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TRANSCRIPT OF THE STUDY CONFERENCE ON OCCUPATIONAL HEALTH OF ASBESTOS WORKERS IN QUEBEC

QUEBEC ASBESTOS MINING ASSOCIATION

Quebec, Canada.

Antigua, B W.I.

November 1965

Tuesday, November 23rd, 1965

LINDELL:

Well, Gentlemen, we would like, first of all, for you to speak in English. We have stenotypists who will be able to take your words in either English or French and if you reach a point where you would rather make your point in French than in English, we would ask you to do so, but we would prefer that you make it in French and then translate it into English.

English seems to be the language common to most of you today. If Dr. Nor[illegible] and I started to speak Finnish, we would be the only ones that understood each other and that would be a little embarrassing for the rest of you.

So if you please, English if you will, and French if you like.

Since I have two doctors' degrees, one would say I have some authority to be here, but I am neither here as a doctor nor as a scientist. I speak only as an engineer with industrial health problems with which we would ask you to help us. We certainly appreciate all of you taking the time to come down here. Even if these are beautiful surroundings, we appreciate you taking the time to come down and join us on the study of the occupational health problems.

I am not going to go through all the newspaper clippings and all the discussions that have preceded this in the past year and caused this problem to become aggravated, shall we say, but I will say only this: that you have received the objective spelled out to you; that you are to define what is the problem. In order to find a solution, naturally one must discuss the disease, if there is any, and to make a diagnosis and then make a recommendation as to what should be done.

So you have the short-range problem; find an immediate reply to give to the public as to what is the truth; and second what has to be done on a long-term basis to put all the information together in order to know what are the right answers.

I will speak only on asbestos to clear up some of the unknown facts about this mineral and health.

Asbestos is a name given to a group of minerals, not one particular mineral. There are two major groups: pyroxene and amphibole asbestos, they are called.

The principal one that is produced in the world today, (in Russia, South Africa and Canada), mostly is chrysotile, but unfortunately even the term chrysotile is not enough to describe this fibre.

Chrysotile fibre is a very silky type. You have hard white chrysotile which has the cross fibre as well as the slip fibre

Now in California, in production, there are some 40,000 tons a year of platey chrysotile. That is a form that looks very much like oatmeal, if none of you have seen this. In Yugoslavia, this type of chrysotile has been in production for quite a few years.

Under an electronic microscope, all of these asbestos minerals look practically the same. The fibres are all hollow cylinders. Even the platey chrysotile of California, which looks like oatmeal, still under a microscope, looks like the other chrysotiles. It can be identified as chrysotile because under a microscope it looks like the other chrysotiles.

So if one were to study the various types or do research on asbestos, just saying that you would like to have samples from a certain company would not suffice because some companies have several kinds of asbestos, all of them chrysotile.

Then on top of that you have chrysotiles that occur in the serpentine rocks. They also occur in dolomitic rocks.

SABOURIN:

I didn't get the last word.

LINDELL:

Dolomites.

And there are some that are enveloped in carbonates also, but they are not of major consequence.

So then the asbestos that is produced will differ from the crude sample that we showed you of chrysotile from Asbestos, Quebec, because what is produced can be any one of the forms generally as I have here : short, medium, long fibre, of which the fine portion is something that will pass through say, a two-hundred mesh screen. This can be either all fibrous material or it may be the ground rock itself. The ground serpentine, the ground dolomite or it could be, in the case of say, crocidolite, you have the banded iron stone : when it's milled, some of the fine deleterious material would be the ground iron stone, which would give the blue fibre a red colour, incidentally.

So besides the chrysotile, you have then crocidolite, amosite, anthophyllite, tremolite, actinolite, brucite, picrolite and some others probably with shich I am not familiar. My knowledge does not cover whether these may have variations amongst themselves.

I would say, that, since in chrysotile operations, the milling will produce and geology has produced altogether a product which can be different. Because they vary. Variations in chrysotile, for instance, are mostly the water of crystallization. In the harsh type fibres there is less water of crystallization than in the silky fibres. So between the two of them, you get the geological factors which come in at the time that the mineral crystallized. So then, geologically, you could get conditions where the temperature, pressure and the chemicals that are available at the time

may not be exactly the same, though the chrysotile could crystallize in very much the same formula.

I would mention also that there are other hydrous magnesium silicates other than the asbestos groups and I will only name one of them just to show you that there are others: that is talc.

Talc is a hydrous magnesium silicate, it is not asbestos. The high quality talc is used for making talcum powders and various other cosmetics. This is probably more used, I would say this hydrous magnesium silicate is more used by the population than any other type of hydrous magnesium silicate. Also, the interesting thing is that commercial talc is not pure talc at all.

Take the seven samples of which I have a record from Gouverneur, New York. They range in variation from talc content of 4% to 24%. With it will be tremolite (15% to 48%), anthophyllite (7% to 38%) and quartz (0 to 3.6%). So that what you know as talc is actually not talc at all: it is a combination of, mostly, asbestos and the lar gest component usually is anthophyllite.

So I only throw that out to you that, using the simple word "asbestos", is certainly not a fair criterion for judging an industry which is producing some three million tons of fibre for the world consumption.

Most of that is the cross chrysotile, the harsh and the silky types, in which the variation is only the crystal in water and probably some effect of the surrounding rocks and most of them in major operations is only serpentine. And the serpentine has the same chemical composition as the fibre.

SABOURIN:

Same composition as?

LINDELL:

Same chemical composttion as the fibre.

Now I have shot my bolt and that is all I am going to talk about on asbestos.

If later on you have any questions about it, I would be glad to answer them.

Now I am going to turn the meeting over to the doctors. You run your own meeting. We are just going to listen, so I have asked Fernand Grégoire if he will be your chairman. The floor is yours.

CHAIRMAN:

Thank you very much Mr. Lindell.

If I have been chosen, I suppose it is for my poor English, so you will excuse my English. You will excuse my inexperience in dealing with these kinds of meetings. I will do my best and I will try to help everybody, give the opportunity to everybody to say what he wants and we welcome all the comments.

Here, listening to Mr. Lindell a moment ago, I was sorry that medicine was not an exact science, being a biological science, as they have with engineering, but on the other hand, if it had been an exact science, we probably would not be here and we would not have had this opportunity to meet in this nice country here today.

I want to take this opportunity to thank Mr. Lindell and all those responsible for this meeting to have given us this opportunity to discuss a problem in which we are so much interested, everyone of us.

Vous me permettrez de dire quelques mots en français; pour ceux qui aimeraient mieux s'exprimer en français ou comprendraient mieux le français. Il est certain que pour moi-même, c'est plus facile de parler français, mais je crois que la majorité, ici, parlant anglais, nous pouvons certainement nous faire comprendre en anglais et avec, je dirais, la compréhension, dans le bon sens, de tous ceux qui parlent simplement anglais, nous pourrons réussir à nous faire comprendre.

Et s'il y a, comme disait monsieur Lindell au début, s'il y en a qui veulent s'exprimer, qui préfèrent le faire seulement en français, et bien, nous essaierons ici de traduire pour les autres, afin qu'ils puissent comprendre les questions et les commentaires qui auront été faits.

[redacted]

Et maintenant, on nous a demandé une chose : c'est de toujours garder les mêmes places, si possible, parce que les sténographes qui sont là, prennent vos numéros et c'est par les différents numéros que vous pouvez être identifiés. Alors, quand vous serez de retour, si vous voulez toujours prendre le même siège, en gardant le même numéro; comme ça, vos noms seront toujours enregistrés par numéro.

They asked me to tell you to always keep the same seat with the same number so that you can be identified easily with the number on the paper.

And now the discussion is open.

I suppose that this paper of Mr. Lindell's. This nice study, will mean questions or comments and you are welcome to start the questions.

Personally I have been very interested to know that there are so many types of asbestos. As a physician, I used to know, that therewerethree main types and I was not aware of the fact that there are eight or nine, as you told me yesterday, different types.

Apparently these different types may have different biological effects.

I don't know if somebody here has done some work with different types of fibre on animals, to find out if there are different responses to the different types of asbestos. Has somebody done some work on that ?

SABOURIN:

What was the question?

CHAIRMAN:

The question is : Has somebody done some experimental work on animals to find if the response, the fibrotical response of the body of these animals is different with different types of asbestos used?

Yes, Dr. Wagner?

WAGNER:

We have done a series of experiments using different types of fibre and, very briefly, the results are that in guinea pigs it is hard to tell whether there is any difference while using the basic crocidolite, amosite and chrysotile. The dust is being taken from the extraction fans in the mills, and in the guinea pigs there is very little difference. In monkeys the difference is very clear indeed. Using the African monkey, the response to amosite after four months is equivalent to that of chrysotile after twenty-four months.

CHAIRMAN:

After ?

WAGNER:

After twenty-four months.

SABOURIN:

I didn't get the last word.

LINDELL:

You have to raise your voice, I'm afraid doctor, if you can.

WAGNER:

The difference became most pronounced in monkeys where the scarring of the lungs, after they had been exposed to amosite for four months, was the same as that seen in the same animals exposed to the same dose of chrysotile as far as we could estimate, after twenty-four months. It was very much more pronounced.

VORWALD:

I get the impression from your report in which you used guinea pigs and rabbits and monkeys that the grading of the biological response apparently in all animals was approximately, as far as I could judge, that chrysotile was the lowest?

WAGNER:

Yes.

VORWALD:

That you got the least amount of biological response with chrysotile and that there was not much difference between crocidolite and amosite. Am I correct in that ?

WAGNER:

Yes. The amosite was done much more thoroughly than the crocidolite.

VORWALD:

But there wasn't essentially much difference between the two?

WAGNER:

No.

VORWALD:

I should like to ask a question Mr. Chairman. Before we, perhaps, go to biological response. I should like to hear someone tell us precisely as to the physical and chemical characteristics (if they are known) particularly the chemical characteristics between at least three forms of asbestos. Namely chrysotile, crocidolite and amosite.

Are there clear-cut and defined chemical difference between these three types?

Do we know this?

It seems to me that it is essential for us in our deliberations, relative to the biological response to asbestos, to know precisely what we are dealing with from achemical and physical point of view and I think that is why the International Union Against Cancer in their brochure

in which they define, and which I am sure Dr. Gilson could talk about it much better than I can, have gone forward in, idenfified and listed quite clearly in their publication, which appeared about ayear ago, these details which must be known relative to the different kinds of asbestos, relative to the chemical and physical characteristics of the different kinds of asbestos which may be used from an experimental point of view and to which workers are exposed. Related also to the incidence of not only fibrosis of the lung, but to the incidence of primary cancer of the lung and, in final analysis, to the incidence of mesothelioma of the pleura.

Because from my point of view, I feel, and I may be wrong in this regard, that there is, there must be, it seems to me, a difference in the chemical and physical characteristics of the various asbestos which in one instance will terminate in simple fibrosis of the lung, interstitial fibrosis if you will, the other under various circumstances will terminate in the occurrence of primary cancer of the lung and then finally there must be a physical and chemical characteristic which defines the mechanisms which allows pleural mesothelioma to develop, which is quite different. And that is why I think it is important for us to deliberate, if we can, the physical and chemical characteristics of these various asbestos which might be considered.

SABOURIN:

I think here, Mr. Chairman, it might be well to underscore that although such answers as those that are invited presently by Dr. Vorwald are important and are desirable and I think they shoud be given, possibly not fully now, but eventually, that the primary object of the meeting is (particularly if the full answers cannot be given and are not known now) is to guide those who are responsible for this gathering so that a proper research centre is established in our community so as to reach, achieve those results which are necessary to give such answers. In short, it may be that the centre of research that is being considered will have to be given orientation by your advice, as I feel that it is not so much the study in depth of the matters which will come under study at that research centre, it is not the study in depth here that we wish to achieve, but when you find that there is a problem to which the answer cannot be given now, well, that you will be able to tell us, according to your own respective subjective knowledge and experience, how in your mind we could achieve such a study best, possibly by referring the study of such problems to specific other centres, sometimes doing it ourselves.

CHAIRMAN:

Well thank you very much Mr. Sabourin. I think we need a man like you to synthesize the work we are doing here. I have forgotten at the beginning to put this session in the proper perspective if you

want, in other words this research centre and it seems to me that the question of Dr. Vorwald is in the line of this perspective because if we know exactly what are the properties, physical and chemical, of these things, well then we don't have to do anything else with it, but if it is not known, if all is not about it, well, then, perhaps in the centre, you will have to study that.

May I ask Mr. Lindell to answer the question Dr. Vorwald put?

LINDELL:

Yes, I would like to answer Dr. Vorwald's question because that is exactly what I was trying to answer.

In other words, instead of giving you the chemical formula for each one of these different types, I gave you only the mineral name.

Now, for the doctors, I can have arranged, after this meeting (I didn't have time to do it before I left), a tabulation showing the exact chemical formula of each one of these different minerals (and each one of these are different). That is why they have different names, because they are different. The only thing I have added to the list is that chrysotile unfortunately has only one name, but there are at least five kinds. You have the cross fibre which can be harsh or silky, in which the water of crystallization is different but we can give you the exact chemical composition of the harsh ones and the soft ones, we can give you the composition of the platey ones and also give you the composition of slip fibres.

The slip fibres have the least water in them. We are able in our plant to simulate the features, the characteristics, the physical characteristics of the slip fibres, which are fast filtering, by chemically treating the fibres and thereby reducing the water of crystallization. Or they can be heat treated and the water driven off. So you make a similar type of fibre and have the same physical characteristics.

Well, we can tabulate this for the doctors and this is exactly why I brought up the subject so that when you use the word chrysotile it has to be defined what kind of chrysotile you use. At the moment, until all the facts are known, one might even have to say where that chrysotile came from, because they are different. And if you realize that geologically most of them are fundamentally different, well then, obviously you are going to have different kinds of compositions.

So, we will prepare such a table. That information is available. I think some of it has been available for as much as thirty years but no-one has attempted to put this all into one document and we will do that for you and distribute that to everybody.

CHAIRMAN:

Thank you very much Mr. Lindell.

Professor Sadoul?

SADOUL:

I think that the question of Dr. Vorwald was a pretty good one from my point of view because we need to know what we have to deal with and it is a very pertinent question and I agree entirely with Dr. Vorwald.

CHAIRMAN:

Yes, Dr. Bohlig?

BOHLIG:

May I ask still a question to Mr. Lindell?

You told us about several kinds of chrysotile. Can you tell me whether you have several or different types of chrysotile in one mine, or are the mines in the Province of Quebec producing all the same type of chrysotile?

LINDELL:

No. That I can answer. They are all different and there are as many as three different types of fibre in the one mine.

Take the Jeffrey mine which is in the main.

SABOURIN:

At Asbestos?

LINDELL:

At Asbestos, Quebec.

It is mainly a silky type of fibre. There, however, occurs in the ore body little pockets of brucite and also some pockets of picrolite...

SABOURIN:

What was the last word?

LINDELL:

Picrolite. And there will be just small veinlets of these that occur from time to time. Actually the brucite has been mined for space age purposes because it has a very high heat resistance.

SABOURIN:

Very high what?

LINDELL:

Heat resistance. And also take one of the mines in Thetford. I happen to know that talc occurs in that ore body. It is a slip fibre mine and in that slip fibre mine there is also talc.

Then you define talc. That's a very good question because I started out earlier to tell you what talc is. You see, there is very little talc found that is really talc. In fact, I think I gave you the figures. The talc itself may run only four to twenty-four per cent, and the rest of it is one form of asbestos or another.

Probably Mr. Messel would know better about the ore-body of Lake Asbestos, which is, again, an entirely separate ore-body from the other mines that are being mined.

You really have to take each ore-body by itself.

Now some of the companies own several ore-bodies, each one being different. Then there is another situation. The early Thetford situation was such that three companies mined the same orebody. So you cannot identify samples by companies. They must be identified by the location, where they came from. But generally you can define them in the classes that I gave you. Slip fibre, cross fibre, harsh platey and silky. And what we will need to do is to give you the chemical formula of these different ones which have never been named,maybe we have to give them a name. But in the table you will get, we will catalogue these separately and probably call them chrysotil e : A, B, C, D, E, if you like.

Mr. Messel, could you say whether you have other than straight chrysotile?

MESSEL:

Well, we have the same thing. We have some brucite and some picrolite but I think what we found most is that the fibre, this is asbestos fibre from the various deposits when analysed do vary somewhat in chemical composttion.

In other words there will be a slight variation in the chrysotile from Asbestos as compared to Thetford and there will be variations in Thetford between the various mines. Each location, more or less, has a composition that is common to it, but it is not common to the whole region. There are variations maybe in the silicate content, may be in the iron content, maybe in the water of crystallization. Each of these vary.

So when you extend this on to crocidolite and amosite you really get a wide range of variations.

So, I think that Mr. Lindell said that the only thing we can do is give you a tabulation of the knowledge which we now possess of these various fibres and their chemical compositions.

I have read some literature where some of these compositions have been carefully analysed and pinned down for analysis purposes but unfortunately that has not covered the whole range that exists. So I think that as far as the mining group goes, I think we'll try to make available to you what information we do have.

CHAIRMAN:

Professor Gilson?

GILSON:

I should like to make a comment in view of what Vorwald said in relation to the Union Internationale contrele Cancer (U.I.C.C.). I should like to suggest that this question really needs dividing into two parts.

We have first to consider the usefulness of having samples of asbestos of known chemical composttion for experimental purposes. We should not however fail to realize that man is clearly exposed to a very mixed batch of materials over the course of his life, and that it is most improbable that we shall be able to characterize with any great precision the past exposures of people. While fully supporting Vorwald in his plea that when we start doing experimental work, we should know as much as possible about the chemistry of the material which is being used, let us not confuse that with the studies of the human problem, where until we have really got any evidence that there are important biological differences in man between one fibre and another, (I'm talking asbestosis now), let us not get too involved in going into the details which may merely obscure the whole picture and we may make very slow progress by feeling that we must differentiate between the human exposure to all these different kinds of material.

It seems to me that we need to keep a balance in this sense.

Well now I think that when, if I may come back again to the experimental approach, I think it is perhaps at least as important if numbers of workers in different parts of the world are going to increase the experimental approach to this problem, which I'm quite sure they will, that perhaps it is at least as important at this stage that people have access to the same material in the sense that they know that the material they are using is the same as somebody else is using in some other country and that gradually we

we shall build up information about the biological effects of these different materials, and perhaps be able to relate them to the chemical differences. But I suspect that at the moment the chemical differences far exceed the capabilities of differentiating them biologically. And I think it would be a pity if we got too deeply into the chemistry at a stage that might perhaps prevent people from getting the cream of the differences, biologically say, between the main types: amosite, crocidolite and chrysotile.

CHAIRMAN:

Dr. Gilson, thank you very much.

I think the importance of this study, this differentiation is perhaps more evident in Quebec than in your country because we have subjects who have been working let's say in Asbestos in the same mine and who have been exposed to the same dust for forty or forty-five years. So it is quite different from the secondary industry where you are using, and the same men may have been exposed to four, five or ten different types of dust during their life. As far as we are concerned, we know that there is quite a difference let's say, as far as pleural plaques are concerned, betweeen Asbestos and Thetford mines. It seems this is very close and then, in one place you have none, and then in the other place you have pleural plaques. So for us it seems important to know which kind of dust we are dealing with, to start with, and to try to see what is the difference,

experimentally and also biologically, because those men have been exposed only to this kind of dust and this is why the opportunity is great in our province to study pure exposure to one or at least to one kind of dust found in the mine. I mean only to the dust found in this particular mine even if there is two or three, I realize that you have other things than chrysotile. You have brucite and picrolite and perhaps a few other things. But this is just small holes : I suppose they don't affect much.

LINDELL:

They are so small that it's only a scientific fact that they occur. The percentage is so very minimum.

CHAIRMAN:

Dr. Vidal?

VIDAL:

Je suis peut-être le plus jeune de l'assemblée, quoique tout de même le plus vieux, mais je remarque que depuis trente ans, nous avons la responsabilité d'évaluer les perturbations pathologiques causées par l'amiante. Nous pensons que l'amiante, peut-être, est le problème général de tout le monde. Après un certain nombre d'années d'observation, on se rend compte maintenant que nos problèmes, dans le Québec, ne semblent pas coordonner avec les problèmes d'Afrique du Sud ou d'ailleurs.

Il y a donc un problème histo-physiologique qui nous dépasse nous, cliniciens.

Mais tout de même, devant la responsabilité économique pour l'ouvrier, de le protéger, d'évaluer son incapacité, de faire un diagnostic et d'évaluer les complications, nous fait responsables du travail de l'ouvrier.

Nous remarquons depuis quelques années qu'il y a différences de coordination, de compréhension, entre les différents diagnostics.

Alors, je pense que nous avons l'impérieux devoir de trouver quel est le motif primordial qui justifie ces différences dans les statistiques. Dans certaines mines du Québec, on ne semble pas trouver les mêmes réactions que dans d'autres mines. Je ne sais pas, je ne suis pas un biochimiste, mais je crois que c'est l'impérieux problème qui se pose maintenant : de faire la recherche de la constitution intime de l'amiante - qui est un terme générique - l'amiante que l'on emploie ici, que l'on emploie en Australie, que l'on emploie ailleurs, afin de commencer à procéder, pour que notre responsabilité de diagnostic soit exacte, précise, selon les objectifs de la vérité.

CHAIRMAN:

Professor Vidal said that for the past thirty years he has studied patients with asbestosis and he found in these cases that there seems to be a different response in the subjects to the kind of dust they are breathing in Quebec and in some other parts of the world and he doesn't know why, but he would like to have research made to try to find the reason why there are such different responses.

GILSON:

May I put in a plea here that we really try and find out what the differences are quantitatively by epidemiological methods?

I only cite our own experience. When we started looking at films of cases of proved asbestosis, pathologically as between South Africa and the United Kingdom, the group thought that they were going to find a big difference in the plaque rate as between South Africa and the United Kingdom. In fact, when you looked at the material the differences were much less than we thought and I think that it's important to be rather more precise than observations based on clinical impressions without proper epidemiological studies aimed at measuring quantitatively what these effects really are.

And I think that before one makes assumptions that chemistry is specifically related to plaques (a particular kind of plaque formation), we want to have the evidence, in man, that there is really a difference, and also remembering that there may be very important differences due to the physical nature of the dust, the dose of the dust, the length of period over which they have been observed, the inhalability of the material and all sorts of other factors which may be quite overwhelmingly important, as compared with chemistry.

CLAASS:

En somme, pour étudier la nocivité, il y a trois approches possibles : c'est l'approche expérimentale, dont monsieur Wagner a parlé. Ensuite, il y a l'approche épidémiologique et puis, la troisième approche, qui est peut-être l'approche clinique et anatomo-pathologique. Au fond, je ne sais pas si j'ai oublié, mais je crois que ce sont les trois approches possibles.

CHAIRMAN:

Yes, Professor Claass was saying that as far as he is concerned he sees it as if we had three different ways to study the problem. The first one will be experimental, the second epidemiological and the third one clinical and anatomo-pathological. Three stages, three different stages, or three different ways to study the problem.

VIDAL:

Trois stages...

SADOUL:

Nous devons tout de même garder à l'esprit que notre approche épidémiologique est seulement assez grossière et que nous avons besoin d'information de toutes natures dans ce domaine. Je crois que nous n'avons pas à mépriser aucune voie d'approche, ni la clinique, ni la radiologique, ni l'étude minérale, l'étude chimique et physique des poussières. Si nous voulons arriver à avoir des informations, il faut garder à l'esprit qu'un travail d'approche doit être fait dans ces différentes directions.

Si je prends par exemple, l'exemple des mineurs de fer, qui est un exemple tout à fait différent, mais où l'on a fait déjà beaucoup de chemin, et il est certain que dans le West-Cumberland, (in England), c'est une maladie tout à fait différente de celle que nous voyons en Lorraine - c'était une maladie, car elle a maintenant disparu, grâce aux moyens de prévention, mais c'était une maladie tout à fait différente de ce que nous voyions en Lorraine. C'est différent aussi de ce que l'on voit dans le Vigerland. Et nous devons garder à l'esprit aussi bien les caractéristiques physiques que, aussi, la façon dont on travaille le matériel qui, évidemment, joue un rôle tout à fait différent.

Si nous mélangeons toutes les voies d'approche, nous ne pouvons pas arriver à quelque chose; mais nous devons garder à l'esprit que pour faire un travail valable, il faut que des groupes différents travaillent par des voies d'approche différentes : clinique, radiologique, fonctionnelle, etc....

CHAIRMAN:

Je vous remercie, Docteur Sadoul.

Thank you.

Professor Sadoul was saying, I'll try to summarise...

LINDELL:

You don't think he can translate for you?

CHAIRMAN:

Yes, I. Perhaps you.

SADOUL:

I can do my best. Yes.

I think we have to rely on several ways to approach the problem. It is necessary to use different tools to look at the occupationsl disease and I think there it no question to lower and to minimize the parts played by the epidemiological studies or by anatomical studies or by pathological studies. Anyway we

SABOURIN:

Or radiology, you said.

SADOUL:

Yes, X-ray pictures.

And we need all of them and we have to rely on all of them in order to get a clear picture of the disease.

That's my point of view.

SABOURIN:

You added, including the physiological.

SADOUL:

Studies, yes.

GILSON:

Could I just add one word to what professor Sadoul has just said?

I entirely agree with him here. I think that it is perhaps easy to assume that the relation of the radiology and physiology may be the same in all people exposed to different kinds of fibre under different circumstances. I suspect in fact that it may turn out to be quite untrue and that in making use of the methods he suggests (the clinical methods physiological methods and the radiological methods) we should keep these quite clearly separate.

But we should nevertheless use them as a whole in order to establish whether or not for a given degree of disability say, the radiological appearance is different in people exposed in industry as compared to people exposed in the mine.

SABOURIN:

They have to be conjugated.

GILSON:

They have got to be conjugated but they must not be conjugated before they have been looked at separately.

SADOUL:

Yes, I agree with you.

Thank you for improving my mind.

CHAIRMAN:

Yes, Dr. Vidal?

VIDAL:

Dans ces données cliniques, radiologiques, pour évaluer un diagnostic, il faut connaître quelles sont les conditions de travail dans lesquelles a opéré l'ouvrier.

J'ai l'impression que, comme dans nos mines du nord, comme dans les mines de métal de l'Abitibi, il y a des mines où il y a une ventilation très adéquate et d'autres, malheureusement, qui sont relativement pauvres.

Et bien, alors, les conditions de travail de l'ouvrier sont absolument différentes.

C'est très important dans l'évaluation finale d'un diagnostic : quelles sont les conditions dans lesquelles opère le travailleur de l'amiante. Elles ne sont pas toutes les mêmes.

LINDELL:

Would you like to say that in English yourself doctor? Your English is perfect.

VIDAL:

I wouldn't call it perfect but anyhow I remarked when we make the evaluation of a diagnostic we have got to know exactly the condition where the labour man was operating and I'm sure in the different mines that's not the same conditions that in industry mills when I start and it was only a mining concern.

SABOURIN:

Conditions vary between one industry and another?

VIDAL:

Sure. We're talking of mines that's all in 1935 but now we're talking of mining and industries built with working different material.

We had, I don't know what kind of material.

SABOURIN:

Under different ventilations?

VIDAL:

And the ventilation is so different in different mines.

VORWALD:

From that stand point is it possible in a retrospective epidemiological study, retrospective study, how far, how far is industry prepared to go, to give us information relative to conditions which might have existed, let's say prior to the second world war? If we are to go back that far, which I think we will have to for exposure time.

SABOURIN:

To ascertain the opposite conditions?

VORWALD:

Yes.

Is industry prepared to, what sort of reference does this have reference to your point which you made in setting up an investigative authority or body, whatever it might be. I'd like to hear John Gilson talk on this too.

It seems to me that our most difficult problem is going to be a retrospective study in order to collate all the evidence, exposure evidence and the kind of asbestos used, ventilation and everything, let us say prior to 1941.

SABOURIN:

We have in our archives, most of the units of the asbestos industry in the Province of Quebec, which groups presently about eight different companies.

We have in our archives dust counts, statistics or history, going back to that period before the first war.

Some mills then were much more dusty that other mills, and more and more now the ventilation is becoming homogeneous, stabilized and better even in this mill than in that mill and I appreciate that Dr. Vorwald is saying that nevertheless very often the conditions that are now being exhibited by, say, human beings in the asbestos industry, these conditions of today go back as to their causality to twenty, twenty-five years' exposure in conditions which were, as to dust for instance, ventilation, working conditions, very substantially different from those conditions which are encountered today.

CHAIRMAN:

Dr. Smith?

SMITH:

If I may, I'd like to add something to that, that the Quebec Asbestos Mining Association has turned its doors completely wide open for an epidemiological survey and we have in our midst today a man who is preparing to undertake that with records going back.

Dr. McDonald would you care to speak about that proposed survey?

CHAIRMAN:

Dr. McDonald?

McDONALD:

Well frankly I'd rather not from the point of view ...I was wanting to have a preliminary investigation, but frankly I'm lost. I don't quite see what the terms of reference of the meeting are. It seems to me that we are reviewing a large number of subjects and I'm not clear as to what the questions are that have been put to us.

Are we reviewing the state of knowledge on asbestos and its biological and experimental effects in the world? Because that would be a big subject.

I had understood we were going to advise, but I'm not clear what we are advising on until the questions have been put.

Are we considering the problem of asbestos in the world or the problem of asbestos in Quebec?

Are we considering the ways in which an epidemiological study could be done in Quebec?

I mean these are very different questions. I'm not sure which one you want an answer to?

SABOURIN:

We are attempting to confess primarily what we think are those conditions that are pertinent to our local Quebec industry, and leaving it to such authority as Dr. Gilson and others to tell us what might be ascertained elsewhere, so that our problem can be studied in the light of the asbestos problem at large in the world, as asbestos is being now incriminated at large.

LINDELL:

Mr. Chairman it's very difficult to define this exactly because those of us who have been trying to define it are not doctors and are not familiar with all the aspects of the problem. That is why we have brought all the doctors together to try to arrive at just what are all the facets and which way should we really go.

We visited, we made two trips to Europe, Ivan and I and we, at that time, felt that the starting point for us would be to establish some place, in a physical location, where we could then bring all the information that is available in the industry and, starting with the mines, because Johns-Manville has early records dating back to the earliest of the mining industry - - X-rays and the clinical data of the various people employed and also all the cases that have taken place and as well as information on what the plants were like during the time.

So we're prepared to. The idea was to find some physical place to bring this together, also acquire a library where we would get together all the presently known information connected with this problem and then from there, having determined that there are other areas of study required, maybe experimental tests which then could be, if necessary there would have to be such facilities established in order to make such animal studies. We would however prefer to use existing laboratories, people who are already today familiar with and have the equipment and are capable of making such studies, that we would then direct and sponsor such studies outside, but if there are no such facilities, obviously it is necessary that we may have to go to the point of establishing such.

