IN THE UNITED STATES DISTRICT COURT

FOR THE EASTERN DISTRICT OF PENNSYLVANIA

IN RE: ASBESTOS PRODUCTS LIABILITY LITIGATION (NO. VI)	Civil Action No. MDL 875

This Document Relates To:

UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MINNESOTA
FIFTH DIVISION

CONWED CORPORATION,

Plaintiff,

-against-

Civil Action No. 5-92-88

UNION CARBIDE CORPORATION,

Defendant and Third Party Plaintiff

-against-

OWENS-CORNING FIBERGLAS CORPORATION et al,

Third Party Defendant.

August 13, 1998
EDWARD ILGREN

August 13, 1998
9:50 a.m.

Continued deposition of EDWARD ILGREN, taken by Plaintiff, pursuant to adjournment, at the offices of Kelley, Drye & Warren, L.L.P., 101 Park Avenue, New York, New York, before Nancy R. Sullivan, a Shorthand Reporter and Notary Public within and for the State of New York.

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E D W A R D I L G R E N

resumed having been duly resworn, was examined and testified further as follows:

EXAMINATION (Continued)

BY MR. BROWNSON:

Q. Good morning, Dr. Ilgren.

A. Good morning.

Q. We are now continuing your deposition on the case of Conwed versus Union Carbide versus others.

A. Yes, sir.

Q. And I wanted to begin by asking you some questions about a series of three recent papers that you have authored which are entitled "Coalinga Fibre - a Short Amphibole-Free Chrysotile," parts 1, 2 and 3.

Do you have those in front of you there?

A. Yes, I do.

Q. Why don't we begin by talking about those.

Now, these papers have been published in a journal called Indoor Built Environment, correct?

A. Yes.

Q. What kind of journal is that?

A. It is a peer review journal that is based in the United Kingdom published by Karger Press in Basel, Switzerland.

Q. How would you describe this journal? Is it a scientific journal, a medical journal or what is it? How would you describe it?

A. I would say it is a combination medical, scientific.

Q. Does it publish articles on medical topics?

A. Some. Occasionally a case report. Some medical articles.

Q. Do you know has it published other animal inhalation studies that you are aware of?

A. I don't know.

Q. Now, it indicates here that the authors of these three articles are yourself and Eric Chatfield, is that correct?

A. Yes.

Q. And I am now focusing on parts 1, 2 and 3 which we have in front of us here and we will get into this in some detail later, but

before you do, can you tell me what part of those three papers was done by you and what part was done by Dr. Chatfield?

A. I've done, I suppose, virtually all of the papers.

Q. Can you -- is there any particular item in the three papers, part 1, 2 and 3, that Dr. Chatfield did that you can point to us so we could mark that as something that was done by Dr. Chatfield?

A. No, sir.

Q. Now, are there further parts to this? I see reference to other things in press as I read these papers?

A. Yes, sir.

Q. What other parts are there?

A. Part 4 is the hygiene analysis which will also include a description of the physical and chemical properties, and there may be a part 5 talking about more general implications of the work, but there is definitely a part 4.

Q. Is that something that is actually being worked on at this point?

A. Yes, sir.

Q. So that is work underway?

A. Yes, sir.

Q. Is that in press, so to speak, or is it prepress?

A. It is prepress.

Q. And does Dr. Chatfield have something to do with the part 4?

A. Yes, sir.

Q. And just so I can understand a little bit about what this is, you say the part 4 will deal with hygiene conditions.

What do you mean by that, hygiene of what and where?

A. It will look at the fiber counts and size characteristics of Coalinga chrysotile as compared to Canadian chrysotile in air as noted on direct examination as opposed to in water following indirect examination.

Q. When you say in air on direct examination, are you talking about a direct analysis technique?

A. Yes, sir.

Q. Basically asbestos fibers are collected out of air through a pump and deposited

onto a filter and analyzed?

A. Yes, sir.

Q. And have those samples been obtained already or are these something that is going to be obtained in the future?

A. They have been obtained.

Q. Where are they from?

A. They are from the Muhle, et al. animal 1987 bioassay and also from filters taken by KCAC in the region of the -- I believe region from the mill but also perhaps the mine.

Q. And who has these materials at the present time?

A. Dr. Chatfield.

Q. How was he able to get air filters from KCAC Company?

A. I requested KCAC to have them sent to Dr. Chatfield.

Q. Did KCAC just give them to you?

A. They sent them to Dr. Chatfield.

Q. Do you do any work for KCAC or how is it that they give you filters?

A. I just called them and asked them to give filters to Dr. Chatfield.

Q. For example, when I called them over the years to get things, they wouldn't even talk to me. I am just curious why is it they talk to you and give you things?

A. I don't know.

Q. It is true, isn't it, because they know you are working for Union Carbide that therefore they are cooperating with you in your work and investigation?

A. Perhaps. I don't know.

Q. Well, who have you dealt with at KCAC in connection with obtaining these filters?

A. I have dealt with Mr. John Myers and Mr. Ed -- well, formerly Mr. Myers and presently Mr. Kleber.

Q. And both of those gentlemen of course know that you are an expert retained by Union Carbide in litigation and are working for Union Carbide, don't they?

A. I don't know.

Q. Well, have you told them that?

A. I haven't told them. They may know or they may not know.

Q. Who was it who made the introduction

to you to Mr. Myers?

A. I don't recall.

Q. Do you remember when you first contacted Mr. Myers about obtaining these air samples?

A. No, not really. Years ago.

Q. So you have had these air samples for several years or these filters?

A. At least.

Q. Is there any correspondence between you and Mr. Myers dealing with these filters or Mr. Kleber?

A. Not to my recollection.

Q. Was this contact between you and Myers or Kleber over the telephone?

A. Yes, sir.

Q. And was this direct contact between you and these two gentlemen or was it through intermediaries?

A. It was direct contact, as I recall; direct contact as I recall, it was direct.

Q. And in terms of these air filters you have obtained from KCAC, do you know when the filters were taken or the air samples were taken?

A. No.

Q. Is there any information that was provided to you that tells you when they were taken?

A. There may be.

Q. Have you seen anything that says when they were taken?

A. They went directly to Dr. Chatfield, so whatever he has may serve to identify the date. I just don't know.

Q. And are you of the impression these were taken in the mill?

A. I believe so. I don't recall exactly whether they were taken in the mill or they were taken at the mine.

Q. Now, with respect to these -- well, let me ask you this.

When do you anticipate this part 4 paper might be published?

A. Several months.

Q. And do you intend to submit it to the same journal, Indoor Built Environment?

A. Yes, sir.

Q. Now, with respect to these samples

from Dr. Muhle, this is the doctor in Germany, is that correct?

A. Yes.

Q. What have you gotten from him or what has Dr. Chatfield gotten from him? Are those filters?

A. Yes.

Q. And are those filters of samples taken within rat inhalation chambers?

A. Yes, sir.

Q. And who has those filters? Does Dr. Chatfield have those filters?

A. Yes, sir.

Q. Did he get those directly from Dr. Muhle or did you obtain those?

A. Directly from Dr. Muhle.

Q. Have you spoken to Dr. Muhle about getting those filters?

A. Yes, sir.

Q. Were you the one who originally requested them?

A. Yes, sir.

Q. And what did you do, call him up or write him or meet with him or what?

A. I can't recall. I certainly spoke to him on the phone several times.

Q. Did you ask him to send those filters to Dr. Chatfield?

A. Yes, sir.

Q. And these are filters from some animal inhalation study that he published in 1987?

A. Yes, sir.

Q. Do you know when those air samples were taken?

A. Not exactly, no, sir.

Q. But it would be sometime before '87?

A. Yes, sir.

Q. And was he willing to part with those filters?

A. Yes, sir.

Q. What did you tell Dr. Muhle that you needed the filters for?

A. For electron microscopical analysis.

Q. Did you tell him that you were working for Union Carbide and were interested in helping them in their litigations with Coalinga fibre?

A. I don't recall.

Q. And who is paying for Dr. Chatfield's analysis of these filters from KCAC and from Dr. Muhle, do you know?

A. Dr. Chatfield.

Q. So he is funding that work himself?

A. Yes, sir.

Q. And will Union Carbide reimburse him or pay him for any of that?

A. I don't believe so.

Q. Why is that is he just doing it --

MR. GERSON: Objection. You are going to ask him why Union Carbide is or is not doing something.

MR. BROWNSON: If he knows.

MR. WILL: Well, you can ask me.

MR. BROWNSON: I would rather ask Dr. Ilgren.

A. It is part of our independent research.

Q. Now, you have told us before about this independent research. Are you telling us, Dr. Ilgren, that the work that you and Dr. Chatfield have done and will do with respect to this Canadian or Coalinga fibre is being funded

solely by you and Dr. Chatfield?

A. Yes, sir.

Q. And you are receiving no reimbursement as part of that from Union Carbide or its attorneys?

A. Yes, sir.

MR. GERSON: Objection.

A. Why don't you qualify this research.

MR. GERSON: I was referring to the form of the question. You asked him in the negative.

MR. WILL: To put it simply, for example, he is being paid for his time here today to talk about these articles, but he was not paid by Union Carbide or by me or the Center for Claims Resolution to do the articles or the research the articles is based on.

BY MR. BROWNSON:

Q. And the TEM analysis that Dr. Chatfield is doing of the filters from Dr. Muhle and the filters from Dr. -- I'm sorry, from KCAC, he is just paying for that himself?

A. Yes.

Q. And as far as you know, he is receiving no reimbursement from Union Carbide or any Union Carbide lawyers or the Center for Claims Resolution in doing that analysis?

A. That's correct.

Q. Let's now turn to the papers we have in front of us, and I am looking at the first paper which is entitled part 1?

A. Yes, sir.

Q. Do you have that in front of you?

A. Yes, sir.

Q. And I am reading from the abstract.

But before we get to the abstract, let's get to the title, which is entitled "Coalinga Fibre - A Short Amphibole-Free Chrysotile."

Do you see that title?

A. Yes, sir.

Q. Are you the author of that title?

A. Yes, sir.

Q. Where in this paper is there any data that shows that the Coalinga fibre is indicated short?

A. I don't believe there are any in this

paper. I would have to go through it again. Off the top of my head, I don't believe that there is any specific data that indicated short.

MR. GERSON: If you need to take time to look through anything, you can take all the time you like.

Q. Would you agree with me as a general proposition that when scientific papers are published and the title of the paper, for example, talks about short chrysotile that as a general matter there is data in the paper that supports that and would let the reader see that this, in fact, is a short chrysotile?

A. I believe there is some reference to a future paper, which -- again I have to go through this -- which makes reference to our forthcoming paper that would let the reader know that there is data to that effect.

Q. And that's the part 4 that we spoke about a moment ago?

A. Yes, sir.

Q. So is that data then or -- I'm sorry, is Dr. Chatfield's electron microscope analysis now complete so that you have the data indicating

that the Coalinga fibre is short?

A. Yes, sir.

Q. What does that data show in terms of the size of the Coalinga fibre?

A. It demonstrates that an aqueous solution Coalinga fibre is 96 percent or more or less than 5 microns in length, and as I recall less than 1 percent shorter than 10 microns long.

Q. And this is analysis done by Dr. Chatfield?

A. Yes, sir.

Q. And do you know why that wasn't presented in this paper where -- part 1 where the Coalinga fibre is described as short?

A. Because the focus is on fibrosis.

Q. Will you will agree with me that this paper and parts 2 and 3, for example, contain references of other scientific papers that have data about the size of Coalinga fibre? Are you aware of that?

