File for Borcina
H+S.

INTERNATIONAL LEAD ZINC RESEARCH ORGANIZATION, INC.

292 MADISON AVENUE, NEW YORK, N.Y. 10017
TELEPHONE 532-0575 (AREA CODE 212)
CABLE ADDRESS INTLEAD NEW YORK

SUBJECT TO REVISION

LEAD ENVIRONMENTAL HEALTH COMMITTEE SPECIAL MEETING
MINUTES

ILZRO Conference Room - 19th Floor
292 Madison Ave.
New York City

July 6, 1967

The meeting was called to order at 10:00 a.m. by the Chairman, Mr. J.C. Sevick.

PRESENT	REPRESENTING
J.C. Sevick	St. Joseph Lead Company
H. Hesselberg	Ethyl Corporation
W.M. Fallies	ESB Inc.
A. Kurayama	Mitsui Mining & Smelting Co., Ltd.
G.J. Stopps	E.I du Pont de Nemours & Co.
E.J. Cay	The Associated Octel Co., Ltd.
F.L. Warner	U.S. Smelting Refining & Mining Co.

STAFF

D.M. Borcina, Secretary Treasurer, L.I.A.
D.S. Carr, Project Manager, Chemistry, ILZRO
J.C. Roumas, Assistant Director, Health and Safety, L.I.A.

GUESTS

C. Thompson	Hill and Knowlton
L. Zahn	Hill and Knowlton

Agenda Item No. 1 - Project LH-115 "Surveillance of Population Lead Levels"

The staff had forwarded prior by mail prior to the meeting copies of (1) the minutes of the May 31, 1967 meeting of the Subcommittee on Population Lead Levels of the Lead Liaison Committee, together with copies of a letter from Professor Vigliani of the University of Milan advising that he is interested in doing work in Milan along the lines of the proposed new six-cities study, and (2) copies of a letter from Dr. Horton of the USPHS to Dr. Radford of Kettering Laboratory inviting him to submit a preliminary research proposal for presentation at a meeting of the Lead Liaison Committee scheduled for July 18, 1967. Copies of Dr. Radford's reply, indicating that Kettering will be pleased to submit research proposals at the July 18 meeting, were distributed at the meeting.

Mr. Hesselberg of the Ethyl Corporation recommended that Ethyl and du Pont personnel be appointed to serve with Mr. Fowler as ILZRO representatives to the Working Group responsible for coordinating the proposed study. It was stated that, in view of the fact that Ethyl and du Pont personnel were involved in the original "Three Cities" study, their experience would be of invaluable assistance in coordinating activities on the new study.

Following discussion, the Committee members agreed that Mr. Fowler should submit to the Lead Liaison Committee, at its July 18 meeting, the names of Ethyl and du Pont personnel who should be appointed to serve with him as ILZRO representatives to the proposed Working Group. It was further decided that in view of the fact that Mr. Fowler is out of the country on a business trip and will not return until July 17, a meeting should be held in Washington on that date between the Ethyl and du Pont representatives and the staff representatives.

Agenda Item No. 2 - Project LH-109 "Study of the Relationship Between the Lead from Motor Vehicle Vehicle Exhaust and that in Consumer Crops"

The staff distributed copies of the budget proposal submitted by the Statewide Air Pollution Research Center, University of California at Riverside for continuation of this work for the second year.

It was noted that a mobile field sampling station was equipped and tested during the past year, and that the contractor is searching for the best methods of plant sampling and analysis.

Following discussion, the Committee members present agreed unanimously that this project should be continued for the second year. Mr. Hesselberg of Ethyl advised that the TEL producers are interested in co-sponsoring a continuation of this work for the second year, on a 50/50 split basis as for the first year. The staff was requested to distribute copies of the contractor's proposal to the total committee membership, asking for a vote on this project.

Mr. Thompson of Hill & Knowlton referred to the paper "The Lead Content of Pasture Herbage" from the Macauley Institute in Aberdeen, Scotland and suggested that it be brought to the attention of all research people who are working on checking lead content of any growing matter along highways.