In the meantime, having all this vast amount of information in the past(and we are self critical about this) because we in Canada, we seem to be the worst authors, I guess, in the world. Everybody else is on paper and we have written very little about what we have done. So now we are faced with that problem of taking this data and getting some of it down on paper so that we can really see what do we have and what is the problem and where do we go.

That is one aspect of it.

There have already and we should go forth with and get this information together and find out what that field, what that field will bring to us.

Now parallel to that, there are all these other problems with which I say we don't feel qualified to say that we should do this, we should do that, and we would certainly wish the guidance of the doctors and engineers and experts, shall we say, in these fields that will tell us what should we do. That's really the purpose of the meeting, tell us which way shall we go and it's not just confined to the mines because my company is very much interested in the factory aspects also and I'm sure the others are also and there is no monopoly on that problem.

Unfortunately factories have a little different situation because, take our own factories, though we are the free world's largest supplier of asbestos, nevertheless, our own factories use asbestos fibre from Australia, from Africa, from other mines in Canada and so that in a particular factory you will find a man exposed to crocidolite, amosite, chrysotile and what have you.

So now what kind of a problem is that and how do you deal with that one?

That I don't know and I would certainly like to get advice on where we deal with that one.

But as a starting point we do have a problem, that is in the papers and we are starting with the mines because this is a group of people that are prepared to finance at least a limited amount to start with, a programme which covers the mines, the information we have and then we move from there.

As I see it that's a long-term programme and we must approach it, I believe, with people that already have the capacity to deal with the problem. I do not think that we should start this by putting some people on the job that have to be taught first what asbestos is and then what asbestosis is and then go on from there.

We are in too big a hurry. We need to find something quicker than that.

CHAIRMAN:

Doctor McDonald does that answer your question, or do you want to...

McDONALD:

May I follow this up?

It seems to me from what you said then that there are two types of questions. The first one is that the industry is conducting in its industry and the findings, whether or not there is a health problem and what its nature is and is seeking the advice, presumably, of how to go about answering this question.

Then there is the question which people outside Quebec would like an answer to which can only be provided by studies in Quebec. But it seems to me that as a first principle in this, some kind of small committee, council or something or other, is needed which will consider both these types of questions and insure that both that suitable work

is done to answer both types of questions and I would have thought that this was stage one.

VORWALD:

May I follow through?

I am somewhat fearful that our discussion may be relatively diffuse and that at the end of the conference we shall not have answers to two fundamental questions for which we are here, which have been cited by Mr. Sabourin and Mr. Lindell. I would urge that and follow through with what McDonald said, namely that we ought to set up a committee which will consider the agenda in order that on the last day we will be in a position to answer these two fundamental questions which are put to this committee. Otherwise the discussion is going to be relatively diffuse.

CLAASS:

Il y a évidemment une source à exploiter dans la littérature mondiale, et c'est ce que l'on fait d'ailleurs en Europe; il y a la Clinique del Lavoro de Milan et l'Institut de Marseille, et plusieurs autres grands instituts qui se sont mis ensemble pour exploiter toute la littérature mondiale sur les pneumoconioses et un bulletin paraît en langue française, allemande, italienne, pour donner une information sur l'ensemble des travaux - même en langue russe et dans les langues orientales - sur les pneumoconioses.

Mais je dois dire qu'en étudian et en se tenant au courant des travaux ainsi faits, on s'aperçoit qu'il y a encore énormément d'inconnus et que des recherches et des approches de ces inconnus sont absolument indispensables, et pour encourager des travaux de qualité dans toutes les voies d'approche...

CHAIRMAN:

Dr. Claass said that the first approach to the problem as far as he is concerned is the literature, world literature on this problem. And from that there are many questions that remain to be answered and then those are the questions we have to work on.

LINDELL:

Mr. Chairman I had hoped to answer Dr. Vorwald's question, that this could be the committee, the committee of the whole to draw up the agenda to arrive at the conclusion we're looking for. But if it were the doctors' opinion that it would be better to select a few of you to sit down and work on such an agenda instead, we would be very happy to name such a committee, You name the committee and we'll adjourn and let the committee come up with the agenda and we'll follow another session when they're ready.

SABOURIN:

Mr. Chairman I would say there that the committee for this North American Continent could well be constituted by those who have been in constant touch, more or less.

I say constant wittingly of the problem of asbestosis for the last twenty years or more. They are Dr. Wright, in physiology particularly and Dr. Ken Smith, who has had the clinical responsibility at the town of Asbestos for years and is now stationed in New York in charge of Johns-Manville medical services and Dr. Grégoire who is now doing the physiology in Canada for the industry at Lavoisier Institute and Dr. Vorwald now of Wayne Laboratory who has done the pathology for a number of years at Saranac while, George Wright was doing the physiology and Dr. Cartier who has had for nearly twenty years the responsibility of directing at Thetford Mines a clinic which encompasses the seven other companies, (Johns-Manville being situated at Asbestos and not being under the Thetford Industrial Clinic) and Dr. Vidal who has been heading the Workmens' Compensation Board and as chairman of the Pneumoconiosis Committee for some twenty five years since the very inception of that board and Dr. Guy who is the pathological man who has had the responsibility for the Board and as a consultant for the industry in Quebec for some twenty years.

Could be these persons, a committee which could meet with a group representing the European Continent : Professor Gilson and Dr. Sadoul of Nancy and Mr. Noro of Helsinki and Dr. Claass etc. And if you would get together I suggest Mr. Lindell at recess period and organize your own session.

Then out of these two groups could come down i: writing an agenda whi ch would circumscribe the debate.

ENTERLINE:

I think that it would be very helpful to have a restatement of the questions because I am not too clear about the discussion, what these questions are.

Could you restate the questions to be answered by the agenda?

SABOURIN:

I think that it is very hard for us to re-state. We have said, I think, about enough to express that there is a huge problem for the industry, and you are the scientists who are presently the consultants at whose door we are presently knocking for orientation and setting up some kind of a centre. The problems, the facets of the problems, well we would have only to repeat what we have already typewritten and what has been said, more or less.

I think Dr. Wright might give us a word here possibly. He is usually concise and much clearer than I can be as a layman, and he knows the problem. He has worked with for such a long time at Saranac and at Cleveland at his Laboratory as a consultant. Also, Dr. Wright, you have a knack of saying clearly what I say with confusion.

WRIGHT:

I don't know that that's always true you know.

I share the quandary that is apparent in the minds of others as to how we might best proceed to get the greatest good of convening a group of people, obviously making up the majority of physicians throughout the world who have a strong interest in the general problem of the result of human exposure to asbestos fibre. I am in agreement with them that if we had a way of stating certain questions which could then be debated we might get further along. I am in accord with this.

I don't necessarily feel their distress as strongly as they did because I have had the general experience that in meetings of this sort, the first day or day and a half is spent in feeling your way around and comments are made which oftentimes seem to have no relevance, one thing to the other. But as time goes by you do ultimately come to an avenue down which you can go and it is apt at the end to be rather clearly defined.

I think the academic people who are so pressed for time like to have committees, they like to set up a specific question and then will proceed to have that answered, and this sometimes works well, but I am not sure that it avoids a very important failing and that is that you omit certain things.

The more narrow you make your enquiry or your line of talk the more apt you are to omit some of the side issues that are very important.

I would however, for the purposes of this meeting, agree that it would be useful if we could, within the next hour possibly. If there are some subjects that could be profitably discussed within the next hour, we could continue doing this and then have an afternoon session made up of a group such as you mentioned where we could phrase the questions a little bit more directly.

I would like to ask one other thing.

Would it be possible for Mr. Lindell to state or perhaps you to state rather directly the questions that have arisen in your mind as a result of having travelled rather widely throughout the world?

Perhaps take ten or fifteen minutes to give us the impressions that you have received. Areas where information is lacking or where there is conflict.

This might be a useful starting point. You have made trips and you have looked around.

SABOURIN:

As Dr. Lindell has said we have been trying to visit most institutions which have been dealing with some research in the fields pertaining to the asbestos industry.

We have been a couple of times to Helsinki visiting with Professor Noro and his associates, and seeing there his vast institution, where they stand I think in a privileged position in the sense that although they haven't got all the varieties of asbestos that we have here, they have by legislation the powers to obtain reports as to the mortality, morbidity, causality and age and conditions of virtually the whole population of Finland, which is small but which is under the control, more or less, of this institute as to research.

As we have found in Nancy, in the clinic and hospital directed by Professor Sadoul, there also a vast institution where a great number of patients are studied as well in the asbestos or other dust

exposed workers and as well in the population and in the non-exposed workers. And, for instance, there we have found that, more or less, in this condition which is quite a problem with us, that of emphysema on which Dr. Wright has written and on which Sadoul has written also, that more or less you check about the same way your views and opinions.

We have found in both places just as at other centres that possibly greater importance is being credited to the importance of radiology at Nancy, for instance, as a factor and we have proceeded to Laenec Centre, the oldest in Europe in tuberculosis, in Paris, in pneumology.

Dr. Bernard Gamain might have been here but couldn't, there is now some examinations going on in this vast institution which prevented him from bein represented here. We have moved to Milano where, a couple of times, we have visited Professor Vigliani and there again we have been faced with a vast institution and hospital facilities and physics and bio-chemistry being very well underscored, and technologically we were much impressed, laymen as we were.

With those sources of consultation which were available at the time to us.

And at Cardiff, for instance, in England. Well we do not have to say, most of us know, have been there. We know of the institutions in the days of Gough, under Professor Gilson and Dr. Wagner,

and associates, we have seen there a tremendous modern organization with great hospital facilities right at their doorstep, giving them a great power of gathering most valuable experience and with data which again under the labour and health legislation of England, has empowered them to have access to so many sources all over the British Isles and elsewhere in the Commonwealth and being their institution responsible only to the British Cabinet and not to the House or to any other body, they have a leeway, a great facility of proceeding in depth to evaluation of the problems that are submitted.

And we have visited at Cleveland the clinic headed by Dr. Wright and his school of medicine which is a tremendous thing which provides great facilities for carrying on research in most of the facets of the problem that we have under study.

And with Dr. Vorwald at Detroit, we have seen there, I say the word tremendous because it is a tremendous organization there. Possibly it is the one that where from the experimental view point as well as in any other field , and nothing is missing in the way of technicians and most advanced technological methods.

In this dust study (pollution of the air , for instance), the problem is given their very, very close attention. Nothing is overlooked. The expenditure is going into millions of dollars.

It is a vast enterprise and, well, in Quebec, you have seen most of our clinics there. Sacred Heart Hospital under Dr. Guy has a new wing now which Dr. Bohlig and others have seen and Dr. Wagner and Dr. Gilson has been there. And although it is now being there. And although it is now being re shaped I would say, and we have at Lavoisier Institute with Dr. Grégoire, who was a student, who has studied under Dr. Wright for quite some time before coming to Canada to specialize in this field of physiology.

And we have visited these centres and others which I might omit, but let's say they are those which stood out.

We could have gone further and thus it occurred to us that it would be unreasonable for the industry to envisage that it should in our province, although responsible for the greater proportion of the chrysotile production the world over in the free world, because Russia is producing more than we are presently, whether it would be unreasonable to expect the industry at home to, on this continent, to envisage setting up those facilities that might be existing in those other laboratories, where we could , say, farm out much of the work to be done.

But, nevertheless, this is possibly the essence of the kernel of our little problem here.

There is something that we should be able to do and that we can do, so that there is a centralization of the ways and means and so that there is, even if it is a limited staff, an office in a permanent way, at home, beside having a library, it might be that we should have certain technical facilities to conduct on the spot such work.

It might be that all our employment and pre-employment and in the course of employment, examination of our men has to be reassessed because we have there a vast reservoir of workers at Thetford Min and at Asbestos. Some are not working anymore, some are at work at home, for instance. We have not the privileged position of Professor Noro. Cancer need not be declared as a cause of death. It is not compulsory in our province.

It therefore makes an epidemiological survey quite a difficult thing sometimes to do with great precision.

It has occurred to us too that we will have to explore that.

Whilst we were in Italy, there is some chrysotile some type of chrysotile at Balangero, the survey of which under Dr. Vigliani possibly could afford there, a privileged position, such as that in Finland and at Milano with chrysotile. Yet the population at Balangero has been rather stable and the Institute of Physiology that you have at Turino.

Am I right?

VIGLIANI:

Yes.

SABOURIN:

is, there are records, they have made studies but this has never been collated into one channel under the heading, "the field of pneumoconioses of asbestosis", or "the various types of asbestos", but this survey of this Balangero, of this Finnish situation and of our situation in Quebec offer together with the survey that Cardiff has been going into will constitute a set of operations that we would like to feel that from our centre we have access to. We would like to feel so.

I think I am being diffuse a bit there but that is the answer to George's request.

Am I right?

LINDELL:

Not quite.

If you like I will give my impression and I won't go into the parts that Ivan has touched.

But this all started out by the industry finding it had a problem and deciding to do something about it, devoted money, and Ivan who has been our advisor in this subject suggested that we should set up a research institute, which prompted us immediately then, because I had to learn something about what is this all about anyway, because I was only a mining engineer.

Therefore, I didn't know enough about occupational health as such to set up such an institute.

So we went around and I would say that some of the things we saw were very interesting.

For instance, in our visit to Helsinki we discussed with them the one mine problem at Paakila and whereas in, you will remember the New York meeting, the term was used asbestos, however, it is anthophyllite at Paakila, and they found a very interesting situation there in that there was a large number of pleural plaques. Not only did it happen in the miners but in people who were not miners. It occurred equally in men and women over a certain age. It had no respect as to whether they were employed in a mine or not and they had made considerable studies in the area. They had taken dust counts and gathered dust in a large area some thirty-three, thirty kilometres in diameter around the mine and found that the incidence of pleural plaques had some relationship to the distance away, but there were, nevertheless, pleural plaques, regardless of occupation or age. In this thirty kilometre area, it was also interesting to note that that happened to be the area of the serpentine belt that was just about the same. Geologically it was all the same rocks and the serpentine didn't occur anywhere else.

Also they found that the incidence of pleural plaques was much smaller elsewhere in Finland. So why then in this particular area? Was it air pollution?

That's an interesting question. Is it air pollutio:

At first that's what you would say, it's air pollution, but could it be water pollution? Because they were all drinking water from wells which came from these serpentine areas. The people were drinking water from wells which were not serpentine.

That was an interesting point.

Or is it as Dr. Noro mentioned to me the other day, possible that the vegetables have something to do with it? Because one thing that I had observed at Asbestos that when we played golf down on the golf course which was down in a valley, the number of mosquitoes are not as high an incidence of large mosquitoes on our golf course as it is in some other parts of Canada, where they really grow big.

And the only conclusion we could come to was - could it be that asbestos fibre having scattered around the area since eighteen eighty-one (1881) has made a higher magnesium content in the soil? And this continual drainage of the rain waters toward the rivers kills the mosquito larva before it can hatch and therefore you don't have as many mosquitoes?

It begins to look as though there are a lot of interesting aspects after our visit to Finland.

Now in Nancy, where they do not have any mines in that part of France, but they are more experienced with asbestosis from the factory incidence. There I was particularly interested in one, two cases that Dr. Sadoul mentioned about, where he had two men of the same age employed about the same length of time, practically about a month, had according to the X-rays, practically the same picture, lung picture of asbestosis, but when he put them on a case of disability, put them on a functional test and found that one man could not sustain an effort as long as the other one could.

Now what is the difference? What are the factors that are involved to make the disability in one case different, whereas on an X-ray picture they were both the same, practically the same exposure, and the whole picture quite the same?

So then you will get a problem that looks like, we not only have a problem of what is the occupational health problem but the environmental health might have something to do with it.

So that is another aspect of study we felt that we had to look into.

Well it was suggested that maybe at Balangero, in Turin, there would be an ideal place to make an epidemiological survey because there was only one mine group and it had been there a long time and these people could very well be identified.

Well I would not be for that, because it happens that Balangero fibre is a slip fibre. And so why am I concerned about that? Well I would hate to have a result come out from Balangero that was unfavourable, especially when it is a different type of a fibre.

Now why do I say that? It is because at Asbestos, Quebec, we do not have any pleural plaques. We don't have any mesothelioma. In Thetford, forty-five miles by air, they have some pleural plaques, they have some mesothelioma.

Now what is the difference then? Where do we go - how do we solve this problem? Do I go crying that, no I'm pure I'm without sin?

I can't do this because asbestos is too big a field and we are too much involved in it. So we have to find all the answers and besides we have not had a new case of asbestosis, I think, in some fifteen, twenty years.

We have some others who have carried over from the past who are still on the payroll. They haven't retired yet, but we also have a man who has been disability-pensioned from asbestosis and he is eighty some years old now and still very healthy.

And so these are the things that we learned as we travelled from one place to another.

At Cardiff, unfortunately, I didn't meet any of the doctors, but I had an opportunity to walk through the hall there and I wish I had a photograph of the information that is on the walls of the passage-way in their laboratory, because there is really a thorough study of what has happened with crocidolite, amosite and chrysotile and my only problem was there I saw that, well, what kind of chrysotile? I had already faced the problem that there is more than one kind. And then my knowledge isn't vast enough to know whether even crocidolites are the same.

However, there is one aspect of that which I then realized, that with the crocidolites they occur in sedimentary rocks, whereas ours occurs in igneous rocks.

So it's quite possible that maybe they have organic compounds apparent which are carcinogenic which we might not have because we have the igneous rocks.

So I think that briefly puts some of my impressions and why I found they were such a problem and why I didn't know how to approach it.

We plan to adjourn for coffee right now.

SABOURIN:

Well I just...so as to complete George Wright's answer?

LINDELL:

Well say it in French and Madame Villaire will take it down.

SABOURIN:

Et bien, en Russie où nous sommes allés, il y avait à Asbest, jusqu'à quinze ou seize mille personnes qui avaient été employées dans l'industrie de l'amiante et là comme à Sverdlosk et à Moscou, les différentes personnes que nous avons interrogées, l'incidence du cancer, nous ne l'avons pas contrôlée, n'était guère plus considérable chez les ouvriers de l'amiante aux mines mêmes, qui produisent incidemment plus de chrysotile que nous n'en produisons au Canada, que dans la population en général. Et je suis allé deux fois en Allemagne de l'Est, à Dresden et Berlin-Est, une fois en compagnie du Docteur Grégoire, et là nous avons pu nous

rendre compte qu'il y avait une possibilité aussi de faire une étude bien serrée parce que sous la législation de l'Allemagne de l'Est, il y a un contrôle des causes de morbidité, de mortalité, qui n'existe pas dans l'Allemagne de l'Ouest, au même point, et non plus dans les autres pays d'Europe que nous avons visités , ou en Amérique encore. Et nous avons pu remarquer cependant qu'à Dresden, le même problème paraissait se poser, qui se posait en Finlande, c'est qu'il y avait une incidence très considérable, dit-on, de plaques pleurales en périphérie des usines où l'amiante est ouvrée à Dresden et ces opération ayant eu lieu plutôt immédiatement après l'évacuation de Dresden par les Russes et alors que les conditions d'opération étaient défavorables et que la pollution de l'air devait être dans une situation des plus déplorables et qui peut, peut-être expliquer pourquoi cette preuve de l'existence de plaques pleurales jusqu'à une quarantaine de milles, dont Dresden pouvait apparaître dans les poumons d'êtres humains et où la présence aussi des mésotheliomas paraît s'accuser.

Et là nous avons pu, tant à l'Institut de Santé - qui est considérable et qui est le pionnier du genre en Europe d'ailleurs - à Berlin-Est, nous avons pu, avec le professeur Holstein et avec le Docteur Anspach et Villers, à Dresden, avec qui le Docteur Bohlig a assez longtemps travaillé sur l'aspect surtout - et en service - l'aspect surtout radiologique, nous avons pu recueillir des constations qui nous faisaient voir

que le problème de plaques pleurales et de mesothelioma pouvait avoir une histoire propre à la région et qui peut difficilement, cette même histoire, se trouver dans les autres pays que nous avons visités.

Et la dernière impression, Docteur Wright...

Dr. Wright, and the last thing that we have found is that we have, we could not find anybody who could satisfy us as to what was the mesothelioma in point as to its characteristics and as to the ways of probing evidence, of reaching probing evidence that would warrant the conclusion that they were in fact mesothelioma and Dr. Grégoire and Dr. Guy have just returned from England and were very much impressed by the statements given them in Belfast, Liverpool and in London by pathologists with whom they have been in touch, who can tell that they see a lot of these mesotheliomas but they have not been convinced at all as to the existence of methods that would allow them to, by analogy, proceed here to assess if in fact we would have here such mesothelioma conditions, in the industry.

CHAIRMAN:

I think if we want, we can get a cup of coffee and then we'll be back in fifteen minutes, please, on the discussion.

(Meeting adjourned until the following day)

Wednesday, November 24th, 1965

GREGOIRE:

Alors, j'espère que chacun a reçu le programme. Vous avez tous votre copie du programme?

Si je commence en français c'est que, apparemment la sténotypiste est prête, celle qui prend le français.

Maintenant, nous allons procéder un peu différemment peut-être, aujourd'hui, de ce que nous avons fait hier. Pour chaque item nous commencerons par demander à quelqu'un d'ouvrir la discussion, si vous voulez, et ensuite, les autres pourront discuter à leur tour, après celui qui aura été demandé d'abord, et qui aura parlé quelques minutes sur le sujet.

I hope everybody has his copy of the programme.

Now we will proceed slightly differently than we did yesterday.

For each topic there will be somebody who will start the discussion. You are required to talk loud enough so that the ladies there, can take all the words, all what you are saying.

Now the first item on the agenda, the general items as you can see : "A review of objectives, opportunities and priorities".

We start this morning with epidemiology and I think Dr. McDonald accepted to open the discussion on this subject.

MCDONALD:

Mr. Chairman, after the New York meeting just over a year ago, as a newcomer to Canada and especially to Asbestos, I was invited to review the question of what epidemiological work could be usefully done, particularly in relation to the mining industry of Quebec. Perhaps not invited, but I also at the same time had a look around at what other things seemed to be related to this question of the findings of the risk to health of asbestos by epidemiological methods. Any my conclusions on this are described in the document which I think has been circulated, which I don't know if you have had time to go through. But what I have to say is essentially a summary of what is in that document.

The first point was that, both from the point of view of the world generally and from that of the mining industry in particular, the first priority seemed to be the necessity to define more accurately and more precisely and with more assurance the risks to those in the mining areas than had been done previously.

Most people will be familiar with the study by Braun and Truan which was done something like eight years ago , and which certainly left quite a lot of questions unanswered and certainly left the conclusion somewhat in doubt.

The study was made at a time before the complexity and potential seriousness of the situation had been as acute as it is today. I don't propose to go into the Braun and Truan study any further than that at this point.

But certainly one could say that it would be possible to start, it earlier and we now have at least eight years exposure. It will take more experience of the working population so that it should be possible to get more reliable data then they were able to do.

This seems to be the first priority.

The second aspect appears to be the question of general community exposure. Here, as you heard yesterday, there is some evidence that pleural plaques do occur in the Quebec mining areas, but there is at least the suggestion that they are not distributed equally between the two principal areas. That they are seen in the Thetford area but apparently not, or much less frequently in the Asbestos area.

If this were true, this would be obviously a matter of considerable importance, and this point needs clearly to be investigated to find out whether it was true.

Moreover, even within the Thetford area where there are seven mining companies operating and a larger number of mines, there was again a suggestion that maybe even within this area plaques were not evenly distributed.

But these are impressions and they need to be put on a sound statistical basis.

The question of whether there was any real risk to health apart from the radiological finding of plaques in the surrounding population seemed to be a matter which was best left for the moment until one has some idea that there was indeed a risk to health in an important degree in those miners who were much more intimately exposed to asbestos. It seemed to be premature to spend too much time on the far less exposed general population, though this might, nevertheless, be a thing for the future.

I felt, nevertheless, that it was worth making a start on the question of pleural plaques.

There then was the question of whether mesothelial tumors occurred in Canada, to what extent they occurred in Canada, and the necessity, if they did, for some epidemiological retrospective case control study of cases of mesothelioma, across Canada and not confined to Quebec, in an attempt to investigate the same sort of thing that the other three or so case-controlled-surveys of mesothelioma have already done.

I felt that one could not take it as certain that these previous studies had necessarily produced the correct conclusion.

I do not feel, in my own mind, satisfied that they were free from bias and I felt that even if they were it was still a matter of some importance to see whether the same sort of picture was true of Canadian mesotheliomas.

And finally it seemed to me worth considering whether, whithin the far more limited secondary processing industries of Canada, epidemiological studies were needed. That I would certainly not give as much priority to. I think, first, studies in the mining industry, secondly, pleural plaques in the adjoining populations, and thirdly, patient-controlled study of mesothelial tumors.

I don't propose to go into the mesothelial question any further than to say that, some may be aware and some may not, that the Canadian Federal Government has a committee, a series of committees on asbestos and health, which Dr. deVilliers is much more competent to comme on than I am, but which has already initiated a system of reporting the mesothelial tumors across Canada for a period of five years past and from now onwards. Cases are coming in to a central point for assessment from a pathological point of view and that it is envisaged, I understand, that this series of cases will eventually be made available for epidemiological study.

So I think that there is every reason to think that that aspect of the thing will indeed go ahead, quite apart from any activity within mining.

So I would like then to confine my comments to the question of what I would think was necessary in the mining industry.

There is nothing complicated about it, it really amounts to nothing more than trying to do a prospective inquiry on the lines of the Braun and Truan inquiry but in a great deal more detail.

I say prospective and I would just like to make that clear that by prospective, I mean we would find a population on entry into the industry and would then follow it through. The fact that it will be done entirely in the past still doesn't prevent it from being a prospective study. It would be a prospective inquiry starting as far back as one could and coming up probably to the present day.

I have had an opportunity of brief survey of records available in the industry and of some review of the sort of numbers of men there are at the present time and who have been employed in the industry, of the kind of records there are, as to their outcome, their medical history, the radiological records that there are. Briefly, it seems to me that there are approximately six thousand employees at present in the industry and since its beginning in the eighteen nineties (1890's) probably somewhere around eighteen thousand employees have, at some time or other been, in the mining industry.

The largest concern, Johns-Manville, has had a great majority of these and has the best record. There are records available, some are somewhat covered by dust and would have to be dug out, but they do exist back to eighteen ninety-five (1895). I believe it is possible to get the names. Now these are very primitive, some of them. They do not necessarily entail more than the name, date of birth and address and job that the man was first employed on, on a very sort of cursory record of what the man did.

Nevertheless, I think these are potentially useful. I don't suppose it is necessarily worth the effort to go back to eighteen ninety-five, but it is not impossible to do so.

The records gradually improve as you go through, and certainly from the nineteen twenties (1920's) on they become quite respectable.

The other companies have a variety of records with varying quality and they go back to varying times. Some go back quite a long way, other mining companies, one or two, have a total exposure of less than ten years, but even so, in these more recent companies I don't think they should be ignored because many of their miners, there are miners and employees amongst them who were employed in other mining companies before that and for whom various clinical records exist.

So I would say one has a good chance of defining a population from the date of employment with the industry, making certain arbitrary rules, probably having a different starting date in different companies but defining the population and then attempting to follow it completely to the present day.

The assessment would then be in terms of, first of all mortality, to find out, are they dead or alive. If they are dead, what was the certified cause of death and what reliance can one put on a certified cause of death? Now that means, that everyone of those cases has

got to be studied in detail to find out the basis of the terminal diagnosis. We can't make up for the fact that perhaps only twenty or so percent of them hav had autopsies. It just so happens that the case that we found we don't know anything about it, but we can, I think, improve the quality or at least enable ourselves to know how much reliance to put on the cause of death by detailed inquiry into perhaps, the terminal illness rated at such special investigation as were done in life to perhaps produce varying grades of certainty as to whether there was a malignant disease of the lung or fibrosis of the lung or not. One would have to do the best one could.

Mortality, secondly, in radiological term. And here the records available are, I am sure, unparalleled.

Dr. Ken Smith has just pointed out an err in my document here, in that in the Johns-Manville company, on page four, I say that pleuroscopic examination of all employees was consigned since nineteen thirty-seven (1937), in fact it was since nineteen twenty-seven (192 and x-ray films have been preserved not since nineteen forty-five (1945) but since nineteen thirty-five (1935).

So we have an annual x-ray of all employe not only miners but of all employees in that company for thirty years, done the same clinic and under relatively standard conditions.

In Thetford, since Dr. Cartier started hi clinic in nineteen forty-seven, forty-eight (1947 - 48) ?

CARTIER:

Forty-five ('45).

McDONALD

Annual x-rays again exist in that population for a twenty year period.

Now these x-rays have been read as they went along and graded but in order to put this radiological assessment on a firmer basis and also in order to make it comparable with other surveys in the world, it is desirable that all these films, or at least a representative sample of films from each individual, should be reassessed by blind reading, by two, or preferably three radiologists using some internationally accepted classification.

I hope that Dr. Cartier and Dr. Grainger and perhaps one other radiologist could undertake this very large task, because it is an enormous task and yet I am sure one that is worth doing so that we would have a radiological classification.

Then there is the question of disability. This is more difficult.

I think that the clinical records of anything other than illness sufficient to disable a person from work probably could not be used.

I think that we could attempt to identify those men that, for whatever reason, were disabled from work, and establish the presumed cause of the disability.

And then finally there is the question of pulmonary function. And here I have had rather further thoughts in the subject since submitting this document, in that I feel that an even greater effort should be made to carry out pulmonary function tests as you suggest that this should be done on this continent. But I think they should be more extensively done on at least a sample, a representative sample of workers with varying degrees of exposure and probably using tests of varying complexity. That we could probably do certain tests in the mining areas on a largish number on a certain level and I would envisage bringing small selective sub samples of those people to Montreal for really exhaustive pulmonary function tests.

So we could assess them, in other words, in mortality, radiological changes, functional disability.

But there is the all important question of exposure. Starting from the beginning with the first index of exposure is, of course occupation. Simply: what did the man do?.

Secondly, from nineteen fifty (1950) onwards there have been dust counting studies carried out systematically at least annually in all the mines and mills of the Q.A.M.A. companies. These have

been done, using the midget impinger method, and though there is criticism of this, they have in fact all been done by the same man since the beginning and they have been done in certain specified points in the mills and mines. The same point year after year.

So we have some objective measure of dust.

Nevertheless, before nineteen fifty (1950) we have no measures of dust systematically carried out and the problem arise: how to grade dust exposure before that. And I would think that what one had to do was to create a register of all occupations in all the mines for all the years of the study. (This would be quite a big job) and then systematically to try to reach a quite simple classification of these occupations by year in probably some very simple provision of dust incidence. It would have to be very simple.

Now the question is how could you get it? Now the only way of getting it is by opinion. This may not be possible.