A. I would have to go through and check.

Q. Now, continuing in the title, it says "Coalinga Fibre - A Short Amphibole-Free Chrysotile," is there any data in this paper that

shows that Coalinga fibre is amphibole-free?

A. It is the same response in the part 4 that will be written with Dr. Chatfield, that information is contained in that as well.

Q. Would you agree with me again as a general matter in scientific papers if you don't have data to support these statements that normally you don't make a statement like that, if you don't have data contained within a paper?

MR. WILL: You have asked him two questions.

MR. BROWNSON: Well, I will ask the second question.

Q. Would you agree with me as a general proposition that in scientific papers if you are going to title it a short, amphibole-free chrysotile, a reader would expect data which would indicate it is short and amphibole-free?

A. I think there should probably be some reference to help the reader find the information in the paper.

Q. But is there any such way the reader could find the information out of this paper?

A. Again I would have to go through. I

believe there is a reference to our forthcoming part 4 paper.

Q. But of course the reader couldn't find that because it is not published, isn't that correct?

A. Soon to be published.

Q. But it is not published?

A. No, sir.

Q. And now I am looking at the abstract of the paper, and about two-thirds of the way down it says, and I am quoting, "The first, from Coalinga, California is comprised of fibres that are almost all less than 5 microns in length and is not contaminated with amphibole."

Do you see that?

A. Yes, sir.

Q. Would you agree with me that there is no data obtained in either this paper or in parts 2 or 3 that support that statement?

A. Yes, sir.

Q. Then you continue and say, "The other two," and I am quoting, "The other two, the Jeffrey fibre and the UICC/B standard, are both Canadian long fibre preparations with a minor

degree of amphibole contamination."

A. Yes, sir. I see that.

Q. Again, you are the author of that line?

A. Yes, sir.

Q. Is there any data presented in this paper that supports the statement that UICC/B and Jeffrey fibre are long fiber preparations?

A. No, sir.

Q. And would you agree with me that there are references cited in your paper, part 1, that describe both as intermediate length fibers and not as long fibers?

A. I think the descriptor's intermediate. I don't -- I think it is called an intermediate type. I don't think it is intermediate length, though I would have to see exactly where that is stated.

Q. Well, you are familiar with the UICC/B Canadian chrysotile sample, that's a standard sample?

A. Sure.

Q. And you are aware of the fact that that's a sample prepared from eight different

types of chrysotile from eight different mines in Canada?

A. Yes, sir.

Q. And with respect to the Quebec rating system, you are aware that that sample is what is known as intermediate length, it is not a long chrysotile as defined by the Quebec rating system?

A. I am not that familiar with the Quebec rating system. I am just looking for the word "intermediate" in the paper.

Q. I can represent to you that it is not in the paper which I found it in some of the references. The paper describes both the Jeffrey and UICC/B as long preparations, would you agree with me on that?

A. Yes, Jeffrey is long.

Q. And your paper says that.

So would you agree with me that your paper consistently calls the Coalinga short fibre and it calls these two Canadian asbestos as long fiber, correct?

A. Well, they are both, in quotes, "long" in relation to the Coalinga -- I mean in a microscopical sense the rating is a

microscopical -- my perception of it, the Canadian grading system is a microscopic system used to sort mining samples where we are talking more about things that are determined microscopically in percentages of fibers over 5 microns.

Q. Do any of the three papers you have published here which describe the Coalinga fibre as short and the Canadian fibre as long provide any microscopical data so the reader can determine if that fact is true?

A. Not in the first three parts.

Q. Again in this fourth paper that may or may not be published, that data may be presented?

A. Yes, sir.

Q. Again I am reading in the abstract. It says, "This report," and I am paraphrasing here, "concerns rats exposed to three," and I am quoting, "types of chrysotile."

Do you see that?

A. You are reading from the abstract?

Q. Yes.

A. Yes.

Q. Now, would you agree with me that in

fact there is only one type of chrysotile. Chrysotile is chrysotile. There are three types of chrysotile. Are there --

MR. WILL: In what sense are you speaking, mineral?

MR. BROWNSON: Mineralogically.

A. Mineralogically, I suppose that is true. Some people say there is a Jeffrey type or a Carey type or another type, but in the sense you are talking, I imagine that's the case.

Q. Again I am reading from the abstract. With respect to the Coalinga asbestos, you stated it "...is comprised of fibers that are almost all less than 5 microns in length."

Would you agree with me that there is no data presented in any of these three papers that supports the statement that almost all Coalinga fibers are less than 5 microns in length?

A. Yes, sir.

Q. Now, let's again focus on part 1 of your paper. As I understand, what you have done here is you have taken some data generated back in the 1978 to 1980 time period, some of which was published by Dr. Kent Pinkerton in a Ph.D. thesis

and you have gone back -- and very recently you have gone back and you have looked at that and you have drawn some conclusions from it, is that fair to say?

A. In small part.

Q. Well, is there any actual experimentation that you did that forms the basis of this paper? In other words, let me put it a different way. Let's ask a different question.

This paper draws conclusions based upon certain rat inhalation studies, would you agree with me on that?

A. Yes, sir.

Q. And those rat inhalation studies were conducted back in 1978 to 1980?

A. Yes, sir.

Q. And you personally had nothing to do with conducting those rat inhalation studies, isn't that correct?

A. That's correct, sir.

Q. And you didn't design those studies?

A. That's correct.

Q. And you didn't participate in any of the experimental work that was done there?

A. That's correct.

Q. Who were the people who did that, as you understand it?

A. Dr. Gene McConnell, Dr. Jack Moore, Dr. Jim Crapo, Dr. Kent Pinkerton, Dr. Connie Stone, Dr. Finas Cavendar, Dr. Bernie Atkins, Dr. Arnold Brody, Dr. Dan McLaurin and others.

Q. Now, were any of these scientists who actually did these rat studies invited to be coauthors with you on your papers where you are talking about the rat studies?

A. Yes, sir.

Q. And which were those?

A. Dr. Kent Pinkerton, and as I recall, Dr. Gene McConnell.

Q. And why was it that neither Dr. Pinkerton nor Dr. McConnell appears as a coauthor on your paper, parts 1, 2 and 3?

A. They didn't want to coauthor it.

Q. Why is that, do you know?

A. I don't know.

Q. Did either of them express any doubt to you as to whether this old data, which now is 18 to 20 years old, could actually be used to draw

the conclusions which you draw in these three papers?

A. No.

Q. Were either of the two asked to read and comment on these papers, parts 1, 2 or 3?

A. Yes.

Q. And did they do that?

A. Sorry, I answered a bit too quickly. Just reask the question.

Q. O.K. Were either of these doctors, Dr. McConnell or Dr. Pinkerton, asked to read and comment on the papers, parts 1, 2 and 3, that you have published?

A. On earlier drafts.

Q. And which of the two looked at earlier drafts or did both?

A. Dr. Pinkerton.

Q. How many drafts did Dr. Pinkerton look at, do you know?

A. I believe one.

Q. And do you know whether the draft that Dr. Pinkerton looked at was for part 1, 2 or 3 of your paper?

A. I believe it would have incorporated

all three. I believe there were two drafts which would have incorporated the first two. Sorry, there were two drafts which would incorporate information in all three papers as I recall.

Q. So it sounds like you had a draft prepared and then later on you split it up into three parts, would that be fair to say?

A. As I recall, that was the case.

Q. And you think there might have been two drafts like that?

A. Yes, as I recall, yes.

Q. And is it your testimony then that one of those two drafts was sent to Dr. Pinkerton for his review?

A. Yes, sir.

Q. Do you know when this was done?

A. I can't recall exactly when I sent them.

Q. And did he, as far as you know, did he read it?

A. As far as I recall.

Q. And did he discuss with you any comments or changes that he had?

A. Sure.

Q. Did he make any note of those or put them in any sort of written form?

A. No.

Q. Do you recall what comments or changes that Dr. Pinkerton had with respect to that draft he read?

A. I don't recall.

Q. Now, I am looking at the introduction of paper number 1, and in your second sentence, you talk about the Stanton hypothesis?

A. Yes, sir.

Q. Do you believe the Stanton hypothesis is correct when it says that short fibers are less harmful than long fibers?

A. Yes, sir.

Q. And do you believe the experiments that Stanton did upon which he bases that hypothesis are valid experiments?

A. Could you qualify "valid"?

Q. Are they experiments that you consider to be valid experiments?

A. What do you mean?

Q. Well, let me back up.

You say that you agree with the

conclusions of the Stanton hypothesis, correct?

A. Correct.

Q. Would you agree with me that the Stanton hypothesis is based upon some animal experiments that were done by Stanton?

A. Correct.

Q. And would you agree with me that those experiments are valid experiments and provided valid data on which you can reach your conclusion that this hypothesis is correct?

A. Well, I think they are insightful. Intrapleural inoculations of different types of fiber, some of which is over and some of which is under 8 microns in length have shown generally a distinction between tumor production and no tumor production.

Q. So you, of course, are aware that the Stanton hypothesis, as you said, is based upon injection studies where rats were injected with different preparations of asbestos fibers and other types of fibers in different lengths?

A. Yes, sir.

Q. And you have described those experiments as insightful.

Would you agree with me that in fact the Stanton hypothesis is based entirely upon injecting rats with fibers. There are inhalation studies there?

A. I am aware.

MR. BROWNSON: Can we take a very short break?

(Recess taken)

BY MR. BROWNSON:

Q. Dr. Ilgren, your three papers are broken into first parts -- I'm sorry, into three parts, and the first part deals with fibrogenesis, is that correct?

A. Yes, sir.

Q. And the second part deals with tumors, tumourigenesis?

A. Yes, sir.

Q. And the third deals with biopersistence?

A. Yes, sir.

Q. And again all three parts of the paper dealing with fibrogenesis, tumourigenesis and biopersistence are based upon these rat studies that we have described that were done back

in '78 and '80, correct --

A. Yes.

Q. -- by Dr. Pinkerton and these other doctors?

A. Yes, sir.

Q. And these studies were at that time undertaken by a group called the NIEHS, correct?

A. And others.

Q. Well, the NIEHS is what?

A. The National Institute for Environmental Health Sciences.

Q. This was a government organization, correct?

A. Yes.

Q. Would you agree with me that back at that time in the late 1970's, this government organization, government laboratory, the NIEHS, was setting up an animal inhalation study of asbestos, correct?

A. I think so. It was more complicated than that.

Q. But whatever the big picture was, I guess one of the things they did was they were setting up a study to dose rats with asbestos dust

in the air and have them inhale it and then study them, is that correct?

A. My only quibble with this is the National Toxicology Program in conjunction with the Medical Research Council of the United Kingdom, and I believe there was also some funding from the EPA, were also involved in this whole study so --

Q. Well, the Medical Research Council in the United Kingdom was doing or had done at that time rat inhalation studies with asbestos, correct?

A. I believe it was contemporaneous with this particular effort, the MRC, Medical Research Council study.

Q. Well, in short, in that era in the 1970's, these two groups were kind of working together, they were going to do two animal inhalation studies with the rats and asbestos, one over in England with this MRC group and then one here with the American government group and see if the results jibed. Would that be a fair summary of what was going on?

A. More or less.

Q. In any event, the American group, if we can use that term to kind of move this deposition along -- can we use that term, the American group?

A. Yes, sir.

Q. The American group was this alphabet soup of government agencies. We had the National Toxicology Program, the NIEH and the EPA gave some funding, but this American government group got rolling and they were going to do these inhalation studies with rats and asbestos?

A. Yes, sir.

Q. And certain experiments were done, and we will get into that in a moment, and inhalation chambers were set up and these rats were dosed with asbestos in these chambers, and they were examined at different periods of time. Would that be fair to say?

