TL116040444

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. 3, 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 ECL'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 report[3] it was found 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 was interested only in your data.

[redacted]

ENVIRONMENTAL SCIENCES LABORATORY [redacted]

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, bore 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).

Years employed	Number exposed	Fibres/cc	X-Ray changes
10-	86	11	0
15-	72	11	1
20-	29		3
25-	46	to	3
30-	6	27	1
	211		7

Group 2

Years employed	Number exposed	fibers/cm^3	X-Ray changes
10-	22	3.5	0
15-	20	3.5	[illegible]
20-	10		0
25-	20	to	1
30-	7	6.0	0
	70		1

The tables from which this slide was abstracted follow:

Hygiene standards for chrysotile asbestos dust

Group 1

Years employed	Number exposed	Mean dust concentration		Number affected		
		particles/cm^3 (see Note 1)	fibres/cm^3 (see Note 2)	Rhonchi	Basal rales	X-ray changes
20—	28	350	11	8	1	0
15—	22	350	11	11	1	1
20—	20	to	to	7	4	2
25—	44	to	to	14	6	3
30—	6	900	27	2	1	1

Group 2

Years employed	Number exposed	Mean dust concentration		Number affected		
		particles/cm^3 (see Note 1)	fibres/cm^3 (see Note 2)	Rhonchi	Basal rales	X-ray changes
[illegible]	22	[illegible]	[illegible]	1	0	0
[illegible]	20	[illegible]	[illegible]	3	0	0
[illegible]	10	to	to	2	0	0
[illegible]	20	to	to	1	1	1
[illegible]	7	[illegible]	[illegible]	1	0	0

My other contemporaneous comments were: "In the departments where the dust counts were 15 to 35 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 20 yr 20 people working at 3 to 4 fibers per cc, only one was reported to have an abnormal x-ray." (Document A, p. 6.)

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/cm^3 and 3.4 to 4.1 fibers/cm^3. 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) and 4(a) You will note, too, that my tables listed 290 men with more than 10 years, exactly as reported in 1968.

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. C. 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 6.30.68. 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 fiberizing, 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 <u>even fewer</u> than 8 cases, of those he reported, with <u>only</u> pulmonary fibrosis. The 1968 paper does not make this clear. If this were so, then the <u>contrast</u> with your 1970 "pulmonary fibrosis" category would be even greater and the discrepency 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 1968 was concerned with earliest evidence of disease resulting from asbestos exposure. (Document B, p. 50.) Indeed, it deemed it advantageous to so consider basal rales 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 rales. I made no reference to basal rales since your paper did not report on these (Document C, pp. 74-75) and

comparison of 1966-Knox and 1970-Lewinsohn 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) also see 4(b) Did you have any cases recorded by you as "pulmonary fibrosis" which did not have characteristic asbestotic interstitial 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 Rochdale and Merewether[4].

	Pneumoconiosis 1968			Cases of Asbestosis				
				1968		1937		1929
No. of years employed	Men	Women	Total	No. of cases	Group Incidence %	No. of cases	Group Incidence %	Group Incidence %
10-14	98	40	138	1	0·72	1	0·54	33·1
15-19	86	24	110	1	0·91	1	0·71	33·6
20-24								
25 over	38	20	86	2	3·33	1	3·03	80·9
TOTAL	224	111	335	4	1·12	3	0·89	36·2

You voice displeasure that the contents of a "private correspondence" I 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 [illegible] figures and you did; my references were then made only to [illegible] your published paper. You did send me new data (comparing pulmonary function abnormalities and radiological [illegible] findings) 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 56, 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.

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Professor Irving J.Selikoff,
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Dear Professor Selikoff,

We have recently received a transcript of a tape recording of your address to the American Industrial Hygiene Conference in Boston on 21st May,1973 which was sent to us because of its references to our work. If the copy enclosed herewith is a true representation of that address and the graph shown by you, we must tell you that we find your assumptions and conclusions to be extremely misleading.

The case made at the end of your paper appears to rest on the fact that the data which formed the basis of our Hygiene Standard[1] was scanty. You then attempt to extract data from the Figures in HCL's publication[2] in order to show a discrepancy between the original data and these Figures. We are astonished at your having communicated the contents of a private correspondence to the meeting which you addressed without having obtained our agreement. We are also very displeased at the manner in which you have used the information we provided in order to launch what seems to us to be a political campaign in the USA aimed at discrediting our Hygiene Standard for asbestos. We cannot accept the implication that the HCL publication allows doubt to be cast on the validity of the BOHS Standard and are of the opinion that such a conclusion is unwarranted.

