

Johns-Manville

Internal Correspondence

To All Environmental Com. Members & Division Managers Group* August 21, 1973
From F. J. Solon, Jr. - 4 North

Copies File & Chrono

Subject Lewinsohn-Selikoff

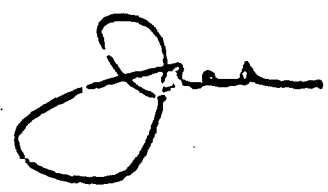
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| G. P. Loubert | F. A. Winterhalter |
| D. H. Lyons | G. W. Wright, M.D. |

w/ attachment

Attached is a copy of Dr. Selikoff's reply to Dr. Lewinsohn's letter of July 12.

It seems to be well documented though there is much of it we laymen admittedly do not fully understand.

The important factor will be the British reply and we will forward it as soon as received.



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	EXHIBIT
	JM-2152



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FIFTH AVENUE AND 100TH STREET • NEW YORK, N.Y. 10029



Department of Community Medicine

August 13, 1973

Hilton C. Lewinsohn M.B., B.Ch., D.I.H.
Chief Medical Officer
Turner Brothers Asbestos Company, Ltd.
P. O. Box 40
Rochdale, England

Dear Dr. Lewinsohn:

I have read your letter of July 12, 1973. Its burden seems to be that you and Dr. Holmes feel that the results of two sets of x-ray studies of asbestos workers at the Turner Brothers Asbestos Company factory in Rochdale, one made by Dr. Knox in 1966 and another by you in 1970, are not contradictory. You believe I was in error in suggesting that there are important discrepancies in the reported results of the two surveys.

Attached is a memorandum responding to the questions raised. I do not consider that your objections are material or valid nor do I find that you have dispelled the doubts held by me and by many other scientists. Indeed, the failure of your polemic to establish coherence and identity between Dr. Knox's study and yours might make one all the more concerned that such identity does not exist.

Correspondence between us is not the best way of settling the matter - and it must be settled. The problem is a serious one. Reports of results of x-ray surveys at the Turner Bros. Asbestos Co., Ltd. have been used to formulate industrial hygiene asbestos dust exposure standards, regulating the permitted dustiness in workplaces in which hundreds of thousands of men and women are employed. If the x-ray surveys have been inaccurately evaluated, then the standards are wrong and these workers are at serious risk of irreversible, often fatal, disease.

The essential contradiction is:

1. Dr. Knox reported to the British Occupational Hygiene Society that he x-rayed 290 production employees with more than 10 years of exposure at the Turner Bros. factory in 1966. He interpreted the films as showing that 5 had changes "possibly asbestotic" (2.7%). I reproduce the table: (From: Ann. Occup. Hyg. 11: 47-60, 1968 (page 55)).

8/13/73

TABLE I. CLINICAL AND RADIOLOGICAL ASSESSMENT OF TEXTILE ASBESTOS WORKERS
FIRST EMPLOYED AFTER 1.1.1933 AND EMPLOYED FOR 10 YEARS AND UPWARDS

Number	Persistent rhonchi present	Per cent of group	Basal rales	Per cent of group	X-ray changes not asbestotic	X-ray changes possibly asbestotic	Per cent of group
250	52	20.8	16*	6.4	12	8*	2.7

*Includes 3 subjects for asbestosis.

2. It was on these data "...made available to the committee by the Asbestos industry..." that the new standard "... is primarily based." (page 49).
3. You reported that you re-x-rayed all 1,287 workers at Turner Bros. (production, maintenance, research, quality control) in 1970. Your results are reproduced in the attached sheet, duplicating Figures 2 and 3 of your paper in the Royal Society of Health Journal in 1972. It is evident that among male workers the following were found:

<u>Years from first exposure</u>	<u>Abnormal x-rays</u>	<u>Pulmonary fibrosis</u>
10-19	more than 35%	more than 20%
20-29	" " 50%	" " 30%
30-39	" " 70%	" " 50%

Among female workers:

<u>Years from first exposure</u>	<u>Abnormal x-rays</u>	<u>Pulmonary fibrosis</u>
10-19	more than 20%	more than 40%
20-29	" " 35%	" " 60%
30-39	" " 35%	" " 80%

I omit reference to your 40-49 year group since Knox reported no comparable group.

4. Differences exist in the two studies (different times; 1966 and 1970; changes in factory staff; different x-ray interpreters and interpretation; different methods of recording etc.) But it was the same factory and, for the older employees for whom the comparison is made, largely the same workers who were x-rayed. The differences in recorded presence of early abnormalities are not trivial, and require explanation.