A. Yes, sir.

Q. And then the experiment ceased and some of it was published and some of it wasn't, some of the results, would that be fair to say?

A. Yes.

Q. And Dr. Kent Pinkerton, one of the

animal inhalation toxicologists working on this, used some of this data for his Ph.D. thesis at Duke University, correct?

A. Yes, sir.

Q. You cited that on a number of occasions in your paper, right?

A. Yes, sir.

Q. And Dr. Pinkerton was awarded a Ph.D. at Duke, correct?

A. Yes, sir.

Q. And now he has gone on and he has moved to California where he is a scientist in the University of California system, is that fair to say?

A. Yes, sir.

Q. Now, you have cited at a number of places in your paper Dr. Pinkerton's thesis which I understand that you had obtained and read prior to publishing your paper, correct?

A. Yes, sir.

Q. And I have obtained a copy here of his thesis and I don't know if you have got a copy there. We can look at my copy if we need to.

At page 23, he is describing the

Coalinga asbestos fiber and these two Canadian asbestos fibers the Jeffrey fibre and the UICC/B.

Do you remember his description of the three types of --

A. If you read it.

Q. O.K. Well, let me read it to you. He says, "For both the Jeffrey and the UICC/B chrysotiles, approximately 75 percent of the combined fibers and fiber clusters were less than 5 microns in length while less than 52 percent of the combined fibers and fiber clusters from the Coalinga chrysotile were less than 5 microns in length."

Do you recall that?

A. Why don't you show me the paper.

Q. I can show you the reference. It is page 23 of his thesis, and I have helpfully highlighted it in yellow.

A. What's the chapter heading?

Q. This is a double-sided thing so let's --

A. This chapter forms the basis of Pinkerton, et al. 1983, within which Pinkerton generally concludes that there is no significant

difference in the percentage of fibers greater than 5 microns long in an aerosol.

Sorry, I didn't mean to be unhelpful in mixing it up.

Q. So you agree with me that Dr. Pinkerton concluded in his Ph.D. thesis and later published the fact that a significant fraction of the Coalinga asbestos was greater than 5 microns in length?

A. In air, yes.

Q. And of course it is fiber in air that people and rats breathe, correct?

A. Yes, sir.

Q. And the data that we have just looked at in his thesis was, as you noted, presented in chapter 2 of his Ph.D. thesis?

A. Yes, sir.

Q. And as you have also noted and told us, that was later published by Dr. Pinkerton and other authors and assigned to the paper, correct?

A. Yes, sir.

Q. So when you said in the abstract of part 1 of your paper that the asbestos from Coalinga, California is comprised of fibers that

are almost all less than 5 microns in length, you would agree with me that that statement is contrary to what Dr. Pinkerton reports in his thesis?

A. Dr. Pinkerton also discusses the length found upon indirect examination and more or less concludes the same on the basis of that mode of preparation.

Q. Well, yes, he concludes again that they are not almost all less than 5 microns in length. About half of them are over 5 microns.

A. In air.

Q. Well, I read the quotation from Dr. Pinkerton's thesis correctly, did I not?

A. Yes, sir.

Q. He concluded that when Coalinga asbestos forms dust in the air, half the fibers, more or less, are greater than 5 microns in length?

A. Yes.

Q. Are you saying if you put those fibers in water, their length differs?

A. Yes, sir.

Q. Why is that?

A. Because they are weakly bound.

Q. Is there some data that Dr. Pinkerton has published which supports that conclusion?

A. Yes, sir.

Q. Where is that?

A. Discussion of the same paper.

Q. In the Ph.D. thesis?

A. It might be in the thesis, but it is certainly in the paper.

Q. But at least in the air, the fibers in the air, you will agree with me that Pinkerton says at least half are greater than 5 microns in length?

A. Yes, sir.

Q. Would you agree with me that despite quoting from Dr. Pinkerton's papers and Dr. Pinkerton's thesis in your own paper you do not present that data anywhere in your paper, do you?

A. No, sir.

Q. And when you say in the abstract that the Coalinga fibers comprise fibers that are almost all less than 5 microns in length, you don't make any distinction about whether it is in air or in water, do you?

A. No, sir.

Q. And also I note in Dr. Pinkerton's thesis, he says that there are a number of Coalinga fibers that are greater than 100 microns in length. Did you see that?

A. Yes, sir.

Q. You are aware of that fact, are you not?

A. Yes, sir.

MR. WILL: What fact? I object to the form of the question that Pinkerton said or the fact that there are fibers greater than 100 microns?

Q. That Dr. Pinkerton has reported that a number of, certain fraction of the Coalinga asbestos is greater than 100 microns in length?

A. Yes, sir, I seem to recall that.

Q. And again that's not set forth in your paper, is it?

A. Not here.

Q. Now, again on part 1 of your paper, you have a section entitled "Materials and Methods." Do you see that? Page 265?

A. Yes, sir.

Q. And you write, "The present study originated as part of a large joint investigation undertaken by the NIEHS in the United States and the Medical Research Council Pneumoconiosis Unit in the United Kingdom to compare the results of similar inhalation studies carried out at two different locations under nearly identical conditions." Is that right?

A. Yes, sir.

Q. That's what we talked about a minute ago in the late '70s, there were these two big rat inhalation studies?

A. 1975.

Q. When you speak of the present study, are you talking about your paper here?

A. Yes, sir.

Q. Would you agree with me that your work here was not part of the NIEHS study or this Medical Research Council study in England, this was some later analysis you have done yourself?

A. In that sense it is correct.

Q. And to the extent that first sentence of the materials and methods may imply to the reader that you are somehow affiliated with the

NIEHS studies or the MRC studies, that would not be correct, would it?

A. As having been involved in the design, for example, or the execution of the study, that would be correct.

Q. And it is also correct that as far as doing anything, you are not affiliated with those two organizations?

A. I have no affiliation with either the NIEHS or the MRC.

Q. And neither of those groups has said to you: Dr. Ilgren, we did these studies back in the late 1970's. Would you now go back and do some further work in connection with that, did they?

A. Kent Pinkerton asked me to help him write these data up.

Q. Well, then why didn't he appear as an author of this if he asked you to do it?

A. I don't know. He didn't want to.

Q. But Kent Pinkerton is no longer affiliated with either of those organizations, he is out in California, isn't he?

A. I don't know what his affiliation is

with the NIEHS.

Q. Neither the NIEHS nor the Medical Research Council in England has asked you, Dr. Ilgren, to do any study or work or analysis of these old rat studies, have they?

A. That's correct.

Q. And again in the "Materials and Methods," if you go down later in the first paragraph, you talk about what you call the "long" Jeffrey fibre and the "short" Coalinga fibre, correct?

A. Yes, sir.

Q. And you present no data in this paper showing that the Jeffrey fiber is long and Coalinga fibre is short, do you?

A. That's correct.

Q. If we didn't go back to Dr. Pinkerton's thesis, which you cite in your paper, the quote which you just read, Dr. Pinkerton's dose, there is more long fiber in the Coalinga than there is in the Jeffrey, isn't that correct?

A. With the qualifications that we just discussed.

Q. Those qualifications are measurements

in air, right? As opposed to measurements in water?

A. They refer to qualifications in air and water, that's correct.

Q. And again going back to these rat studies that form the basis of your papers here, rats are breathing asbestos, they are not drinking it, correct?

A. That's correct.

Q. Then you continue, under "Materials and Methods," now I am in the second paragraph, and I am down on the second sentence of that paragraph, you write, "None of the Coalinga and UICC/B morphometry data have been published in scientific, peer-reviewed journals though some have appeared in a thesis published by Pinkerton," correct?

A. Correct.

Q. I have read that correctly?

A. Yes.

Q. And the thesis published by Pinkerton was the one we just looked at, his Ph.D. thesis published in 1982?

A. Yes.

Q. Then you say, "And in several small abstracts." Then you list a bunch of abstracts, correct?

A. Correct.

Q. Now, you cite those abstracts in your paper, and I just wanted to look at a couple of them. One of them you cite is an abstract in the American Review of Respiratory Disease, correct?

A. Yes, sir.

Q. And you cite that at note 33 of your first paper, so I want to turn to note 33?

A. Note is the same as reference?

Q. Reference No. 33, correct?

A. O.K. thank you.

Q. I'm sorry, let me look.

Reference 33 in your paper is the Pinkerton thesis?

A. Yes, sir.

Q. And then you write, "And in several small abstracts by Crapo et al." reference 34.

Do you see that?

A. Yes, sir.

Q. "Pinkerton et al.," references 35 and 36?

A. Yes, sir.

Q. "And O'Neil et al.," reference 37?

A. Yes, sir.

Q. So if we now, to pick some of those, went, for example, to the Pinkerton one you cite at reference 35, this is the one in American Review of Respiratory Disease, correct?

A. Yes, sir.

Q. In 1981, in volume 123 part -- or number 4, part 2, right?

A. If you say so, but as I recall, that's the part.

Q. I am just reading from your reference 35.

A. I don't have a part. I just have volume 123, page 21.

Q. Now, would you agree with me that the conclusion that you draw and report in part 1 of your paper is that Coalinga asbestos is not fibrogenetic?

A. Fibrogenic.

Q. Fibrogenic, right.

Does not cause fibrosis in rats?

A. Yes, sir.

Q. Would you agree with me if you look in this abstract of the American Review of Respiratory Disease published by Dr. Pinkerton Pratt, Brody and Crapo, they do not say that, do they?

A. May I see your abstract, sir?

Q. First of all, have you read this before?

A. I believe so, yes. Thank you.

Q. My question will be can you show me where they say that Coalinga asbestos does not cause fibrosis in rats?

MR. WILL: That isn't what the article is cited for. To that extent, I would answer to your question, is that what you are asking him or a different question?

A. Your question to me is where in this article do they say Coalinga does not cause fibrosis?

Q. Right.

A. It's not stated in this.

Q. And in fact, that's not stated in Dr. Pinkerton's thesis either, is it? He doesn't come out and say Coalinga does not cause fibrosis in

the rats?

A. No, sir.

Q. In fact, if we look at this article in the American Review of Respiratory Disease that you cite at reference 35 by Dr. Pinkerton and these other doctors, Pratt, Brody and Crapo, they say after three months exposure the rats, if they are exposed, that after three months' exposure, all of the fibers including the Coalinga or the Coalinga cause injury to the epithelium and the interstitia, isn't that what they report?

A. Yes, sir.

MR. GERSON: We need to take a five-minute break.

(Recess taken)

BY MR. BROWNSON:

Q. Dr. Ilgren, another reference that you cite of another article published about this rat study which forms the basis of your paper is another article in the American Review of Respiratory Disease which is your reference 36, right?

A. Yes.

Q. And that's again by Dr. Pinkerton and

Dr. Pratt and Dr. Crapo of Duke University, right?

A. Yes, sir.

Q. And you reference that as a paper in 1981, although I notice it was actually published in 1980. Is that just a typographical error in your reference?

A. Probably.

Q. We are talking about the same paper here, aren't we?

A. Let me just double-check 36 against your -- yes, sir, it is probably a typographical error.

Q. And in that paper which is reference 36 of your part 1, these authors again say that after three months exposure, the patterns of injury in the rat lungs were similar in both of the treatment groups, and here they are talking about the Coalinga and the UICC/B, correct?

A. If I could just see that quickly. Thank you.

The NIEHS intermediate is actually the Jeffrey fibre, but that's what it says.

Q. So what we are looking at here -- thank you for that clarification -- is the

Coalinga and the Jeffrey fibre?