Agenda Item No. 3 - Other Business

a. Project LH-79 "Toxicity Testing of Organolead Compounds"

The staff distributed copies of summary tables containing health and safety properties of 17 organolead compounds on which tests have been completed.

b. Catalytic Afterburners for Auto Exhaust - The staff reported that the ILZRO Lead Metallurgical Committee had recently discussed the steps being taken by the State of California to reduce and possibly eliminate lead additives from gasoline, and had requested guidance as to whether or not ILZRO should become involved in research on exhaust treatment devices which would not be affected by lead compounds.

Mr. Hesselberg of Ethyl pointed out that catalytic exhaust devices are not currently in use. He pointed out that there is no legislation anticipated through 1970 that will require catalytic cracking units, and that the automobile manufacturers prefer not to employ catalytic devices because of their complexity and expense. Instead, they are developing modified engine exhaust systems and air injection systems which are more economical exhaust control devices for meeting federal requirements at a small cost to the consumer.

Mr. Gay of Associated Octel agreed with these comments and stated further that the automobile industry prefers to modify the engine rather than to hang on another device which would substantially increase the cost of the automobile.

Following discussion, it was agreed by the Committee members that ILZRO should not become involved in research on catalytic cracking units as there is enough other research on exhaust control systems currently being carried on. It was the consensus of the Committee that Mr. Hesselberg should review this subject with the ILZRO Lead Metallurgical Committee.

c. The following news item and articles were discussed:
1) News item from the June 14, 1967 issue of Detroit, Michigan Free Press titled "Fallout from Car Fumes on the Rise" - reporting on a paper read by C. Patterson at the annual meeting of the Air Pollution Control Association, and containing erroneous statements on lead in the atmosphere by Dr. J.P. Lodge of the UNMS. The staff reported that a letter had been written to Dr. Lodge asking him to provide further information on the statements attributed to him. (note: For your information, a copy of Dr. Lodge's reply is attached.)

2) Article from June, 1967 issue of Wire and Wire Products, titled "Prevention of Lead Poisoning in the Patenting Shops" which contained erroneous information concerning preventive action. The author will be appraised of appropriate authoritative information on this subject.

3) Letter to the Editor in June 10, 1967 issue of the British Medical Journal stating that an article on the subject of lead absorption in children which appeared in the May, 1967 issue is misleading. Copies of the article in question, which had been distributed to all Committee members on June 29, 1967, reported on the results of a study that showed no evidence of a relationship between mentally defective children and high blood lead levels in children. The committee felt that the letter to the Editor, a copy of which is attached, does not refute the article.

There being no further business, the meeting was adjourned at 12:15 p.m.

James C. Roumas, Recording Secretary
Health and Safety

JCR/jv
Attachments (2)

NATIONAL CENTER FOR ATMOSPHERIC RESEARCH

LABORATORY OF ATMOSPHERIC SCIENCES
BOULDER, COLORADO

Telephone
303-444-3151

Post Office
Box 1470

19 July 1967

Dr. James C. Roumas
Assistant Director, Health and Safety
Lead Industries Association, Inc.
292 Madison Avenue
New York, New York 10017

JUL 20 1967

Dear Jim:

My apologies for the delay in replying to your letter of 19 June. In all honesty, it became TLIF; I have been gone much of the time, and things have pyramided horribly here.

As you can imagine, I was a little shocked at the clipping from the Detroit Free Press. At first, I wondered if they had talked to someone else, then I realized what had happened. The item that resulted was the product of the usual process of removing all of the hedges, qualifiers, disclaimers, etc. I did say, certainly, that a small fraction of the population had serious environmental exposures--a statement which I think you cannot dispute. I then stated that some people were of the opinion that the kind of latent injury described could occur. When pressed for an idea of how prevalent it might be, I said, more or less, that I had no idea since there were no data available, but that if in fact it did exist it conceivably ran as high as 10 percent of the population, since that many people are exposed to substantial lead concentrations from time to time. I stated, however, that this was a number plucked out of thin air and a pure supposition on my part. I rather think that if the phenomenon exists it could be that prevalent, but I certainly did not present it as a scientific conclusion.

I can see that the result gave you boys a bad day. This was unintentional although it is my private opinion that the environment would be greatly improved if other uses than gasoline additives could be found for our lead production.

