinue publication of the *Archives of Environmental Health*.

Have labor unions and Dr. Morton participated with the IMA in these endeavors? The IMA even managed in 1960 to get the Permanent Commission and International Association on Occupational Health to hold its XIIIth Congress in New York City. American industry contributed $100,000 to make that meeting an outstanding success, thereby making industrial physicians in this country aware of the ideas and advances of their counterparts all around the world. Did labor unions contribute to the success of that Congress? The IMA has also repeatedly pointed out the inadequate support that budgeters within the government have given to its own Bureau of Occupational Health. The annual budget of that bureau for years was abysmally small, so small that very little of a positive nature could be accomplished. Were labor unions and Dr. Morton cognizant and interested and joining the protest about such inadequate budgets? Recently, of course, the voice of labor has been heard in strong support of appropriations for OSHA implementation and NIOSH work, which is very welcome to us.

We who have been fighting the battles of occupational medicine for these past 50 years and more, are delighted to see the significant interest being exhibited by labor unions in occupational health. We hope that they have come to join us in the battle. We welcome them not as an adversary, as Mr. Brodeur would lead one to believe, but as an ally. Our aims and objectives are identical. We are both interested in the health of the American worker. We can achieve this not by working at cross purposes with each other, but by joining hands and working together for our common goal. If inequities or bad practices exist, we must work together to correct them. This will not, however, be achieved by attempting to divide the profession into the "medical-industrial complex" on the one hand and "the independent medical and scientific community" on the other. I have tried to show how this is just not the case. Let not the brush stroke be too broad, for to do so will cover many diligent, capable, hard-working occupational health physicians who have been doing an outstanding job under difficult and trying circumstances for over 50 years. Our future still lies mostly ahead of us. There is much to be accomplished as we are well aware. Let us get on with the job at hand.

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