A. Yes, sir.

Q. Now, I note again what is described, what you have told us now is the Jeffrey fibre, is again described as intermediate range chrysotile fibers?

A. They refer to Jeffrey fibre repeatedly as the intermediate fiber, and I find this very confusing myself, where the intermediate size preparation is the UICC/B, so there is some crossing over of terminology.

Q. Right.

A. But the NIEHS intermediate is the Jeffrey fibre.

Q. You would agree with me that the NIEHS does not classify the Jeffrey as long fiber, they call it intermediate?

A. I don't think it is referenced to length, but again I don't know why they call it intermediate.

Q. Then I am looking at your note 37 in your paper which is another published abstract by these researchers of the rat study upon which you base your paper, and this one again was published

in the American Review of Respiratory Disease in 1981, correct?

A. Yes, sir.

Q. And this again is by Drs. O'Neil, Dr. Ken Pinkerton and Dr. J.D. Crapo, and it is entitled "Morphologic" -- I'm sorry it is entitled, "Lung Volume Changes in Rats Exposed to Chrysotile Asbestos," correct?

A. Yes, sir.

Q. And I am looking now in that paper and I am reading just below the middle table they write, "Interstitial fibrosis was seen histologically in all exposed animals at one year and increased in severity during the year in air," correct?

A. If that's what it says.

MR. WILL: Is that what it says?

MR. BROWNSON: Again I highlighted that in the yellow.

A. That's what it says.

Q. So if I can summarize here, the original or a group of the original scientists who exposed these rats back in this inhalation study in 1978 to 1980, have published and we have now

looked at a Ph.D. thesis and three abstracts, all four of which you have cited as references in your paper, part 1, correct?

A. Yes, sir.

Q. And would you agree with me that as we have just seen in all three abstracts, they say that all of the three types of asbestos including the Coalinga fibre cause changes in the lungs of the rats?

A. Yes, sir.

Q. Now, I am continuing in your paper, part 1 in "Materials and Methods," I am now down to the third paragraph?

A. Yes, sir.

Q. And you report or you write, "The records, histology slides, paraffin blocks and wet tissues of the animals on lifetime test were located in November 1995 at the NTP archives by Drs. Sils, Hamlin and Bridges as inventoried documents." Right?

A. Yes, sir.

Q. Then you continue, "All of these were reviewed solely by the senior author" -- that's you, right?

A. Yes, sir.

Q. -- "Aside from 45 cases that were studied in conjunction with Dr. J.C. Wagner," correct?

A. Yes, sir.

Q. Now, I want to ask you about that.

A. Yes, sir.

Q. I will try to move this along here, but in a nutshell, are you telling us there or writing there that what you did is you managed to find some of these old materials from the rat study in an archive, at the NTP archive, and you managed to find them and locate them and then you looked at some of them, would that be fair to say?

A. Yes, sir.

Q. You did this beginning in November of 1995, that's when you found them and then thereafter you looked at them?

A. Yes, sir.

Q. And then you published this series of three papers based upon your review of that material?

A. Yes, sir.

Q. Now, what I am not clear about is you

talk about 45 cases that were studied in conjunction with Dr. J.C. Wagner. When you found these in November of 1995, at that point did you give half of them to Dr. Wagner or was this some work he had done like before that or how did that work?

A. I asked Dr. Wagner if he would come to the United States in April 1996 to review some of these materials with me.

Q. So after you located them in November of 1995, you then asked Dr. Wagner -- is it Wagner or Dr. Vagner -- how do we pronounce that?

A. I don't know.

Q. It is the same on the paper anyway.

You asked Dr. Wagner to come over to the United States and look at 45 of these rat slides?

A. A subset of the slides.

MR. WILL: It is Chris.

Q. And did he do that?

A. Yes, sir.

Q. And he did that, he came over in the spring of 1996?

A. Yes, sir.

Q. And did you and he then sit down and look at some of these materials?

A. Yes, sir.

Q. Where was that done?

A. At the NTP.

Q. That's down in North Carolina?

A. Yes, sir.

Q. At their archives there?

A. Yes, sir.

Q. Did you find these materials to be well organized and in good order, or did it take some real digging and work to get them?

A. That's two questions.

Q. O.K. Well, it is two questions, right.

Were they easy to find?

A. No.

Q. They were hard to find?

A. Yes.

Q. But once you found them, did they appear to be complete and in good order?

A. Yes.

Q. And you and Dr. Wagner then sat there at the archive and then looked at them, correct?

A. Yes.

Q. What was it exactly that Dr. Wagner did with these 45 things? I mean strike that. Let's back up.

What were these 45 things that he looked at, were they slides?

A. They were the slides representative of 45 animals.

Q. And is there any reference in your paper here in any of the three parts of the paper as to which 45 rats were examined by Dr. Wagner?

A. No.

Q. And we are going -- in a minute I want to go through some of your tables and data that you present where you talk about all these different rats. But when I read these papers, I couldn't tell, you know, which of those had -- were those examined by Dr. Wagner and which of those were examined just by you, and would I be correct in saying there is nothing here that specifically will tell us that?

A. Not in these papers, no.

Q. Was there any laboratory equipment made available to you and Dr. Wagner at the

archives to do this or did you just look at them by eyes or how did you look at that?

A. They gave us a double-headed microscope. They said if we wanted to look at any of the tissues, we could do so under a covered hood.

Q. So you could look at them through the microscope, but you had to use the hood, is that right?

A. No. We had an ordinary light microscope, and the slides were made available and so we just went through, say, for each animal just for an example there might have been 30 or 40 histology slides, so we would go through those and if we had any questions about wet tissues or if we had any questions about, say, paraffin blocks, they would find for us, but we didn't have to do that. We just looked at the histology slides.

Q. Through the microscope?

A. Yes, sir.

Q. How long did that take?

A. About -- as I recall 8-1/2, 9 hours.

Q. And on that trip in the spring of 1996, where did Dr. Wagner come from, England?

A. He came from England.

Q. At your request?

A. At my request.

Q. Was that specifically to come and look at these slides that you have found in the archive?

A. Yes.

Q. And again just so we are clear, these are slides of these rats that had been exposed to the three types of airborne asbestos, some 18 years before?

A. Yes, sir.

Q. And on that particular trip, did Dr. Wagner look at anything else or do anything else in connection with the work which resulted in these three papers?

A. We spent several days going over the various data sheets and information that I had received from the archives just talking about study overall, talking about how in his opinion it originated and who was involved.

Q. And was Dr. Wagner ever asked to be a coauthor on any of your three papers?

A. Yes.

Q. But I see he is not. Why didn't he do that?

A. He didn't want to.

Q. Do you know why?

A. No.

Q. Who paid for Dr. Wagner to come over -- fly over from England and spend several days in North Carolina looking at this stuff?

A. I did.

Q. Did you pay that out of your own pocket or did you get reimbursed by anybody for that?

A. Out of my own pocket.

Q. So you didn't submit any of those bills or travel expenses or vouchers or anything to Union Carbide or their lawyers or anybody?

A. No, sir.

Q. Now, before we go on to ask you about the rats, let me back you up to page 265, part 1 of your paper.

A. Yes, sir.

Q. And on the second full paragraph on that page, you say, "Aside from the study by Davis and Jones, the most recent short fibre chrysotile

inhalation investigation was conducted at NIOSH," which is all capital letters, "by Platek et al."

Do you see that?

A. Yes, sir.

Q. Now, in reviewing the literature and even references that you cite, I note that Dr. Muhler -- Muhle in Germany had a study published in 1987, which was an animal inhalation study, chrysotile. Are you aware of that?

A. Yes, sir.

Q. And isn't it correct, then, that the most recent study was really Muhle's study and not this one by Platek in '85?

A. As published, my explanation is that the NIOSH work as cited in this paper, it has been continuing. Platek published a rodent phase, but also a sort of monkey phase that is being completed now. So I suppose I was thinking that that work is still actually ongoing, the Platek work, but as cited, you are right, you are correct.

Q. And again in that same paragraph, the next sentence said, "These workers exposed rats and monkeys via inhalation to a highly purified

short fibre chrysotile sample," right?

A. Yes, sir.

Q. When you say "highly purified," what do you mean by that?

A. I suppose a sample that was very short, highly ground down to a short length.

Q. So in other words, it had been milled?

A. Yes, sir.

Q. Because in fact if you look at Platek's paper, you will see they used a mill, they used a ball mill?

A. Yes.

Q. They didn't purify it. They just milled it in a -- ground it up?

A. Yes, sir.

Q. So to the extent your term "highly purified" may imply that it was pure chrysotil with no impurities or no contaminants, that's not correct?

A. That's correct.

Q. Now, let's continue then in your paper in the "Materials and Methods." Now I am on page 266, and you have the heading "Animals."

Do you see that?

A. Yes, sir.

Q. Here is where I did not find your paper to be clear. I cannot figure out how many animals are involved here, and I would like to run through with you and take a little time to try to figure that out, O.K.?

A. Yes, sir.

Q. You begin by saying "Four hundred 5-week old specific pathogen-free (SPF) male and female Fischer 344 rats" were involved in this experiment, correct?

A. Yes, sir.

Q. And again, just so we are clear here, you did not select these rats or have anything to do with the rats or raise them or do any of these experiments. You are talking about this old experiment that Pinkerton published his thesis on?

A. Yes, sir.

Q. Then you continue by saying three hundred and thirty of these form the basis of this study."

And by that, do you mean your paper?

A. Yes, sir.

Q. Now, my question is how did we go from 400 rats to 330? Was it that when you went to the archive you could find material on 330 of the rats or did you find all 400, or how did that come about?

A. There was an additional group to which animals were exposed to a JM 100 microglass fiber preparation. And that group is not discussed in my paper. But it would be amongst the 400 animals. Is that clear?

Q. I believe so.

You are saying back in this study of the rats, some of the 400 rats were exposed to a Johns Manville asbestos?

A. No, a JM 100 microglass, a glass fiber.

Q. A glass fiber?

A. Right. There was untreated control, Coalinga, UICC/B, Jeffrey and JM 100 fiberglas in the original study. So the discrepancy between 400 and 330 pertains to the absence of the JM 100 group.

Q. Now, let me just start then with the controls.

Why was it that you did not examine or look at the JM glass fiber treated rats?

A. I just didn't have time.

Q. So you did not look at them, and they did not form a part of this paper and you still haven't looked at them, would that be fair to say, you set those aside?

A. Yes.

Q. Do you remember how many of those there were?

A. I believe there are 70 or 80 cases. Split male/female, half and half, more or less.

Q. Well, you write in your paper 330 of the rats formed the basis of your work, so it must mean there were 70 of them, would that be fair to say?

A. About 70.

Q. So that left 330 then that were exposed -- that were either control rats or were exposed to the three types of asbestos, Coalinga UICC/B and Jeffrey?

A. Yes.

Q. Now, let me continue under "Animals" because I need some -- I need to be careful here.

The next sentence you write, "Upon receipt at the NIEHS, 5 animals of each sex in each exposure group of the 360 rats were randomly selected and killed for morphological study."

Do you see that?

A. Yes, sir.

Q. Now, my first question is you say of the 360. See, now earlier you said there were 400, but now we are down to 360, and I am wondering what the difference is there?

A. That 360 may need to be 330. So there may be an error.

Q. So is that the same 330 as the 330 that you looked at, is that what we are talking about?

A. Yes, yes.

Q. So that is simply an error. What that should read is 330 rats were randomly selected or five of each sex in each exposure group out of 330 were randomly selected, right?