I would envisage taking thirty or forty old timers and getting them to assess these jobs on this classification. Not only the jobs before nineteen fifty (1950) but the jobs since nineteen fifty (1950) and then to compare their efficiency in judging dustiness for the periods where we have objective records. And from this series of thirty or forty men to try to see whether there are any people who appear to be good judges of dustiness in terms of memory.

Now it may be that there aren't any and then that's that. But if there are, I think it is worth making an effort to use their judgment. We don't have to use it in the final analysis, but we can try to use it.

There is then, in addition to the question of how much dust, the quality of the dust. We consider first of all, of course there are different mines, different mines, you could practically, different distribution ranges of dust, of fibre, colour, of fibre size and I would think that also, in the analysis, should be taken account of the identity of the mine, the type of fibre they use, that they produce and year by year a measure of the distribution of fibre size of that particular company.

The follow up then becomes the next questior to find out are they alive or dead. And if so, if they are dead what did they die of.

Now the possibility of doing this at all is greatly increased by the peculiar nature of this population : The French Canadian population in a very rural part of Quebec. A part where the populati characteristically does not move and doesn't want to move, even when they ar layed off work they still don't want to move. And where the people know a lot about the movements of others.

So I believe that it should be possible, mainl by local inquiries. And in this it is most important that we have confidence

and I refer to this again, this study, the fullness of the follow up depends a great deal on collaboration of the working people. They must feel that this study is important to them otherwise we won't get the information.

But simply by local inquiry I would hope that we would find out what happened to most of these people.

I can't say how successful it will be until one tries, but I think it should be a good hope.

But on top of this, of course, there are certain other sources of information. Some of the companies have got various insurance plans, pension plans, various systems of keeping contact with old workers who have left the employ and I think that through these various regions it should be possible to trace a very high proportion.

It will inevitably be yet a final proportion whom nobody knows about.

Now we don't have the advantages here of the British medical system of records, of being able to trace people through official records, but in Canada in nineteen sixty-four (1964) and sixty-five ('65) there has been carried out a complete registration by the Unemployment Insurance of all adults working in Canada.

SABOURIN:

I think Mr. Chairman, we could give a rest of two minutes to the typist?

McDONALD:

Mr. Chairman, maybe I am speaking at too great length and too great detail.

LINDELL:

You want to keep your eyes on Dr. McDonald

McDONALD:

If it is too great detail please say so.

CHAIRMAN:

No it is perfect.

McDONALD:

Yes there has been a registration of all adul workers throughout Canada and from inquiries through the Commission in Ottawa, I have reason to believe that providing this particular scheme has the approval of the Department of National Health and Welfare, it should be possib to use these records.

There is the question of confidentiality, but it sounded to me from the preliminary inquiries that providing the government said this is something that is justified, that we would be able to do this, and this might be a very valuable way of getting at what had happened to the five, ten percent that have disappeared. At least it might indicate whether or not they were alive or dead in nineteen sixty-five (1965).

So one way or another I am fairly optimistic that we should be able to do a very good follow up and I would emphasize from an epidemiological point of view how important this is.

Every person who is missing is potentially a case of lung cancer, potentially anything and in fact, the one good reason for being missing is that you are dead of course, and it is a very important that we put an awful amount of effort into trying to trace, if possible, as near as possible to a hundred percent.

There will then be this question of attempting to improve the quality of the cause of death. There is no mystery about this. It is simply that we have got to do our best. We get as close to the evidence as we can, try to see the final clinical records, try to get information from the doctor who attended the case. This is all very time consuming and we then have to grade our diagnosis as to whether they are based on autopsy or whether they are based on, shall we say, clinical records in hospital plus radiology, whether they are simply nothing more than a death certificate.

This will be important for analysis and also important for comparison with the findings of others. In particular, one comparable study that is going on presently in Britain where they are doing this.

So really that is essentially the type of study that I think is necessary from the point of view of supervision.

At McGill in my department I would be very happy to supervise the epidemiological aspect. We have been very fortunate recently in adding to our department a very close friend of mine, a professor of statistics who, I am sure, we have had no chance to discuss this, I am sure will be very happy to undertake and to advise and help in the statistical aspect for which, of course, there are considerable statistical problems here.

And then finally from the pulmonary function aspect we have in Dr. Margaret Becklake and Dr. David Bates at the Cardiorespiratory Unit at the Royal Victoria. They are very interested in the pulmonary function work and they would be very happy to advise and supervise and help in pulmonary function.

In conclusion I feel that through this we therefore have a major and immediate epidemiological study in the mining industry, which I estimate, which would perhaps take about three years, though Dr. Gilson says I am being optimistic but there are other related studies.

I would feel that it's desirable to set up some kind of small research epidemiological unit to carry out this study and so far as possible use any spare resources to explore the possibility of other services.

It is inevitably wasteful to do only one epidemiological study. It would be very much better if this unit could be

given some freedom to explore things outside the main study. Inevitably you can't keep everybody occupied all the time on one particular job. It is much better if they can also, shall we say, start off another study of pleural plaques. It might even be that some aspects of the mesothelial retrospective inquiry might be carried out partly by the same group.

I think that summarizes it.

CHAIRMAN:

Thank you very much Dr. McDonald

Now your paper is open for discussion.

Yes Dr. Noro?

NORO:

Mr. Chairman, I read with great interest the research plan of Dr. McDonald last night and it is very funny that without knowing what you are doing here in Canada we planned, just half a year ago, a similar research programme in Finland.

I think it will be somewhat easier for us to perform our study because our mine is rather small, having only two hundred fifty miners. And we are following just the same lines Dr. McDonald has explained here.

Even that is very interesting to see how much it cost a year. Our estimated annual budget was fifty thousand dollars and yours is forty-five. That is cheaper here.

And we are trying to continue this work during five years because I feel in three years it might be too short a time.

So we feel that having so few miners, we have better opportunities to look through the population around the mines, because until now we have found about a thousand three hundred pleural plaques in the population living around the mines and it would be very interesting to follow, say five years from now what will happen to these people having this kind of change in the pleura.

Anyway I feel that it might be useful to have very good collaboration with your Canadian research workers and plan perhaps a bit more in detail. Also that we can compare our results.

Another interesting thing might be that our asbestos is anthophyllite asbestos. There we might find something else or some other research, for instance pleural plaques which are so common in our environment.

Anyway I feel that this is a very well planned research programme and we would be most happy to be in collabroation with you Canadians.

CHAIRMAN:

Thank you Dr. Noro.

Dr. Wright?

WRIGHT:

I was a little disappointed that some other studies which could be done parallel to this one are apparently not being plan

A question of chronic bronchitis which has been raised a number of times by the British group, seems to me, could be included here without much increase in effort, at a time that you bring any samples for pulmonary function tests which you could use that same sample, if properly chosen in a parallel way to, at least to do a fairly good questionna and perhaps some sputum measurements in order to get some idea of the frequency of chronic bronchitis by history. And you are going to have measurements so you're going to have some information about chronic constructive phenomena. I would hope that you would include that.

Now this, of course, will pose another problem because now you will have to go outside the employees to get a control group and I really think that you should think twice about leaving out an outside control group in regard to almost everything that you do. Ultimately you will always have this as a weak place in the study. I am sure you are aware of that because you bring it up in your folio. And I would urge the support for this sort of thing.

The problem of tuberculosis is apparently one that periodically is raised and this could also be looked at. It will requi some outside control group.

You don't list the sort of pulmonary measurements you are going to make. I don't know whether this will be the subject of further discussions, Mr. Chairman, but it would be interesting to know what is proposed in this study.

I would say, on the basis of our own personal experience, we have had no trouble carrying out the most detailed of studies, if you desire to do them. And it can be done at the working place. Equipment is transported much more readily than workers and I wouldn't stint on what you are going to do in any way because you will find it impossible to do it.

McDONALD:

Mr. Chairman may I just

CHAIRMAN:

Yes.

McDONALD:

make a comment?

First of all I do agree about this. This is really an example of the fact that with you having a unit there are a variety of additional things that, no doubt, could be done and should be reconsidered.

I was trying to concentrate on what were the primary objectives but you do raise one question that I forgot to comment

on and that is this question of controls in relation to the main question of the asbestos as it is associated with asbestos. I thought very hard about this question.

Obviously what we would like to have is a control group to see how the asbestos workers differ from non asbestos workers, but I came to the conclusion, having looked around as hard as I could, that there just wasn't. That these people in Asbestos and Thetford are subjected to a way of life and to a standard of medical supervision, radiological supervision, even autopsy attention, that is not like any other place that I could find.

I am stillopen to suggestion and I felt that anything other than a true control group would be worse than useless.

This still doesn't exclude the possibility of perhaps at a later date in the light of findings, it seems very desirable to compare this with some other population, consider, say, some other mining group, Nova Scotia Coal Mining and Thetford Metal Miners. But there I was worried about specific hazards that might be associated with those occupations and basically the plan that I am making depends upon comparison within the industry. And therefore this question of attempting to identify exposure is all important.

Essentially what I am trying to do is to compare people who have different levels of exposure to asbestos by controlling within. It would be nice to have an external one but don't exclude the possibility of adding it on.

CHAIRMAN:

Yes?

CRALLEY:

We have a study of some magnitude going on in the United States, an epidemiological study of all the asbestos product manufacturing industries.

After looking at this outline I know there is one, the procedures of one will parallel that of the other very very closely. It is very heartening for me because it gives an increased credence to the workability of this approach.

Our study has been going on for two years now. It covers asbestos textile, asbestos cement products, asbestos friction materials and asbestos insulation materials. We have not planned to cover the mining in our country, at least not at this time because there is too small a population to follow this approach.

Our first approach is to gain all the detaile questions we can on the environmental exposures, within the categories and follow that with detailed questions, information, with each employee within the plant.

We have covered between thirty-five hundrc and four thousand workers to date. We plan to cover, eventually, ten to twelve thousand, more or less equally distributed among these four groups. And of course, we could go eventually into other areas that might be indicated

Our first effort has been to define in detail the nature of the exposure, not only to asbestos, to the other superimposed exposures, which within themselves are potentially injurious, which may be additive some way or another. We don't know. Ours is in the manufacturing of it, not in the mining. Not only that because the asbestos must be shipped in and most of it comes from Canada, we find it necessary to do a great deal of medical research defining the quality of the fibres coming in. That would help most definitely, in fact in your project, and I think we have many areas of cooperation and inter-relationship here.

We are defining in detail the nature of exposure and we are getting information from each employee on identifying this person whom we follow through his lifetime. Information on where he has worked before he went into the asbestos industry and since.

Complete smoking history and other. In addition to this we are including an area of, we call it hobbies.

SABOURIN:

Hobbies?

CRALLEY:

Hobbies.

We are finding that, we are finding that increasingly somebody has a hobby in the basement. It may be wood-working, it may be metal working, it may be modelling of some sort, log counting or anything you wish. All of them are hazards.

The exposures down in a basement are things that industry would not tolerate in any instances. This person would go on his job and he would observe all the precautions necessary for safety. When he gets home he forgets about this. And we are getting as vigilant as we can on the hobbies. This group includes gardening and everything where you have exposure to toxic material outside of the eight hours on the job.

We will follow these courses through the lifetime and then we will follow the pattern established here, of cause of death, lung density patterns and then tie these back then into the overall data on the individual and his environment.

We will later on develop these other areas, clinical, some experiments are going on now and Dr. Pendergrass will discuss that.

Dr. Vorwald discussed some of our areas in pathology and Dr. Wright will in physiology. They are all consultants to our study.

We found out that we can go back ten to fifteen years exactly, backwards with rather reliable exposure data. In many instances, we can, in some cases, we cannot. This will be study until we have the equivalent of lifetime exposure on a statistical number of people that we will carry this on prospectively another ten, fifteen plus years. But we would get information from time to time and see what is happening to trends and say it seemed like the studies have been listed here parallel and at least supplement the others very closely.

We must have common methodology and procedure so that the data we have will mean something to the data the other people need and vice versa.

So I am very heartened and I say we will very strongly endorse this approach and we like to cooperate in areas we can.

CHAIRMAN:

What physiological studies are you doing on these people?

CRALLEY:

That has not been developed yet? We are first thinking of defining in detail the environment. We need a questionnaire or the information development and cohort. Then we are going to in the way you suggest here and we study, we will study in depth these other areas, a selected group.

SABOURIN:

I hate to interrupt but I represent the league of rights of women.

It should be coffee time to give a well deserved rest, otherwise we won't have accurate reporting.

CHAIRMAN:

Yes.

Excuse me but perhaps in French.

We still have....

HOUBERECHTS:

En vue de, je crois, répartir également le travail; étant donné mon très pauvre anglais, je préfère parler en français

Je trouve que l'étude dont a parlé le Docteu MacDonald, est une étude extrêmement intéressante, au point où nous en sommes.

Il s'agit de découvrir la relation entre l'exposition aux poussières et l'état de santé.

Et monsieur MacDonald propose, pour cela, d'utiliser toute la documentation qui est disponible. Il se propose d'examiner les dossiers médicaux de douze ;mille, dix-huit mille ouvriers, tous ceux qu'il trouvera. Il se propose également de rechercher dans les entreprises, les mesures d'empoussiérage et, par conséquent, il s'adresse au passé. Le temps qu'il faudra pour cette recherche, c'est le temps qu'il faut pour la terminer, puisque ce n'est pas une recherche sur une évolution d'ouvriers qui sont exposés aux poussières.

Seulement, il y a un certain nombre de remarques qui sont à faire : Tout d'abord que, pour les années les plus anciennes, puisque monsieur MacDonald veut repasser aux années '35, '40 et '45, et bien, là, je crains que la rigueur des renseignements qu'il pourrait avoir sera assez limitée. Tout le monde sait que dans le domaine des mesures des poussières, on a fait des progrès considérables du côté médical; on en a certainement fait également.... on est déjà sûr que les poussières respirables sont celles qui sont inférieures à 5-microns, en 1935; tout cela ce sont des questions qui peuvent laisser planer un doute sur le début de cette recherche

Deuxièmement, je pense qu'au cours de la recherche, les objectifs vont se modifier, car on va vraisemblablement déjà découvrir, au bout de la première année, que certains groupes sont in-intéressants.

En relation avec ce point, je voudrais demander s'il n'y a pas des renseignements qui sont disponibles dans l'industrie et qui s'adressent à des groupes plus petits. Peut-être y a-t-il des pièces pathologiques sur lesquelles on peut faire des coupes anatomo-pathologiques pour se rendre compte - sur des groupes plus petits - de ce que peut provoquer l'inhalation, donc, de l'asbeste. Ceci, ce sont des renseignements qu'il serait utile d'avoir également.

Ensuite, pour le futur, il faudra que cette étude puisse conclure au choix d'un certain nombre de groupes d'ouvriers, de telle façon à ce que l'on puisse définir le danger de certaines fonctions spécifiques dans l'industrie de l'asbeste. Il est certain que toutes les fonction ne sont pas également dangereuses. Dans les charbonnages, il en est ainsi également : si l'on définissait quelles sont les fonctions spécifiques où le danger existe, on pourrait immédiatement conclure que c'est là où il faut faire de la prévention et surtout, de la prévention technique.

Je vous remercie, monsieur le Président.

PRESIDENT:

Merci, professeur Houberechts.

Maybe I should try to translate partly what you you have said. So, since I took a few notes here.

He said that the first thing that you should do i try to establish a relation between the exposure of the workers to dust, and at this moment now you propose for that the utilization of the available documents at your disposal. Also, you could use the dust counts that are available and then by that you are trying to study the past.

The time of the study will be ?

McDONALD:

It will be the time it needs, because you do not have to study a growing evolution. You study something which is completely past.

CHAIRMAN:

And then he says that for the past, probably the exactitude of the documents you have is subject to great caution, let us say as far as the measurement is concerned. They don't have the same method today, the same apparatus and also.

McDONALD:

The same apparatus.

CHAIRMAN:

that twenty years ago they had, let's say - I think you did not count much?

SABOURIN:

Five microns.

CHAIRMAN:

Five microns. Well

SABOURIN:

It was five microns but he said, "I doubt whether it should be considered today".

A five microns failure today could be accepted as meaning what it was supposed to mean.

CHAIRMAN:

And then with the same thing he says probably much progress was made also in medicine and when we judge today the criteria we have now applied to the work, it is difficult to make a good study.

He says also that along your study, you wil probably modify your objectives because after a year or two you will find tha some groups are more interesting than others.

And then he proposed to use in the study what the industry may have as far as, not only in the way of documents but, let's say data, on chronic bronchitis.

I suppose you mean all round pathology. If they have not only the slides but some other material that could be used.

Also for the future he would like to propose or be interested that you try as soon as possible to make the choice of the group of workers who have functions, who are employed in more dangerous parts where the hazard of exposure is greater than some others and then try as much as possible to relieve that, to relieve, to change the conditions. I suppose that. . .

SABOURIN:

And he concluded by saying that the survey should take the time it warrants and not be lead necessarily by any specific duration.

LINDELL:

Yes, and as far as dust count is concerned, Dr. MacDonald said they have been taken by the same man since nineteen fifty (1950) or by the same method.

I am not concerned about that aspect, what I am concerned about is that the dust has changed

In nineteen fifty (1950) when they started to take the dust counts the quality of the fibres that were produced at that time, w made for an entirely different product. For instance, the highest quality fibre at that time contained rock dust, or fibre dust, up to a minimum cleanliness of thirty-five to fifty percent and that same high quality fibre today is down to the twelve to fifteen percent category. And in the product that was sold the most, let's say group four, in nineteen fifty (1950), the dust was as high as fifty five and sixty percent. Today this is in the twenty-eight to twenty-nine per cent dust count.

Also in the fifties ('50), take in every mine even in Asbestos, the percentage of dustiness was seventy-five percent , whic was very high, now it stands at twenty-five percent, five hundred thousand tons of this material was produced. So it isn't the method so much as the change in quality of the dust that might have quite a bit of influence.

CRALLEY:

During the meeting of the A.T.I. in Thetfor I requested of the mining companies many samples of the products of the same grade, back as early as you can get, even to nineteen forty-five, in the same grade, so we can do the same type of analysis and get a clue as to what, whether there has been a change or not. This is very important.

CHAIRMAN:

Thank you Dr. Cralley.

Yes, Dr. Vorwald?

VORWALD:

I just want to say, very briefly, cite a few of the difficulties which have been confronting us relative to two rather comprehensive epidemiological studies which we are conducting on certain industrial populations and this study is being conducted in, one at least, in the epidemiological department in the school of public health in the University of Michigan, in addition to our own department at Wayne State University.

The one problem, one area of epidemiological study concerns about ten thousand men in, currently in the study. They are so-called machine tool operators. And we divided these into a number of groups but I would like to point out, only, make a point on certain things, that we have been confronted and somewhat disenchanted with first the change in the quality and nature of the cutting oil compound that has been used during the course of years. Our major difficulty is in the era prior to nineteen forty-seven (1947). If we go back as we are to the year ninetee thirty (1930), it becomes increasingly difficult. During the war years, we are practically unable to identify the nature of the cutting oil which was used. There are two types. One has come in only recently.

So that is a point which I should like to make and it has relevance.

It seems to be the quality and the nature of the asbestos to which these men were exposed.

Secondly, the point I should like to make is that prior to nineteen forty-seven (1947) and even extending to nineteen fifty-five (1955), we have had extreme difficulty in identifying the concentration of the aerosol or of the challenging agent to which these men were exposed.

In large measure there is little or no sound evidence relative to the atmospheric concentration to which these men were exposed and certainly less when we deal with the size of the particles to which these men may have been exposed and have inhaled.

The third point I should like to make is the duration of employment and the precise employment that we are dealing with.

An industrial population, a group of individuals who belong to a union which is relatively closed we selected because of a number of reasons. But we thought that this group would be subject to clear definition as to their employment and their duration of employment in a given situation prior to nineteen fifty-five (1955) and certainly during the war years, from nineteen forty (1940) approximately to nineteen forty-seven (1947) or forty-eight (1948).

It has been almost impossible either to use the badge number, the department number or the job number of each of these men, which they know, and identify them from it and the precise nature of the work they did.

If the individual should still be alive and if he is fortunate enough that he can remember, he can say although he was identified by job number and department number to be working as a machine tool cutting operator, that he in fact, for a relatively short period of time did indeed work as a machine tool cutting operator and that he had a variety of exposures, some of which may have reduced pulmonary impairment.

The fourth point that I should like to make is that this programme, although relatively confined to a population of workers in an industry, presumably belonging to a relatively closed union, that this is indeed a costly venture. Just from the pure epidemiological point of view and all that it means. And currently in both of these programmes there are approximately fourteen or fifteen people occupied full time: a biometrician, an epidemiologist and other things to save some of the pathologists' time to review the evidence relative to the historical evidence, relative to a surgical procedure which may have been done or an autopsy finding.

We have been confronted with going back to the hospital, the various hospitals gathered in and around Detroit and we haven't got far afield yet because many of these individuals have yet to find out why they were in a hospital and for what reasons, and if there is tissue available.

And we have been confronted with the situation that relatively few of their pathologists will retain their material for a period longer than ten years.

And these are some of the problems. But first the changing of the compound, the concentration of the aerosol, the duration of employment, the precise nature of the employment and the cost involved.

I should like to speak a little more about the last item.

I don't believe that in our country at least a comprehensive programme of this sort can be done for fifty thousand dollars annually. I don't believe it.

McDONALD:

Dr. Vorwald said and what others have said, in so far as difficulties of defining the job, defining exposure, getting clinical information, every one of these difficulties I completely agree with I have no doubt that they are very difficult.

The answer is that, I think, in any epidemiological study the accuracy, the reliability of the study is just simply proportionate to the effort we put into trying to make these estimates of these variables as accurate as we can.

Now if we could say, "right, this isn't very accurate. Let's go and do it somewhere else, where it is accurate," that would be fine, but what we have got is a situation which we cannot repeat. We have fifty years' experience in this industry, an opportunity to draw such conclusions as we can from the evidence that is available and therefore my feeling would be that we must, somebody must attempt to do his best with this. And as I was saying just now, I think it is a question of effort.

I would like to refer back to Dr. Houberechts. He commented on the unreliability of early dust counts and of early dust estimates with which I agree completely. He also referred to the possibility of trying to limit a study to special groups from whom we would get more useful data.

Now I believe, so far as the study that I outlined is concerned, there is no group that we can say is un-important. It is equally important to follow theoffice workers as the men with heavy exposure. The point is that we are trying to make comparisons. It is just as important to follow them all and this is particularly true in view of the difficulty of getting outside control.

I think that also, regarding cost, I am sure that in this one aspect I have been very optimistic.

CHAIRMAN:

Thank you.

Yes, Professor Sadoul?

SADOUL:

I will try to speak first in French then to summarize in English.

Je pense que l'expérience que nous avons eue sur une étude rétrospective faite chez les mineurs de fer de notre région peut être utile.

Nous avions la chance d'avoir des renseignements très précis et des dossiers extrêmements bien tenus. Tous ces malades avaient été vus dans mon service hospitalier au cours d'une hospitalisation, où nous les avions interrogés avec précision sur leurs antécédents professionnels. Nous avions des examens cliniques, des examens fonctionnels très précis.

Et nous avons voulu voir quelle était la destinée de ces malades, cinq ans après - cinq ans c'est une courte période.

Nous avons pu déterminer, pour quatre-vingt-quinze pour cent d'entre eux, la cause des décès, mais nous avons été malheureusement désappointés par le peu d'informations qu'en définitive, nous tirions de tout cela. Alors que nous pensions que ces milliers de dossiers allaient nous fournir une information extrêmement importante, je dois avouer que, finalement, ils ne nous ont donné que deux ou trois informations intéressantes.

Certes, ces informations étaient importantes, puisqu'elles nous ont permis de voir une fréquence très hautement anormale des décès par "coeur pulmonaire". C'étaient deux points très importants.

Mais, comparativement au travail énorme qui avait été fait, comparativement à la qualité des renseignements qui avaient été rassemblé sur ces malades, c'étrospective nous a paru une montagne qui donnait naissance à une souris; la souris était très vivante, ou très intéressante - ou plutôt les deux souris - mais c'étaient simplement deux souris.

SABOURIN:

En quel sens cela vous a-t-il tellement désappointé? Vous ne nous avez pas dit.

SADOUL:

Parce que j'espérais trouver des arguments pronostics extrêmement plus précis, savoir par exemple, obtenir les corrélations sur la durée de survie d'après tel ou tel résultat fait, obtenu cinq ans auparavant. Si vous trouvez telle caractéristique clinique, telle caractéristique fonctionnelle cinq ou sept ans auparavant, chez un malade, est-ce que ce malade a plus de chances d'être parmi ceux qui meurent dans le délai de cinq ans ou de sept ans? Ou, est-ce qu'il n'a pas plus de chance?

Nous espérions trouver quels étaient les signes les plus "reliable" - pour employer un anglicisme - que nous obtenions pour avoir, pour estimer le pronostic.

Well I think retrospective studies are sometimes a little bit disappointing but I think it is well worth while to do them because we can get, by such ways, very good orientation for future research. But/comparison in to the huge amount of documents you have, you get only very particular information. Of course, I don't want to say little information but it is on very small points and sometimes it is important. For instance, this retrospective study I made recently in our iron mines. We had very good records of the iron miners and we looked at the destiny or the follow-up of these iron miners during a five year period and we were able to get two informations.

One was that there was abnormal frequency of lung carcinoma in this given population and the other was the abnormal frequency of chronic cor pulmonale. But we were not able to find any close correlation between physical results, clinical results or radiologic X-rays appearances observed five years before the death and the time of survival for instance.

So I think it is worthwhile to work on records. But don't hope too much from these results.

I think it is worth while to work on records only for, not a short period of time please, because anyway it takes time, a very long time, but only for a given period of time and after. But don't try to obtain more sophisticated results, more elaborate results.

SABOURIN:

Do you mean don't expect too much?

SADOUL:

Yes.

ENTERLINE:

I think that one of the reasons why the results are sometimes inconsistent is not because the records are bad as much as it is because we don't consider all of the factors that cause the results.

We concentrate very often on occupation exposure which may be very insignificant in terms of producing cancer or whatever you are looking for. But I don't think it is because of bad records. I think it is because we don't consider enough of the variables.

SADOUL:

I agree with you.

Je pense, si monsieur le Président, veut me donner la parole un instant encore, je pense justement que l'intérêt de ces études rétrospectives est de trouver quelles caractéristiques doivent être étudiées pour les études futures. Par exemple, dans le cas de cancer du poumon.

Alors évidemment, il sera nécessaire de prendre en considération les habitudes au point de vue tabac, n'est-ce-pas, tandis que, aussi bons que soient vos "clinical records", peut-être que vous n'avez pas pris attention et que vous n'avez pas noté quelle était la consommation de tabac...

SADOUL:

In your retrospective study or in my metabolic study I think it is worth while to go in with retrospective study in order to select the exact characteristics you want to take into account in the other study, in the following study.

VIGLIANI:

I am not as pessimistic as Sadoul is about the value of retrospective studies. Provided you don't pretend too much from these studies and you have reliable records and above all, as far as asbestos is concerned, you have the X-ray films of the people concerned. As a matter of fact if we want to do only prospective studies we have to wait too long. We must know at least what kind of problems we are confronted with and what is the magnitude of these problems in the different industries or areas. This can always be a key to a prospective study, taking into account all the limitations which are inherent to this particular type of study.

We have done retrospective studies of about seven hundred and fifty certified cases of asbestosis who received compensation , in two regions of Italy. And of these people about one hundred and eighty died. These retrospective studies went back to nineteen forty-three (1943) covering approximately twenty eras, periods.

I think also that prospective studies will give you much better yields. In any case, our results have been not so bad as we thought from the very first beginning. Also because the patients were not all seen by us but were seen in different hospitals and so the records were not always comparable but at least the cause of death and some features of the disease could be rather well studied.

And in any case very good hints could be obtained for the programme of a prospective study, just as Sadoul has pointed out now.

NELSON:

In a modern asbestos mine only about ten per cent of the men are significantly exposed to the dust. I get figures from the mine during those operations. So, if we were to study a group as of now, we would have essentially ninety per cent control and ten per cent non-control. Now thirty years ago or forty years ago I am sure the percentage of men employed who were exposed to dust in mines may have been thirty per cent, or forty per cent. But this brings me to my thought. Would it not be possible, since time is somewhat precious and money will be too, would it not be possible to get some meaningful information if one took as the first phase of this proposed study a group classified as the most heavily exposed (and you could set a number perhaps), and then an equal group of non-exposed individuals from within the industry and then compare the two to see whether or not anything emerges?

CHAIRMAN:

Dr. McDonald you want to answer this question?

McDONALD:

I think it is a perfectly reasonable suggestion. I think that the problems as I see them are that (until the problem of estimating exposure is going to be a big job anyway), it won't be easy to pick this limited size group of heavily exposed workers until we really get down to classifying the population in terms of exposure. I agree that it should be possible. We wouldn't need to go through the whole thing, we could probably cut it short a bit and define two groups. One heavily exposed and one not.

My point is that I feel that if we did this, we would miss so much of the valuable information that is undoubtedly available.

There are possibly variations between mines. Even with the eighteen thousand, and we shall not be able to study all of them, shall we say we get down to about twelve or so. By the time we break these twelve thousand down into different exposure groups, into different mining groups, the number we shall have available will be none too big. In fact not big enough, not for all the problems we have to look at.

I would say it is uneconomical to spend time not to do the job properly.

SABOURIN:

I should like to point out to the dust exposure conditions in Thetford Mines, which could still be prevailing to some extent.

And these dust exposure conditions were such and to some extent might still exist somewhere in some particular location.

I am not sure that it goes without saying in my mind that therefore the survey must embrace all of the possible occupations in the mills, in the factory where there is a factory around as well as the environment.

For let me say, yesterday before the committee I had a chance to say, and I think it is quite interesting, the accusation that is leveled, following say, the New York meeting, implies that the presence of some asbestos bodies in the lung, coupled with the presence of a malignancy would lead to a most probable conclusion, let's say, which would be not only coincidental in its causality but would be that pathological condition as a whole, the sequence of the absorption at some time of those asbestos bodies.

Now in Thetford Mines in the last fifty years up to about fifteen years ago the conditions were such that the visible dust particles in the air in the locality were in such a proportion that an automobile parked at the door of a hotel in the town away from the mine, would find itself covered with a flaky snow-like coverage within an hour or so. To a point that you had to clean your car if you wanted to see what was the colour of your car under that coating.

I might be exaggerating a bit but not so much.

And therefore it seems to me that if these conditions existed and I recall Dr. Gardner is considered on this continent a most authoritative source of information in this field and was head of our school at Saranac for a number of years, I remember him saying, "If this is visible outside the mill in the locality, well any pair of lungs in this locality upon autopsy, upon some anatomological study, would disclose the presence of asbestos bodies, including an autopsy on a six-month baby", so convinced was he that there was a very heavy concentration of invisible particles in the air all around Thetford.