THE MEDICAL SURVEILLANCE OF ASBESTOS WORKERS

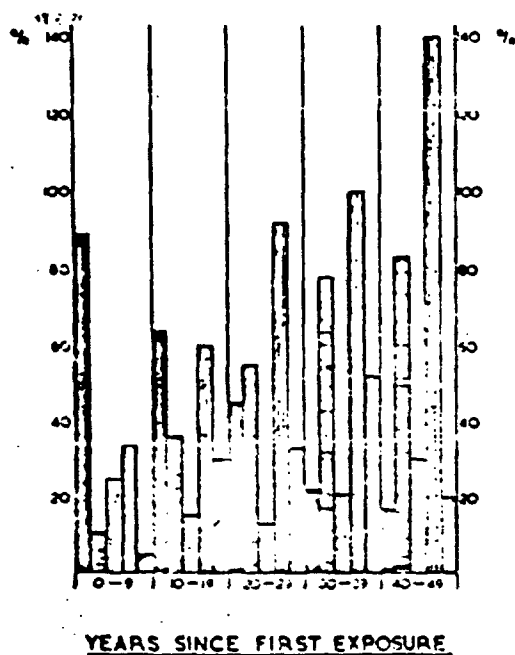


Figure 2

The percentage incidence of normal and abnormal X-ray in men related to 10 year exposure groups. (The incidence of pleural thickening has been calculated by using all the regional distributions of thickening irrespective of whether more than one area is involved in an individual. The number of pleural areas exceeds the number of subjects resulting in the high percentage figures.) (PI-Pleural thickening)

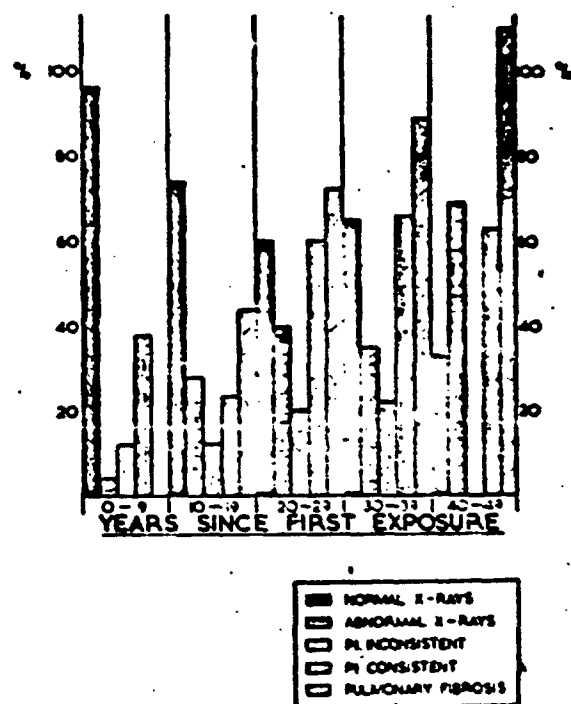


Figure 3

The percentage incidence of normal and abnormal X-rays in women related to 10 year exposure groups. (PI-Pleural thickening)

8/12/73

5. Explanation is possible. The 1966 x-rays exist in the files of the Turner Bros. Asbestos Co., as do the 1970 x-rays. Both sets of films can be reexamined and re-appraised. This is what many of us assumed to have been promised by Dr. Holmes when he said in Lyon on Oct. 2, 1972,

"Thus the information, although the best available at the time, was, to say the least scanty for the purpose and some of us who were associated with it have become increasingly concerned with the authority with which it has become invested in the international field. The time has therefore probably arrived for an up-to-date re-appraisal of the original data and the study of new data which has since become available elsewhere."

It may turn out that reappraisal of the x-rays will prove Dr. Knox to have been correct and that the figures published by you do not mean what they seem to mean. It may result in the opposite: that Dr. Knox's evaluations were in error. Perhaps you were both correct, and differences are more apparent than real. Without reappraisal, one cannot be sure.

I am confident that skilled independent British experts will be able to provide an accurate answer to this unresolved question.

6. The reappraisal is long overdue for another reason. The standard derived from the data made available by the Turner Bros. Asbestos Co. Ltd. refers only to prevention of asbestosis. The BOHS committee, on which Dr. Knox and Dr. Holmes served, stated in its 1968 report that "...the quantitative relationship between asbestos and cancer risk is not known...consequently it is not possible, at this time to specify an air concentration which is known will be free of risk in this respect." (page 54).

Unfortunately, asbestosis is only one serious hazard of asbestos workers. In some cohorts approximately 80% of excess deaths have been found due to one or another type of asbestos-associated cancer. Such cancers, as mesothelioma, can occur at dust exposure levels below those which result in asbestosis.