A. I believe that's true.

Q. So --

A. If you look for clarification in the legend at table 1 and you will see in the second

line at "Time zero," five animals were sacrificed for morphological analysis. As the animals arrived, five were taken out of each group and removed just for baseline studies, and when I say upon receipt at the NIEHS, five animals of each were removed, that's what it refers to, is as the animals came in. They may not have been of each sex.

Q. That was my next question.

You report in the text under "Animals" that upon receipt back at the NIEHS study in the 1970's five animals of each sex in each exposure group were selected and killed for morphological study, and then as you note up above at note 1 of your table 1, you say five animals were sacrificed.

So my question is this: Was it five that were killed at the outset from each group or was it ten?

A. I think the number is -- numbers work out in the following way: I know I have just told you that the 360 should be 330, but it may be the other way around. If upon receipt, 360 animals arrived and at time zero 40 animals were taken out

to leave 320 animals divided four ways, one for untreated controls, one for Coalinga, one for UICC/B and one for Jeffrey, 10 of each were removed, meaning 4 times 10 are 40. This leaves one with 320 animals remaining, all in test or 40 per -- or 40 in each group after the five were removed.

Q. Now, do you know that that's what occurred or are you just surmising that by looking at this?

A. That's -- that's what I know to have occurred in the sense that 40 animals were on test at the time zero, and from those four animals were removed at each of the subsequent time points being 3, 12 and 24 months, so in table 1, if you see at time zero, there is 45, to take off five, that leaves 40. At three months there is 36 from which to take off 4, giving 32; and then at time -- at point 12 months, they take off four to give 28.

Q. Before we get to the actual inhalation and how they killed them at various lengths of time, I want to go back and start with what they started with because I am still not

clear.

Are you saying that at this initial killing or sacrifice, when they started, they took a total of 10 rats -- well, strike that.

I will ask you the question. 400 rats arrive at the NIEHS sometime in the late 1970's?

A. Yes, sir.

Q. Then you describe that a bunch were killed right up front for morphological testing?

A. It would appear 40 were taken out for JM 100, and then a set were taken out for morphological baseline testing.

Q. So we start with 400 and some were then to be used with the JM 100 fiberglas?

A. Yes, sir.

Q. You are saying you think that might have been 40 but earlier you had said 70?

A. No, I was mistaken.

Q. O.K. So we start with 400. 40 then were to be exposed to the JM fiberglas?

A. Yes, sir.

Q. That leaves us with 360, and now you are telling us of those 360, then 40 were killed

right up front?

A. Yes, sir.

Q. And that would be 10 of each sex?

A. Five of each sex.

Q. Five of each sex?

A. In four groups. So that's 10 per so-called group, a group being either a treatment group or the untreated control.

Q. And at this point, they haven't been treated with any asbestos, yet they have just been divided into these groups?

A. Yes, sir.

Q. And they killed some to get some kind of a baseline before they start the experiment, is that fair to say?

A. Yes, sir.

Q. So they kill 40 and that leaves us with 320 rats, right?

A. Yes, sir.

Q. And those 320 rats then, as I read your paper, were then put into four groups, untreated controls, Coalinga exposed, UICC/B exposed and Jeffrey exposed?

A. Yes, sir.

Q. 320 divided by 4 is 80, so did they put 80 in each?

A. Yes, sir. 40 male and 40 female.

Q. Now, let me move back up earlier in the paragraph and again focus on your statement that 330 formed the basis of your study. In other words, you looked at 330 of them.

My question is: If we start with the 400, we pull out the 40 JM 100 exposed animals, then we have the 40 that were killed up front, leaving 320. And then of the 320, they were divided into four groups of 80.

Where does the 330 that you looked at factor into that equation?

A. I think the 330 then would be 320.

Q. So when you said 330 in your paper, you meant 320?

A. I believe so, yes.

Q. Now, when you say you believe so, do you know that or are you just again surmising or what?

A. Well, to the best of my analytical ability discussing it with you and from recollection of what's written in the relevant

papers where these data appear such as Pinkerton's thesis, Pinkerton et al. 82, the Becton-Dickinson documents and other documents, it is what I would conclude.

Q. So can we then state that in your paper when you say you looked at 330, that's an error, you really looked at 320?

A. I believe so, sir, yes.

Q. Now, we are down to the 320 rats that formed a part of the experiment, and we have got them divided into four groups of 80, 40 males and 40 females?

A. Yes, sir.

Q. Then as I understand this experiment that was done again at the NIEHS group back in the 70's those animals were then exposed to asbestos, the controls weren't 80 were not -- 240 were exposed?

A. Yes, sir.

Q. Now, of the 80 controls, I saw reference in your paper to the term "sham control," and I was a little confused by that.

As you understood the experiment, were the 80 control rats just exposed to nothing

or were some somehow hooked onto the apparatus or how did that work?

A. I think they were totally untreated animals. There was no so-called shamming or put-on apparatus. This is as far as I understand the study.

Q. So 80 were put in cages --

A. And whole bodily set aside, untreated controls.

Q. So that then left 240 rats exposed to asbestos, is that right?

A. Yes, sir.

Q. And again we went over this a moment ago, but of the 240, 80 were exposed to UICC/B chrysotile, 80 exposed to Jeffrey chrysotile, and 80 exposed to Coalinga chrysotile?

A. Yes, sir.

Q. 40 of each sex exposed to each of the three types?

A. Yes, sir.

Q. And then based upon this information that you found in the archive and what you reviewed, et cetera, you saw that at different intervals of 3, 12 and 24 months some were killed

out of each group, and you report that in your table 1?

A. Yes, sir.

Q. Now, is table 1 a table that you prepared and designed or did you take that from some of the earlier published work?

A. No, I designed that table.

Q. So table 1 -- well, let me do it. This will be faster.

Why don't you describe for us what table 1 shows. Run through that and tell us what that is.

A. Table 1 indicates the so-called design of the study with reference to the number of animals found at each time point, so at time zero with 45 animals of each sex in each group, that makes 90 per group or 4 times 90 is 360, hence the 360.

At time zero, looking just at one sex in any one group, five animals are removed for the baseline sacrifice to be studied morphologically leaving 40 animals to go on to this lifetime test.

Q. Right.

A. So that at time zero, four additional

animals are taken off to be studied using the electron microscope for morphometrical analysis, and then the same is done at 3, 12 and 24 months, so that absent the animals taken off for this baseline morphological study, leaving 40, as you take off 4 at time zero, you end up at three months with 36.

And then again if you take 4 more out of that, for morphometrical study, you end up with 32 by 12 months.

And again if you take 4 more at that point, you end up in theory of 24 months with 28 animals of each sex on a lifetime test.

Q. O.K. Now, did your review of these materials in the archive indicate that that is actually what occurred?

A. I did find -- when I actually looked at the data at the archive, one or two so-called extra animals, so that instead of at the 24-month time point, there being, say, just 28 months on test in one or two cases there was 29 or 30, and I couldn't work that out. I don't know where the extra animal came from. But by and large that -- this is -- this conforms to what I found in the

vast majority of the cases.

Q. Now, did you find something in the study design or study protocol where they said here's what we are going to do. We are going to kill four of each sex at zero time and 3 months, at 12 and 24 out of each of the groups and -- stop there?

A. Yes, sir.

Q. That was the intent of the original study?

A. Yes, sir.

Q. Then what you are saying is when you went back to the archive and looked at it, you determined that that in fact is what they did, but as you have noted, you found that they also had one or two extra animals?

A. In one or two instances.

Q. And were these one or two extra animals that you found in one or two instances killed animals at one of these time periods or were these animals that remained alive?

A. They appeared to have remained alive.

Q. You report, now continuing under "Animals," a total of 96 rats out of the 320 were

used for morphometric study.

Is that what you mean there, these are the ones that were killed along the way at 3, 12, and 24?

A. Yes, sir.

Q. So again backing up, I am trying to be as clear as I can here, the study started with 240, four groups of zero control and the three asbestos groups. 96 were killed along the way, so we should really, to determine how many are left, subtract 96 from 240, and we get 144. And then what you are saying is you noted that there were one or two extra ones here or there, am I right on that?

A. There would be -- well, the 96 doesn't appear to take into account the morphometrical analysis of the time zero animals. It would just appear to be 3, 12 and 24 months.

So just for example, you have at any one time point four animals of each sex of any group untreated or treated removed. So that would be 32 animals for -- at any one time point in all. Do you understand?

Q. Right. So should there really be 108

that were killed in the original experiment and not 96?

A. Right, I believe it is 120 --

MR. WILL: Wait, can we just go off the record for a second.

MR. BROWNSON: Sure.

(Discussion off the record)

MR. BROWNSON: Let's go back on the record, and Dr. Ilgren will continue the answer.

MR. GERSON: Why don't you repeat the last question.

BY MR. BROWNSON:

Q. Let's start at this present position.

I think we were clear, Dr. Ilgren, until we ran into this number of 96 and now you are now explaining to us that that number really is something different and what it is and can you describe that for us?

A. Yes, I could.

Q. O.K.

A. The 96 rats pertain to the number of animals that were analyzed morphometrically at 3, 12 and 24 months. It does not pertain to the

morphometrical analysis of the animals studied at time zero, so there should be an additional 32 animals added to the 96 to give a total of 128 rats.

So the sentence that reads, "A total of 96 rats out of 320 were used for morphometric study" should actually continue as at 3, 12 and 24 months. In fact, a total of 128 rats in all out of 320 were used for the entire morphometric study.

Q. So there should be an additional sentence in here that says a total of 128 rats were used for morphometrical study?

A. Yes, sir.

Q. So can we then take the 240 rats ready to be studied and subtract 128 that were killed along the way beginning at time zero to get our remaining live rats?

MR. WILL: 240?

MR. BROWNSON: 240.

A. It should be 128 out of 320, I believe. Is that what you asked me? 128 out of 320?

Q. No, that's not what I asked you.

MR. WILL: Because 240 is not the right number.

Q. Let me do this.

Again I don't want to rehash what we already did but after the -- when we took -- when they took the 400 rats that came into the laboratory and then after taking out the JM 100 rats which was 40, we ended up with 360 and then they killed off five of each sex out of four groups, 40 more?

A. Right.

Q. And that brought us down to 320 rats, right?

A. Right.

Q. O.K. Now I see where my confusion is, O.K.

So we have 320 rats ready to begin the experiment. 80 were controls and 240 were to be controlled to asbestos, is that fair? Is that correct I mean?

A. Yes.

Q. Total of 320. And then what you are saying is 128 were killed along the way at zero months, 3 months, 12 months and 24 months?

A. For morphometric study, yes.

Q. So what we need to do to determine how many rats remained alive is subtract 128 from 320?

A. I believe so, yes.

Q. What do we get there?

A. 192.

Q. So that leaves us with 192 rats?

A. Yes, sir.

Q. And again without going into great detail and belaboring this, your paper here, parts 1, 2 and 3, was a review by you, and I guess to some extent Dr. Wagner of these 192 rats or -- I'm sorry, the remaining slides or tissues or whatever of these 192 rats that remained alive after the study to see what happened to them at their death. Is that fair to say?

A. Yes, sir.

MR. GERSON: Could you repeat the preceding question.

(Record read)

A. Plus an analysis and synthesis of the morphometric data in connection with the morphological or histological studies based on

those materials.

Q. So if -- I am trying to summarize this to put it in laymen's terms.

A. Sure.

Q. What you were trying to do here is, going back to this archive and pulling out this old material of the rats who now are all long dead, and you were going to examine them -- one of the things you were going to examine is of the remaining 192 living rats, after the experiment they eventually all died or were killed and some tissues or slides were kept of those and you were going to look at those, right?