All on account of the pleural plaque problem and the whys and wherefores given by Dr. McDonald and by Dr. Wagner and the comments that we read of the experience of the South African region. I think it is proof that the survey, if the survey is to take place, might be quite expensive as Dr. Vorwald pointed out.

But very respectfully, I would say that if a survey is to be made, it must be such that the book will be complete. The results might not be very considerable but they will prove to the whole world and to the powers that be and to the population and to the governments and to everybody and sociologically we will have a chief power, a primary duty to do all that we can to find out the causes of such conditions that are being incriminated.

LINDELL:

Well, speaking on Mr. Nelson's point first, that if you take a number of the people that are supposedly in the occupation that were previously dust exposure jobs.

Since nineteen fifty-seven (1957), fifty-eight ('58), I would say that the new mills, mines and also including our operations, (it is as recent as fifty seven ('57), that you move into an era in which probably, and we believe it is so, there are no hazardous dust exposure positions. We think that you will not find anything in the last five years in these particular locations. But we need to include the new mines and mills in the study because there will be the people that have, some of them, been employed in the other operations previous to that. So that in order to cover them all you will have to take the old as well as the new.

Relative to Dr. Vorwald's remark, he is not against this study, he is just pointing out some difficulties.

VORWALD:

I have an industrial population, and in the final point I meant that it is going to take a long time for a good retrospective point of view study. All the reports are scattered everywhere. You can't confine it to the medical department.

You have got to go to the employment department, the personnel department, the insurance department, the compensation department. All of these departments must be scrutinized for evidence relative to the retrospective epidemiological study of the current population.

When you can take the man and interview him personally, results present relatively little problem.

I did not wish to imply that I am pessimistic in any way about this, I am optimistic, providing it is done with precision and detail and recognizing that this is going to take time and adequate support.

WRIGHT:

Mr. Chairman, I would like to make one other comment that I am sure a number of others here at the table could make just as well.

In Dr. McDonald's proposal there is one very short paragraph in regard to what is truly a prospective study and I would like to speak in support of this very strongly with the desire that it be initiated at the beginning and not later.

I think that the truly retrospective study starting with a new population and observing them over a period of X number of years is important as a means of testing the effectiveness of what it is you do.

There will be no question but that as time goes by no matter what results you obtain from retrospective study there is going to be some question about it and it will only be answered in terms of a well done prospective study.

The time to do it is now I think, for the following reasons which I will give you, and this is largely a psychological one. If as a result of a retrospective study you find rather meagre evidence of impairment or injury of workers, there is a natural tendency on the part of the management to not quite understand why you need to do further prospective studies. I can understand this.

Secondly, if the worker is in Canada as he is in the United States, the first time a study is introduced he accepts it quite well when we need him to do something. At a subsequent time the question is always asked, "why do you need to do something different now?"

By urging the prospective study to be done at least at the present time it is primarily because of the psychological aspect. It can now be much better introduced than it can at a later date and I feel certain that you will want a good prospective study at some time.

I am in agreement with Professor Sadoul that the retrospective studies tell you what you need to do with the new things but I think we already have information as to what we need to do in retrospective studies.

Quite a bit is known about the effect of asbestos fibre and we could introduce something since at the present time a retrospective study could be dropped if on the basis of a prospective study you don't need to do them. But I think something should be done in this regard. I support Dr. McDonald very strongly on this about which he really hasn't said anything.

CHAIRMAN:

You want to say something about it now Dr. McDonald?

McDONALD:

I suppose I have been trying to keep, in spite of what Dr. Nelson said, I have been trying to keep this proposal to the minimal.

But I agree completely that I do refer to the fact that there is a new mine in Newfoundland with extremely good opportunity to start a careful review of a group of workers who in relatively recent times have had no contact with asbestos whatever and who have come into a new mill. I have never been there but I believe it runs on the best modern lines.

LINDELL:

If it isn't I would be really surprised.

McDONALD:

This is a group that seems to invite careful supervision starting today and moving indefinitely. And, of course, I agree that if we are taking a group of workers when they started work in the past and then following through to the present time, obviously it shouldn't stop today. I agree with you, if you can do it we should continue. This can be a starting point in the operation. We should continue forwards, perhaps not on them all but certainly carrying a study of these people further relating it to the much more precise informatinn that we can now obtain about them. But it does cost more.

CHAIRMAN:

Any other comments?

VORWALD:

Of course in your prospective study you are not excluding the retrospective study. Certainly a prospective study should be done but let us all recognize, as Vigliani said, that it may be a long time before you really answer the questions, if you ever will answer them, purely on a prospective study, particularly in the area of carcinoma of the lung and mesothelioma which may necessitate a prospective study for thirty years.

WRIGHT:

That is not very long. Twenty years ago all the questions that are being raised, most of them, could have been answered if we had had a prospective study at the time. And it is time to get it started. You and I won't be here, but someone will want the information.

VORWALD:

Sure, I am taking an optimistic point of view. I think the prospective study should go on but I don't believe that the retrospective study has to be excluded.

SABOURIN:

Dealing with the town of Asbestos and Thetford these are the only two centres in the whole of our continent where the population has been there, living out of the salaries or wages most of them earn at these mines. It means virtually the only source of employment. So there is a stable element there in that populatinn which you can examine, where you don't have those conflicting, like Detroit's epidemiological studies. However, in Asbestos, most likely it will be a sister or a brother who has been engaged in this operation.

LINDELL:

Here is another example : Monro Mine, in Northern Ontario. It closed a year ago. It operated for fifteen years.

That was the only asbestos mine in Ontario. Anyway if you are going to make a study of those people, that ought to be done pretty soon because they will be scattered all over Ontario and Quebec in a not too distant future.

GILSON:

I should like to support this proposal that McDonald has made. It seems to me that this is an unusually good opportunity for retrospective and prospective studies despite all the doubts that we have about it. Because it does seem that the records in this particular instance with the large number of X-rays and quite good dust figures, at least in terms of consistency have been going on for an unusually long time.

It seems absolutely essential that the best possible retrospective and prospective studies be made of this material both from the point of view of the Quebec industries but also from the international groups because these are the only large groups of people who have been exposed to one kind of fibre uniquely. They haven't been exposed to other kinds of dust but at least a very few of them must have been exposed to amosite, and crocidolite but no others.

In national studies there seemsto be very important differences in relatinn to the tumor site problem. I also am a little uncertain as to how much information will be got from the very detailed analysis of the past exposures, I think for a slightly different reason.

That is that we have not very clear biological concepts which can be expressed mathematically as to exactly what has been happening in terms of dust and the length of time it is in your lungs.

In some of the simpler forms of pneumoconiosis you can assume apparently fairly reliably that time and concentration are interchangeable. Therefore you have a fairly simple mathematical concept to work on. In the case of asbestos you haven't got this situation as we do not know what happend to people who have been exposed for a certain period of time and therefore time concentrations are not necessarily equally important. You have to take into account a more complex concept for expressing your results. And this makes the whole thing much more difficult, especially when you have uncertainties about your past dust information.

Nevertheless taking all these doubts into account you still have in this population a quite unique opportunity looking at the mortality data and I would emphasize a point that McDonald made I think, which wasn't included in the previous report, that we must look not only at the lung cancer but at all kinds of tumors. I think it is obviously, and I think Vorwald has emphasized this, already we must have a complete listing of all kinds of cancer because it may be that this full listing may throw up some additional information that at present we have only very shaky ideas about.

I would like to support most strongly Dr. Wright's comment. I think first of all three years is quite an unrealistically short period. I don't think that five years will be any too short a period for even achieving what Dr. McDonald has said in fair detail. But I think it is also necessary to start prospective studies because as far as I know there are no recorded retrospective studies where there were reasonable grounds of supposing that there were scattered people. Don't start retrospective studies unless there is a hazard there. It seems very important to build these two projects in together as far as possible. They obviously overlap to a very considerable degree because in making the studies of the present population from the mobility point of view obviously this forms the base-line of the population identification. I think that it means that you have to look to planning the financial support for this programme on the assumption that you are not going to be able to close the book at the end of five years.

CHAIRMAN:

Well we are still at the first line. However, with an agenda we know where we are.

Now we can pass on to the clinical data.

Perhaps I can ask Dr. Paul Cartier if he would like to tell us about the kind of data he has? How he did it.

CARTIER:

Quelques remarques, Monsieur le Président, quelques remarques bien courtes.

Il semble évident qu'une étude épidémiologique doit être considérée essentielle pour augmenter et contrôler nos connaissances actuelles sur les divers problèmes de l'asbestos, qu'ils soient cliniques, radiologiques ou histo-pathologiques.

Personnellement, je suis convaincu qu'une telle étude épidémiologique va permettre aussi de mieux orienter les méthodes de travail actuellement employées dans les deux cliniques de Québec et d'établir, dans la suite, la priorité des études postérieures à réaliser.

Il est certain que cette enquête va permettre aussi d'indentifier les lacunes qui existent dans les examens médicaux qui sont pratiqués annuellement chez nos mineurs d'amiante.

Dernière remarque, je ne crois pas opportun en ce moment, de fournir des détails sur la question des plaques pleurales, sinon d'ajouter aux remarques du Docteur McDonald que cette enquête épidémiologique apportera des renseignements préliminaires précieux sur cette question importante des plaques pleurales.

It appears obvious that an epidemiological study should be considered essential to improve our knowledge on all the

aspects of asbestosis, whether they be clinical, radiological or histo-pathological. Personally I am confident that such an epidemiological study will permit also to improve and to better orientate the work performed at the two Quebec clinics.

I do not think it proper to make at this moment any comment on the problem of the pleural plaques but I would like to add to the comment of Dr. McDonald on this subject that an epidemiological study will help to some extent to bring in precious preliminary data on this important question of pleural plaques.

CHAIRMAN:

Thank you Dr. Cartier.

Dr. Grainger you want to add some comment on the clinical work you are doing on asbestos?

GRAINGER:

I wonder if Dr. Smith would like to answer that? We used to, we collect the same type of clinical information and Dr. Smith has seen the institute and the paper work in that group. He should be able to comment better than I.

SMITH:

I will toss the ball back to you Roger, because I have been away too long.

I would just say this that as far as clinical records are concerned Dr. Cartier and I got together in nineteen forty-six (1946), forty-seven ('47) to develop forms which are comparable in Asbestos and in Thetford Mines and later on when I left Jeffrey and went to New York, we elaborated slightly on that form to gather more information. But the type of clinical examination carried on today, I can't comment on that, except to say that I am quite satisfied with the results that are taking place. When I look at the health of the workers following this programme that Dr. Grainger and Dr. Cartier are executing I think they are doing an excellent job. I know that problems are going to arise in the future. We don't know about them, therefore, I think this epidemiological study retrospectively and prospectively is very important. But how you are doing it today, Roger, the minute details I don't know, I will have to leave to you.

GRAINGER:

I have copies of the clinical form that we use, properly explained. The type of information that we obtained.

SABOURIN:

I don't think we could, if we wanted to, go through our entire programme. I don't think we should go into too many details, otherwise we will not have finished with epidemiology by Friday morning.

LINDELL:

Not going ahead with the survey. They should speak now if no-one wants to go on with the survey.

SABOURIN:

These are details.

LINDELL:

That would pretty well settle the issue. Is there a dissenting vote?

WRIGHT:

I don't have a dissenting vote but I have in my mind a question as to whether the records are going to give you an indication of the presence or absence of mesothelioma. This is an important part of your project. Are the records adequate to give you some information about this. Clinical, radiological and everything else. Are they adequate, will you know whether they were there or not?

McDONALD:

We know some were there and some were not. This is hard to say. In fact we do review the evidence that there is on causes of death. I must admit that I think it will be on mesothelioma that this study is least likely to give precise information. On the other hand, I think we may well get an idea of whether a high prevalence, a worrying prevalence of it is there or not.

All the sorts of situations from other sources of information is of mesothelioma o curring in people with sometimes very low amounts of exposure in comparison wit the sort of thing that even a person in Thetford a few years ago would have experienced.

It should be possible at least to say at the end, the situation here is clearly quite different from that, or that there is indeed some thing to worry about.

I think that is all I can say on this. I think this mesothelioma question should not be left solely to this particular study. I do refe to the fact briefly that efforts are being made nationally to have all cases of suspected mesothelioma reported for assessment and I am sure that this is complimentary and doesn't replace this study in any way. I think that needs to happen also.

CHAIRMAN:

Thank you Dr. McDonald. Yes Dr. DeVilliers?

DeVILLIERS:

I would like to say very briefly we had an opportunity to hear about Dr. McDonald's plan and the study and to discuss it. We were very interested and certainly would like to support it. I would like to correc perhaps just one slight misunderstanding if I may, and that is the National Research Council as such is not interested in carrying out studies themselves. Rather they would like to support and provide for an opportunity for people to

meet and to discuss their problems and in such a way perhaps stimulate the interest and provide for thought, evaluate resources that are available for studies to be carried out in Canada.

CHAIRMAN:

Any other comments on clinical aspects?

GILSON:

Can I just raise one point about the completeness of the other occupational information in the record?

It seems there was a coordination of records right back to nineteen forty (1940). How much information was recorded in other occupations before the man entered the industry. This is the thing about mesothelial tumors which must be recognized. That there are some people who apparently work in one place but when you go into it, in great detail, you find in fact that they have had previous exposure somewhere else. You can't uniquely and certainly say that it is an association with one kind of asbestosis.

SABOURIN:

Perhaps Dr. Cartier could say what is the extent of the questionnaire in that sence?

CARTIER:

Most of the people have started at sixteen years of age.

SABOU RIN:

You said sixteen years of age. There was no previous history of employment?

CARTIER:

People just out of school and accepted in the mines are in very large proportion in Thetford and secondly there we ask the men, of you can accept the version of every new employee coming to the industry. We ask especially if he has any previous exposure. Sometimes we don't have any details but we are very careful to know about previous dust exposure. To some extent there is some information.

CHAIRMAN:

Do they change mines often? When they go for one mine they usually stay there for their life?

CARTIER:

Usually.

LINDELL:

There is only one mitigating factor in Jeffrey Mine that hasn't been brought up about occupation. That is many of the men that worked at the mills were farmers and they ran behind a plow, behind a team of horses and worked in the mines the rest of the time.

And our experience in Northern Ontario is that a farmer had a greater silicosis exposure than the miner did. That is a factor which doesn't appear on the records.

SMITH:

That's following Dr. Cralley's comments about hobbies and moonlighting!

GUY:

I wonder in a retrospective study how much reliance can be put on the evaluation of the degree of exposure. Every time I receive some pathological exposure case, I have the clinical history. I am not an expert in the job of asbestos but the clinics tell me he has been exposed significantly or that there was just a slight exposure or a moderate exposure. Now, some of the cases who came to me after the clinic said slight exposure. I was surprised to find there was no asbestosis or fibrosis and I went back to the files and looked it up to find out if there is some correlation with the degree of exposure and the degree of fibrosis. In many cases they tell me he has seen significant exposure and we find no fibrosis or very slight fibrosis.

So I wonder in a retrospective study how much reliance can be put on degrees of exposure.

CHAIRMAN:

Probably Dr. Vidal will say something.

VIDAL:

Ce que nous recevons, ce sont des patients qui viennent pour subir leurs test au point de vue compensation et je vois que les rapports donnent : "modérément" - "légèrement" - "non significatif" - "Très avancé" - " peu significatif". Ca veut dire quoi? Ca veut dire une impression.

Mais si on pouvait dire que ce type-là a été exposé à tant de fibres d'amiante par pied cube, il me semble que ça serait plus précis que de marcher encore avec l'ancienne notion : en bas de cinq millions de poussières par pied cube, c'est une zone de sécurité pour tout le monde. Ce n'est pas suffisant.

On a besoin d'énormément de renseignements au point de vue de notre opinion, qui supporte tout de même une responsabilité économique. Je ne suis pas un médecin de mine, je suis le médecin qui doit dire :"Ce monsieur-là a droit à une compensation totale, 50%, 25%...", peu importe. Mais alors, pour se baser exactement, il faut des notions sur la constitution au point de vue physico-chimique de l'amiante - ce que nous n'avons pas suffisamment actuellement.

Il faut ensuite - peut=être que nous avons l'avantage, avec monsieur Grégoire, de discuter sur la valeur de la physiologie, de discuter sur la valeur de la physiologie respiratoire -ce que l'on trouve maintenant et ce que l'on trouve chez un homme de 59 ans qui serait dans une autre industrie ; il y a probablement des déficiences.

Alors, où se trouve la vérité?

La vérité c'est une pucelle qui est très difficile à séduire. Comprenez-vous? C'est ça la vérité.

Et cependant, si au point de vue conscience, nous voulons engager la question économique de l'industrie, bien, nous devons autant que possible chercher la vérité, non pas l'impression probable; c'est suspicieux. Voyez-vous, de temps en temps, nous parlons de doute raisonnable, parce que dans le doute raisonnable, on n'est pas sûr, mais il y a des possibilités assez marquées. On donne le doute à l'ouvrier, parce qu'il y a un préjudice dans cette opinion finale. J'aime mieux le donner à l'ouvrier. Le patron peut supporter cela. On accepte cette mentalité-là assez facilement.

Mais ce doit être un doute raisonnable et non pas sentimental. Ca fait une grande différence entre ces deux doutes-là. C'est là tout le problème.

I am not a mining doctor you know, I am the chairman of the Board of Compensation. We have to determine the amount of disability and that is an interesting and economical question. If you want to put a judgment, you should be honest and be sure. There are so many questions pending now to go on a doubt.

A doubtful opinion happens sometimes, when they say, yes the length of exposure, his age, and some signs, you say, well it may be possible to give a reasonable doubt.

Never make an opinion acceptable and so conform to the opinion of your own conscience. That's all. And we need some more information exactly.

CHAIRMAN:

I think this is especially true with the employees since, let's say, thirty years we don't have enough dust count, data is very poor at that time?

VIDAL:

The men who were examined in the asbestos mining industry have an average of thirty-seven point six (37.6) years of age, of exposure. But 30 years ago, it was more than that.

SABOURIN:

More and more Vidal has been expressing the important function of respiratory tests as being the thing of the day. More so, he feels (it is not my field at all but I know he would like to say that), more so than the X-rays, than the X-ray pictures which he seems to take more and more with a grain of salt. And years ago we used to say and I am still of that school, (to what extent I am qualified to say, that is not a question I would like to answer myself) but he doesn't think with his vast experience that we should insist so much today on the existence of the X-ray patterns.

I still believe we should but he said today more and more, it is the function of respiratory tests that seem to bring to me the conclusion that will allow me to assess.

VIDAL:

He wants to talk for me, he thinks that I can't...talk.

When I saw our expert radiologist he says, "asbestos has a big amount of exposure, fifty-eight, fifty-eight sometimes sixty". Say what do you mean by big amount? "Well," he says, "we get more bronchial tumors," and he says, "we work thirty-five years in a mine".

"I know that, but if that case comes up to your office, we don't say, we don't say a word of his occupation. What would you do?"

"Oh, well it looks like the type of change due to the age".

"Well listen, if it's due to the age, what about asbestos dust?"

Seems to me since about nineteen twenty-two (1922) that X-rays are very poor.

I remember in Paris, was a plate and you put your finger and it made a stain. Now it seems to get poor X-rays, we don't give so much importance of the cause. But even the X-rays, that information is not exact. We saw so many.

SABOURIN:

Unless it is the typical pattern?

VIDAL:

Sure he showed only his lung - no specialty. Tuberculosis may make picture negative marks but at biopsy it is cancer.

SABOURIN:

For the purposes of the record, this hasn't much bearance.

Every claim that is allowed means about forty thousand dollars, cost in dollars, that the industry will have to pay. It is not assessed over the general population, it is the industrialist who is called upon to pay.

This will place in evidence, let's say, the selfish point of view which is warranted, however, of the industry in wanting to nevertheless discharge its burden honestly, sociologically and to the best of its ability.

DUVAL:

Before we close the discussion on epidemiology, I would like to make a suggestion for the future. Any prospective study, so far as I can see from the records on dust counts, have been rather uninformative.

SABOURIN:

You are meaning the metal mining industry workers in Quebec, not in asbestos.

DUVAL:

No, no, not asbestos. But we have started a recent survey of all our cases and we have started dust counts and we have

sent material to the Department of Health in Ottawa. Dr. DeVilliers here can confirm that, and in the future we shall take into account and what kind of exposure the man is going to have and we shall place our man according to these findings in the future.

I don't speak of clear pat cases of silicosis but doubtful cases, and we try to manage, but with the data obtained by accurate dust counts. Besides that we have the value of the ventilation system which exists in each mine. And that survey has been conducted for the last five years.

We know very well that in the past, a few mines had very poor ventilation and, in fact, reviewing our statistics recently, we have been able to find out that five mines have more than fifty per cent of all silicosis cases which have been compensated for in the last ten years cases. I will speak later of that.

SABOURIN:

Out of what total?

DUVAL:

About thirty mines in operation. We have now about forty-five mines in operation. We have had an average for the last twenty years of thirty mines in operation and we have now forty-five mines in operation and we have a greater number of workers.

We have now reached twelve thousand workers but I think this average will get a very valuable data from when we start now the survey on the dust counts and the survey on ventilation systems. And it seems obvious to us that the mines who had the good ventilation system and good dust count, some mines had good dust counts, evidently the incidence of silicosis was much lower and I presume that in the future we shall go ahead with this prospective study and of course I would like to have a system like they have in Germany where they have an index card, you know, it's an I.B.M. card or something similar where they can list the amount of exposure a man is having every year and the type of exposure and the amount of silica to which he was exposed, and that we also should take into account the various types of exposure that the man has had or is having in his occupation, the type of dust and the amount of each element of this dust. We should compile something like Germany does and they have much more accurate data than we have. It is fascinating to study their system.

We are fortunate to have an engineer who is in charge of the ventilation for the Tyndalloscope and he knows much better than I do about the German system, that their system is most accurate.

Thank you.

CHAIRMAN:

In accordance with that, I remember this morning Mr. Lindell said that since nineteen fifty (1950), you are very satisfied with the dust count. Do you mean in asbestos?

DUVAL:

No.

LINDELL:

I didn't say that. The same man has been taking dust counts in the entire industry. It will be at least uniform. If it is lousy, it will all be lousy.

VORWALD:

Relative to the recent remark on the dust counts; are you contemplating, or do you contemplate taking random samples of your collective atmospheric dust for analysis by electron microscopy for evidence of submicroscopic fibres. Are you doing this?

DUVAL:

The method used in Germany I don't know exactly.

The Tyndalloscope it seems, our chief engineer seems to be very much satisfied with this new equipment and he can work much faster or very fast.

He can read two hundred samples a day with the impinger method and it takes a long examination under the microscope. With the Tyndalloscope you have good readings and fast readings and you can cover many mines in a relatively short period.

VORWALD:

My question was : are any of the samples taken at random for electron microscopy to find evidence of submicroscopic particles.

DUVAL:

No, no.

CLAASS:

Tout à l'heure nous avons entendu un médecin du Travail dire l'utilité et l'espoir qu'il fonde dans les études épidémiologiques.

Ensuite nous avons entendu un expert parler de l'importance pour les expertises, pour l'évaluation de l'incapacité, je crois, quelque chose qui rejoint justement les déclarations du Docteur Duval. Cela n'est pas la question qu'en Allemagne en particulier, je pense aussi en Belgique, ceci intéresse énormément l'ingénieur d'exploitation.

En effet, il y a l'espoir qu'avec les études épidémiologiques, il pourra mieux orienter la lutte contre les poussières et modifier entièrement l'exploitation dans les mines.

Je pense, à l'heure où l'on fait le bilan de l'utilité des études épidémiologiques , ça valait la peine d'être dit.

CHAIRMAN:

Dr. Claass said that at the present time many spoke about the importance of the epidemiology and in Germany, they attribute great importance to epidemiology works, especially in relation to, with the orientation of the work against dust. And for the future also to try to eliminate as much as possible the quantity of dust and arrive at the...

DUVAL:

And it is based on ventilatinn and dust counts.

CHAIRMAN:

Now the physiological approach has been touched on by Dr. Vidal a minute ago.

I think that Professor Sadoul could comment about the physiological part.

SADOUL:

Je pense que les études fonctionnelles nécessaires dans les enquêtes épidémiologiques, doivent être distinguées complètement des études fonctionnelles entreprises pour d'autres buts, pour atteindre d'autres buts.

En effet, on peut faire des études fonctionnelles pour arriver à une meilleure connaissance de la maladie, à une meilleure connaissance des troubles que cause cette maladie chez un individu donné. Ou bien on peut faire des études fonctionnelles pour mieux apprécier l'incapacité de travail d'un ouvrier donné. Ceci, c'est deux buts.

Pour atteindre ces deux buts, on utilise des études fonctionnelles tout à fait différentes de celles qui doivent être utilisées en épidémiologie.

En épidémiologie, nous devons utiliser des tests simples, qui ne demandent pas plus de vingt minutes au grand maximum, par sujet, je pense, et qui sont néammoins, malgré leur simplicité, très fidèles. Pour employer un anglicisme, je dirais que c'est un "big challenge"; c'est à la fois d'avoir quelque chose de simple, de rapide et de fidèle.

La nature des tests utilisés varie aussi selon les troubles susceptibles d'être observés.

En effet, on n'utilisera pas les mêmes tests pour la Byssinose ou pour l'asbestose oupour la Berryliose. Ces tests fonctionnels ne peuvent être interprétés qu'en tenant compte du contexte clinique. Si nous n'avons pas de renseignements cliniques valables, pour interpréter les tests fonctionnels, nous arriverons à des erreurs.

Au point de vue clinique, les antécédents devront être pris en considération, non seulement par un questionnaire, comme le font volontiers les auteurs britanniques, mais aussi en discutant avec le médecin du Travail, les causes d'absence. Par exemple, dans les antécédents, l'examen physique, la mesure des crachats auxquels faisait allusion le Docteur Wright tout à l'heure, doivent être considérés.

L'examen fonctionnel doit être suffisamment sensible, car nous voulons le mettre en oeuvre dès le début de la maladie. Peut-être qu'il doit nous apporter des informations dans le cas particulier de l'asbestose, avant la radiologie et, par conséquent, il doit être un test extrêmement sensible. La spirographie, malgré son apparente facilité, est une technique difficile. Elle utilise des appareils simples, mais elle doit avoir des techniciens particulièrement entraînés. C'est une technique qui nécessite un peu d'art - comme la cuisine - et il est beaucoup plus facile de faire de la mauvaise cuisine avec de bonnes recettes, que de la bonne cuisine. C'est la même chose pour la spirographie

On doit faire non seulement confiance à la capacité vitale, mais je crois aussi, au volume résiduaire qui permet de vérifier la correction de la spirographie, dans une certaine mesure.

On doit aussi mesurer le VEMS et peut-être est-il utile, si la note bronchitique est importante, de recourir à ce que les auteurs européens appellent des tests de provocation, c'est à dire la mesure du VEMS après nébulisation, d'acécoline, d'histamine ou de broncho-dilatateur; les échanges respiratoires doivent être, à mon avis, étudiés dans le cas de l'asbestose, même dans une enquête épidémiologique. Mais dans ce cas-là, quel test? Nous savons très bien que le test de transfert au C.O. en inspiration unique, est un test qui donne chez le sujet normal, une dispersion - comme cela - puisque je parle français vous m'excuserez de faire des gestes, mais cependant, chez un sujet donné, il donne des tests très reproductibles. Et, si on prend un groupe de sujets donnés, comme se propose de la faire le Docteur McDonald, pendant une période de temps, alors je crois que l'on peut utiliser très bien le D.C.O. ou le transfert du C.O. en respiration unique, en "single breath".

Pour les tests d'exercice, je pense que, malheureusement, il sont difficiles à étudier en étude épidémiologique et, cependant, ils seraient très intéressants. Ceci est encore discutable et à Genève, à la récente réunion du Bureau International du Travail, nous ne sommes pas arrivés à nous mettre d'accord sur un test d'exercice.

Je voudrais, en terminant cette trop longue intervention, insister sur deux points et dire mon accord complet avec le Docteur Wright sur la nécessité formelle d'un groupe contrôle et la possibilité de transporter, d'utiliser un laboratoire transportable auprès de la mine. Personnellement, pour les études épidémiologiques, nous avons l'habitude de toujours transporter notre laboratoire auprès de la mine, même s'il s'agit de voir un petit groupe de sujets. Laissez-moi dire, par exemple, cinquante sujets seulement, nous aimons mieux les voir dans la mine qu'à l'hôpital, où nous disposons de beaucoup plus grandes facilités; par exemple, pour les tests d'exercice, nous observons très volontiers une hyper-ventilation, si les mêmes sujets sont vus à l'hôpital, parce que les conditions psychologiques ne sont pas les mêmes.

Donc, je crois qu'il faut employer un laboratoire transportable, même si l'on veut faire des tests complexes et qu'il faut employer nécessairement un groupe-contrôle.

I will summarize now in English.

I think pulmonary function tests useful for epidemiological studies must be clearly separated from those which are used for scientific purposes. However, the nature of the tests used in field studies depends on the nature of functional disturbances which can occur.

For example, in byssinosis, lung volume measurements and F.E.V./1 will be interesting and will be enough but in berylliosis respiratory changes have to be done, clinical observations must be always taken into consideration if we want to interpret them correctly. As a pulmonary function test it is very important to know exactly what is the amount of sputum of the patient which has taken a lung volume, a lung messurement or something like that.

Respiratory function tests must be very sensitive because we want to identify in epidemiological study early trouble, particularly in asbestosis because from my point of view, I think functional disturbances due to asbestosis can occur before X-ray picture shows a distu rbance.

Pulmonary function tests must also be relatively selective of the asbestosis or of the trouble susceptible of a cure in asbestos workers.

Lung volumes, F.E.V./1 are of course useful but they must be made together with gas exchange studies and I suppose DCO single breath will be useful for epidemiological study.

This wide gathering, everybody knows for this reason maybe it is better to use steady state DCO instead of single breath test. But it is a question for the specialists to discuss and also of the material which is available, of the instrumentation which is available.

Blood studies are rather difficult to undertake, to make in epidemiological studies and it is a pity because it is very interesting to know what is partial pressure of oxygen, for instance, during rest and at work in asbestos workers. But unfortunately it is rather difficult to make in an epidemiological study.

I think exercise tests are worthwhile to perform in epidemiological study, but unfortunately it takes time and up to now we, the specialists, have not reached an agreement on this point of the test, exercise test.

Anyway, I must end by saying my agreement with the necessity of control groups, of course, and on the possibility to go along with transportable laboratories.

I don't know if it is correct as to the English point of view, but I hope you understand my meaning, as George Wright pointed out only a few hours ago.

VIDAL:

Etes-vous d'avis, dans une expertise pulmonaire, que l'interprétation des données de physiologie respiratoire doivent être discutées et appliquées, au point de vue clinique?

SADOUL:

Evidemment, si je suis déjà d'accord sur ce point, au point de vue épidémiologique, alors combien plus je le serais au point de vue expertise. Et je pense que le problème de l'expertise est différent et qu'il n'a pas, je pense, à être envisagé ici.