Sincerely yours,

James P. Lodge
Staff Chemist

Operated by the University Corporation for Atmospheric Research
Under Contract with the National Science Foundation

analysed completely. Nevertheless, the initial results in these prophylactic trials do not appear to support the totally negative view which arises from the experimental work on the therapeutic effect of ascorbic acid which has been reported from Salisbury. These results will be described when the analysis has been completed.—I am, etc.,

C. W. M. WILSON.

Department of Pharmacology,
University of Dublin,
Dublin.

Lead Absorption in Children

SIR,—The indiscriminate use of the terms absorption, exposure, intoxication, and poisoning by Dr. Neil Gordon and his colleagues (20 May, p. 480) is misleading. They measured only blood lead concentration and inferred that it reflected absorption.

The blood lead concentration represents an equilibrium in which absorption, excretion, and tissue deposition participate. Recent animal studies in this department have shown that lead can accumulate in the liver following oral administration of lead compounds without significant change in the blood lead. Conversely, while the significance of a raised blood lead is not disputed, it is not necessarily indicative of absorption but could represent release of tissue lead.

Lead poisoning in childhood usually occurs between the ages of 1 and 3 years; death and cerebral damage are well-documented sequelae.[1] It is questionable whether the measurements reported by Gordon and others in older children and adolescents are relevant in any way to the aetiology of mental handicap.—I am, etc.,

D. BARLTROP.

Paediatric Unit,
St. Mary's Hospital
Medical School,
London W.2.

REFERENCE

[1] Byers, R. K., Pediatrics, 1959, 23, 585.

Treatment of Choriocarcinoma

SIR,—Dr. K. D. Bagshawe (15 April, p. 170) appears unduly concerned that our paper (4 March, p. 321) may have weakened the case for the treatment of choriocarcinoma in specialized centres such as his at Fulham Hospital. We wish to assure him that we have the highest regard for his good work, and, although we do not support the view that all cases of choriocarcinoma should be treated in specialized centres, we do agree that difficult problem cases should be channelled without delay to such centres for chemotherapy by experts like himself.

In his eagerness to defend his position Bagshawe has made certain statements which require examination. In the opening paragraph he says: "Before chemotherapy was applied to choriocarcinoma few clinicians had seen more than a few cases." This categorical statement may have been true of clinicians in Britain, but not in Asia. Before chemotherapy was introduced into our hospital we had seen two dozen cases or more, and there must be other clinicians in Hong Kong, Indonesia, the Philippines, and other parts of Asia who have seen many more. Such inaccurate statements do not strengthen the cause which Bagshawe is trying to promote.

Two papers on choriocarcinoma from Singapore have recently appeared.[1,2] Bagshawe compared the figures and concluded: "The virtually simultaneous presentation of totally opposite conclusions, drawn from the same data, must cast doubt on their value." This hasty conclusion ignores the fact that the first paper[1] was on pulmonary choriocarcinoma and the second[2] was on all types of choriocarcinoma. Our second paper also included more recent material than that reported in the first, although the duration of follow-up was up to 1966 in both presentations. There is no contradiction in our conclusions or discrepancy in our figures. Neither was there recourse to "spontaneous resurrection," as Bagshawe terms it. The careful reader will find that Bagshawe's assumption is in error. The deaths in the "hysterectomy-chemotherapy" group were 12 in both papers, with one case under treatment. Bagshawe repeatedly refers to our cases of malignant trophoblastic tumours as "hydatidiform mole." He may disagree with our classification, but there is no need to misrepresent the facts.

We find it difficult to understand the self-contradiction in Bagshawe's thinking on the value of hysterectomy. To quote him: "The reader is led to believe that some workers have claimed that hysterectomy is valueless . . . Such claims have not, I think, been made. . . ." Here he appears to hold no bias against hysterectomy. Then, in the second last paragraph he describes at length the evils of elective hysterectomy and concludes, "unless the uterus has perforated, hysterectomy is better deferred." Uterine perforation is a rare complication in choriocarcinoma. This means that Bagshawe opposes hysterectomy in the majority of cases. In his paper,[3] which we had referred to, Bagshawe also elaborated on the alleged evils of hysterectomy and concluded: "Hysterectomy is thus not only ineffective but also disadvantageous in some patients." Let his own statements be his judge. In our paper we had advocated that such clinics "deserve careful study before hysterectomy is abandoned altogether." Indeed, unless a strong pattern were found these claims (based on flimsy evidence) would mislead gynaecologists into giving up hysterectomy altogether.