A. Yes.

Q. And that's what you did, correct?

A. Yes.

Q. And then in addition to that, you have just told us that you also in your paper talk about some of this morphological data of the killed rats that Pinkerton and these other prior people have done?

A. Well, they did morphometrical studies, studies done -- largely by an electron microscope, some light microscopy, but I suppose

what I was saying before was that in 1992, I was sent morphometrical data and some tables which presented some pathology data, and I was asked to put together a manuscript and publish this material.

And at that time when I looked at the data tables for the animals on lifetime tests, you know, we are talking about the animals that you and I have been talking about just now, it was clear that 40 to 50 percent of the animals were missing, and so I satisfied myself with -- through discussions with Kent Pinkerton that I had most of the morphometrical data.

But at that time neither Kent nor I knew where the rest of the animals were that had been on lifetime test, so at that point we agreed that we needed to try to make that determination, and so the study was originally conceived of an analysis not only of the morphometrical data but the so-called -- when I say morphological data, I am talking about the animals on lifetime test as analyzed by traditional light microscopic means -- but Kent and I both agreed that the study should be an attempt to integrate both his own work done

by an electron microscopical analysis and morphometrically with these lifetime studies which had never really been published before.

Q. Now, you say you were asked in 1992 to publish the morphometrical data. Who asked you to do that?

A. I said to publish the whole thing as an integrated work.

Q. Who asked you to do that?

A. Kent Pinkerton.

Q. Did he come to you and say, "Dr. Ilgren, I have selected you to do this," or had you gone to him before then, or how did this contact originate?

A. In 19 -- I believe in 1991, I came across Kent's 1983 paper entitled something like "A characterization of the size of Three Fiber Types in an Aerosol," the three fiber types being the Coalinga the UICC/B and the Jeffrey fibre.

And I read the paper through, and I believe as we have discussed in the earlier depositions, he had indicated in his paper, in that 1983 paper, that he felt the implications of his findings had a biological relevance and the

manner in which the data had been set out, the thoroughness and the completeness in which the data had been set out in this 1983 paper suggested to me that this was a part of a much larger study. It was the so-called exposure data analysis of a large inhalational bioassay.

And so just to sort of confirm or refute that particular suspicion, I tried to find Kent Pinkerton, and I looked in the 1983 paper and he was listed at Duke. And so I called the Department of Pathology at Duke and I asked for Kent Pinkerton, and they put me on to Dr. Crapo. And Dr. Crapo had said that he had moved to California.

But since Dr. Crapo was involved in this particular study, I said, "Well, I am calling because I have a suspicion that there might be other data attendant to this particular study. Is that true?"

And he said, "Yes, there is mountains of unpublished data."

And so I said, "Well, I would be very interested in speaking with whomever might be able to tell me more about these data."

And he said, "Well, I will give you Kent Pinkerton's telephone number in the University of California."

So I believe late 1991 or some point maybe in early 1992, I called Kent Pinkerton in California and I told him that I have been looking at the issue of long versus short fiber for some years and in particular the Coalinga fibre, and he said that indeed he had a great deal of unpublished data and that he wrote me a letter and he said, I believe it was in December 1992, in this letter that he would be pleased if I sort of cut, paste, did anything I want with this to put it into a written manuscript.

Q. Did he give you permission to use the data out of his doctoral thesis and the tables and that sort of material?

A. Yes, sir. He sent me chapter 4 of his thesis, two rough manuscripts, five tabular summaries of the lifetime bioassay, the animals in lifetime test, and there were also two letters, one from Jim Crapo from 1979 addressed to Gene McConnell and another 1991 from Dr. Crapo to Kent Pinkerton. I believe that's all he sent me at the

time.

Q. Do you still have those materials?

A. Yes, sir.

MR. BROWNSON: Can we get those, Trevor? Just the letters.

MR. WILL: Let's talk about it.

MR. BROWNSON: O.K. let's move on here.

Q. And is it then this paper, part 1, 2 and 3 that we have been talking about this morning that is the resulting published work by you that originated back in 1991 when you called Dr. Crapo and Dr. Pinkerton?

A. Yes, sir.

Q. And again I am wondering why neither since they -- this was their original work, is a coauthor on your papers?

A. I beg your pardon?

MR. WILL: I already asked him that.

MR. BROWNSON: I asked about Dr. Pinkerton. Now I am asking about both.

Q. This was the original animal toxicology work for Dr. Pinkerton and these other doctors in the 1970's at the national program we

talked about, and you now have examined some of that data and published it and you told us that Dr. Pinkerton invited you to do that and sent you a bunch of material.

My question is do you know why none of those doctors who did the original work are coauthors on your paper?

MR. WILL: I object to that. He only talked about the three that he was in contact with, McConnell, Pinkerton and Crapo. Earlier he gave you a much longer list of doctors.

MR. BROWNSON: Now I am asking how come none of them are on the papers as coauthors.

A. I believe I said they didn't want to be on the paper.

Q. And again is it your testimony that none of them gave you a specific reason why they didn't want to be on the paper?

MR. WILL: Here is my problem. You have now expanded the question.

MR. BROWNSON: I know.

MR. WILL: But you have done it in a

way that makes it suggest that he talked to a lot of doctors about being on the paper when there isn't any testimony that he did.

MR. BROWNSON: Let me back up and clarify this then.

BY MR. BROWNSON:

Q. Is it true that you just asked two people to be coauthors -- I'm sorry, three people to be potential coauthors, Wagner, McConnell and Pinkerton?

A. To my recollection.

Q. And none of the three or all three declined to be coauthors?

A. As I recall, yes.

Q. And now my new question is: Do you know any of the specific reasons why any of the three decided not to be coauthors?

A. Well, I didn't ask Chris so specifically, but I had the sense from our discussion and interaction that he didn't feel that he had done, and again I am surmising, but I had the sense that he felt he hadn't done that much work on the overall projects, i.e., this specific present study that he wanted to be a

coauthor. That's my -- that's what I surmise. I don't have a -- and I believe the same with McConnell.

Q. How about Dr. Pinkerton, though, because this, after all, was his data?

A. I don't know.

Q. So what you have just told us, if I can again try to sum this down to a nutshell, is that your own interest here began really when you saw this 1983 paper by Pinkerton and Brody and all these authors and that you read it and you wanted to follow up on it, and you placed this call to Dr. Crapo, and away you went?

A. And away -- sorry, I was -- could you just repeat that?

Q. This work that you have done has now resulted in the three-part paper that we have been looking at, again to summarize and paraphrase, really began when you found and read this 1983 paper by Dr. Pinkerton and Crapo and other authors sometime in about 1991 or so, is that fair?

A. Well, these specific papers but I -- I mean I had gone to visit Dr. Muhle in Germany in 1988, and I have been talking to him about

Coalinga fibre at about that time or somewhat earlier. So I suppose the inception time in my own mind is before my 1991 discussion, but the data that goes into these specific papers is really time-lined or begins in that 1991 period that we just talked about.

Q. And kind of a seminal event, if you will, is when you found this 1983 paper called "Three types of" -- called "Characterization of Three Types of Chrysotile Asbestos after Aerosolization," reference number 45.

A. Yes.

Q. Now, your first contact then with the authors of that old rat study we have been talking about all morning was the telephone call you placed to Duke University, and you ended up getting Dr. Crapo, right?

A. Yes, sir.

Q. He then put you in contact with Dr. Pinkerton, right?

A. Yes, sir.

Q. Did you speak in addition to any of the other coauthors, that would be, Dr. Brody, Dr. McLaurin, Atkins, O'Connor, Pratt?

A. Yes.

Q. And which of those did you speak to?

A. I spoke with Dan McLaurin, Bernie Atkins, I think it is Dr. O'Connor,

Q. How about Philip Pratt?

A. No.

Q. How about Arnold Brody?

A. I can't recall. I don't believe so.

Q. Do you know if at any time between 1991 and today whether you have told Dr. Crapo that you are doing work for Union Carbide in consulting work in asbestos litigation?

A. I don't believe I told him. I believe Kent Pinkerton and I talked about that at some off and on, and he was aware of that from the beginning.

Q. Now, in your papers, parts 1, 2 and 3, you have various acknowledgements to different people, correct?

A. Yes, sir.

Q. Now, here in either of the three parts of your paper, however, is there any acknowledgement to Union Carbide, is there?

A. Not that I can see, no.

Q. You would agree with me, however, that during the time period you worked on this paper and up until today, you have been doing consulting work for Union Carbide and its attorneys in cases involving Union Carbide Coalinga asbestos, right?

A. Yes, sir.

Q. Do you think, Dr. Ilgren, it would be a good practice to have made some mention of that fact in the acknowledgements so that the reader of these three papers would know that while you were concluding that Coalinga asbestos is, to use your term, a nuisance dust, you are consulting with and helping Union Carbide in its asbestos litigation?

A. I didn't think that influenced my analysis and interpretation.

Q. Would you agree with me that that is a bias that some reader might be interested in knowing, however?

MR. WILL: Well, I object to the form of the question.

It is not common practice in the scientific industry for authors to put down everybody they consulted with.

MR. BROWNSON: No, it is not but -- strike that.

BY MR. BROWNSON:

Q. You told us at a prior deposition that during the time over the past few years, and that includes the time you were working on these papers, your main source of consultation income has been from Union Carbide in its asbestos litigation, correct?

MR. WILL: Well, I object to the form of that question, too. He said what he said during certain periods of time. I don't think that is true for the entire period of time.

A. I would have to receive the Q and A. I don't believe I said that.

Q. Over the past several years, you have been doing consulting for Union Carbide in asbestos litigation, correct?

A. Yes.

Q. Including this case, by the way, right?

A. Yes, sir.

Q. One we had versus Union Carbide?

A. Yes.

Q. During the same period of time, you were working on these three papers, correct?

A. Yes, sir.

Q. And you have published and in your papers concluded that although Canadian asbestos can be both fibrogenic, tumourigenic and biopersistent, Coalinga asbestos is not?

A. That's what the data shows.

Q. That's what you have concluded, correct?

A. On the basis of the data, but that's my conclusion.

Q. Now, with respect to the asbestos that was given to these rats in the Pinkerton study back in '78 to 1980 --

A. Yes, sir.

Q. -- Dr. Pinkerton in his thesis and Dr. Pinkerton in his published paper in 1983 describes that as "Coalinga mine chrysotile," is that right?

A. I believe so, sir.

Q. And as you know, Dr. Ilgren, there have been historically at least three operating

mines in the Coalinga deposit. We know which of the three this chrysotile came from?

A. COF25 Calidria

Q. How do you know that?

A. I have the documentation.

Q. So this is Union Carbide Calidria chrysotile that these rats were exposed to?

A. Cyclone overflow.

Q. From the Union Carbide mine, correct?

A. Yes.

Q. By the way, and I disagree a little bit.

Are you aware that the California chrysotile that Meltoni used in his experiments was also from the Union Carbide mine because I see you take some pains in your paper to raise some doubt as to which mine that came from?

A. He never responded a reply to that effect, so what do you base that on?

Q. I based it on the fact that Dr. Langer gave it to him and Dr. Langer got it from that mine?

A. That's interesting.

Q. So now you know that there is a

helpful piece of information?

A. It is another piece of information.

Q. I am looking at the 1983 paper that kind of started all this study for you that we have talked about, and I take it you have read that paper and you familiar with it, correct?

A. Yes, sir.

Q. Now, that paper has a table called "Table 1," which is entitled "Gravometric Measurements for Each Chrysotile Preparation in the Exposure Chapter," O.K.

And again this is the paper that is talking about the rats that were exposed to the three types of asbestos?