Je serai très heureux de discuter cela avec vous dans le privé, mais je pense que le temps s'envole et que nous devons rester avec l'agenda.

May be I am wrong Mr. Chairman, but I don't think it is useful to go in with compensation problems because it is another problem and we need other tests and epidemiological tests and I want to put the emphasis on epidemiological study. Excuse me, but...

CHAIRMAN:

Thank you Professor Sadoul.

Now I think we have to adjourn.

As we did not go through the programme (we proposed to go through this morning to experimental pathology), we still have three more items and this is a difficult thing to discuss. We will have another session this afternoon and then we expect to be able to go a little bit on the programme to-morrow. That looks to me very much too large to be discussed. Perhaps we will have to have a morning and an afternoon session. So this afternoon at three.

(meeting adjourned until the afternoon)

GILSON:

Just from the point of view of the record, I should feel that the statement that Professor Sadoul made, only the English translation, in which he suggested that the scientific studies needed certain time, I would like just to stress the point he made that a number of tests which have been in use in laboratories and in clinical practice for a number of years have reached the stage where they look as though they might be useful for epidemiological purposes. A great deal of effort has been put into improving their simplicity from the point of view of the man on whom the test is being done. A great deal of work is being put into making the results highly repeatable and as free as possible from instrumental and biological variables. But I think this is an aspect which will enable us to use these more complex tests for epidemiological purposes and I think that things have changed in an important way in the last two or three years so that we may now be able to use these tests for epidemiological tests in the study of asbestosis and asbestos exposed workers, whereas three or four years ago I don't think it was possible to. I think it is an important development which happens to coincide with the I. L. O. attempt to standardize capacity tests and the opportunities for studying them in this field.

CHAIRMAN:

You want to talk a little more about this?

GILSON:

I don't think so. This is going rather too much into detail. I think that as Professor Sadoul suggested the details of the tests are better left for specialists to decide. He has himself been chairman of the I.L.O. which I know has been a group of experts which has been considering this. They have come out with a report which I think will be a great help in achieving some international standardization. They made quite specific points which is a very major step ahead on an international basis I think. I don't know when the report will be available.

SADOUL:

In a short period of time. One month or two.

WRIGHT:

Yes, I'd like to make one or two comments about this general aspect of the problem. As Professor Sadoul indicated the use of pulmonary measurement can be classed in various ways. There are times when taken together information is of diagnostic help and is commonly very useful in trying to assess the condition of the man for epidemiologic purposes.

I think its primary use is simply to recognize whether there is injury. It is not expected to tell you the nature of the injury. So they have a very good and important role to play because oftentimes, and I know this is true from our own study of asbestosis, you will find abnormalities revealed in these measurements under circumstances when it is not shown let us say in the test film and for this reason I believe that an adequate epidemiologic study does require the use of these measurements. I, after considerable experience with it, have the feeling that for some reason or another, epidemiology is always thought of as having to be something that must be simple. I don't agree with this. I think if the end is important, whatever means you use will be justified in achieving that end. Repeatedly at meetings of this sort I hear the statement we must have simple procedures to do something epidemiologic. I would like to go on record as not agreeing with this at all. I think you should use, for expediency, the method that will give the answer that you need. But, in order to get the answer you must do something that is rather sophisticated. I have two comments in regard to the studies that you envision. The first of these is that these methods now are rather well demonstrated, most of them, to be very repeatable in a single individual. In other words, by any one of these measurements that you would do on me I will have exactly the same result, done the next day and the next day and the next. The answers are not matters of personal judgment, which is a great advantage.

Chest film unfortunately is a matter sometimes of judgment. These are rather more objective in their character and they are highly repeatable. Now why do I say this? I say this because for any study that can be set up where a man can serve as his own control, in other words a measurement is made early on in any study and then watched over a period of years. Measurements of this kind are, I believe, very sensitive. Unfortunately from a retrospective point of view they are rather insensitive in the individual because of the great variation from person to person in their ability as regards any of these measurements with perhaps the exception of arterial blood cancers where there is somewhat less scatter. I say this because I feel that this supports the contention that we should have begun long ago and surely should begin now prospective studies, if you are interested in using pulmonary function method as one of the indices of some injury that may have occurred to the male because in this way you are using these studies at their maximum sensitivity.

The other matter is since I am sure that some of these measurements will be included in the proposed study in a not necessarily retrospective matter but as a part of the study. In other words the measurements will be made after the exposures have occurred. I think this would be important to give some thought to the variation in the anthropometric measurement of different populations.

I have the impression, and I stand to be corrected on this, that the population in the Thetford and Asbestos area, tends to be rather short and heavy. Perhaps not as completely heavy as I suggest but at least they are short people and certain of these measurements are related rather strongly to body size. So that if you planned to compare your population to let us say, a population studied in the States, where we are inclined to be larger people with a somewhat different body build, you might find that your data is slightly in error unless you have your own standard. This goes again to the question of some external control which I believe will be necessary, at least to the degree to show whether in any one of the measurements you choose a normal population that fits well with the normal population of other groups.

I think Mr. Chairman, those are the only comments I have to make.

ENTERLINE:

I would like to make a comment on Dr. Wright's observations regarding simplicity of tests. I think that the value of the tests in epidemiologic studies can bring returns, but with information about population in which you are interested and very often tests are used which are of such a nature as to discourage response.

That is they are complicated, sometimes painful or embarrassing, sometimes very sophisticated tests, and I think the net results of this may be a loss of information. I have in mind particularly a study during which we worked with capacity tests on coal miners and obtained about a sixty per cent response. The reason for the forty per cent non response was that the men were physically incapable of taking the test. So it was impossible to generalize about the sixty per cent who participated.

WRIGHT:

How did you know if they were too impaired that you couldn't do the test? Then you had the one extreme of the responses to your test.

ENTERLINE:

What value do you assign to these people?

WRIGHT:

You assign the value of not being able to do the test. An absolute minimal of not being able to do anything.

ENTERLINE:

Unfortunately the decision on who was able and who was not able in this kind of a situation is a pretty flimsy one.

WRIGHT:

This is a part of knowing the error of method where I agree with you one hundred per cent.

ENTERLINE:

I think that simplicity on epidemiologic tests held very often in getting a good response. I think that a good response is lost sight of in an effort to improve the quality of the information and I think this always has to be kept in mind as to whether you can get a response with this kind of capacity tests.

GILSON:

There is a slight confusion about the use of the word simplicity. I used it specifically in relation to what the man who is being observed has to do. The test itself may be extremely complicated and require elaborate equipment and require computers and all to be done in a reasonable time, but this has nothing to do with the simplicity of the test in the sense that I used and I am quite in agreement with Dr. Wright here that the test must be one which a high proportion of those people who are going to be studied can do without straining and without any discomfort.

CHAIRMAN:

So now we'll go to the radiological aspects. I would like to ask Dr. Pendergrass if he would comment on the radiological aspects of asbestosis.

PENDERGRASS:

I thought these were details that possibly should be referred to a small group because of lack of time. There are details that I am very anxious to talk over with some of the radiological members of this committee and I thought that this might be deferred until we could illustrate these with examples. We can't do it here. We can only talk about it because of the light situation. I am told that this can be done in the hospitality room and this would accommodate fifteen to twenty people if such a number are interested. I have with me some films illustrating this very well. I have some slides of isotopic studies that I thought might complement pulmonary function studies and complement X-ray studies. It is simple and can be carried out with safety on a population that, I mean it requires no particular effort on their part. These are the sort of things that I hope we might discuss.

You said something this morning concerning classification. Dr. Cralley talked about this. We have been giving great thought to the use of an international classification and as yet our panel has not met on that. I have some ideas of my own which I would like to share with some of my colleagues here and see what they think.

It is our hope that we can develop a classification susceptible to reproducibility, time and time again say, in individuals and probably by panels that could be recorded so we could follow the progress of the disease.

CHAIRMAN:

Would you like to have this meeting let's say after dinner tonight?

PENDERGRASS:

That would be wonderful.

CHAIRMAN:

I would like to ask Dr. Guy if he would like to make a few comments on the pathological aspect of asbestosis.

GUY:

Mr. Chairman, I see we have pathologists here. We have Dr. Wagner and Dr. Gross and Dr. Vorwald under which I studied in Saranac and I will try to say what I know about compensation cases of asbestosis.

This is a mining district and of course, if you look at a long compensation basis I don't think you look at it very well the way you would look at it very leisurely. You have to consider other things. You have to consider other persons, you have to evaluate the opacity of that fibrosis, the asbestosis in that man. There are many things that we know about asbestosis and there are many things still unknown. As Dr. Wright said this morning we are still asking the same questions as twenty years ago.

We know where the lesion started, we know that there is presence of asbestotic or asbestosis bodies. We know it is a proliferative lesion. The pneumonitis, we know, is systemized fibrosis. We know it can also give hypertrophic mutilating fibrosis but I rather wonder if the mutilating fibrosis in asbestotic lungs, is extensive, whether it is not due to some other causes.

We know its distribution in the lower lobe. In some parts of the world you can find asbestotic cases in the upper lobe without much asbestosis in the lower lobe. Well here again we are probably dealing with something else.

In this country I was told there is a high percentage of silica and a high percentage of T.B. and probably we are dealing with something other than asbestosis.

In relation to asbestosis a few years ago I had the opportunity of comparing the incidence of asbestosis cases with the general trend of the climb of the general population. Well, I found out that the curve of the climb of T.B. was exactly identical with the curve of the general population. Well, it was not the same with the silicotic where the curve was more slowly declining.

So I say that pulmonary T.B., if it exists, certainly has no relation with asbestosis such as we see in silicosis for example.

Now in relation to cor pulmonale, we find that cor pulmonale happens in advanced cases of asbestosis and we have not found cor pulmonale in minimal or moderate asbestosis. But there are also many unknowns on fibrosis of asbestosis.

For example if we know where the localization of the lesion is, we certainly don't know where the exact initial intimation of the lesion was. Then the other factors than the fibre, the other dusts, infection and stroma infection especially in my mind plays an important role, and this has not been properly evaluated.

Now, we were talking about mesothelioma. I think we should teach the average pathologist in our country. I don't know if it is the same in England, but certainly we in Canada, we should teach the other pathologists asbestosis is not an asbestos body and fibrosis we should teach the pathologists not only what the lesion is but the degree of the severity of the asbestosis. We should certainly so try.

If we remember that mesothelioma can occur in people presenting minimal fibrosis and we don't expect to find them in cases which are sent to us for compensation.

Now there is also the question of whether the fibrosis continues after the man is taken out from employment. Some people contend that it does and some others that it does not. This should be very well evaluated.

Another question is the transport of fibres by the lymphatic system and here we have definite contradictory pronouncements. Myself, I have looked at many nodes and certainly have not seen asbestos bodies. Perhaps, and I am sure there are some microscopic particles in these lymph nodes, but we must say you do not find fibrosis if there is some doubt. This is important because with some people there is the theory of peritoneal mesothelioma caused by the asbestos fibre and the transport by the lymphatic to the lymphatic of the peritoneum and evolution for a mesothelioma, and the role of microscopic part which has been investigated very thoroughly. Do they play a role in the fibrosis? Do they play a role in pathology in the malignant tumor? This should be investigated.

The immunising factor we know because in New York, Dr. Bennett told us that in asbestosis there is no immunising factor such as is seen in silicosis and cancer of the lung.

I can only speak of the material I have. I have studied about a hundred cases of silicotic cases, a hundred cases of asbestosis, and in the silicotic group there were twelve cases of cancer of the lung and the surprising part of it is that until 1958, I had only three and then after '58, I had the rest and it made a total of twelve.

In the asbestos group there are approximately fifteen cases of bronchial tumors and then I maue a little experiment which is a very intimate experiment. I asked a patient of mine, I gave him the list of the asbestos cases, the ages, they were men, a hundred cases with the ages. The technician, I told him to get me a hundred cases of general autopsy of the same age men. He didn't know what I wanted to do with it. He just went through the files and he got the causes of death.

SABOURIN:

General autopsy in hospital?

GUY:

Yes.

In that group we had ten cases of bronchial carcinoma. So there seems to be a little incidence of more bronchial carcinoma. It is a small group it is a very selective one on the part of the silicotic group because these people presented lesions for the general compensation.

Dr. Wright was saying this morning we should add that to chronic bronchitis. I said this morning that perhaps we should be cautious about degree of exposure and evaluation in retrospective studies. Well perhaps it is another cause, perhaps these people, and I think you have written about that in 1955 or something like that about it, that you find people working in heavy exposure for many years and they did not present evidence of fibrosis or very minimal fibrosis.

We need a good study of the broncial tree especially in view of the relation with the broncial carcinoma and we know of course, that there is no definite type of carcinoma associated with asbestos and we need to know that there is no relation with cancer, with even minimal fibrosis and moderate fibrosis and we need, of course, other material than compensation material.

We should, and I make a plea here, we should have more autopsies in that region and not necessarily for a person coming up for compensation. We should have a lot more autopsies. Complete and well done autopsies. Otherwise this material is not suitable for good examination.

Our visit to England has been very valuable in seeing their work, in seeing some of their tumors, in exchanging views concerning mesothelioma.

Of course, this should bring on exchange in evaluation of material, not only in Canada but between parts of Canada, we should have pathologists who have set of slides where everybody is talking about the same thing. Where everybody is talking about the same lesion, or everybody is talking about bronchial carcinoma and is talking about mesothelioma

and this is the plea, that in our province that is you need in the world that we should have a pathological set up which would be complete and which will extend far more than the peoples' compensation, so that we can get scientists with material comparable with other countries.

CHAIRMAN:

Thank you Dr. Guy.

Do you want to add some comments Dr. Vorwald?

VORWALD:

I can't add any significant additions to what Dr. Guy has said.

I would like to say this, however, that the area of pathology is perhaps no less complicated than is that which confronts the physiologists, and in my experience I am greatly concerned not only with reference to the histo-pathological diagnosis of bronchitis, or bronchiolitis, or of an interstitial fibrosis profuse throughout the lung. This can be done but the interpretation of this fibrosis in terms of causation is not easy.

The pathologist cannot, from my point of view, merely look down the two tubes of a conventional microscope and conjure perhaps that what he sees microscopically is indeed due to the emulation and pulmonary deposition of a pulmonary asbestos fibre.

I do not believe that the mere presence of an asbestos body in the lung or near the pleura necessarily means that the causation of pathology which is observed is the asbestos fibre. I think this is clear because certainly there are many other disease entities which have no apparent relationship to the pulmonary deposition of the asbestos fibre, which haveno relationship to the exposure to asbestos.

Now when we come to cancer of the lung the problem becomes a bit more complicated clearly and simply on the basis of knowing from a histo-pathological point of view when we are indeed dealing with a carcinoma. Sure, it is easy when the carcinoma is full-blown, but I respectfully call your attention to the problem dealing with proliferation and metaplasia of the basement membrane of the airways, and when are we to call this really a malignant neoplasia.

The same problem extends itself to cervical cancer and to breat cancer in many instances and it is no less complicated when we are dealing with this very complex system, the lung.

The problem, with respect to the pathologists, in dealing with a frank carcinoma of the lung, is to make some effort relative to the classification of the kind of tumor which we see. Because ultimately it may be possible to clearly show that a certain histo-pathological carcinoma has reference to the pulmonary deposition of an asbestos fibre, of one kind or the other.

I think it necessitates not a single section but a multitude of sections through the cancer because in my experience I have found relatively few pure histological types of pleural sections. That is they are frequently mixed. They are frequently of the adenocarcinoma type and even extend one serial section far enough and you may find fibrosis of the epidermoid and squamous cell proliferation.

Now the mesothelioma of the pleura is perhaps not more complicated than all the others, not only from the biological diagnosis, and when we are dealing with a malignant neoplasia of the pleura as opposed to a simple pleural proliferation of fibrosis, it is further complicated by the fact that a primary carcinoma of the lung can extend itself to the pleura and a primary malignant mesothelioma can extend itself to the underlying parenchyma.

This problem was the subject of a recent debate which was attended by some of us at the Armed Forces Institute of Pathology to try and differentiate as best we could from a purely objective histologic point of view certain malignant mesotheliomas of the pleura from a primary carcinoma of the lung and we spoke at great length about the use of differentiating techniques. Particularly the staining technique with mucicarmine and colloidal iron and this hyaluronadase tumor and this is not easy, and I am somewhat disenchanted with the use of the hyaluronadase sensitive or negative test.

At least in the material that has come to me. Because we received this material from a pathologist sometimes poorly fixed, sometimes it is improperly fixed, and thus is not amenable to detailed studies for hyaluronic acid content.

So then I leave with you the plea that the pathologist, because of the situation he may even wish to do study culture work both in vivo and in vitro concerning the mesothelioma of the pleura, that he must have adequate material at his disposal, properly presented so that he can identify quite clearly what he is dealing with and he can report on it as he sees fit for a definitive diagnosis if this be possible.

I think that is all.

CHAIRMAN:

So there is good agreement between the pathologists.

Would you like to comment Dr. Wagner?

WAGNER:

I agree with the pathologists who are concerned with this problem. The trouble is that as has already been emphasized, the majority of these cases are not seen initially.

I don't think that mesothelioma diagnosis is as difficult as you like to make it. The criteria are there amongst the people concerned with it and the best way of getting this information across is, as Dr. Guy suggested, with sitting around with sets of slides showing the variations in the form of these tumors.

I agree with Dr. Vorwald. The big difficulty of diagnosis is the small percentage of these tumors which mimic adeno- carcinoma of the lung very closely and the form of peripheral carcinoma of of the lung of the adeno type. And here the histo-chemistry is of importance and it is imperative that the tissue that the pathologist receive is either fresh or in suitable fixative.

The same fact has occurred in the diagnosis of asbestosis. One of the features one must declare in all these investigations is that we obviously classify asbestosis, and we agreed on these at the U.I.C.C. meeting in New York, into four categories as a minimal, slight, moderate and marked and although the agreement among the pathologists who are concerned with the problem is pretty clear, this is only a small group of the pathologists concerned and once again it is very important that demonstration centres be set up where people can come and study the accepted classification. Slides should be available to be sent to those people who will become interested in the problem.

Now another factor which I think has been very strongly emphasized and probably rightly so is the question of pleural plaques, and just from the point of view of pathologists is far more extensive than the radiologists observed. The majority of cases of asbestosis that I have seen where the disease is definitely present show the presence of pleural thickening of some sort or another. Usually some of the plaques show a form of pleural thickening and the minority of these cases are calcified.

We have done some work on these and further work has been done in Ireland.

About ten per cent of the plaques that one sees in pathological examination are plated and the question of concentration of the plaques is rather concentration on a small facet of those plaques that are calcified. These are not the majority of the plaques.

Another point of extreme importance is that of asbestos bodies more particularly then.

Paul Gross and others have shown this very high number of asbestos bodies in lung smears taken from routine necropsies.

Now this is a very important means of finding out the extent of asbestos in the atmosphere, providing it is done by people who really know what asbestos bodies are and we have the very great fear that following the work of Thompson, Paul Gross and others more people

are going to do these smear techniques and we are going to get this done by people who are not convinced in their own minds, or clear as to what asbestos bodies are and who are very likely to get a lot of false fragments seen in the lung smears which are an awful lot reported as asbestos bodies.

Here we must develop a technique of checking this and this is the contribution that the industry can help us with in working out methods.

This has been done by several teams in Britain to get a method where one can identify a single, get a single fibre on a glass less than a hundred microns in length, five microns in width and also possibly what type of asbestos one is concerned with.

On the experimental side of the problem, I think, one of the factors which we are working on and may be of considerable value is the possibility, first suggested by the work of Dr. Beattie, that the chrysotile seems to be extremely soluble in the lung and the possibility of chrysotile disappearing rapidly from the lungs may be a factor in the suggestion of stability.

With amphibole asbestos, the disease is definitely progressive after the ceasing of exposure. With the chrysotile, there does seem to be some indication, but this is not proven as yet, that the disease is not progressive after exposure has ceased and this of course will

be a very important factor in dealing with cases of asbestosis in the Province of Quebec.

This comes out fairly clearly in the malignancy experiments we have not completed but are involved in doing where we inject various types of asbestos into the pleural cavity of experimental animals, using disease-free rats, and much to our surprise we produced eighty per cent tumors when our native chrysotile was injected against about fifty per-cent using the anthophyllite and crocidolite and this experiment obvious had to be repeated on the inhalation method because it would seem quite possible that if one exposed the animal by inhalation because of its marked solubility chrysotile may be completely dissolved before it reaches the pleura.

Finally I see that someone has given us a copy of Harington's paper.

I think we should have brief comment possibly under pathology on the position of the oil that you see in this.

It is reprinted, it has changed somewhat.

The first factor here is his original work where he used only cyclo-hexane extracts and this produced a certain amount of strongly fluorescent yellow oils on evaporation. Further work done by Harington and Dr. Commins of the M. R. C. showed that if you use a series of solvents you will get off considerably more oil. So the actual amount of oil produced is far less than actually occurs in the fibre.

The other point that occurs is that, although the paper deals entirely with the natural oil, the virgin oil, the oils in the virgin asbestos, it has been shown very clearly, that as we all know, all types of asbestos have very great absorption properties. That asbestos will absorb onto it any oil on which it comes into contact with.

This was shown very clearly with asbestos bags. Some of the work by the asbestos council in Britain has shown that about eighty per cent of the oil will be absorbed in as little as three months.

When one considers that five per cent of the bag is actually oil, which has been applied to the fibre in the process of making the bag. It is a considerable amount of oil.

In addition we know certain oils are used to cut down the dust in the spinning process and oils are added to the fibre. And so the oil story becomes an extremely complex one. One of the things that we are trying to do at an early stage (and it is remarkably difficult), is to find out whether the oils have any significance. In one experiment, we injected crocidolite fibre with oil and crocidolite fibre from which as much oil as possible had been removed. It showed that slightly more tumors occurred in which there was no oil in the fibre, than occurred in the fibre in which the oil was present.

The other disturbing fact that came from these experiments was that previously we thought that chrysotile had no oil at all. The fibre was produced from mines in Quebec Province.

All these tumors are bound to have a fairly significant oil content. This occurred in addition on a further sample from the same mine. The oil content is somewhat different, and possibly some of the more carcinogenic oils are found in one specimen than in the other. There is tremendous variation from fibre to fibre and, also you have a tremendous variation from outside. We feel that, certainly in Quebec Province, the industry could be of very great assistance to us by giving us some indication of the oil content in the various mines in the Province of Quebec. Whether their oil content in the fibre was the same, how much the methods of drying affected the oil content, etc.

CRALLEY:

Was it a virgin mineral? Was it at Asbestos where you found the oil?

WAGNER:

This was a sample that was sent by Dr. Cartier.

CRALLEY:

Bear in mind that there is perhaps an outside possibility of the process itself being involved.

In the earlier processing, the drying was done using counter flow direct gas contact where soft coal was used as a means of providing the heat and we know that soft coal burned under certain conditions will produce these aromatic polysaccharide compounds. We also know that, even if we go over to some of the bunker fuels, they, have, concentrated in them, many of the metals and also the polysaccharide aromatic compounds. These theories should be disproven, I think, one way or another.

WAGNER:

The experiments you see were both done fairly recently. The first was done at the end of nineteen sixty-two (1962) and the last one was recorded in nineteen sixty-four (1964). I think on the question of the oils, although the amount known is very small, one must consider the work of Rashabat in Russia, and Savioti who is now in Chicago, in which they have shown that well-known carcinogenics will produce tumors in the lung exposing the man to a very low percentage of the hydro-carbons that are absorbed onto some particular materials. But for this asbestos fibre coupled with the presence of carcinogenic hydro-carbon, becomes a slow leaching process; this leads to one of the ideal methods of attacking the pulmonary tract.

NORO:

Mr. Chairman if I may refer to pleural plaque cells. Dr. Wagner mentioned some work about them. He said that it might be only about ten per cent of them can be seen in X-ray.

Well, I have perhaps given a different picture here telling the incidence of pleural plaques around the asbestos mines and these are on findings.

Just now Dr. Merman has made pathological anatomical studies and there you can see he has taken just general autopsies, autopsy material from different parts of the country, and his material is four hundred thirty cases. Age is more than fifteen years and he has found in this general material totally in thirty-nine per cent pleural caustication.

That is a surprisingly common finding, but if we go through the country with X-rays we use nowadays in T.B. with seventy millimetre picture and we take in Finland about one million picture a year. So we can cover the entire population in four years. And in these pictures we very seldom see pleural calcification but only in districts where we have asbestos mines.

It means that if you take about a million pictures a year and if you compare these with general autopsy material

which Dr. Merman just has studied, you should find about four hundred thousand pleural calcifications ayear in this X-ray material. But if you find some hundred, that is good; but in this asbestos district you already have found one thousand three hundred of them.

Why I just ask Dr. Wagner? What would be the explanation for this? Is it that some other, perhaps, mineral metabolic things in this area are rate? Might it be drinking water, calcium magnesium or something like that which makes that the calcifications are better seen in that region? That is a very interesting point.

I just would like to mention that for you here in Canada, would like to look a bit more this side of the problem.

CHAIRMAN:

Thank you Dr. Noro.

Would you like to add a word here, Dr. Wagner?

WAGNER:

I think this is the whole problem of calcium metabolism.

What you get in plaques is dense fibrous tissue which becomes so dense that the blood supply is completely cut off and you get death of the tissue.

In process now in these narcotic areas you do get an accumulation of calcium and this must vary in individuals as to their actual calcium metabolism and the amount of calcium they have.

This is a problem of calcium metabolism and not of asbestosis.

CHAIRMAN:

Now we go to the next, . .

SABOURIN:

Have any tests been conducted since our last conversations with you as to the contents of these calcified pleural masses? Have they been analysed as to their content?

WAGNER:

Not as far as I know.

McDONALD:

Either spectrographic methods, X-rays and so forth? No?

GUY:

Dr. Wagner, do we see fibre, asbestos bodies in pleural plaques?

WAGNER:

Yes. We have never seen them on the ordinary microscope but using special techniques they have been shown to be there.

GUY:

Fibres?

WAGNER:

Yes.

CARTIER:

In two cases we have seen it doesn't identify any fibre at all except phosphate of calcium and iron pyrites. That was in the report from the Department of Mines, Province of Quebec. But they didn't identify no talc or no asbestos fibre. They looked only at the plaque. We didn't send the adjacent lung with that.

WAGNER:

Using the plaque and putting it under a microscope you see a lot of the fibre in them.

CARTIER:

In Quebec, I don't know... It didn't succeed.

GILSON:

Yours (pleural plaques) come from chrysotile which has been dissolved.

SABOURIN:

Before closing this chapter I think (Dr. Cartier told me a moment ago), it might be useful for some of you to know Beattie and Wagner for instance, that there is now a system being adopted by the clinic at Thetford, is it? Or the companies in general?

CARTIER:

The Quebec Asbestos Mining Association will ship, as you know, at the request of the International Union against Cancer, it was requested that about three thousand pounds of the same grade of asbestos, Group seven for instance, taken under the same conditions, be shipped to South Africa. They will have a fibre bank. A bank of asbestos fibre for experimental work.

SABOURIN:

How are they packed?

CARTIER:

Well, they are supposed to be packed in plastic bags covered with jute bags to prevent contamination from oil, if it is such a problem.

CHAIRMAN:

No other comments?

We go to the next item, indices of exposure.

May I ask Dr. Cralley to comment on that please?

CRALLEY:

If I stand here you may hear a bit better.

A number of methods are currently available to quantitate and evaluate airborne exposures to harmful materials. The available methods and procedures were quite limited two decades ago in contrast to the more sophisticated methods now available. Constant improvement is taking place, so that a decade from now many new techniques will be available.

In choosing methods for evaluating airborne exposures, the objective of the programme, as well as the intended use of the data, must be considered. If the intended purpose is research to correlate the effects of exposure on workers, the most sophisticated techniques available should be used. For we are groping to find out not only what specific airborne exposures may be harmful, but also their chemical and physical characteristics as well as parameters of measurements which may be meaningful.

For example, if particulates are involved, the air sampling method must be capable of collecting that fraction which is respirable. It is well known that the chemical and physical characteristics of the respirable fraction of a dust may differ considerably from the overall airborne dust load or the parent material from which it is derived.

If the dust contains fibres the characteristics of the respirable fraction of the fibres and their relationship to other associated particulates must be determined.

In mixed exposures it may be necessary to use more than one sampling procedure for there is no one universal method which will serve all purposes. After the sample has been collected equal consideration must be given to quantitating the sample so that the most meaningful data can be obtained. For example, various kinds of optical microscopy, electron microscopy, electron diffraction, X-ray diffraction, atomic absorption, spectrophotometry, fluorospectrometry, mass spectrometry and many newer instrumental methods of analysis each give important bits of information on a sample where their use is applicable.

With this detailed information chemically and physically characterizing the agent or agents under study, as well as toxicological and other information on their biological effects, parameters of relationships can be developed which show the most uniformity and reproducibility in relating the agent or agents to their effect on health. With this information it may be possible to select a number of indices of exposure, providing there is a uniform and predictable relationship of the index to the overall spectrum of exposure which the index represents.

If the purpose of the evaluation is primarily one of monitoring to determine if levels of exposure are above what would normally be expected in a specific environment where engineering controls have

been instituted, it may be possible to choose less sophisticated measuring techniques such as the tyndallometer or automatic dust counting devices. These must, however, be calibrated so that the index of what they are measuring has a predictable relationship to the characteristics of the environment under measurement.

With concurrent epidemiologic research going on in many countries on the effects of environmental exposures on health, it is very important that methods and procedures of characterizing the exposures be detailed. Where possible the several methods which may be used in different countries should be used and their findings inter-related so that maximal application can be made by others of the data developed.

CHAIRMAN:

Thank you very much, Dr. Cralley.

It seems to me that this is very important if you want to compare the work that you are doing with the work being done in other countries. There then it will mean that you have a kind of international meeting to decide about the method of these.

You have something to add on that topic?

NELSON:

Well I am a little ahead of my story but I would like to comment on a relatively new means of measuring dust that can be used in the asbestos industry as well as the mineral industry.

This is the photo-electric counter. It has been developed in several combinations but the most recent one is Bausch and Lomb. This instrument weighs about thirty pounds, it is completely portable and it gives an instantaneous dust count on a meter scale.

With such an instrument it would be possible to do a rapid survey of an asbestos mill, perhaps in one hour or less. It would give answers right now instead of wasting hours or days to have the samples counted and valued.

There may be objections to this kind of instrument on the grounds it differentiates between fibres and small dust particles but it does not differentiate as well as one could with a microscope. But I don't think that is a serious objection.

We are looking for an index of air cleanliness, the efficiency of air ventilated power operations. Photo-electric instruments will give us that index.