Comparisons such as those you have made between the Knox data and the HCL data are invalid for the following reasons :-

1. The paper by HCL was not intended to justify or verify the BOHS Standard, and no reference is made in this paper to environmental dust measurements. The results presented in HCL's paper were preliminary results and in the discussion section it was clearly stated that 'from the information currently available only tentative opinions can be expressed '. We are

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surprised therefore that you have been able to read far more into the incomplete picture presented than the author was able to do or even intended to do.

2. The populations reported by Knox and HCL were not identical nor were 'the very large majority' of asbestos workers 'common to both surveys', as you suggested. 'Office personnel' were not included. The population studied by HCL consisted of workers in the recognised asbestos areas, together with maintenance, quality control and research staff.

3. Knox reported on 290 men (not 913 as stated by you) with at least 10 years exposure since 1st January,1933, when the Asbestos Industry Regulations 1931 took effect, and who were still in the Company's employ on the 30th June,1966. HCL on the other hand has analysed the X-rays of a much larger population (1,287) as of November,1970 irrespective of date of first employment; some were in fact first employed before 1st January,1933.

4. Knox sub-divided his population according to actual cumulative exposure whereas HCL's was sub-divided by decades elapsed since first exposure, irrespective of the actual number of years exposed. In view of these basic differences and others which we will point out in due course, we fail to see how you derived the relationship between cumulative exposure and percentage of individuals affected on the graph which you showed (copy attached). The heading on the graph is obviously misleading as HCL at no stage in his report equated the radiological changes with 'asbestosis'.

5. Because it was possible to estimate dust exposures for the Knox population, the BOHS Sub-Committee were able to arrive at figures for cumulative exposure expressed in fibre years/cc. No such exercise was ever attempted by HCL. This is a further invalidation of the graph since you have attempted to relate radiological features described by HCL to what we can only regard as speculative exposure data.

6. Knox only recorded X-ray changes as being present which are possibly asbestotic, after taking the whole clinical picture into consideration. 'Radiological changes considered significant included those of increased general opacity of the lower lobes, blurring of the cardiac outline, pleural thickening and adhesions'.[1]

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HCL on the other hand attempted to analyse his radiological findings according to the UICC/ILO classification, an entirely different set of criteria, and took no account of the clinical picture as a whole.

7. The BOHS sub-committee decided that basal rales were the 'key symptom' since all the men with X-ray changes, by the Knox criteria, had basal rales, but some with basal rales had no X-ray changes.[1] HCL points out in his paper that he has not yet attempted to correlate physical signs and X-ray changes.

8. We are obliged to point out that the use of the words 'pulmonary fibrosis' by HCL in his paper may have misled you, as they were used to describe small opacities which prior to the advent of the UICC/ILO classification may have been regarded as consistent with fibrotic changes. The use of this term does not allow for sub-division into profusion and type of opacity as is now possible with the UICC/ILO classification. For simplicity in presentation, HCL grouped all cases showing evidence of these changes together, irrespective of severity. Knox only included X-ray changes consistent with the clinical diagnosis of asbestosis. It is not possible to say, from the data that HCL has compiled, how many persons would fulfil the criteria for a clinical diagnosis of asbestosis, and to attempt to illustrate these two very different sets of data on the same graph is simply not permissible. The clinical significance of the radiological changes observed cannot as yet be defined.

You appear to have ignored completely 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)[2] 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.[3] This lower incidence has occurred in spite of the fact that the Pneumoconiosis Medical Panels are better equipped to diagnose asbestosis than Merewether was. In addition, during the years immediately following 1933, there still was substantially greater exposure in certain jobs than in more recent times.

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In view of your comments and our previous correspondence we should like to remind you that we are in the process of reviewing and bringing up to date all the available data, and hope to present it for publication in the near future.

Dr.Holmes' remarks at Lyon were never intended to suggest that the BOHS Standard was not stringent enough. The BOHS Standard is 100 fibre years/cc and therefore application of a 2 f/cc limit for periods of less than 50 years has the effect of providing an ample safety margin. The BOHS Sub-committee, of which we are both members, recently reviewed the Hygiene Standards for Chrysotile Asbestos in the light of the latest available data and recommended that no change be made at the present time.

We are forwarding a copy of this letter to the President of the American Industrial Hygiene Association, the Chairman of the ACGIH, the Director of NIOSH and also the Asbestos Information Association of North America in view of their obvious interest in this important subject.

Yours sincerely,

H.C.LEWINSOHN, M.B. B.Ch. D.I.H.
Chief Medical Officer.

S.HOLMES,
Chief Health Physicist.

Encl:

REFERENCES

1. Hygiene Standards for Chrysotile Asbestos Dust - Annals of Occupational Hygiene 1968.

2. The Medical Surveillance of Asbestos Workers - Royal Society of Health Journal, April 1972.

3. Report on effects of asbestos dust on the lungs and dust suppression in the asbestos industry - Merewether & Price. HMSO 1930.

4. Annals of Occupational Hygiene Vol.16, 1973.