A. Yes, sir.

Q. And that table indicates that the concentration of dust to which the rats were exposed -- strike that -- that table indicates the concentration of dust to which the rats were exposed?

A. As milligrams to per cubic meter.

Q. Exactly. And it reports both the total dust in the chamber and milligrams per cubic meter, and then it reports what is called the

respirable concentrations in the dust chamber in milligrams per cubic meter?

A. Yes.

Q. It is true, is it not, that the respirable concentration of Calidria Union Carbide dust to which these rats were exposed was only about a third as much as the two Canadian chrysotile dusts to which those rats were exposed?

A. I think it is 42 percent as opposed to 76 versus 82 percent.

Q. It is 42.3 Calidria, 75.7 UICCB and 87.1 Jeffrey.

And by respirable concentration, they mean the dust that gets breathed by the rats?

A. Yes, sir.

Q. Now, let me ask you a few more questions about that.

You have noted that the percentage, if you will, was 42 percent for the Calidria, 75 for the UICC/B Canadian and 87 for the Jeffrey Canadian, but it is also true, is it not, that the total concentration of dust from which the respirable fraction was derived was different, wasn't it?

A. I believe so.

Q. And I can show you this. I mean I am not trying to be tricky.

A. No, I know. But just show me the numbers real quick and I will -- yes, that's fine.

Q. So what we have got, the total concentration of what's called chamber dust or dust in the chamber was 11.36 milligrams per cubic meter for the Jeffrey chrysotile?

A. Casella or Cascade, there are two methods, the top is Cascade or the top is Casella.

Q. It looks like "Cascade," but you can check it.

A. Yes, it is the Casella.

Q. So according to table 1 in Pinkerton's paper 1983, he reports that the chamber dust concentration by mass for the Jeffrey Canadian chrysotile is 11.36 milligrams per cubic meter, correct?

A. Yes.

Q. For the UICC/B Canadian chrysotile, it is 10.99 milligrams per cubic meter?

A. Yes.

Q. For the Calidria Union Carbide, it is

7.76?

A. It is not 3.28?

Q. No, no, I am talking about the total chamber dust?

A. I'm sorry, right, right, right, sorry.

Q. Then when you turn to the same table, he next reports the respirable concentration of the dust which is what the rats breathe?

A. Right.

Q. And he reports that the Jeffrey Canadian chrysotile is 9.9 milligrams per cubic meter, correct?

A. Correct.

Q. And the UICC/B Canadian is 8.2 grams per cubic meter?

A. Right.

Q. And the Coalinga Union Carbide is 3.28 grams per cubic meter?

A. Right.

Q. Now, would you agree with me that what that tells us is that the Coalinga Union Carbide asbestos is about a third as much as the Jeffrey in terms of the respirable concentration?

A. Yes.

MR. WILL: By weight, by mass?

A. By mass.

Q. As reported here, it is about 9.9 for the Jeffrey and about 3.28 for the Coalinga?

A. Yes, sir.

Q. So it is fair to say, is it not, Dr. Ilgren, that these rats back in the NIEHS experiments got about -- breathed about a third as much Union Carbide Coalinga dust as they did the Canadian chrysotile?

A. That would be the assumption, yes.

MR. WILL: Again, by mass, not by fibers.

MR. BROWNSON: Well, as reported in this --

A. As reported by mass, yes.

Q. And would you agree that we do not have any actual data as to how many fibers each rat or the different rats breathed, but we do have the data as to the mass or the amount of asbestos they breathed?

A. Yes, that's correct. Per that paper. You are talking just about on the basis of those,

that paper, right.

Q. Now, can you tell us where in your papers, parts 1, 2 or 3 you report that in fact the doses or the exposure amounts that the rats got to the three types of asbestos was different?

A. I believe there is a presentation or representation of those mass data in part 3, I have to check that. It might be in another part. Again that's discussed at great length in part 4, which as you have pointed out, is not yet published. I thought we had repeated the mass data, but I guess we hadn't repeated it.

Q. Again I will go to the abstract of part 1 of your paper which is entitled "Coalinga Fibre - A Short Amphibole-Free Chrysotile," part 1, "Evidence for a Lack of Fibrogenic Activity," and what you say in your abstract and what you then say in the paper is that the exposed rats back in this Pinkerton et al. experiment showed fibrogenic responses to both asbestos types from Canada but none from the Coalinga chrysotile, correct?

A. Yes.

Q. But you never say anywhere in there

that they only got a third as large a dose of Coalinga chrysotile as they did of the Canadian?

A. It is not discussed in this paper.

Q. Would you agree with me the inference that the reader is left with is that these rats who were all exposed to the three types of asbestos in equal amounts and those exposed to Canadian asbestos got sick and those exposed to Calidria did not?

A. They were all occupational exposures but one would infer that.

(Luncheon recess: 12:30 p.m.)

AFTERNOON SESSION

1:45 p.m.

E D W A R D I L G R E N ,

resumed and testified further as follows:

EXAMINATION (Continued)

BY MR. BROWNSON:

Q. Dr. Ilgren, I want to once again plow through the number of animals, but hopefully we can finish it fairly quickly.

But before I do that, one thing occurred to me which I could not get -- find an answer from your papers and maybe you know.

Do you know if any of the sacrificed rats, you know, the ones we have talked about earlier that were sacrificed at either before it started, before the experiment started or at zero, 3 or 12 or 24 months, do you know if any of those rats had tumors? Do we have any data on that?

A. Yes. Yes, there is mention of, I believe, two tumors at 24 months and I put this -- this should be in my paper. It is in part 2. It is in part 2, I believe.

Q. I don't want to jump ahead then.

A. That's fine.

Q. We will get to that.

Now, going back to part 1 of your paper, you had spoken about one of the things that the federal government groups who were doing the rat inhalation study was doing was they had this big group of Fischer control rats that they were looking at.

Do you know what I am talking about there?

A. Controls?

Q. Not on this particular study we have been talking about.

A. The Solleveld.

Q. Yes, the Solleveld?

A. Yes, the Solleveld group.

Q. And I pulled the reference that I found in your paper with respect to that. I found a paper. This is one of the papers that you had referred to. It is called "Natural History of Body Weight Gain, Survival and Neoplasia in the F344 Rat," by Solleveld and some other authors?

A. And the question is?

Q. Well, first of all, I want to confirm

is that the paper that --

A. Which I have cited in here?

Q. Yes.

A. I believe so. I would have to go and check, but I am very -- I am pretty sure this is the paper which is cited. Yes, it is reference 42.

Q. Now, I am going to work off my notes here and let me see if I got it correct.

If you turn to page 942 of that paper at table 1, have I got that correct?

A. Yes.

Q. I count 5,747 rats in this big group, is that right?

A. Yes.

Q. Now, just can you explain for us, Dr. Ilgren, what this is. I understood this was kind of some big general study of the Fischer 344 rats for general purposes. It wasn't tied into this particular study we have been talking about this morning, is that right, or is this just --

A. No. My explanation as having put the same question to Chris Wagner, his explanation rather to me was that when the study was being

the companion concurrent collaborative study that was run in conjunction with the Medical Research Council in the United Kingdom, so as the study was originally set forth the -- there were two groups that were run in parallel, an untreated control group and a UICC/B chrysotile exposure group, and they were, to my knowledge, started almost at the same time in the United Kingdom and at the NIEHS. And that was the primary purpose of the study.

The NIEHS group also added the Coalinga short and the Jeffrey long to their protocol, but those two were not added to the United Kingdom protocol. O.K.?

Q. Just so we are clear, this collaborative study that was done back in the 70's and 80's as well?

A. Right, that's McConnell 1984 was cited.

Q. You mean Wagner?

A. No, it is actually McConnell et al.

Q. Now, continuing in table 2, we come to the Coalinga lifetime rats, and again these are slides that are being looked at by you, right?

A. Yes.

which is the Solleveld paper, it lists all these different types of neoplasias that these control rats had. None of these rats were exposed to asbestos, just so we are clear?

A. No, sir.

MR. WILL: Is that correct, they were not exposed.

A. That's correct. They were not exposed to asbestos.

Q. If you look down on this list of males, when we come to mesothelioma, it says two rats, and they were both mesothelioma, and they are just .4 percent, I guess?

A. I believe so.

Now, could I see that? Were they cited as NOS.

Q. I don't know.

A. Yes, I think they are NOS. Well, they would be plural.

Q. In any event, would you agree with me that out of this control group of some 5,748 rats were not exposed to asbestos they found two mesotheliomas?

A. Yes, we have cited those in our 1991

paper and in the book.

Q. And I did my own math, and that comes out to .03 percent. Does that sound about right?

A. If that is what you come out with, sure.

Q. So would you agree that in at least among this control group of the Fischer 344 rats which were used in the study we have been talking about today that the naturally occurring or background level mesothelioma is about .03 percent?

A. I would say yes. The only thing that comes to mind is I think there were a couple of other studies of untreated Fischer rats, and I can't remember what they found. I think it is a slightly higher incidence reported by others, but yes, I would agree with that.

Q. I want to go again to the 1983 Pinkerton paper we were talking about before lunch, and again this is the paper cited by you as reference 45 in part 1 of your paper?

A. Yes.

Q. Now, I am looking at table 2 of that paper. Are you familiar with that table? I will

show it to you.

A. Yes, I am familiar with the paper.

Q. And this table 2 is entitled "Optical Microscopy Fiber Characterization Percentage of All Fibers Greater than 5 Microns in Each Size Class," right?

A. Right.

Q. And what this table shows is of the fibers greater than 5 microns how they break down into different lengths, correct?

A. Yes.

Q. And these are the data as reported and published by Dr. Pinkerton in 1983 with respect to the Coalinga asbestos and the two Canadian types, UICC/B and Jeffrey, correct?

A. Right.

Q. Do you agree with me that as a general proposition, that of the fibers greater than 5 microns, the three asbestos types, there isn't a great deal of difference in their breakdown in the different categories?

A. Agreed.

Q. I am now turning back to the "Materials and Methods" section of part 1 of your

paper. I am at page 266, and down at the bottom of the right-hand column, you have got the heading "Histopathological Analysis."

A. Yes.

Q. And you write, "Animals on lifetime test were analyzed histopathologically as described by McConnell et al."

Now, my question is who analyzed these? When you say they were analyzed, who did that?

A. I did with Chris Wagner.

Q. This is that review you told us about earlier this morning down at the archive when you looked at the slides under the microscope?

A. Yes.

Q. Then in the second paragraph of that section, you say, "The interim sacrifices were scored," and then you talk about how they were scored. Who did that? Is this something that you did again or is this going back to the early work?

A. That's early work, chapter 5, table X, thesis Pinkerton, 1982.

Q. Now, I then turn to table 2 of part 1 of your paper, which is certain fibrosis scores on

the rats on lifetime tests, and you look at the three different asbestos types, right?

A. Yes.

Q. First of all, let me go back and make sure I am clear.

When you are talking here about lifetime tests, are you now talking about those rats that lived past the 24-month period?

A. Yes.

Q. That's what we are talking about?

A. Yes.

Q. And just again, that is how many rats?

A. 28. As I indicated before, there were -- in one instance, there appeared to be one or two more such as in table 2. If you look at the UICC/NIEHS 1996, you will see for males, there is 30, so I mean in that instance, I found two extra animals.

MR. WILL: And that's 28 per sex per group.

THE WITNESS: Yes.

Q. So let's back up again just so I can be clear on this.

Of the rats that survived the 24 months and were not killed as of 24 months --

A. Yes.

Q. -- and we may need to work the math here, there were -- what was it, 192, we had figured earlier that were left at that point?