If I were managing an asbestos plant I would want to know from day to day and from week to week, how my dust control was doing. With this photo-electric instrument I could have that information within minutes. With electrostatic precipitators and all the other devices, you just cannot get the information that fast. Too many man-hours are required.

I had hoped to have this photo-electric counter of Bausch and Lomb here. Unfortunately it was lost somewhere between Rochester, New York, and New York City where I was to pick it up. They promised me to send it by air-freight but I don't have much hope.

SABOURIN:

It was in use in Russia in nineteen sixty (1960).

NELSON:

There are some devices that are not adapted to moving around.

CRALLEY:

I make one point that you might confirm. This instrument must be used by somebody experienced in the use of it and in interpretation, in identifying between polluted water, somebody smoking, a burning flour field adjacent to it. They will all, they will all record on this instrument and it will not differentiate at all so that you could get some very misleading data showing very high concentration of particles. You don't know what they are from, you don't know what it is.

NELSON:

But anyhow you can investigate right then and there.

BEATTIE:

We have three of those instruments at the moment working in factories in England in three different locations. These three instruments are of this type. They have been calibrated against each other with known particle size material and we are quite convinced now that they are reliable in telling you whether dust counts are going up or going down.

Now it is possible that we are developing out of this one recorder with sixteen different pick ups so that a works manager can sit in his office and if an increase appears he can immediately find out where that counter is and try to track down the cause of this increase in dust concentration.

So it is instantly possible, using membrane filters at selective points at selective times, to correlate the electrical changes that you get in your recording with specific counts.

We have scientific distribution so that in about another six months or so we shall have a very good idea, not only of dust counts, meaningful dust counts, but the variations throughout the working day and the causes of the changes which we are getting. These can be related to definite events that are going on inside the factory.

The other thing about it is this. You must remember they don't tell you anything at all about the nature of the actual material which you are counting. This is a matter where you have got to go down and use the millepore filter type screen. This is particularly important because the actual character of the dust that you get, say in a spinning or weaving part of the factory, will differ considerably from that part of the factory which has handling.

The relatively crude asbestos as it is delivered in England from the Canadian mines, the mineral composition differs completely. Very large quantities of serpentine do not go to handling but come to weaving and spinning. But there is a method of knowing that from your millepore, that from the point of view of dust control these are of value. But we must calibrate.

It is no use taking it as it is, you have got to prepare, calibrate each instrument against a known standard.

CHAIRMAN:

Thank you Dr. Beattie.

Are there any further comments on this subject?

GILSON:

I would like to make a point.

Going back a bit to Dr. McDonald's proposed investigation. As I understand it he is going to make use of impinger figures of dustiness which extend back something like ten, fifteen years and which are believed to be fairly highly reliable. In a sense, the same method has been used. These samples have been taken in a fixed point in the whole mill or whatever it may be and have been done a number of times. This is obviously an excellent state of affairs. I think that they will be useful from this point of view. If some comparative studies were now made using other instruments and on a random selected individual basis, using random sampling methods so that one could have greater surety that the figures which are available for the impinger bear some relationship to what the individual is now being exposed to, you will now obviously have to make an assumption that they are now reasonably representative. They were representative in the past.

This is something that could very well be done now and would be useful for helping the certainty in which my interpretation of what Dr. McDonald's ideas are.

This, obviously, would link up with some of these different methods of counting dust on which there is no universal agreement at all at the moment as I see it, but which we know, for example, that Shell Service at the moment have already published comparative studies in different parts of the industry comparing some instruments with another.

And finally for the reasons that Dr. Beattie just mentioned, that you in fact find that two instruments do not give the same results when compared to different parts of the land.

It seems to me that this work is now going on in the Public Health Service and it would be excellent if it could be extended into the Canadian mills where, obviously it is a different part of the industry, where we need the data for comparative biological studies.

CHAIRMAN:

No other comments?

Yes, Dr. Cartier?

CARTIER:

May I ask Dr. Gilson what is the safety limit in this country at this moment, in Great Britain?

GILSON:

Zero. We are quite sure of that.

CARTIER:

And it is possible, you think, to reach that level?

GILSON:

That is a supplementary question!

CHAIRMAN:

I am sorry but it is four-thirty but we have another topic, statistical, that we have to see today.

Would Dr. Enterline like to comment on stastistics please?

ENTERLINE:

All right, I will be brief if I can. Somehow statistics always elicit that kind of response!

I would like to talk just briefly about two things that have come up during the day. One is on the use of control groups. This was brought up when we talked about Dr. McDonald's project this morning. And the other is the problem of measuring things. We talked a lot about that this afternoon. How you go about measuring things.

First, regarding the use of epidemiologic studies, I think we have to recognise that epidemiologic studies are really a compromise between what we would like to do and what we are able to do. What we would like to do is take a population of men, in the case of asbestos, and divide these into two groups. Expose one group to the asbestos particles and not expose the other group and see if there is any difference. See if the group exposed has more mesothelioma or whatever we are interested in.

For we are able to merely observe people who have selected themselves into the asbestos industry by some means that we don't know much about, and see if, indeed, these men differ from some expectation.

Now the big problem is how do we know what to expect for any group of men who have selected themselves into an industry? Well, the fact is we don't know how to measure, how to estimate the expected rate precisely.

However, we do know some ways in which we can approximate an expected rate. For example, we like to compare, in the case of asbestos miners, men who are of the same age, who live through the same time period, who live in the same kind of area, who have the same ethnic background, possibly compare them with the asbestos miners and see if there is a difference in the rate at which some particular disease develops.

It has been difficult in the past to do this because of the amount of clerical work involved in developing expected rates. It has been very expensive. However, in the last few years we have developed computer programmes that do this clerical work for us and so we can develop and we have developed an expected number of deaths in study of asbestos product workers.

I did, last year, an expected number of deaths standardizing for a large number of variables. And the point that I am really making is that we are doing much better now in formulating and developing control groups in the epidemiologic studies than we did a few years ago. And the reason we can do so much better now is because we do have computers to do for us a great amount of the work that we were unable to do with just pencil and paper and a library of books.

Now the second thing I want to mention is this problem of measurement.

I think that every time I have been in a meeting, we always seem to bog down a little bit in how do you measure things. I think that the reason we bog down is because we don't really agree why we are doing these measurements and how we are measuring.

Pathologists, by and large, are interested in classifying people, putting people into groups. They are generally not interested in the individual. Epidemiologists, by and large, tolerate a certain amount of care in the interest of achieving other kinds of comparability of the other people such as achieving a very good response from a large population the thing that Dr. Wright talked about earlier, and as a result sometimes they seem to do things that, from a clinical stand point, appear to be inappropriate.

Now I think that all this discussion about simple tests and what tests are appropriate and the kind of studies that McDonald was talking about, for example, evolves around the decision, the decision as to what we are going to do with this information. If we are only interested in putting people into groups we probably could tolerate different kinds of approach. Treating the individual where the individual is important. The kind of measuring being developed being quite different than for the groups but I should emphasize that it doesn't mean the epidemiologist isn't interested in the errors he makes, he is able to tolerate an error frequently where clinicians are unwilling to tolerate an error in classifying people.

The other thing deals with the matter of classifying measurement. We talked about this, this morning, and I think the general principle here is that the need for precision in measuring, say dust in the environment or accumulating previous dust exposures, depends on how strong the relationship is that we are interested in observing. I feel, generally, that if the relationship is so, that each has to require extremely elaborate measurement devices, then maybe the relationship isn't so terribly important. I think these are the kind of things we are dealing with. I think probably the relationships are not that weak and that we can, maybe, be somewhat crude, somewhat imprecise, in averaging the environment of these workers.

I have had some experience recently with working with Dr. Cralley when he did the environment measurements. I got the diagnostic data together despite the fact that we both had extreme reservations about the crudity of the data measurements of the environment. The final data worked out very nicely. There did appear to be a very nice relationship, in this response. In the case I am speaking of we are talking about mortality from cancer of the lung. Despite the environmental response the relationship came out rather evenly, almost linear fashion about as well as one might expect if, indeed, a relationship did exist in the people we took samples from.

So I think I would just say what my two points are. One is that there are ways in which we can try to develop control groups for epidemiological studies that were not available a few years ago by using computers and second, when we talk about measurements in epidemiological studies, we may be able to tolerate some kind of errors that maybe the clinicians would not be able to tolerate in their own work.

CHAIRMAN:

Thank you very much Dr. Enterline.

Does somebody want to comment on that?

CRALLEY:

I wonder if Phil and I maybe have got. Maybe somebody may think, assume that I left out certain details on environment and Phil maybe talked.

Actually we had two different situations in mind. Information I was able to give Phil which did correlate. We were not able to put values down but we would give magnitudes without values.

I make a plea that if given environment to geological effect we must have values somewhere along, criterias of safe levels. So although we, I agree with Phil, we tried drawing people with certain exposures in certain environments so that they got removed for another group.

It wouldn't be final enough to enlist safe levels but as a next step, after we do what we did here. So I don't think that we altogether failed.

SABOURIN:

I approve of the dialogue between Phil and Cralley.

NELSON:

May I ask what is the basis, may I ask Dr. Gilson what is his basis for his zero?

GILSON:

I was joking, the situation obviously called for an answer in jest.

We haven't a figure at the moment which we would be willing to publicize.

NELSON:

I was interested because you, or someone, made the same comment at the New York conference last year. I was stunned at the time and I was stunned again.

GILSON:

Not me.

I would just like to say that I agree very much with what Enterline has just said and I think it is very relevant to this problem of using radiographs for this type of work where, it seems to me, that one aim is to try and get off a piece of celluloid as much information as you can in as reproducible a way as possible and as consistently over a longer period of time and this is really far more important than making the diagnosis in a single case. The most obvious example of this is that if you look at half a million films of coal miners, as we have to in our country now because now everybody has been X-rayed, in this half a million obviously maybe one man or more may have some other disease which exactly mimics the X-ray appearances of pneumoconiosis.

From the simple point of view of the individual, this may be important in which case he may have to do something about it. And that reading from the point of view of statistics of the change of prevalence of pneumoconioses over a number of years and establishment of whether or not things are improving or not. For these errors are of no importance over only two or three cases. They are entirely irrelevant. And I would like to support fully what Enterline said on this point of measurement.

CHAIRMAN:

Dr. Wright you have a comment?

WRIGHT:

We were talking at recess about the same point of view, diagnosis in such studies. It isn't necessary to make a diagnosis when you are making a study of epidemiology of the disease. It isn't necessary to speak in terms of taking a look for evidence in alteration and I think this has to do with the pathology of the story. Repeatedly the term was used to make a diagnosis of asbestosis. I suspect that pathologists, if they were really put to it, would have great difficulty to do it. If we only had the tissue and no knowledge of the person, it isn't necessary that we know the person, all I would need to do is to say there is something not right with this lung and in what way it is not right. You are looking for evidence that environment in years affects the man, not necessarily a diagnosis.

CHAIRMAN:

Thank you.

No other comments?

Then you are invited to be here at seven for the X-ray radiological conference.

(meeting adjourned until the evening)

Wednesday, November 24th, 1965 Evening

Abstract of Remarks by E. P. Pendergrass

1. Roentgen methods are important tools in an epidemiological study of chest conditions, but they have limitations.

   The methods include fluoroscopy which can be recorded on tape and subsequently each frame studied individually are recorded on cine film if important varients are seen. This will provide an opportunity for correlation with pulmonary function studies and continually be available for comparison, when necessary. It is recognized that cine and tape studies can record dynamics of the heart, the lung and other structures which cannot be seen by the eye.

2. Discussion of techniques employed in a given asbestos industry included the illustration of roentgenograms made in several positions :

   P.A. - A.P. - Lateral positions.

   In discussion, Drs. Bohlig and Noro emphasized the importance of oblique A.P. tangential views made with the view of recording the posterior lower lobe segments.

3. Radiation safety criteria of protecting the gonads and the whole body from excess radiation should be continually exercised and the records of radiation exposure recorded.

4. Isotope techniques of recording the perfusion of the lungs through its vasculature by the use of isotope-bound molecular aggregates of albumen were discussed. Normal and abnormal patterns were illustrated; and the use of the techniques to complement pre-employment and continuing, roentgenologic and pulmonary studies were illustrated by slides.

5. The discussion of the limitation of the roentgenogram in recording normal and abnormal changes was illustrated by a series of slides that showed examples of premortem and post mortem findings in the lungs. The technique of the post mortem studies has been published (reference will be supplied).

A plea was made by Pendergrass to consider doing a similar technique (roentgenographic) in all individuals included in the environmental study who will be subjected to post mortem studies.

It was suggested that the post mortem, roentgen and pathologic studies would include macro-roentgenograms of the "Gaugh sections" with the hope that additional information may be obtained.

6. A tentative classification of asbestosis was discussed. This classification has been built on the I.L.O. 1958 Classification and modified by the use of additional symbols which apply particularly to asbestosis.

(This classification can be furnished, if wanted.)

Thursday, November 25th, 1965.

CHAIRMAN:

We start immediately with experimental pathology.

I would like to ask Dr. Vorwald if he would like to open the meeting with mechanisms related to effects of asbestos on man.

VORWALD:

Mr. Chairman, it is not easy to speak of the total area of experimental approaches for the resolution of some problems which we see as they attend the exposures and inhalations to asbestos.

It seems to me that the experimental pathologist or the experimentalist sets up certain hypotheses which he attempts to validate, to prove or disprove. And in this regard I have been very much concerned about a number of facets of the problem, as I see it at least, relative to asbestos, asbestosis.

First of all you will recall that at the opening session I mentioned the importance of knowing the nature of the challenge. I think this demands very clear and precise consideration.

From my point of view I cannot mount an experimental programme if I do not know precisely what I am dealing with. And this inquiry becomes rather complex when we deal with asbestos and the large number of a variety of asbestos fibres, their length, their smallness, their chemical composition and all these things. I like personally to deal with pure compounds, and deliberately mix pure compounds, if I wish to, to determine whether there are any neutralizing effects or cenogistic effects in the pulmonary tissue imposed by one component of the challenge in relationship to another component. I think this should be given serious consideration in any experimental point of view, any experimental effort.

The second focus of the experimentalist is t to try to identify the mechanisms where-by the challenge accomplishes its effects. And in this regard and looking at, from a human point of view, I see a number of separate, but somewhat inter-related, foci which demand attention. We are concerned with the capacity of the challenge to induce, in a unit period of time, a chronic bronchitis, a bronchiolitis, a diffuse interstitial fibrosis of the lung attended or unattended by, what I call, anatomical emphysema, which may be associated with demonstrable functional impairments.

The other focus, of course, is primary cancer of the lung. All the changes in sequential events which occur not only structural, but at the cellular level relative to the hyperplasia and metaplasia and neoplasia of the epithelial elements of the total respiratory tract and lung.

And the next focus, of course, is m. ..thelio: mesothelioma of the pleura. And to try to answer the question as to whether the pleural plaque made up of fibrous tissue and, perhaps, in some instances, a deposition of calcium, whether this, indeed, is the forerunner of a malignant neoplasia of the pleura.

Now in this area of study I am particularly concerned with the test subject. I recognize the difficulties in interpreting the results from, in an animal, a biological system, interpreting those in terms of what may happen with respect to the human individual : man. And the selection of the test subject is determined in some measure by the objective, the primary objective, of the experimental venture. If, for example, we were to study chronic bronchitis, and bronchiolitis, in our country at least, I am tremendously concerned with the so-called episotic bronchiolitis and bronchitis that occurs in the albino rat. I recognize that there are strains of albino rats which have a very low incidence of chronic bronchitis, episotic bronchitis and bronchiolitis, and we are making every effort to obtain such a colony and to maintain a colony without the increasing incidence, in

accordance with age, of chronic bronchitis and bronchiolitis. But this imposes a problem relative to the study of any aerosol, inhaled aerosol and its effects upon the lung.

Second, if we are dealing with diffuse fibrosis, in an attempts to mimic, if we can, the type of interstitial fibrosis that we see in the human lung, relative to asbestos, the deposition of asbestos fibres, then probably another type of animal test subject may be necessary, may be required, and in this regard I try to select the test subject which enables one to go forward with certain functional tests. This is not easy to do with a rat and a mouse, but it may be more easy with a rabbit and with a monkey and with a dog.

If the focus be, ultimately, one of carcinoma of the lung or mesothelioma of the pleura, the selection of the test subject, now, requires serious consideration, because you attempt to select this test subject which at least has, what I call, the biological capacity to respond carcinogenic-wise or mesothelial-wise to the asbestos fibre.

I would not use a mouse, for example, only because of the difficulties of tending the so-called pulmonary adenoma. We have used the rats to advantage because we know that if the challenge is the correct challenge that the rat, at least does have the biological capacity to respond with epithelial proliferation and metaplasia and all that neoplasia mean

To wit: with beryllium we are constantly producing primary pulmonary carcinoma which in the majority of incidences is an adenocarcinoma, but which in twelve per cent of the cases, or slightly more, is the epidermoid type of carcinoma in the white rat and this takes roughly thirteen months and usually, in our hands, at least, with a beryllium sulphate at the threshold limit which is promulgated for man, that at seventeen months we have approximately a hundred per cent of primary malignancy in the white rats.

But this wasn't satisfying enough. So we went to the monkies, the primate, in anattempt to ease the problems of interpretation in terms of human events. And we have succeeded in producing primary very anaplastic metastasising carcinoma of the lung of the primate, of the rhesus monkey. And currently skunk tail monkies that are found in Ethiopia are under investigation but it took us four years for the first cancer to be found, and I now have six monkies all of which have developed primary pulmonary cancer and the last one was eight years, eight years after the original challenge.

The challenge with beryllium sulphate has been continuous depending upon the clinical status of the monkies. They have to be watched very carefully, otherwise they may die of chemical pneumonitis

So the point I wish to make is that it is important to select the proper animal depending upon the objective of your experimental approach, and the problem which you may wish to, somewhat, resolve, if this be possible, from an experimental point of view.

I would like to speak for a moment about the aerosol and, indeed, coming back to the nature of the challenge. It seems to n that in terms of the problems attending asbestosis, that we must consider different particle sizes. I am wondering whether it is indeed not the extremely small particle size in the ultra-microscopic form which induces very significa interstitial fibrosis, but more than that, whether it is not this small particle which has a very defined preference to the induction of mesothelioma of the pleura and indeed of mesothelioma of the peritoneal cavity.

There is another alternative, if however, in tl certain asbestos fibres, either by themselves or having absorbed another substance, are relatively soluble in the lung and that this problem of mesotheliom. of the pleura does not indeed necessitate the transport of a particulate substanc but merely a cell, for example, which has phagocitized a small, extremely small particle, or which has been influenced by substances, which has been extracted by tissue fluids from, or from about the asbestos fibre per se.

This may explain the absence of definitive asbestos bodies in many of the pleural mesotheliomas and the absence of the asbestos body, as far as we can detect it, in the mesothelioma of the

peritoneal cavity. But in all of this when the total story is told, with respect to the experimental point of view, where we can do certain things sequentially, where we can sacrifice them, where we can define the nature of the challenge precisely, when it is all finished we are still confronted with the interpretation of our results in terms of human disease.

This is always a problem and we must recognize it, we must not draw too firm conclusions. For example, in the beryllium story even though we are developing beryllium tumors, carcinoma, as yet we have not decided, and I don't think anyone has decided, that the beryllium per se, the beryllium iron per se, is capable of inducing primary carcinoma of the lung in the human subject. Oh, sure, there have been cases found, but I think that the beryllium suffers often because of association and not because of the rat's carcinogenic capacity, at least in the human study.

So, I think from an experimental point of view, and I should certainly like to hear Dr. Guy and Dr. Wagner and my friend Dr. Gross to speak on this. I think in setting up an experimental approa that one should try, if one can, to utilize the animal to best advantage for the study of a number of parameters, not merely expose, and equate, to see whether mesothelioma or carcinoma of the lung ultimately develops. There are many things which should be done during the course of the exposure.

We are doing bio-chemical studies of the cellular levels and we are finding some very interesting things, at least, in the experimental animal subjects. Whether they will ultimately have a reference to further understanding of the carcinogenic mechanisms in man remains open for debate. We don't know this, you see, but maybe they will.

So, Mr. Chairman, with that, I should like to close and give my colleagues an opportunity to speak to this subject.

CHAIRMAN:

Thank you very much Dr. Vorwald. Dr. Gross

GROSS:

To my mind one of the purposes of experimental pathology is to attempt to reproduce the disease that one wishes to investigate in animals, so as to study the mechanisms of the disease. And the purposes for studying the mechanisms of the disease would be to get some information, or some ideas, as to the prevention and/or possible treatment of this disease.

Now there are number of pitfalls which are encountered doing this type of investigation and Dr. Vorwald has nicely covered a number of these pitfalls. One of the most important ones which he has covered is the variation in re-activity of the different animals.

For example, as he has indicated, the lung of rats may react to the inhalation of beryllium dust with the development of a lung cancer. However, such a development in the human, while it may have been recorded, it has never been proven that, inother words, the development of a carcinoma of the lung in the human has never been proven to be caused, to have been caused by beryllium. On the other hand, nobody has, as yet, been capable of reproducing the chronic pneumonitis, as it occurs in the human, in animals. A type of pneumonitis is produced in animals but it does not, it is not equivalent to the human type of chronic beryllium disease.

These are examples of pitfalls which the experimentalist may encounter.

Another type of pitfall is to be found in the investigation of silicosis. For example, Dr. DeVilliers has recently found that silicosis in hampsters is entirely different from silicosis in other animals. The silicosis in hampsters, the silicotic nodules in hampsters, do not mature, do not form collagen. It is also recognized from the work in England that mules and horses in mines do not develop silicosis where the workers may have developed silicosis exposed to the same dust. This has also been noted in Germany. Einbrodt in Germany found that the lungs of horses exposed to large concentrations of dust, at death, over many years, at death showed, showed contained very little dust and we have also recently

found that mice and rats exposed to the same dust concentration of quarts dust, for the same length of time, in the same chambers, the rat will develop silicosis and the mice will show no evidence of having been exposed to the dust.

There are, however, other purposes for performing experimental pathological investigations, for example, to test the pathogenic potential of an unknown dust or other unknown substances. There are pitfalls in this type of investigation and which I will not go into, except to say that one of the pitfalls is related to the telescoping of time. One attempts to perform the experiment, or the exposure to dust, in a year, whereas in the human such an exposure would extend over a lifetime, or nearly a lifetime, The dosage which one exposes, to which one exposes the animal is very much exaggerated over that which the human would be exposed.

These are all pitfalls which have to be taken into consideration in the interpretation of the results.

Now in regard to asbestosis there are a number of problems which lend themselves to experimental investigation. One of these is a problem which was touched upon by Dr. Vorwald, I believe, around twenty-five years ago, dealing with the susceptibility of the human, as well as animals, to a dust.

It is known that a relatively small percentage of workers exposed to the same dust, for the same length of time, only a relatively small percentage will come down with this dust disease. And somewhat similar variation, variability in susceptibility has been encountered in the human. But up to the present time, although Dr. Vorwald has emphasized this point as long as twenty-five years ago, very little investigation has been conducted on this particular question. And it certainly deserves increased attention.

Another problem which the experimentalist might well concern himself with, related to asbestosis, is the relationship of dust clearance, that is the ability of the lung to clear itself of the inhaled dust, to the development of asbestosis. I think it may be considered axiomatic that a pneumoconiotic reaction, that is a dust reaction, will occur only where and when the clearance mechanism of the lung has failed. This failure of the clearance mechanism may be absolute or relative, it may be focal or generalized, multifocal, multifocal failure of the clearance mechanism is exemplified by the multiplicity of nodules that occur in silicosis , whereas asbestosis would represent the result of a diffuse failure of the clearance mechanism.

Still another problem with which the experimentalist may concern himself in regard to asbestosis, is the progression of asbestosis. Asbestosis, according to many reports, is a progressive disease. I would like to point out that this term progression is a loosely used term, it is usually used by the clinician and when you want to pin the clinician down as to what he means by the disease being progressive he generally means that the patient is pursuing a downhill course and he automatically attributes the downhill course to the inhalation of asbestos dust. But I would question very much whether this interpretation is justified in all of the cases, or even a large majority of the cases.

When you analyze clinical progression, what thorough phenomena are encountered, you will find that this clinical progression may mean a progression in the opacity, or the increasing opacity of the X-ray picture of the chest. Well the worsening of the X-ray picture of the chest may be due to fibrosis and if the increase in fibrosis is due to the inhalation of asbestos dust, or the presence of asbestos dust, I would subscribe that the interpretation that the progression is an asbestotic progression, I would agree with it.

However, how can one tell first of all, that the increasing opacity of the X-ray plate is due to fibrosis when other processes in the lung can likewise produce increasing opacity.

Also fibrosis may be caused by other subsequent dangers to which the lungs are exposed, such as infections to which the asbestotic is also subjected to, just as any other individual. Exudations may cause an increased opacity of the lung and, of course, neoplasia. All of these may produce a worsening picture, a worsening X-ray which the clinician can blithely attribute to the progression of the asbestotic changes.

A second feature in the patient which the clinician may ascribe to a worsening of the asbestotic process in an increasing or a development of short-ness of breath which the individual has, maybe either either of cardiac, it may be due to the right sided failure of the right side of the heart, in which case it may be due to asbestosis. But there are other factors which may cause failure of the right heart. One of them being emphysema which may not be at all related to the asbestosis. Supposing we considered pulmonary causes for the increasing shortness of breath which develops in some individuals exposed to asbestos dust. The shortness of breath due to pulmonary causes may, of course, be increasing pulmonary fibrosis which, as I indicated before, may, perhaps, be due to a progression of the asbestosis. But chronic bronchitis which is often related to excessive cigarette smoking, may also be a cause of shortness of breath and if we care

to associate emphysema from the chronic bronchitis complex then we must also admit that emphysema may also be a cause of the shortness of breath and there are, of course, other causes of shortness of breath as well.

So I have now discussed the clinical concept of the progression of asbestosis and the pitfalls in the conclusions drawn from an examination, either of the X-ray pictures or from the physical examination of the heart or lung.

Then we come to the pathological concept of the progression of asbestosis and there, I would say, one can be fairly definitive in what constitutes progression. I believe there should be very little disagreement of opinion if we say that asbestosis progresses if, after cessation of exposure, the fibrotic process does not extend, furthermore, the fibrotic process matures and finally develops into a more or less cellular scar.

The clinician, many clinicians believe that emphysema, as we generally recognize it, mainly the centre lobular or pan lobular emphysema, may result as a result of exposure to asbestos dust. This is difficult for me to comprehend because asbestosis is a proliferation disease, it produces tissue, so far as I know nobody has ever observed tissue destruction caused by the inhalation of asbestos dust, I have never seen it. Yet according to the W.H.O. definition, asbestosis is a destructive disease.

I cannot reconcile panacinar or pan lobular or centre lobular emphysema which is a destructive disease with asbestosis a proliferative disease.

Well another problem which arises is the relationship of other chronic pneumonidities, in the human at least, to asbestosis. There is an increasing amount of chronic pneumonitis being reported in the general population. The cause is unknown. Presumably such causes can be multiple. It is, therefore, perfectly possible for such chronic pneumonitis, with the associated fibrosis, can occur in people who have been exposed to asbestos dust. We know that the presence of asbestos bodies are not, in the lung, are not necessarily an indication of asbestosis. It is possible that an individual may have a chronic pneumonitis of unknown cause and yet have asbestos bodies in the lung due to an insignificant exposu to asbestos dust.

There is also the problem of cancer which Dr. Vorwald has touched up on I merely wish to bring you up to date on the experimental aspects of lung cancer so far as asbestosis is concerned.

We have, we are near the conclusion of a three-year study in which we exposed rats, hampsters and guinea pigs to large concentrations of chrysotile dust for two years, almost two years, and in some of the rats the clearance mechanism was disturbed by painting the trachea with a strong chemical which destroyed the epithelium and did not allow for a re-growth for two weeks.

Animals were exposed to the dust prior to the regeneration of this epitheliu Out of thirty-one rats that lived, sixteen months or more, three cancers were developed. One epidermoid carcinoma and two adenocarcinomas.

About a hundred and twenty rats which we similarly exposed for the same time in the same chamber but later than tw weeks after the painting of the trachea with the chemical, none of them developed tumors. Also forty rats that were exposed to the same dust in t same chamber at the same time, without any painting with the chemical, nc of them developed tumors. And twenty-five rats which were laboratory controlled, not exposed to dust and not painted, none of them developed tumors. However, of nineteen rats in which the asbestos dust was injected intra-tracheally two developed cancer, adenocarcinoma.

So it seems to me that in view of the spontaneous, the figure for a spontaneous development of epidermoid carcinoma in the albino rat is one in about thirty thousand. Isn't that right McDonald?

The spontaneous development of experimental carcinoma in the white rat, is about one in thirty thousand?

DeVILLIERS:

Yes, it is.

GROSS:

And no adenocarcinoma of the lung has ever been reported as a spontaneous development in the rat.

It seems that there is good evidence then to believe that, experimentally at least, the lung cancer and the exposure to asbestos dust in large quantities of asbestos dust, - I must emphasized that, because even with the exposure of the animals to tremendous concentrati actually the concentration used was one hundred milligrammes per cubic metre, which is a very heavy concentration, - the vast majority of the animals under such circumstances did not develop cancer. It is only when the clearance mechanism was disturbed by the painting of the trachea with lye, (five per cent sodium hydroxide), only under those circumstances was lung cancer produced. Furthermore, only when the clearance mechanism was overwhelmed by the intra-tracheal injection of large amounts of the dust, we had to give six successive injections in one rat and four in another, the other rat which had received less dust did not develop cancer.

So there is a relationship between dosage and the development of lung cancer in the animal.

Now this may have a practical implication, namely: it is recognized that cigarette smoking may impair the clearance mechanism of the lung. Hence in a heavy cigarette smoker, you may have the conditions duplicating the experimental conditions.

Still another problem which is worthy of investigation is the effect of secondary infection to the development of asbestosis. How does it affect the fibrosis of the lung in animals? And from that we might get some inkling as to the relationship of secondary infections to asbestosis.

And Dr. Vorwald has alluded to still another problem which lends itself to experimental investigation, namely, the effect of mixed dust on the lungs, a problem which has intrigued Dr. Smith for many, many years. In as much as most of the exposures which occur in the asbestos industries are no longer pure dust but are mixed dusts. And this problem has not been investigated even though it is, the solution to that problem is in dire need of solving.

It has been suggested that mesothelioma of the pleura occurs because the clearance mechanism, by virtue of the clearance mechanism of the lung the dust is brought up and then swallowed. The implication being that there is a transmigration of the swallowed asbestos dust through the intact intestinal wall into the peritoneum. We were intrigued by that hypothesis, we did not believe it, but, nevertheless, we thought it might be worthy of a trial.

So we set up a group of sixty rats which were fed a fifty per cent mixture of food with finely ground chrysotile asbestos dust over a period of two years. The rate of gain of the weight of the rats was, of the animals, fed asbestos dust was identical with control rats receiving a normal diet.