This unhappy omission of cancer from consideration in the setting of asbestos exposure standards, makes it all the more necessary to be diligent and prudent in evaluating data related to the establishment of such standards.

Sincerely yours,

Irving J. Selikoff, M.D.
Professor

IJS:vr

8.13.1973

Memorandum: Review of letter of Dr. H.C. Lewinsohn
and Dr. S. Holmes to Dr. I.J. Selikoff, July 12, 1973.

Documents

- A. Dr. Selikoff's remarks at the American Industrial Hygiene Conference, May 21, 1973, as recorded and transcribed by undisclosed sources and submitted by Drs. Lewinsohn and Holmes.
- B. Committee on Hygiene Standards of the British Occupational Hygiene Society. Ann. Occup. Hyg. 11:47-69, 1968.
- C. H.C. Lewinsohn. The Medical Surveillance of Asbestos Workers. Roy. Soc. Health J. 92:69-77, 1972.
- D. G. Berry. Hygiene Standards -- Theory and Application. Lyon, Oct. 2, 1972, Paper 22.
- E. S. Holmes. Criteria for Environmental Data and Bases of Threshold Limit Values -- Environmental Data in Industry. Lyon, Oct. 2, 1972, Paper 20.

Comments (reference to numbered paragraphs in Lewinsohn-Holmes letter).

- 1(a) The paper "...was not intended to justify or verify the BOHS standard." Nowhere did I say it was so intended.
- 1(b) "The results presented in HCL's paper were preliminary results."

It is possible that you will have further findings but at the time of my presentation only those were available that you had published, and defined in your paper as follows:

"Figures 2 and 3 illustrate the incidence of radiological changes, in ten years exposure groups, in 970 males and 317 females exposed to asbestos. In the previous report⁵⁶ it was shown that radiological abnormality was dependent upon exposure, age and smoking habits but that exposure appeared to be the most important single factor." (Document C, pg. 74.)
- 1(c) You argue that it was clearly stated in your Discussion that "From the information currently available only tentative opinions can be expressed." I fail to see the relevance of the implied complaint. Nowhere did I quote your opinions. I am interested only in your data.

- 2(a) You state that the populations reported by Knox and HCL were not identical nor were "the very large majority" "common to both surveys." I would be interested in data to support this assertion, with reference to the employment categories specifically referred to by me in my presentation, i.e., those 10-19, 20-29 and 30-39 years from onset, and reproduced in my tables [see 3(a)]. These were the groups in which radiological changes were seen, and to which reference was made. (Document A, pp. 10-11.)

The thrust of my discussion, and the contrast to which I pointed, was on the individuals x-rayed by Knox (Document B, p. 55) in 1966. All of these individuals were at least 10 years from onset and, unless no longer working at Turner Bros., were in the 1970 groups x-rayed by you. Do you contend they were not? If so, please explain.

I made no issue of individuals 40+ years from onset, nor were they included in the graph (see 5) since they had begun work before 1933, and were not included in Dr. Knox's report.

- 2(b) Insofar as you would have preferred that I had designated the additional individuals x-rayed by you as "quality control and research staff" rather than "office workers" I can only plead the shorthand of an extemporaneous presentation designed to fit a limited time allotment. Incidentally, my experience with U.S. research staffs is that they do spend a good deal of time in offices.
- 3(a) You complain that I stated that Knox reported on 913 men with at least 10 years exposure. You misquote me. My exact words were: "Dr. Knox had examined only men in the scheduled areas — that is, the production areas, 913—, whereas Dr. Lewinsohn included all employees...." (Document A, p. 10.) The figure 913 was given to compare a production area group with the larger population (1,287) x-rayed by you. No statement was made by me that such a number was in the 10 year or over groups. (Document A, p. 8.)

Indeed, my references to Dr. Knox's findings were specific and exact, since I showed a slide which presented the precise data contained in the tables in the paper published in 1968 (Document B, p. 51) and attributed to Dr. Knox (Document B, p. 54).