In our paper we have not claimed that our "results equal those obtained elsewhere," as Bagshawe asserts. We have reported the results of all 33 consecutive cases of malignant trophoblastic tumours seen, treated, or both, irrespective of the stage on admission to hospital. We have included no fewer than 11 deaths in patients who did not have the benefit of chemotherapy. Bagshawe's results were based on 23 treated cases out of 28 seen. The hopeless cases were excluded from his results. Obviously no worthwhile conclusions could have been drawn by trying to compare our results with his.

Bagshawe places great faith in the Joint Project[4] which assessed "616 cases of suspected trophoblastic tumours" from 18 different centres in Asia over the period 1955-58 for study by a panel of pathologists in the U.S.A. The non-representative nature of this pooled material led the authors themselves to conclude that "no accurate rate of choriocarcinoma to hydatidiform mole cannot be established . . . and there is no way of determining whether the large number of choriocarcinomas is due in part to a large number of hydatidiform moles." Yet Bagshawe makes the [illegible] statement that the Joint Project "found no evidence that mole was a relatively more frequent antecedent to choriocarcinoma in South-East Asia than in the U.S.A." We are of the opinion that the value of the Joint Project is severely limited by the absurdly small number of cases studied, and out of a population of several hundred millions or more. Your readers might be interested to know that for the four-year study period of the Joint Project the Singapore participants contributed 41 cases and that these were rejected from the final analysis because of inadequate data. We have studied over 300 consecutive cases in the past seven years. Our results may be expected to give a more truly representative and accurate picture than the collation of hotch-potch data from 18 different sources.

We find Bagshawe's mathematics perplexing. In calculating the ratio of molar to non-molar pregnancies in the last 25 cases of our series he arrives at the figure of 12.5:1. We are at a loss how this figure was arrived at. He states that the "expected number of fatal cases without treatment" in our series would be "about 24." On what grounds? He also states that our "fatality rate is twice that of a somewhat larger series" from his own hospital. May we suggest that all these unfounded statements and self-contradictions do not strengthen the case for specialized centres and may weaken the faith of Bagshawe's supporters. Finally, may we reassure Bagshawe that we do not oppose the treatment of problem cases in his specialized unit, but we do strongly contest the unfair allegations which he has made against hysterectomy.—We are, etc.,

W. S. H. TOW.
W. C. CHENG.

Department of Obstetrics
and Gynaecology,
University of Singapore,
Singapore 3.

REFERENCES

[1] Tow, W. S. H., Proc. roy. Soc. Med., 1967, 60, 236.
[2] —— and Cheng, W. C., Brit. med. J., 1967, 1, 321.
[3] Bagshawe, K. D., Lancet, 1963, 2, 1103.
[4] Joint Project for Study of Choriocarcinoma and Hydatidiform Mole in Asia, Ann. N.Y. Acad. Sci., 1959, 80, 178.

Pressurized Aerosols in Asthma

SIR,—I am grateful for your published disclaimer (27 May, p. 580) that I do not support the views of Dr. R. Munro Ford (6 May, p. 373) or others of your correspondents who condemn the use of aerosol preparations in the treatment of bronchial asthma. Indeed, the agent which I find most useful and use most frequently in the treatment of chronic asthma is 1% isoprenaline delivered from a De Vilbiss No. 40 hand nebulizer. As with other medicaments, I take pains to explain clearly to the patient the way in which it should be used and its limitations.

I deplore the present "hue and cry" approach to the problem of deaths from asthma, and still more the emotional witch-hunt on one valuable form of therapy. A recent paper by Tai and myself[1] indicates how severe is the functional state of many patients with asthma who do not appear clinically very ill. This paper suggests that we should look afresh at our attitudes in the field of "expectations," rather than talk too glibly of "unexpected" deaths from asthma.—I am, etc.,

JOHN READ.

Department of Medicine,
University of Sydney,
Sydney, N.S.W.,
Australia.

REFERENCE

[1] Tai, E., and Read, J., Lancet, 1967, 1, 644.

SIR,—I have been following with great interest the correspondence in your columns concerning the excessive use of pressurized aerosols in asthma. Little has so far been written about the older asthmatic child in this context apart from the very instructive case described by Dr. W. Pickstone (25 March, p.