I have not examined all of the animals but in those that have been examined, the organs were perfectly normal.

Still another facet which lends itself to experimental investigation is this recent discovery in Germany, Grosstüther, of a chemical compound, P_2O_4 it is called, chemical name is polyvinyl pyrodine enoxide. This compound has the intriguing capacity of completely inhibiting the development of silicosis. It might be interesting to see whethe or not the asbestotic reaction of the lung to this dust is likewise inhibited.

And finally, not least, there is the problem of the finding of asbestos bodies in the lungs of general population which would lend itself very nicely to experimental investigation.

There is no reason for believing that the asbestos body, which is a reaction of the lung tissue, predominantly to the presence of asbestos fibres, whether this reaction, with the formation of the body, is specific to the asbestos fibre. It seems to me that there is at least a good possibility, if not probability, that similar bodies can be formed from other fibres, as indeed has been shown by Dr. Davis

at Cambridge, and Dr. Collet in Paris, that very similar bodies, at least from a chemical point of view with the deposition of the ferrotin around the bodies, I mean around the substance, can form in coal-miners, for example, and to amorphous material of unknown composition. Well, if such materials can form to amorphous material one might certainly, well, one can hope that similar bodies would be formed as a defence to fibrillary, fibres, fibre type bodies.

So as a suggestion for future investigation one might investigate any kind of fibrous material which can be rendered in sufficiently fine form so as to be respirable.

And finally the question which has been raised by Dr. Thomson in South Africa, that perhaps the asbestos lining of breaks which contribute to, theoretically at least, to a considerable amount of street dust, to which the population, the general population is exposed, may contain asbestos fibres to owhich the general population responds with the formation of asbestos bodies in the lung. I think that can be put to the test by separating out from the break drum dust, possibly by magnetic separation, the iron and injecting or exposing animals to the plastic plus asbestos fibres.

I think that about concludes what I have to say. Thank you.

CHAIRMAN:

Thank you very much Dr. Gross.

Dr. Vigliani?

VIGLIANI:

I wish to comment very briefly on a point here of experimental pathology in the agenda.

First of all I am not quite happy with the expression, "experimental pathology". I think that considerable progresses and information has been given by the study of human cases as very recently Davis has very nicely shown. So, I think that if we could do basic research this will probably comprise better what we are intending that is etiopathogene of the asbestosis and of the related carcinomas of the lung and pleura.

May I mention here, very briefly, which are the principle points of interest, at least to my mind, which should be studied in this basic type of research ?

First of all what is the action of asbestos fibres on the macrophages? We know that macrophages are not destroyed by asbestos fibre, contrary to what happened before. But, nevertheless, it may be possible that macrophages undergo some changes. They can change into fibroblasts, or they can change into epithelioid cells. I think that this may be an important step in the understanding of what is really the origin of the fibrosis.

Because, to my mind, there is a relationship between the changes in the macrophages and the onset of the fibrosis. The fibroblast must receive a stimulus, which may be a chemical stimulus released by the macrophages, in order to multiply and to produce collagen fibre. And it would be very interesting to see whether the change of a macrophage to an epithelioid cell would provide such stimulus. This has also a general pathological interest because there are many diseases where epithelioid cells appear and the final onset of that is a fibrosis of the lung.

Now another problem which I think would be of interest, is what is the importance, if any, of the ultra-microscopic single asbestos fibres? What we know under the optical microscope, in the lung, is generally a bundle of asbestos fibres. We do not see the single fibrils. Now what happens? Is the interstitial fibrosis due to the bundles of fibres, or is it due to the spreading throughout all the lung of ultra-microscopic particles? I think that this would be a research which would not be too complicated to carry out with the aid of an electron microscope.

Another problem is related to pleural thickening. Is the mechanism of the pleural thickening the same as the mechanism of interstitial lung fibrosis or is it another? Are they in the pleural thickening asbestos fibres or asbestos fibrils? Or to the extent are they responsible for the pleural thickening?

Or is there any other mechanism? I was, for instance, thinking of a sort of dilated hyper-sensitivity. Because we know that in the pleural thickening, at least in the worst type, there is a lot of monoclear cells and which are very difficult for the dilated type of hypersensitivity.

Now as far as the tumors are concerned th first question I would like to be answered is this : whether lung cancer and mesothelioma of the pleura develop only upon a pre-existing lung or pleural fibrosis? Or may they develop also in an apparently normal lung and normal pleura? I think that this is an important problem to be answered just in connection with the term "asbestos-induced" lung tumor or mesothelioma of the pleura. It may be that these tumors develop because of a trait-existing fibrosis, or it may be that these tumors develop by the mere presence, or specific carcinogenic activity of the asbestos fibre, or of the asbestos bodies. And this seems to be a question which must be answered if we have to deal with those cases of mesothelioma of the pleura who are occurring in people non-occupationally exposed to asbestos, or not showing any radiological or even pathological sign of asbestosis.

And finally a very difficult problem is why these tumors develop. I don't want to enter into the pathogenesis, the etio-pathogenesis of the tumors, but these tumors have a special feature, so they are claimed to be connected with asbestos.

Now what is the mechanism whereby the asbestos may increase the natural occurrence of lung tumors, or the occurrence of pleural tumors? It may be that asbestos will increase the natural rate of genetic mutation and thereby giving rise to an increased number of tumors, or it may also be that asbestos will prevent the clearing of the lung and of the pleura from the naturally occurring tumor cells.

I don't know whether I have made myself clear but the problem, concerning this tumor, tumors generally, is that tumors cells, tumor cells ordinarily occur in all humans, due to the naturally occurring genetic mutation, but there is a special immunological process whereby all these cells which are not considered as healthy will be destroyed in the organisms. And this is the reason why we don't get all cancer. Because all our cancer cells arising in our organisms are destroyed before they can multiply and they are destroyed by a specific mechanism, immulogical pathology.

Now it may be possible that asbestos may interfere with this patrolling system of the lung which destroyes the naturally occurring tumor cells. This is something which is related in some way to the sarcomas occurring after the implantation of some foreign bodies in the organisms, plastic sponges or plastic materials producing a fibrous reaction.

In any case, this is a very interesting problem and specially for a better knowledge of the action of quartz on the etiopathogenetic mechanisms of these tumors.

So what Professor Gross was talking about the clearance mechanism of the lung, then there is also a clearing mechanism concerning the tumoral cells and this may be impaired by the presence of asbestosis fibrosis or maybe by the fibre itself.

Now to summarize I think that many of these basic problems have not only an academic interest but they also have a practical interest and I think that we have to trust on the convenience of conducting this basic research, maybe because of the hope they may lead to some unexpected help of the serendipity type.

CHAIRMAN:

Thank you very much Dr. Vigliani:

It seems that there is no question that there are a lot more unknowns in asbestosis than one can think.

It is sure that a lot of research, basic research is necessary but now the industry would like to know if you think that the industry itself should start to recommend, should start animal studies on the affect of various fibres in the lung, or should it leave it to the other bodies at this time?

VIGLIANI:

Well I don't know whether it is for me to answer this question. I don't know. To me very simply it means that if there is nobody else who would undertake that research it would be wise for the industry to undertake such research. It depends on the.....

CHAIRMAN:

Dr. Wagner?

WAGNER:

I think that we have heard a very interesting and very diffuse discussion on the whole application of the basic and fundamental problems of the research into the whole question of pneumoconiosis. But I would, all apologies, here I feel that we are concerned with some specific problem in a very small area of applied experimental work. And we have got some specific questions to answer, and I think if we do those questions first we shall then see what part of industry can play in it.

Now the first of these questions is, before we tackle the thing experimentally, and by experimentally I mean in animals, is... can asbestosis be produced in experimental animals? Well this Dr. Vorwald and I and others, have shown this can be done.

The second point is, do all types of asbesto: produce asbestosis in experimental animals? This has also been done. We have been able to show that, certainly using the main types of asbestos, with the exception of anthophyllite, that one can produce lesions in experimental animals which completely mimin those seen in man.

The third problem on asbestosis is; are all types of asbestos which produce asbestosis in experimental animals, are these all progressive? And in animals you have a tremendous advantage over human from the fact that you can kill the animals in any stage of the experiment and the humans rather object to this!

SABOURIN:

Progressive in what sense?

WAGNER:

Well I was just coming to this.

By this in the animals you can dust them for a certain length of time with a known dosage of dust and then kill a certain number of the animals to give your statistical results and then leave the other animals to live for longer. And by doing this one can see either by straight histological examination and chemical examination of the lungs whether the fibrosis is continuing after the end of exposure or not.

This is a very important and fundamental question, as we mentioned yesterday, in the whole understanding and possible treatment of the disease.

The next point is, can carcinoma of the lun be produced in experimental animals? Now straight carcinoma of the lung in animals exposed to asbestos dust has not, as yet, been produced, except where some physical or chemical alteration has been made to the lungs of the animal, such as the experiment which Dr. Gross described.

At the International Meeting in Perugia on experimental carcinogenesis in animals it was definitely decided there, that tumors, to accept the fact that you can produce a carcinoma in experimental animals.

Firstly the type of animal is very importar It was decided generally that mice were unsuitable because they spontaneousl produce these adenomas and the adenoma is not acceptable as a carcinoma, and that the type of tumor which would be required in rats would be the epidermoid type, the squamous type of carcinoma would be taken as the basi criteria. And, as I say, this has, so far, not been produced in any animal, to my knowledge, that has been exposed to asbestos dust without some interference in the natural clearance, lung clearance, or some chemical irritation of the lungs of that animal.

The third question is, can mesotheliomas be produced by all types of asbestos dust? This we have done, we have produced these tumors and they have been accepted. The method we have used is not acceptable, we admit this. We wanted initially to see if we could produce these tumors by inter-pleural injection of asbestos dust. The dose we used wa extremely large compared to human exposure and the site as I have said was the cardio and physiological.

We are now repeating this experiment by inhalation of know amounts of dust both high concentration and on the concentration which will be more acceptable to that which man is exposed.

The fundamental point, as Dr. Vorwald raised, is the question of finding a pure asbestos dust for the basis of these experiments. And it is also very important that we should use all those doing experimental work to use the same type of asbestos as a basis. For this purpose the U.I.C.C. Committee has, as a lot of us know, with the support of the industry, set in progress a mechanism for collecting a type of these specific dusts. And I think Dr. Gilson would like to talk about that later.

But I think the other thing that I would like to talk about is, we must agree on a basic experimental animal. And once again going back to the Perugia Meeting and to our experience, and experience of others, the animal which is required for an experiment of this nature,

with all its variagated possibilities, is basically, first of all, a small animal because this is where the statistical control of the experiment is extremely important. So you must have a small animal which you can keep a large number of, it must be an animal which has been known to produce the disease which we require, it must be an animal which will live through the time of th experiment without dying of intercurrent infection, and it must be an animal about which a considerable amount is known of the natural tumors which it develops, because after exposure to any carcinogenic substance.

It therefore, seems fairly clear to us that the animal which is most desirable for this, the basic animal, is the pathoge free rat. This is the rat mainly of the wistos strain which was first bred in New York which is now being bred elsewhere in the world in increasing numbers. And this particular rat has the great advantage over the other laboratory rats in the fact that it does not have the inherent bronchitis which occurs in most rats in laboratories. We therefore, have a healthy animal which will survive the length of the experiment without developing bronchitis and probably dying of intercurrent infection which will invalidate any statistical analysis of the experiment. For control this will be used as a basic animal.

Obviously the question of chronic bronchitis that both Dr. Vorwald and Dr. Gross have raised, one can use the ordinary laboratory rat which is a very useful animal as it has a built-in chronic bronchitis by comparison. But to get the right sign on all these experiments I feel very strongly one must agree on the experimental animal and on the type of dust to be used.

And answering the question that, sorry I am going a long way from the question the chairman put, I think the greatest contribution the industry can make at this stage is helping us to provide the fibre for these experiments. The fibre that has, the fibre whose physical characteristics are purely known.

CHAIRMAN:

Thank you very much Dr. Wagner.

It is time for coffee break.

(meeting adjourned for twenty minutes)

CHAIRMAN:

I think that Dr. Gross wants to just say something.

GROSS:

Mr, Chairman, I would like to correct a minor point which Dr. Wagner has made in regard to my animals. He stated that no cancer of the lung has been produced with asbestos except in animals that, whose lungs had been treated chemically. That statement is not quite correct in as much as in two animals which were not treated chemically but who were injected intra-tracheally with chrysotile asbestos dust carcinoma developed, two out of nineteen.

CHAIRMAN:

Dr. Gilson now you want to say a word?

GILSON:

I feel rather timid after the very detailed account of the difficulties, the experimental pathological approach involved and I think perhaps some of us had a sense of despair that the experimental approach won't produce any results until the industry has long been succeede by some synthetic one.

And what I would like to say is just a very small contribution to this problem that was outlined by Dr. Wagner about the materials that are to be used. It seemed to us and to the members of the U.I.C.C. working party which met, as you know, in New York after the New-York Canada Science Symposium, that there would be an expansion of the experimental work, or basic research as Professor Vigliani would like us to use the word, and if this expansion was to take place rapidly and most efficiently and if we were to get results which were as clear cut as they could be, in a field where there are already very many variables, we could at least start with insuring that different workers in different parts of the world knew that when they had injected subcutaneously, or put into tissue culture preparations, or had put up a dust cloud for inhalation, that at least somebody in some other laboratory was using the same material. And it was this simple content which led to the suggestion that we should try and with the help and we have had very active help from the industry, arrange to collect in one place samples of the principle types of asbestos collected from areas where we knew something about the medical exposures of the people. This is why the particular samples that have been and are being collected were chosen. And they were chosen to cover the crocidolite type, because, as you know, many studies have been made in the crocidolite mining areas.

They were chosen from the amosite area because, here again, is an area where the medical supervision is being very carefully maintained and every effort is being made to detect any tumors arising in that area, anthophyllite was clearly a material which we thought should be added, partly because Professor Noro and Dr. Kiviluto had already contributed a great deal to the knowledge of people who may have been exposed to this material. And more studies were planned of the men, women, in this area so that anthophyllite was added for this reason. And we then came to the problem of chrysotile. Well, now of course, chrysotile because it is much the largest, much the most important, of the types of asbestos and is mined in a number of different places represented the most difficult problem in deciding where we should get it from. We suggested that one of the places might well be Shabanie in Southern Rhodesia because their chrysotile has been mined for a long period of time. There has been medical supervision of the population in that mine since the twenties (20's) and so far no mesothelial tumors and, indeed, very little asbestosis, has been discovered in that particular population. And it is a material, kind of chrysotile which is being very extensively used in the United Kingdom.

We then, of course, came to the problem of how do you obtain a sample from Canada where you have two large areas producing roughly equal amounts, and where the material was different in certain important respects, as Dr. Lindell has mentioned in the beginning.

How do we get a sample from Canadian chrysotile which might be acceptable for an international standard?

I would like to emphasize that the main object of these international standards was to insure that people had the same material wherever they were working with it and in the case of Canada, the first idea was, well let us get samples from all the principal mines and then perhaps we shall be able to cover the whole field. We felt, however, that this was, perhaps, impracticable. We may have been wrong about this, we felt that the chances of getting enough material from all the principal mines, in sufficient quantity and setting them up as international standards, getting the money to pay for the preparation of some of these standards, was, perhaps, more than we could expect.

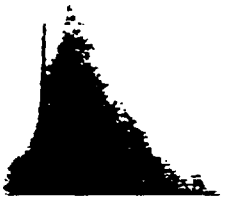
At the beginning we also felt that even if we did do this we should be in real difficulties that the size of the experimenta approach that would be needed would be so large that we might never get enough experiments started and people who has a particular interest in a particular mine or a particular kind of chrysotile would say, - "Well, I won't bother about the other ten standard samples of chrysotile from Canada, we will only work on one", - and then the results of this experiment would have to be compared to the results of some other experiment in some other part of the world where somebody didn't believe in this particular material and has selected some other material.

We shouldn't know then whether the differences were in fact due to differences in material or differences in all the other variables which had been so admirably and clearly described by Dr. Vorwald.

And it is for this reason that it was proposed to pool the samples from Canada, but that the samples would, we hoped, be sent to South Africa and before they were pooled material would be taken from the bags and this material would be kept and, we hoped, would be analyzed in detail for the physical and chemical features in the way in which Dr. Vorwald has mentioned.

I am less optimistic than he is. I think that we can't in fact, contemplate knowing all about the material, which we are likely to be using, in the near future for experimental purposes.

Take the oil material alone. Here you have evidence that there are waxy materials naturally in asbestos. A small number of the constituents of the material have already been identified, but if we take other examples in the hydro-carbon industry it might well take ten years to identify perhaps two or three hundred materials which are, no doubt, present in this waxy material which appears on the fibre. And thus the chances of characterising the material fully, in terms of chemistry, quite apart from the physical and crystallographic aspects of it, seemed to the committee, to be very remote.



And we thought it was more important, therefore, to start with a limited number of samples, get these available as soon as we could in a form in which they could be used, inhalation or injection material, and then ask all the laboratories of the world who had an interest in this problem to start helping in an international project of characterizing these particular samples in any ways in which they were specially skilled. And this material would be collated together in hopes that ultimately some of the information relating to chemistry and physics and the material and crystallographic structure etc., could be related ultimately to the biological response and, of course, ultimately to the molecular biological approach which we hope will be develope in the end by people who we believe will get interested in this material, perhaps, particularly those people who have a fundamental interest in carcinogenesis. Because it seemed to us that here was a remarkable material which hadn't been fully explored by those people interested in experimental carcinogenesis.

Well, now I realize that there is hesitation on the part of the industry in Canada to any idea of pooling the material.

For Canada, I feel that some, perhaps, compromise on this might be achieved without unduly enlarging the samples and yet maintaining something which might be representative of what people in the mines, or elsewhere, using chrysotile from Canada have been exposed to.

I think one should, however, finally emphasize that there is a very big gap, after all, all we can do now is take the material which is mined at the moment and use this as our standard samples. But we must all be very well aware that we are now seeing, particularly in the cancer problem is the result of exposures over years in the past and that anything that is now being mined may differ importantly, biologically, from what the men were exposed to at the time they started work in this industry.

However, this is just one of those imponderables and difficulties which we have just got to face, it seems to me, and this is the best compromise we can achieve.

But I would like to emphasize just before ending that our main object was to insure that people working in different parts of the world would, at least, know one thing, that they were using the same material as somebody in some other land was working with.

CHAIRMAN:

Thank you Dr. Gilson.

Mr. Lindell you want to comment?

LINDELL:

No comment.

CHAIRMAN:

So you told me that the Q. A. M. A. will send each doctor a table giving the physical and chemical characteristics of all fibres you have in the industry, all fibres you have in Canada?

LINDELL:

Well, we will send the information that we have, which at the moment is quite a bit, both physical and chemical characteristics. And there is a study now going on the inorganic, the organic side of the picture and as soon as that information develops we will pass it on

McDONALD:

May I make a brief comment?

CHAIRMAN:

Yes, Dr. McDonald?

McDONALD:

It seems to me that Dr. Gilson has emphasized that the important thing for these samples is that they should be standard rather than necessarily representative. In fact the problems of having any samples representative of anything would clearly be very difficult if not impossible.

I just was wondering rather than the compromise suggested, of perhaps, not having a single sample from Canada.

What then, would not be another alternative method, that of pooling all chrysotile samples? What is the justification, in fact, of a separate Shabani chrysotile and Canadian if neither can be considered representative of either? Would it not be reasonable to have a single sample for lab. work of crocidolite, amosite, anthophyllite and chrysotile, to standardize the laboratory results without necessarily enabling anyone to say, - "These results apply to any particular geographical area?"

LINDELL:

I will withdraw any objections to Canadian mines sending one pool sample, provided that you don't call it a Canadian chrysotile. Maybe you can call it chrysotile "A" and chrysotile "B" and then I don't care what you do with it.

CHAIRMAN:

Dr. Vorwald?

VORWALD:

This pooling of the Canadian chrysotile disturbes me somewhat. For the simple reason that it would appear that there are problems in one area, namely: Asbestos.

SABOURIN:

The town of Asbestos?

VORWALD:

Yes.

As opposed to problems in another area. And if these two samples are pooled completely we will not know from at least an experimental point of view if we attack it, the results, we can say very little about the results, relative to one or the other geographic situation. Because currently, at least it is alleged, that the problems are much more severe in one situation than in another geographic situation ninety miles removed, and if these samples are pooled how will this problem be resolved?

Would it not be better to have a sample "A" and sample "B", one from the Asbestos area, another from the Thetford area, and compare these, which may ultimately have reference to the epidemiologica study which is going to be conducted in both areas?

McDONALD:

What is worrying me, Mr. Chairman, is that it is just exactly that conclusion that one might be tempted to make.

I think that it would be very difficult to get a sample that could conceivably be representative of one mine, let alone one area, and that at this stage surely the main object is to ensure that the laboratories throughout the world are at least examining the same material, without attempting to go from that to draw epidemiological conclusions.

LINDELL:

Mr. Chairman?

CHAIRMAN:

Yes, Mr. Lindell?

LINDELL:

The reason why I wanted to withdraw that was to avoid the debate here.

I think the U.I.C.C. has already debated it, so I am willing to go along with their conclusion.

WAGNER:

May I just say a word?

CHAIRMAN:

Yes.

WAGNER:

These samples are intended to be used as a control and obviously any specific sample that anyone wants to test should be tested against a control. This is your baseline.

The combined sample of chrysotile gives you a combined baseline of chrysotile to try any other type of dust that you want to against it, so that your results will then be comparable with those of anyone else in the world against the same baseline. But they are not meant for characterization of the fibres in particular districts. These are just samples to give you a baseline to work from. Of known samples which will be, presumably, well enough mixed to be a standard sample. These, we are informed by the industry, can be done.

VORWALD:

Of course, there is then, the problem of dosage. Not only of the chrysotile fibre per se, but of all other complex substances that may exist with the chrysotile and differ from one situation to another. And you may thereby be diluting, in fact, these substances by mixing.

WAGNER:

Providing the samples are standard.

VORWALD:

What do you mean by standard?

WAGNER:

These are samples that the industry, and Mr. Lindell can surely correct me on this, the industry have been for a long time, have been mixing asbestos from different areas in a way by which they get a standard result.

By using their method from this, one hopes the samples will be sufficiently mixed so that everyone will receive exactly the same.

VORWALD:

I recognize this that everyone will receive the same, but I am concerned about whether it is, indeed, the same one and two, whether we are ever going to be able to analyse the various components.

SABOURIN:

Well, Mr. Lindell could answer this.

LINDELL:

The U.I.C.C. has recommended this because basically all the processes, all the large processes I should say, really do not use just one fibre in any process. And since they use a mixture of fibres they take, for instance, in the manufacture of asbestos pipes. And years ago you used only crocidolite from Cape Asbestos and chrysotile from Shabani• that was used in the first pipe manufacture. Since that time due to the non-availability of some of these fibres there has been a mixture of fibres since nineteen fifty-two (1952), when "T" and "N" said they could no longer supply the manufacturer with "C" and "G" fibres.

In effect you have this composit insular fibre, and I think Dr. Noro can tell you, even in Finland you go into the asbestos cement factories and they still have fibres from two mines in

Thetford, fibre from Russia and they may have blue fibre from Africa and this is what has lead to the U.I.C.C. to consider as a control sample a melange.

CHAIRMAN:

Including Russia?

LINDELL:

No Russian fibre around here, no.

So I go along with what Dr. Wagner has said, that, all right, you have a control sample which runs that in series and if you really want to know what is going on in your particular ore body well that would have to be another study by itself.

VORWALD:

I would agree to that. That is all right.

CRALLEY:

Mr. Chairman, would this mean that this is the only source of fibre available for research as far as the scientific community is concerned then?

SABOURIN:

The scientific what?

CRALLEY:

Community.

GILSON:

Good gracious no I should hope not!

LINDELL:

The idea is that if you wanted to compare, do research work to see whether you could reproduce the results that someone else has found, you will be able to get fibre of the same characteristics, presumably.

CRALLEY:

Fibre from a particular areas would be available though to do research on, and compare it against this total standard

GILSON:

Obviously one rather hopes that eventually, after the status has been set up and you have learnt a little bit about it, the mixing and uniformity of the product, and you have got the industry and all other laboratories that can contribute to the analysis material, chemically, physically we have got them well characterized as far as possible, other material of special interest as, for example, as Dr. Vorwald has just suggested, differences between two areas in Canada, differences between areas in the States and in Canada, will all come along later and people will graduall build into their experiments other kinds of fibre which one would imagine they

would be using to test this specific hypothesis, that fibre "A" is different because we have got some medical evidence, or some physical reason for believing, or some other reason, and this should be a series of logical hypotheses. And the logical hypotheses we have at the moment is that the four major types may produce different biological effects.

CRALLEY:

I see another advantage to this. We can use these standards to test out our tenhniques. Sure, we know what we are doing before we go to a second source to determine if it's different. We will have at least that uniform technique of approach.

GILSON:

Yes.

CHAIRMAN:

Dr. Smith?

SMITH:

I have nothing to say here.

CHAIRMAN:

All right.

Now we go to the number three, environmental conditions.

May I ask Dr. Gilson to comment on that, please?

GILSON:

I think I have talked quite enough.

CHAIRMAN:

Well I think you are the one who could talk about environmental conditions, number three.

GILSON:

I feel that there are many more competent people than I am, to comment on this. However, I would just like to say, perhaps, a little about what seems to us to be an important aspect in looking at the environmental problem, and this is the differentiation which I felt that yesterday there was some, perhaps, confusion about, the differentiation of the measurements which are aimed primarily at discovering local sources of dust production in terms of time and in terms of site, in order to improve general conditions within a mine or a mill or whatever it might be. And the other purpose which seemed to be one of obtaining information about the dustiness which might be relevant to the actual exposures of the man.

Obviously the dust prevention aims ultimately at preventing the disease but, nevertheless, it does seem that there is a stage in the improvement in most processes where the engineering requirements are somewhat different from obtaining sufficient evidence about the exposure, the likely exposure of the man, and the differences are both in

relation to the type of instrument you use, the time of sampling, and what you do with the results, particularly instrument sample when you have got it. And it seems to me that if I judge what is happening in certain other general fields of dustiness and measurements, looked at from the human exposure point of view, certain important points come out.

First of all, the first one is this, that all studies have shown that there are big fluctuations in dustiness, both in time, and in space, in nearly all processes in mines and, therefore, we need to have some means of getting an average sample because this is, in fact, what the man is exposed to when you look at his whole life history.

His lungs act as an admirable sampling device, and collect the material in relation to what is presented to them. And there has been, certainly in my country, and from a discussion which took place shortly after the Cambridge meeting in London, a very clear trend towards using sampling instruments for this purpose which would be long-term ones. By which I mean you wouldn't particularly want to get a sample for less than one shift and in some circumstances, if the dust was sufficiently low, you might leave the instrument in the appropriate place for as long as a week or even a month and save a great deal of analysis of individual samples by allowing the instrument to collect over a long period of time in order that at the end of the month, perhaps, you would get something representative of the general environmental conditions to which the man has been exposed.

Long-term sampling for these types of dust, certainly, seems to be becoming increasingly thuught to be the right procedure.

The other thing that, the trend that I see developing is towards moving away from number counting because of the difficulties of interpretation of number counts. Some of the results that were presented at the New York Academy of Sciences, showed quite clearly what was well known before, that number counting is very much dependent upon the particular technique you use, the particular instruments, the microscopes and all the rest of it. You can get ten or even a thousand to one differences according to variations in the precise techniques.

Also with certain types of dust, serious problems have arisen in counting techniques due to the overlap of particles, giving you an underestimate of the counts. And for this reason there has been a powerful trend, I think, towards trying to develop instruments which would sample the respiratory dust based on a knowledge, as far as we have at the present, of the parameters which control the deposition of dust in the lungs. And as you know in the nineteen fifty-nine (1959) meeting in Johannes burg, certain specific recommendations were made for the characters of an instrument designed to collect gamma metric ally this environmental dust.

And the Cambridge ;meeting of the British Hygiene Society brought new evidence on this whole problem together and left, I think, most people who attended it with the feeling that so far we have not got the answer, in that there is still a marked discrepancy in the apparent size of the dust when determined in two different ways, one experimentally, short-term measures of dust retention in the lungs and what you find when you analyze the resuspended dust from man and animals after they have been exposed to dust for some long period of time and allowed to retain itin the lung.

Well, now this seemed to many of us to be an unsolved problem at the moment, of which there are many technical reasons why it should be so. And I think that there will, no doubt, in the next five years be a clarification of this whole problem. We shall know more about the dust that is retained in the lungs in terms of its size and particularly the aerodynamic properties of the dust, not only its size, and I know I have no doubt that we shall also have much better information as a result of the acute exposure type of study using isotopically tank dust and other methods.

I think that all I would like to suggest is that in this field of asbestosis we shouldn't neglect the possibilities of using these illustrative gamma metric samplers for assessing the dustiness from a biological point of view.

There are many unknowns here but there are in many countries comparative studies now going on using these gamma metric samplers against the standard methods that have been used in the past.

Nobody suggests throwing away all the standard methods used in the past because there is a great deal of evidence and volume of data on their usefulness. Perhaps the best recent one published is the silicosis follow-up study published by the U.S. Public Health Service. But I think that it is very important that those people concerned with this general problem should be moving with the times in this field of dustiness and to keep in line with the present views on other kinds of dusts which are rapidly developing particularly in the field of coal mines and in the field of silica-containing dusts.

CHAIRMAN:

Thank you Dr. Gilson.

You want to add something there?

CRALLEY:

I don't know if I can add very much, I certainly couldn't improve on Dr. Gilson, and the importance of this approach perhaps just to know that there is no such thing as a universal sampler that can be used for all purposes.

If we are actually going to use several types of samplers and from the data collected try to devise an index of exposure that can be used as a criteria of a safe level. A lot of work has been involved in doing this.

Now as far as the mass sampler is concerned there seems to be some evidence, and especially this is supported by the toxicologists, that the relationship of mass in its present dosage has got to be just a little bit more accurate than on counts.

However, I would like to point out that the chances for error even in mass sampling in analysis is just as great as in the counting, we just don't see but it is there because we are dealing with infinitesimal small amounts and if it is not in the hands of an experienced, qualified person the range and difficulties will still exist. So even that it not a panacea.

CHAIRMAN:

Thank you very much Dr. Crall ey.

VIGLIANI:

I will just make a very short comment on what Dr. Gilson said about the instruments to be used in dust counts. It is very correct as far as nearly all types of non fibrous materials are concerned but I am not quite sure whether these same concepts are useful too in the case of asbestosis because what we normally conceive as respirably size dust cannot be straight applied to asbestos.

And, therefore, even the illustrator and the other apparatus we generally use a pre-sampler apparatus for the sampling of respiratory dust, must perhaps, be checked very well against asbestos dust in order to be sure that they are working properly also in this particular case. And moreover the gamma metric instrument will give you an idea of what the total weight of the dust is respective of the amount of fibres and particles.