Group 1			
<u>Years employed</u>	<u>Number exposed</u>	<u>fibres/cm³</u>	<u>X-Ray changes</u>
10-	58	11	0
15-	72	11	1
20-	29	to	2
25-	46		3
30-	6	27	1
	211		7

Group 2

<u>Years employed</u>	<u>Number exposed</u>	<u>fibers/cm³</u>	<u>X-Ray changes</u>
10-	22	3.5	0
15-	20	3.5	0
20-	10	to	0
25-	20		1
30-	<u>7</u>	6.0	<u>0</u>
	70		1

The tables from which this slide was abstracted follow:

Hygiene standards for chrysotile asbestos dust

Group 1

<u>Years employed</u>	<u>Number exposed</u>	<u>Mean dust concentration</u>		<u>Number affected</u>		
		<u>particles/cm³ (see Note 1)</u>	<u>fibres/cm³ (see Note 2)</u>	<u>Rhonchi</u>	<u>Basal rates</u>	<u>X-ray changes</u>
10-	53	320	11	5	1	0
15-	72	390	11	11	1	1
20-	29	to	to	7	4	2
25-	46			14	8	3
30-	6	940	27	2	1	1

Group 2

<u>Years employed</u>	<u>Number exposed</u>	<u>Mean dust concentration</u>		<u>Number affected</u>		
		<u>particles/cm³ (see Note 1)</u>	<u>fibres/cm³ (see Note 2)</u>	<u>Rhonchi</u>	<u>Basal rates</u>	<u>X-ray changes</u>
10-	22	163	3.5	1	0	0
15-	20	163	3.5	3	0	0
20-	10	to	to	2	0	0
25-	20			1	1	1
30-	7	290	6.0	3	0	0

My exact extemporaneous comments were: "In the departments where the dust counts were 10 to 15 fibers per cc, of the large number of workers there employed, 7 showed some x-ray abnormality which could have been asbestosis. On the other hand, of 70 or 80 people working at 3 to 4 fibers per cc, only one was reported to have an abnormal x-ray." (Document A, p. 8.)

The designations "10 to 15 fibers per cc" and "3 to 4 fibers per cc" were derived from Mr. Berry's table in 1972 (Document D, p. 11). He gives them as 10.9 to 15.2 fibers/cm³ and 3.4 to 4.1 fibers/cm³. I believe my reference is accurate.

3(b) My use of the phrase "...could have been asbestosis" (Document A, p. 8) was based upon the column heading in the 1968 paper (Document B, Tables 1 and 2, p. 55), "X-Ray changes possibly asbestotic," and therefore seems entirely faithful.

3(c) You will note, too, that my tables listed 290 men with more and than 10 years, exactly as reported in 1968.

4(a)

These were compared with the results in those workers x-rayed by you in the same general employment categories, i.e., "20-29 years, 30-39 years, 40+ years" from first employment." This is admittedly not absolutely identical with your predecessor Dr. Knox's "years employed" but, if anything, your employment categories ("years from first employment") could not have reflected more than Dr. Knox's ("years employed"). It could have been less (if some workers had periods out of work), but not more. Therefore, if anything, your x-rays should have shown less disease than Dr. Knox's, by employment category.

4(b) You complain that the term "asbestosis" was used on the graph. This word was used by Knox while you used the term "pulmonary fibrosis." We therefore had the option of using either. Perhaps you might advise me if you think I was incorrect in using Dr. Knox's "asbestosis," and if you believe it is stretching things to so label your "pulmonary fibrosis," when you describe such x-ray findings in workers employed in an asbestos factory for 20, 30, 40 or more years. I seem to remember an old English saying, to the effect that circumstantial evidence is very strong, as when one finds fish in the milk.

5 You say that you did not attempt to calculate fiber years/cc. I did not say you had. I did show a graph prepared by Dr. William J. Nicholson of our Laboratory comparing your x-ray findings with Dr. Knox's using an overall average cumulative exposure for the entire population. The exposure experience was not taken out of thin air but was derived from the very complete Turner Bros. dust data analysis presented by Mr. G. Berry in Lyon on October 2, 1972 (Document D). These data were given as the latest available. Unfortunately, you did not present comparable results in your own paper -- we would have liked to have seen them.

The asbestos exposures used on the graph in my presentation were estimated as follows:

The dust levels reported by Berry (Document D, p. 11) and presented at Lyon served as the basis for estimating cumulative exposures prior to 6,30,66. To each of these integrated exposures was added 22.5 fibers/cc-years, which was obtained by averaging the dust level data listed in Table 4, page 57,

of Document B for the fibroizing, carding and spinning departments and multiplying by 4.5 years. Ten year average exposures were obtained by averaging the data in appropriate 5 year categories, weighted by the number of men working in 1966. Because we had no way of associating observed x-ray changes with individuals in high and low 10 year exposure categories and in order to be as conservative as possible, the dust exposures assigned to groups of workers were those of the highest exposed workers although 1/3 to 1/2 of the individuals represented by each point were exposed at levels 1/3 less.

The estimates of the number of individuals affected were also as conservative as possible. All questionable parenchymal x-ray changes and all pleural changes not consistent with asbestosis were considered as negative.