What we want to know also in asbestos studies is the relationship between the amount of fibre and the amount of particulate matter. And therefore I think that in any case a preliminary and or any other studies that define the composition of the dust in terms of length of fibres and so on should be done.

In the end, I don't know whether it will be correct to apply this new trend and development in the study of the common type of pneumoconiosis trait to asbestosis and to abandon the old system of numeric counting of the dust, whereby you can also have an idea of what are the relationships between the fibres and the particulate matters.

CRALLEY:

In our study we are using all three methods We are using light impinger, light fuel counts, we are using membrane filter with phase contract properties. We are also using mass samplers to try and get some correlation, maybe get some clues as to what the state will mean

in terms of the various types of collecting systems and plantations being used.

VIGLIANI:

If you use all methods I give it up.

CHAIRMAN:

Dr. Gilson?

GILSON:

We shouldn't abandon old methods. I didn't suggest that we should abandon old methods. I didn't say that we should abahdon the existing methods. I specifically said that I though that these should be added to the instruments that we are using now, hoping that eventually we may be able to replace the older instruments by more efficient ones.

But I think that this is a field essentially where the experimental animal approach can give us a great deal of information. It is perfectly possible to clarify the clouds to which the animals are exposed using these various instruments and then measure the mass of dust in the lungs of the animals quite equally and we can also do a certain amount of compositional analysis within the animals as well.

It seems to me that it is an approach wher quite apart from the biological effect of the dust on the animal, we can use tl instruments and the animals to discover which particular instrument gives t best index of what is actually retained in the animal.

CHAIRMAN:

Thank you.

Dr. Houberechts?

HOUBERECHTS:

Monsieur le Président, toutes les mesure de poussière, d'empoussiérage, plutôt, sont des mesures extrêmement difficiles, mais je dois reconnaître, en y réfléchissant, que la mesure d'un empoussiérage de poussière d'asbeste est encore beaucoup plus compliquée et je ne saurais que confirmer ce que monsieur Gilson a dit sur la nécessité d'essayer de se représenter ce qu'un poumon retient d'un empoussiérage de poussière d'asbeste.

Deuxièmement, dès que l'on saura cela - et je crois qu'il faut s'enpréoccuper, car les autres courbes de rétention que l'on connaît ne peuvent pas être, à mon avis, transposées dans le domaine de l'asbeste - dès ce moment-là, il faudra que l'on fasse tout de même très attention, notamment aux propriétés aérodynamiques des poussière qui, dans le cas actuel, doivent jouer un rôle beaucoup plus grand, étant dor né leur caractère fibreux, leur caractère long et leurs sections petites.

Et alors, n'est-ce-pas, on peut se demander : Est-ce que, réellement, les méthodes que l'on utilise - et on sait, elles sont imparfaites, elles sont toutes imparfaites, même si c'est pour des poussières beaucoup plus simples - est-ce que les méthodes que l'on utilise, est-ce qu'elles peuvent créer avec une rigueur suffisante une coupur de ce qui est inhalable et de ce qui ne l'est pas?

On dit que c'est vers 5 microns. Oui, c'est vers 5 microns pour une poussière qui est assimilables à une sphère par exemple, mais si c'est une poussière, n'est-ce-pas, qui est fibreuse, et bien, jusqu'à quelle taille est-ce que les poussières sont inhalables? Et je ne connais pas du tout les empoussiérages de l'asbeste; je ne sais pas répondre à cette question. Mais c'est tout de même une question dont il faudra tout de même que l'on se préoccupe tout particulièrement.

Et alors il y a, à mon avis, une dernière chose à laquelle il faudrait se rattacher aussi, mais cela ne devrait pas peut-être faire l'objet de mesures de routine, n'est-ce-pas, mais bien d'une investigation à caractère de recherches. Est-ce que dans les empoussiérage que vous avez, par exemple près des moulins, avez-vous une grande quantité de particules submicroniques?

Parce que cela c'est également très important. Le professeur Vorwald, ce matin a encore insisté sur cette question. Et bien, on ne le sait pas, n'est-ce-pas.

Il faudrait vraisemblablement que l'on fasse là une investigation beaucoup plus en profondeur.

Je vous remercie, monsieur le Président.

CHAIRMAN:

Est-ce que vous voulez essayer de traduire en anglais, s'il vous plaît?

HOUBERECHTS:

I have said that every dust measurement is very difficult and I feel that when it is on asbestos it is much more difficult than when you have another one because, other dust measurements, because the aerodynamic properties like Professor Gilson said the aerodynamic properties are of great importance.

I feel that, it seems to me that the retentio curve, the classical retention curve of dust in the lung is not valid if it is asbestos.

Therefore, studies should be made to have an accurate curve with asbestos dust and then when we have that we must try to find with, even with the different instruments, we must try to find som thing which we can reproduce a measurement which corresponds to the retention of the lung.

And secondly, but this should not be a routine measurement, it should be a research work, maybe, because it may be very difficult to be done for a long time, we should know if there is an important amount of infra microscopic. How do you say it? Submicronic dust in the dust you have, for instance, in the mills. Because it can be of big importance. You hear what Professor Vorwald said about interstitial and diffuse fibrosis.

CHAIRMAN:

Any other comments?

NELSON:

Mr. Chairman?

CHAIRMAN:

Yes?

NELSON:

Mr. Sabourin yesterday spoke about the fall-out of dust on automobiles and so on around Thetford and I would point out that perhaps is the unimportant dust. That dust consists of the large particles, the floculated particles that you cannot possibly breathe and it is not really an indication of the requirable dust. So what we might have in the future is some programme of measuring community dustiness. I think that such measurements will be somewhat of a surprise in that although a dust cloud may hang over the town of Thetford, this cloud may actually consist of

less dust than we imagine because of the depth through which you are viewing that cloud. For example in Phoenix, Arizona, the people are disturbed about their air pollution problem if they cannot see the mountains fifty miles away. Actually the absolute air pollution, the amount of particulates in the air is really very, very low in Phoenix, but they think that it is extremely bad.

I point out that it might, I think, be interesting to do some measurements of the dust in the community air.

I think that the asbestos industry, the mining industry, might look into some design changes of ventilation in regard to a screening equipment and milling equipment. But that is a matter aside really from the conference.

Finally the matter of dust sampling. As I mentioned yesterday I believe that our future lies with the photo electric counter, so long as we have a level of dust which we believe is safe, so long as we use an instrument which will measure that level and levels above and below that level, we can monitor our environment to insure that no asbestosis is created in the future. I don't believe we can apply in practice all the various kinds of sampling instruments to our dust problem. There are practical problems in the mill. We must use the device which gives us the most information with the least expenditure of time and money.

CHAIRMAN:

I think Dr. Noro has a question.

NORO:

Well Mr. Chairman I trust... In my mind what Nelson already said it is a different problem to study the outdoor air around the factories and there we need quite different instruments and methods. And I would like to ask our American colleague, have you had la time, any collaboration with your air pollution department, this is not, if they have developed some new study or investigation or survey for asbestos dust in Canada environment?

CRALLEY:

I know that the air pollution has a national network sampling system created throughout various cities in the country and they collect various samples and analyze these to get trends, particulates and gases, a broad spectrum of whatever they might be looking for use in the trend what is going on at various levels of the community pollution.

They are interested in knowing the fibre content of some of these samples naturally, but the task of finding that in a large volume of gunk is so tremendous that they have not succeeded with that procedure, to my knowledge.

We are certain of that but that is asking for quite an assignment to get hundreds of cubic meters of air and everything that is in it and try and find out if there are a few fibres in it. And there would be just a few fibres in relation to everything else you would get. They would be to the point of minority. They are liable to get lost and any electron microscopy or any other technique of trying to isolate and detect them.

NORO:

I think it is not so difficult if you use just general air pollution instruments.

We are in Finland using British instrument just to follow the falling dust of one month, two months, so then we might have enough dust already to measure the asbestos in that. And other very good study in the environment is to study the smoke. In winter time here, you might have, not here I am sorry, you might have the same kind of circumstances we have in Finland so you just take a sample of the snow in winter time or in spring and there you will see then only the dust which is coming though the air and there you don't have the effect of the dust from the earth which is mixing your peoples, perhaps, in summer time.

That is one way to study it.

LINDELL:

Six months snow-and six months for taking it easy.

CHAIRMAN:

Yes, Dr. Vorwald?

VORWALD:

The first statement I have to make is on community air problem. As some of you know there has been under way the past five years a rather comprehensive study of community air proble And I must point out in a certain geographic situation right in the suburbs in the heart of the industrial area of Detroit and we are monitoring aroun the clock and have been doing so daily for four years... fifteen different components. And this is done on automatic instrumentation and it goes o tape and the tape goes to the computer and the computer reads out all the relative values, day and night, hours of the day to tell you when the graph is high and when the graphic is low and the carbon monoxide and the carb dioxide and... and these are just some of the samples we have been monitoring among them.

And as Dr. Cralley has said, when you come to analyze these in detail you find such a complex amount of materi in this sample and we find fibres, all kinds of fibres but the real question in our minds is, is it an asbestos fibre or is it a fibre of something else?

These are the problems. We have tons of them, not tons but we found pounds of it and if any of you want any particula material from the City of Detroit because we have it all in plastic bags, not burlap bags, so you can use them to advantage if you want to study suburban industrial compounds.

I must assure you that Detroit is not like Los Angeles. In other words, although it has been said that General Motors still circles the nation, but I can also say that as Los Angeles goes, so does not go Detroit, with respect to air pollution. Los Angeles is unique.

Well then the next question which I should like to ask Dr. Nelson, is do you really believe that the photo electronic measurement is going to assist in the resolution of the problems concerning the asbestos industry? Particularly from the point of view that we have talked of, the biologic activity of sub-micronic forms?

NELSON:

I think it is going to get at the crux of the matter which is the prevention of dust.

VORWALD:

Well, yes.

NELSON:

That is all that I am saying, that it is for, for the future for asbestos companies in Quebec and elsewhere. And for the mining and smelting industry, in our business of copper levelling.

VORWALD:

At least dust that you can see with photo electronic devices.

NELSON:

It is the what?

VORWALD:

The dust that can be detected.

NELSON:

Oh, yes.

VORWALD:

Photo electrically and you would agree that there are, may be, many, many, many particles in the range which you will not pick up?

NELSON:

Yes, but I believe that there will always be a relationship between the number of ultra-microscopic particles and the particles which can be seen in the microscope.

For example, it takes a tremendous amount of energy to produce ultra-microscopic particles. In any milling process you break down materials in proportion to the amount of energy you put into them, roughly. I am not a milling expert, but, and there is a distribution curve of the fineness of the, yes of the fineness of the product which is produced and to get out to that ultimate region you would really have to grind and grind and grind... And although it would be interesting to study the ultimate particle size of t dusts we are concerned with in the asbestos industry, I don't think it's a practical matter. We need to worry about it if we have a device which mig give us a general measure of the general condition. A photo electric count

LINDELL:

Dr. Nelson I was going to ask you, yesterday, you mentioned about calibrating this machine. Which dusts, of which parts of the mill do you use to calibrate it? Because there will be different conditions for the mills.

NELSON:

The instrument does have some particle size selection. You can discriminate between particle sizes. I would use it in various sections of the mill and I would be taking simultaneous samples by a standard method, the impinger method in your case. I would compare the two. I am sure that you would find there is a conversion

factor so that the information from the counter could be converted to the impinger or vice versa. And I think that the conversion factor would remain fairly constant under a variety of conditions.

CHAIRMAN:

Dr. Bohlig?

BOHLIG:

If we consider pleural plaques and mesothelioma as indicator for the space in which we have to look for asbestos fibre it might be that dust measuring, for example, in Germany, comes too late.

The air pollution from our asbestos factories was just to be seen like the conditions Mr. Sabourin yesterday described in Thetford Mines. But industry has learnt to get the asbestos from the dusty air in the plants and factories, you know asbestos is expensive stuff and now you cannot see any air pollution by your eyes in the environment of the German asbestos plants and factories. And the plaques we do find today and the mesotheliomas we do find today, are the answer of the dust inhaled twenty years or thirty years before.

So I would comment we might be coming too late. The conditions are of course not so in Finland and in every environment of mines and not so inthe district where is, where is done inhalation work. But just the environment of plants.

CHAIRMAN:

Thank you Dr. Bohlig.

GILSON:

I would just like to comment on the remark that Dr. Vorwald made because I think it is of very, very great importance and if he indeed has got, which I am sure he has, samples of the dust collected in Detroit over a period offour years and that there is fibrous material in this we really should make a very serious effort to try and discover what this material here is.

This is a superb example of a piece of research started for some other purpose which has thrown up material which could be of immense value with a problem that we are all being asked. Is there in fact any important amount of the fibre in the general atmosphere? And it seems to me that these samples that he has got should be looked at by anybody, and this is just the sort of thing that the industry might very well put a very considerable amount of effort in within their own laboratories

What they have is skill and the methods of identifying the fibre has not been perfected for this purpose but I think this is absolutely unique material that ought to be looked at very energetically.

VORWALD:

We will send some to you if you want, John!

GILSON:

Unfortunately I am not in a position to make good use of it. I wish I could. I am sure there are others here who could.

NORO:

Mr. Chairman, I quite agree with Dr. Vorwald. It is very difficult to get out asbestos from the general air pollution sampling in the cities like Detroit or even like Helsinki. It is a small city compared to Detroit but having this kind of isolated industry, coal mine like we have in Finland. So that is not so difficult because you don't have too many other dusts in the air. So, for instance, here I have some surveys made sixty-one ('61) and sixty-three ('63) with British deposit coal. You remember this one so we could see that, for instance, when observation sides were about half a kilometer from the mines so we could find in one direction (it depends on the wind and so on,.) thirty-five grammes of asbestos on a hundred square meters per month and if you go farther on, let us take four kilometers to the north from the mine, the same thing was 0.7 grammes per square meter per month. So it is not, I think, impossible to measure this kind of environment if you don't have too many dusts.

CRALLEY:

Answering Dr. Vorwald's question. What air volume is represented in the pound of gunk which you have?

VORWALD:

Fourteen thousand cubic feet per minute. And it goes down to, in the warm weather because we cool it a bit to try to maintain our animal exposure system at seventy-two degrees, that means that it will in the cold weather reduce that probably down to ten hundred, I mean one thousand cubic a minute as opposed to fourteen, one thousand forty cubic feet... and so our air flow will be... in order to move the air in summer time, in order to quantitate what is...

VIDAL:

There is a tremendous amount of air you are sampling though?

VORWALD:

Tremendous.

Fourteen thousand cubic feet.

CHAIRMAN:

Dr. Enterline?

ENTERLINE:

I would just like to endorse this notion of measuring atmospheric pollution in these asbestos areas but maybe for a little different reason than you have been discussing. We talked some yesterday about studying other respiratory diseases. Minor chronic bronchitis and emphysema and so forth.

I recently had an experience in West Virginia where we compared the amount of chronic bronchitis in coal miners living in two different towns. Our first observation was that the coal miners in one town had a great deal more respiratory disease than in the other town. Now there is a difference in type of coal mined and we first think that maybe it is the kind of coal that produced this thing. Then we observed that the wives of coal miners also had different amounts of respiratory disease comparing the two towns. Now these women presumably are not exposed in mines and this was rather puzzling. We then observed that other workers were also different and finally observed that the level of air pollution inthese two towns was quite different.

Now we might conclude that the high amount of respiratory disease in coal miners, among coal miners in one town may have been due as much to the air pollution of the town as it was to their work.

So their pollution might well have been an important factor in chronic respiratory disease, and it is fortunate now, in retrospect, we did look at this.

LINDELL:

I would like to point out an interesting observation. Most of the mines, I don't know how they do it but they build a mill on the leaward side of the tailings dump.

When the wind blows over the tailings dump, it exposes right on the mill for one thing and also you will find the village is on the same side too. It must be really engineered to get it that way!

VORWALD:

I suppose it would be easier for you to change the wind direction!

CHAIRMAN:

No other comments?

Shall we go to item number four, supervision of health of workers.

I would like to ask Dr. Noro who has had vast experience, to comment on this, please.

NORO:

Mr. Chairman, looking at the title of this conference I read here : "Study Conference on Occupational Health of Asbestos Workers" and until now we have spoken most on asbestosis which is, of course most interesting health problem among asbestos workers but I think it is a minor problem if you take the total health of asbestos workers you know that is very small proportion in the total health problem and that is because it might be good to just to discuss very briefly also other health problems of

asbestos workers which usually could be found, or are many times connected with the findings in connection with pre-employment and periodic examinatio and so on.

I just had the opportunity to look through th medical forms of Johns-Manville Company and as far as I see the forms are complete if I compare them with European medical forms they might be mor complete than ours. And concerning the pre-placement medical examination forms so I think all of the points which we have taken into consideration at the European side and looking through the periodic physical examination form I only would like to stress that in this kind of occupational health work, we nowadays must take into consideration more and more also the general healt problem of workers. Starting from the general disease, like cardiac disease, lung disease, mental and so on and so on, which from the industry's point of view are most important if we are talking about sickness of workers or permanent disability.

So asbestosis is rather small portion there But the main diseases are the, just this kind of common diseases and we hav to take them into consideration in planning and developing medical services in industry and occupational health problems.

Nowadays, especially in my country, and i Scandinavian countries, so the placement of workers and re-placement of

disabled workers is becoming more and more important occupational, medical problems and of course workers having asbestosis have always spec re-placement or rehabilitation problems and we don't have any time to go int these workers in this respect. But I only would like to take this point under discussion because I feel that we are perhaps too much talking now on asbestosis and forget the all very important general medical and occupationa health aspects, especially if tomorrow we talk about the industrial health institute.

So that is important to keep in mind also other occupational aspects of asbestos workers.

CHAIRMAN:

Somebody else wants to comment on pre-employment forms?

Dr. Duval?

DUVAL:

Mr. Chairman, I believe that if the prospective study of Dr. McDonald's would be carried out in our district. I mea if the asbestos mines were located in our area, the problems of investigatio would be tremendous. Fortunately this study will be much more easier because the mines are located in an area where there is no other type of exposure.

I mean it is rather a unique situation but when we come to other types of exposure in the industry I think I can give you a very different picture. And I suppose that in the near future, or maybe in a few years I should say, three or four years, there will be operations in asbestos mines in our district too I presume that there will be. And then we will have the problems of confronting different types of exposure, either to asbestos or to minerals. Especially our gold, copper, zinc and lead mines we have lithium, we have molybdenite and these mines have different orebodies, different chemical compositions and so forth. Besides that their source of labour is unique. They have been able to draw their labour in the community, in the surrounding community without any trouble and they still have a surplus of labour.

SABOURIN:

In Thetford and Asbestos?

DUVAL:

In Thetford and Asbestos. While recently they have, they were obliged to dismiss a certain number of these people and they came to our place to get a job, because we are, in the country we are short of labour, we look for labour and, believe me, in all the companies, we have forty-five companies in operation in our district and all of them look for labour. They look for engineers, they look for technicians, they look for everything.

For the last three years we start to hire Indians from the north from the I reserves. Some of them in Chicoutimi have also hired Eskimos. Some Eskimos have started to work in our mine, especially in Lake Albanel are I mean this area is now brand new.

SABOURIN:

Lake Albanel?

DUVAL:

Lake Albanel is north of Mistassini, I mean adjoining Mistassini Lake and these companies are looking to emplo these Eskimos because in this remote area it won't be easy to import labc say from Thetford Mines or Asbestos. I mean people are , as Mr. Sabourin said, they like to live there. I suppose they have been well treated they don't want to move.

This situation is that we have made stud special studies of pre-employment examination and special artificial history forms that we have drawn and we have tried to get as good inform as we could.

These forms were filled at the time of employment or by all men employed in our mines, and if Mr. Couture wo pass a sample of these along it might help to understand a little what we h in mind.

Well this form was prepared specially to investigate in past employment, previous employment. And it has enabled us to find out that when we start some statistical work on the cases of compensation that we have had, we found out that over fifty per cent of our cases of compensation were immigrants.

Our shortage, our problem of labour is twenty years old. I should say in some instances thirty years old because these mines are in a remote district. We had to get people to work in there so the mines were inviting immigrants to work in our areas.

Unfortunately the screening has not been carried out very well as they were in need of workers. They almost hired everybody that would like to work there, with the final result that we have over fifty per cent of our compensation cases of which, who belong to the immigrant group. And as I said previously the other cases of compensation are mostly coming from five mines that have not reached the good standard of ventilation or a proper measure of prevention.

And now we hired people from Vancouver, from all provinces, from Newfoundland, even from Quebec Hydro and they too go to work in Quebec Hydro where they do some tiling work and then they come back to us and so forth.

We have many workers who have been in Ontario in the Elliott Lake area where they have a high content of free silica in the dust. These are uranium mines, they spend five, six years over there and then they come back to us for a job.

So as you see the picture is not very encouraging for the future. And we shall have, I should say our troubles are not finished, we are just starting and I believe that if we have, I do not hope that we have asbestos in our area, but if we have, eventually, then I would like to ask you.

SABOURIN:

But you know you have asbestos deposits?

DUVAL:

Yes, I know that.

I would like to ask if someone has experie here with double exposure to asbestos and to silica dust and what is the outcome of double exposure? Is there some relation between them? There ca be some relation between dusts, silica and asbestos dust in the same mine? If there is relation?

CHAIRMAN:

Dr. Smith?

SMITH:

I would like to say, preface my remarks by saying that I have reported repeatedly to my management that we have no problem with asbestosis in Johns-Manville. We have cleared up the dust. We have a problem with asbestos fibre though, because most of our processes are mixed dusts. Chiefly asbestos and silica. Asbestos-silica in cement and asbestos-diatomaceous earth cement and everything like that.

As you heard, I think it was yesterday, in this big mill that we have no cases of asbestosis for over twenty years. We are producing six hundred to seven hundred thousand tons of fibre per year and we are still having no asbestosis within our plants and I have eighty-seven reporting locations where we are using mixtures and all I can say, Dr. Duval, is that may the Lord help you if you ever have a mixture of asbestos and silica and don't control it because you will get the most bizarre disease you have ever seen. There is no question about it.

LINDELL:

Dr. Duval should study my X-rays. I have fifteen years in nickel mines and twenty years in asbestos. I have got the exposure you want!

SMITH:

There is a very serious problem when you get mixtures of the dusts and they are not controlled to the greatest extent.

CHAIRMAN:

Dr. Gilson?

DUVAL:

Now about the employment examinations in Quebec. I don't want to review the legislation but we have to account for legislation. And you know that we have only two years now of probation and the situation is really difficult too, here what can you do? You may have a normal X-ray at the time of pre-employment and then three years, four years later, then you have something showing on the X-rays.

Fortunately we have had five years with Quebec and it has been changed to two years and I don't know why there was some pressure but we have to face that too in the future. There is also legislation to be reviewed about the difficulties of the, and in order to clari this, I mean it is not, it is not the place to discuss this question because it would take too long. But anyway, we have to face this fact that we have onl two years of probation and after that each company is responsible for any case of asbestosis or silicosis in the Province of Quebec.

And besides that it works according to a merit rating. The last employer is responsible for the case.

Anyway, I would like to know what would your feeling about having a common source of information?

I should say that the occupational history is most important, at least in my case in my district. It is not as important as in the asbestos mine at the moment but in the future, I think we should have a common source of information. I think that we should have, at least centralize our information for all the workers in the mines of Quebec.

And already with this type of, that was my idea, we have four copies, we always type four copies of special history, that you have a copy on hand, and the fourth copy gives information to the man, to the employee, how to use the certificate and so on, but I think that one of the sheets, I suggest that I am ready do to it, one of these sheets could be filed somewhere in a central place. If a mine needs information about the previous exposure then it would be available. I am ready to do this on my part.

CHAIRMAN:

Dr. Gilson?

GILSON:

I would like to make just one general point about periodic examination because in this particular field of asbestos exposed workers, we, in our country, have shown, had experience of what I think is success in one way and failure in another. It consisted of periodic examination since nineteen thirty-one (1931) has been enforced on people

who are in scheduled occupations. But while this, I think, has provided some protection for the employer and the employees, perhaps insured that some people who might not have got compensation have got compensation, possibly people who would have got early disease may have been removed in time to perhaps lessen the rate of progress of their disease. But the answer is we don't know of any of these pieces of information and the reason why we don't know is that no systematic scheme was set up at the time that the periodic examination or the initial examination was established to make use of the information. Thus at the time they were started in thirty-one ('31), obviously it was relatively much more difficult than it is now with the sort of computing facilities that are available. And I would urge very strongly that when new schemes or existing schemes are being reviewed very serious thought is give to what information can be got out of the vast amount of labour that goes into completing these sort of forms and which is a loss in general, while obviously is of value to the individual.

And it seems to me we have a very good example in Dr. Cartier's records, I am sure Dr. Grainger's records, perhaps, in Asbestos there is a vast amount of information which, if it had been coded in a form which could be readily extracted, would have saved a great deal of the sort of problems that are now facing Professor McDonald.

I have in mind the sort of thing which I am sure applies to other industries just as well, but which was set up in the London Transport Executive, for example, in, by Dr. Lesley Norman, whereby certain information, admittedly rather crude in the medical nature, was linked automatically with the information in the personnel department in such a way that very rapidly answers could be provided to questions such as, for example, "Are drivers aged sixty and over likely to have more accidents than the younger?" This can be answered immediately within a week.

And there are many other questions like it like the difficult one that they had which was presented to them a little while ago - "are colour-blind drivers, in fact, more likely to get accidents, or do they in fact know the position of the coloured lights and completely overcome this difficulty?"

And this sort of thing was immediately answered because all the records were there. The accident information about individuals plus their colour-blindness was all fed into the records and you could extract it from the analysis immediately. You could, perhaps, make a special study of them.

And this is the type of investigation that I think we ought now to be building into any new periodic system. It involves a certain amount of extra money, a certain amount of extra personnel, but with the sort of facilities that are available it is going to be of enormous value for our successors.

SADOUL:

Je voudrais simplement souligner que lors de la réunion récente des experts en matière d'exploration fonctionnelle, réunis à Genève par le Bureau International du Travail, pour déterminer quelles étaient les explorations fonctionnelles à faire dans les pneumoconioses, cette Commission, après discussions, a recommandé très vivement l'emploi des tests fonctionnels dans les examens d'embauche, comme lors des examens périodiques.

Ces examens fonctionnels périodiques pourraient être ceux qui sont employés pour les études épidémiologiques, c'est-à-dire des examens fonctionnels simples. Ces tests, comme le Docteur Wright le disait hier, sont très fidèles chez un travailleur donné et leur changement peut donc donner, leur variation peut donner, leur variation peut donc donner des indications extrêmement intéressantes.

Dans l'esprit des experts l'embauche éviteraient certainement des discussions et des hésitations lorsqu'il s'agit de compenser un travailleur qui a une maladie professionnelle.

Mais je crois qu'il y a là un moyen, un outil qui ne devrait actuellement pas être négligé et ces examens fonctionnels périodiques dans le problème de l'asbestos devraient être considérés, à mon

avis, comme aussi importants que les examens radiologiques périodiques. Dans l'asbeste, ils doivent jouer un grand rôle, aussi important que dans la byssinose, par exemple, où ils sont en train de jouer un rôle.

SADOUL:

Il will now summarize in English. I hope correctly and not with such nonsense as I did yesterday.

The I.L.O. Commission recently settled in Geneva about dealing with the proper use of pulmonary function tests in occupational diseases, recommended the use of lung function tests in the pre-employment examination and in periodic examination.

The experts agreed on the point that these tests are quite reliable on a given worker, and they thought that in asbestosis, for instance, ventilatory tests must be used in conjunction with D.C.O. tests, for instance. It was the only suggestion for D.C.O. tests but the lung function tests were, anyway, highly recommended in pre-employment examination and in periodic examination.

The experts thought that the changes observed by such functional tests are very interesting to appreciate, maybe, the occupational hazard in a given occupational environment. And the Russian expert pointed out very strongly the value of such tests for this special purpose. But other experts pointed out the value to appreciate the extra

occupational facts such as atmosphere, local atmospheric pollution, such as smoking habits, or alcoholic habits, or maybe some congenital factors. And also to evaluate the individual susceptibility in such workers.

And at last, to finish with that, I think, and the experts thought, that this pre-employment examination with pulmonary function tests will avoid some discussion in compensation in the very, very far future.

CHAIRMAN:

Thank you very much.

Mr. Sabourin?

SABOURIN:

Inasmuch as Dr. Wagner has told us that the condition of asbestosis was progressive even after removal from exposure. It seems to me that possibly a question in point to put to Dr. Gilson would be, to ascertain if there is any kind of follow up procedure in the British Isles as to those cases that might be already recorded of asbestosis alone, or accompanied with some other pathological conditions after the cases have been, let us say, adjudicated upon and particularly in those cases where the claimant has been compensated, gone to some other form of employment, is his condition, in short, followed up periodically so that you may be in a position to ascertain if the condition is one that is aggravating or is becoming stationary or is going downwards?

In our province I must say that we have to confess that this system is not followed, is not in existence and we have quite a number of claimants, therefore, who have not been compensated for some fifteen, twenty years sometimes, and we don't know if they are not to-day operating some automobile or working in some other mine in the section of Dr. Duval.

GILSON:

Well, I think the point that Sabourin has raised is just an example of this point I was making of the ineffectiveness of our system in our country that although the periodic examinations have been undertaken, when the man is thought to have asbestosis he is then removed from the industry, or may be removed from the industry, and is followed up subsequently, this is since nineteen forty (1940) about, because he may be eligible for increase in the compensation sum paid under the national scheme.

The whole of the records of this particular system are not one that are readily acceptable for study for various technical reasons. A limited investigation of these was done by Dr. McVitty and was presented at the New York Academy of Sciences, which indeed showed that after asbestosis was diagnosed by our Compensation Organization, by the doctors in the organization, the prognosis was bad in a rather surprising number by five years or so.

I think it is quite possible that there are real differences in our European experience from the experience here in Canada and indeed this is one of the things that I hope Dr. McDonald's study will reveal.

In our country in addition to the compensatio scheme and for the last three or four years certain industrial companies hav been making investigations of this type and investigations linked with detailed lung function and I hope that within, perhaps, a year or so we shall be in rather a better position to answer one of the points that I am sure all of us are interested in and that is, if you have a man who you think on lung functior studies has got early evidence, an abnormality probably linked with asbestos exposure and you take him out of the industry does this improve or does it no And this is something which we are a little way along the road towards answering but we haven't yet been able to do it.

CHAIRMAN:

Thank you very much Dr. Gilson.

I think we will have to close the meeting now because the girl is unable to go on any more, so we will start tomorrow morning. But we have this meeting tonight at six fifteen with Dr. Vorwald with his slides.

So good afternoon.

(meeting adjourned until the following morning)