As can be seen by comparing the graph with your figures 2 and 3, our conservative approach has resulted in the graph showing less x-ray abnormality than do your figures.

I would appreciate either reworked or new dust data to go with your x-ray results and will then prepare another graph. In the meantime, we will have to use dust data presented from Turner Bros. by Dr. Holmes in 1968 (Document B, p. 57) and by Mr. Berry in 1972 (Document D).

6. Since Dr. Knox included in his recorded x-ray changes pleural abnormalities as well as parenchymal disease, it may well be that there were even fewer than 8 cases, of those he reported, with only pulmonary fibrosis. The 1968 paper does not make this clear. If this were so, then the contrast with your 1970 "pulmonary fibrosis" category would be even greater and the discrepancy even more puzzling, since we compared all of Dr. Knox's "possibly asbestotic" x-ray changes (Document B, p. 55) with your more limited category "pulmonary fibrosis."

A further point is in order. The BOHS paper in 1963 was concerned with earliest evidence of disease resulting from asbestos exposure. (Document B, p. 50.) Indeed, it deemed it advantageous to so consider basal rates since these could occur before x-ray change. You state you used "the U.I.C.C./ILO classification" (mislabeled; it is the ILO U/C classification!) to analyse your radiological findings. Your appropriate use of this international x-ray classification system rather than a personal one has surely allowed you to approach the goal of the BOHS committee, the use of "the earliest demonstrable effects on the lung due to asbestos," as the criterion for "asbestosis." I believe your recorded x-ray findings (Document C, Figures 2 and 3, p. 74) are likely to be accurate, and the reported presence of parenchymal and pleural abnormalities "consistent with" asbestosis to reflect what actually exists among workers at the Turner Bros. Asbestos Co., Ltd.

7. Basal rates. I made no reference to basal rates since your paper did not report on these (Document C, pp. 74-75) and

comparison of 1966-Knox and 1970-Levinsohn was thus not possible. Parenthetically, neither did I compare pulmonary function findings, which were reported by you but not by Knox (Document C, pp. 75-76).

- 8(a) Did you have any cases recorded by you as "pulmonary fibrosis" also see which did not have characteristic asbestotic interstitial
4(b) fibrosis if they later came to post-mortem?

- 8(b) You state that I ignored "...the fact that the number of cases of asbestosis in 1968 among 335 employees (men and women) exposed for 10 years or more since 1933 in Rochdale and still employed, was 1.12% (i.e., 4 cases) as compared with Merewether's finding of 44.4% (i.e., 59 cases) among 133 workers exposed 10 years or more in the same industry in 1929. This lower incidence has occurred in spite of the fact that the Pneumoconiosis Medical Panels are better equipped to diagnose asbestosis than Merewether was."

I charitably made no reference to this observation of yours (contained in Document C, Table II, p. 71), since it was such an obvious error: indeed, I regret that you have again called attention to it. The Pneumoconiosis Panel cases were new cases first diagnosed in 1968; Merewether's data gave the results of a study of the prevalence of asbestosis among a group of workers in 1929. One should not confuse annual incidence of a disease, with its prevalence. (Your Table II follows.)

TABLE II

New cases of Asbestosis (still employed) diagnosed in 1968 (with no employment prior to 1.1.1933) compared with the data of Bamblin²⁰ and Merewether¹⁹.

PERSONNEL 1968				1968		CASES OF ASBESTOSIS 1957		1930
No. of years employed	Men	Women	Total	No. of cases	Group Incidence %	No. of cases	Group Incidence %	Group Incidence %
10-14	93	49	139	1	0.72	1	0.44	33.1
15-19	86	24	110	1	0.91	1	0.71	33.6
20-24 & over	58	28	86	2	2.30	1	1.00	80.9
TOTAL	234	101	335	4	1.12	3	0.90	26.2

You voice displeasure that the contents of a "private correspondence" were communicated. I hardly think that a paper published in the Royal Society of Health Journal is "private correspondence." I requested that you send me numbers to translate the figures and you did; my references were then made only to what you reported in your published paper. You did send me new data (concerning pulmonary function abnormalities and relation of x-ray abnormalities to smoking that were given in a paper by you at Salford) but I made no reference to these data since, while I had them and while you had made bibliographical reference to them (Document C, Reference 30, p. 77), they had not yet appeared in print and others could therefore not readily study them.

You suggest that the information is being used "to launch what seems to be a political campaign in the U.S.A. aimed at discrediting our Hygiene Standard for asbestos." I note you sent your letter to the Asbestos Information Association of North America. In contrast, my analysis was given in an invited paper to a scientific society (the A.C.G.I.H.), before my peers, and available for discussion and for presentation of alternate views, if such be held.