A. I believe so. Yes.

Q. And those were again in the four groups, control group, Coalinga rats, Jeffrey rats and UICC/B rats?

A. Yes, I believe so, yes.

Q. And as a general matter, as I understand what you are telling us in table 2, there were equal numbers of each except these couple of extra ones here and there that we will look at here, is that right?

A. That's to the best of my recollection.

Q. So, for example, on the control rats, you report there were 28 males and 26 females. Is that what that says?

A. Yes.

Q. Now, let me just see if I have got this terminology straight.

Table 2 starts by saying "Control/NIEHS 1996"?

A. Yes.

Q. And what these are, are -- again what all of these things are in table 2 are the rats that survived more than 24 months?

A. Yes.

Q. When you say "Control/NIEHS 1996," what you are describing there are the slides of rats that were dead but had lived more than 24 months that you and Dr. Wagner looked at when you went down to the archives?

A. Right. In conjunction with the autopsy protocol and the gross pathology findings, but that's right.

Q. You went looking for the NIEHS, you found them in this archive at the NIEHS in North Carolina?

A. That's correct.

Q. So if I was to more, in a longer sentence, describe what those control rats are, those are control rats who lived -- I'm sorry, let me back up and start again.

If I was going to describe what those

control rats are, those are the control rats who were not killed in the original experiments, up to 24 months, but who later died and the slides were preserved in the archive and you found them and looked at them in 1996, is that fair to say?

A. Yes.

Q. Now, do we know in terms of what you call the lifetime rats or these lifetime scores or lifetime tests, do we know how long this lifetime was?

A. Yes.

Q. How long was that?

A. It varied.

MR. WILL: Which rat?

Q. Were they allowed to live out their life or were they all killed at some point?

A. No, they were allowed to live out their life and the survival data for the individual rats are given in individual data tables. I think if you look at the figure legend at the bottom of table 2 where you see 750 plus days, well, that pertains to MRC data, but there were survivors generally in excess of, as I recall, 600 days, 700 days for most rats, but

every rat that died has a survival duration for it. O.K.

Q. So these rats that were not sacrificed as part of the experiment were allowed to live out their life in a cage and then when they died, somebody noted how old they were and cut them up and to preserve these slides, is that fair to say?

A. Yes.

Q. Then this was all filed away within the archive. Then you came along in 1995 and '6, found it and looked at it, and you are now reporting in this table 2 what you saw, is that correct?

A. Yes.

Q. Now, if we then look at table 2, and I will try to get through this as quickly as I can, the first thing you report is the controls that you looked at, and there were 28 males and 26 females, right?

A. Right.

Q. Then you say "Control MRC, 1984, 34 total rats." What is that data?

A. Well, those data are the data from

the companion concurrent collaborative study that was run in conjunction with the Medical Research Council in the United Kingdom, so as the study was originally set forth the -- there were two groups that were run in parallel, an untreated control group and a UICC/B chrysotile exposure group, and they were, to my knowledge, started almost at the same time in the United Kingdom and at the NIEHS. And that was the primary purpose of the study.

The NIEHS group also added the Coalinga short and the Jeffrey long to their protocol, but those two were not added to the United Kingdom protocol. O.K.?

Q. Just so we are clear, this collaborative study that was done back in the 70's and 80's as well?

A. Right, that's McConnell 1984 was cited.

Q. You mean Wagner?

A. No, it is actually McConnell et al.

Q. Now, continuing in table 2, we come to the Coalinga lifetime rats, and again these are slides that are being looked at by you, right?

A. Yes.

Q. O.K. And if I read this correctly, there were 27 male rats exposed to Coalinga fibre and 24 females who were allowed to live out their lifetime, right?

A. That's correct.

Q. So that gives us a total of 51 --

A. Maybe I should -- there were data found in the archives for 27 animals. The number -- the proper number at the start would be a calculated 28, but in the actual data sheets that I found in the archives, there were records for 27.

Q. That was my question. 27 males and 24 females?

A. Exactly. And the ones that could be used for scoring that were not confounded by, say, leukemia, as I indicated in the tumor paper it would be Coalinga males 21.

Q. Now, do we know or do you know whether 28 rats -- 28 male rats and 28 female rats were exposed to Coalinga and allowed to live out their lifetime or you couldn't find records or they really only did 27 females or 24 females?

A. I don't know.

Q. So we don't know which of those two it was?

A. Correct.

Q. But you found records of 27 male rats and 24 female rats exposed to Coalinga fibre and allowed to live out their lifetime?

A. That's correct.

Q. And then in terms of giving a score to those rats for fibrosis in their lungs, you determined that 21 of them were -- 21 males and 17 females were able to be scored?

A. That's correct, with Dr. Wagner. He and I went through, as I recall, all of the Coalinga and the untreated controls together as I recall.

Q. So there were --

A. I believe -- excuse me one second. I don't think we specifically state which cases we -- no, we don't specifically state which ones we looked at, but he -- Chris and I went through a fair sampling of the control and the Coalinga slides together. We didn't look at any of the UICC/B together and we also looked at a number of the Jeffrey together as well.

Q. But when you say you looked at a fair sampling of them together, you personally looked at all of them?

A. I looked at all of them, and he and I looked at a sampling of them.

Q. So in terms of trying to figure out whether the Coalinga exposed rats that lived out their lifetime had fibrosis in their lungs or not, you determined that there were 21 male rats and 17 female rats that could be examined for that purpose?

A. Yes.

Q. And you also determined that there were six males and seven females or a total of 13 that you were unable to score because there was some problem with the slides?

A. Yes.

Q. So of those 13, we don't know whether they had fibrosis or not because you weren't able to make that determination because the slides were obscure or whatever?

A. We discussed the possibility of that and there is a discussion of that point in the paper itself.

I just want to see here.

As I have indicated on page 267, column 1, paragraph 2, with reference to the animals which we could not score but we believe that there wasn't any reason to believe that the larger group that we could sample was not representative of the whole.

Q. And why do you say that?

A. Just on the basis of the consistency of what we saw in the group -- in the cases which we could see. There was no suggestion whatsoever in the 38 animals exposed to Coalinga which could be histologically scored of fibrosis.

Q. Well, O.K. You did, however, find age-related lesions on those that could not be scored, correct?

A. Sure.

Q. And was the age-related lesions the reason they couldn't be scored? That seems to be what you are saying here.

A. Well, I am saying that these particular so-called age-related lesions, which includes the Fischer cell leukemia and includes a mode of death very common to these rats, which is

a renal failure due to amyloidosis and secondary uraemia and pneumonitis causing edema, there was no reason to believe that these were necessarily obscuring an underlying fibrosis. It is not that there was an age-related fibrosis per se.

Q. You call it age-related lesions?

A. Yes, exactly.

Q. But then you say that the age-related lesions were probably treatment-related. What do you mean by that?

A. Where are you reading?

Q. I am reading in that paragraph you just cited me to the second paragraph on the first column on page 267.

A. I think what we are saying here pertains to competing causes of death. The Coalinga and the untreated animals died of so-called age-related lesions. The age-related lesions was the kidney disease and this form of cancer, this leukemia.

The Canadian-treated animals died prematurely because of the pathogenic and fibrogenic and cancer-inducing effects of the Canadian asbestos, and there was -- as I recall,

there was less age-related lesions in the Canadian-treated animals because they were dying from asbestos exposure or the attributable effects of the asbestos exposure.

Q. And that is determined by you looking at these slides in 1996?

A. And all the other data that I had, the autopsy reports.

Q. So it is determined by you looking at all this material you found in the archive in 1996?

A. Yes, sir.

Q. So if I can then try to summarize the results as described by you under the heading "Results," you're saying that after you looked at the archival materials, 38 Coalinga-dosed rats that lived out their lifetime and some control rats and some Canadian chrysotile rats, you concluded that those data showed that the Coalinga chrysotile is not fibrogenic but the Canadian is?

A. It is absolutely clearcut.

Q. That is in contrast to other data including data to papers you cited in your references which would show that the Coalinga

asbestos is fibrogenic in certain instances, correct?

MR. GERSON: I object to the form, of course.

A. I don't see the data. I saw the statement.

Q. Well, we looked at those three abstracts a moment ago that you cited.

A. You showed me a line in O'Neil, Crapo 1981 that said histologically there is interstitial fibrosis, but I never saw the data.

Q. Did you look for it?

A. I looked for every single piece of data associated with this study.

Q. But, Doctor, this is data presented in a paper, and you rely on papers and all scientists rely on papers, and if these eminent scientists like Dr. Pinkerton said that Coalinga-exposed rats got interstitial fibrosis, you don't doubt them, do you?

MR. WILL: I object to the form of the question on several grounds. Let him finish the question.

Q. Just because it only took Pinkerton

one line to say it, you don't doubt what he found, do you?

A. Is that the question?

Q. Well, you seem to be concerned that it -- that I quoted one line, so let me ask the question.

You will agree with me that in the same group of rats, that is, the Coalinga-exposed rats, but those that did not live out their lifetime, those that were killed at various time periods along the way, Dr. Pinkerton and these other doctors who did the study did find interstitial fibrosis caused by the Coalinga asbestos?

A. I have no idea what they based that on.

Q. But they found it, didn't they?

A. Well, Kent Pinkerton and I communicated. He sent me all of his data. We have had numerous discussions of what he sent me. I don't find anything in terms of the data that he ever sent me or in fact anything he ever said to support that statement.

Q. Doctor, you don't find it, but you

will agree with me that Dr. Pinkerton reported in the American Review of Respiratory Disease in three different abstracts he published there that he found interstitial fibrosis in Coalinga-exposed rats from this experiment?

MR. WILL: I don't think it says interstitial fibrosis.

A. It says in O'Neil, Crapo in 1981 that histologically they found interstitial fibrosis. As I recall they had looked at 24 months. I think they go to 24 months, is that correct?

Q. Yes, they do.

A. And I am saying that. I have never seen any light microscopical histological data which either Dr. Pinkerton or Dr. O'Neil or Dr. Crapo have ever compiled to support that statement, and I have been given all the data, I have reviewed all the data. I have discussed with them their data, and in my opinion they have -- there is no basis to say that or to support that particular line in O'Neil et al. 1981.

Q. Which really is 1980?

A. I'm sorry, 1980.

Q. So are you saying that Dr. Pinkerton

was simply wrong when he reported in the American Review of Respiratory Disease that there was interstitial fibrosis caused by the Coalinga fibrosis in rats?

MR. WILL: Can you be more specific with the article. I think what you said this morning was lung changes, if I remember the quote correctly.

Q. In one article Dr. Pinkerton and these other doctors including Drs. Crapo, Brody and Pratt said that all fibers, and this includes the Coalinga, caused injury to the epithelium and interstitium, correct?

A. You are reading from Pinkerton et al. 1981, that particular --

Q. Yes, I am.

A. And in that -- before you go on, in that particular paper, do they report 24-month data?

Q. I don't know if they do or not, 12-month data?

A. Well, in that particular paper, they don't report the complete data.

Q. Let me ask you this. If a rat gets

asbestosis in three months, does -- that asbestosis doesn't go away and disappear?

A. Do they say asbestosis or do they say injury?

Q. O.K.

A. They are talking about acute reactions which are clearly reversible for Coalinga, all the data clearly indicate that the acute reaction, the accumulation of very small accumulation of cells and matrix in the exposed, high exposed Coalinga animals are reversible and disappear. They disappear at the end of life. They disappear at the end of 24 months.

Q. You say that?

A. Their data say that. Their data is presented in this paper. These are their data.

Q. The data that you have dug out of an archive and you have presented in a paper you say says that?

A. The data which I was sent by Dr. Pinkerton which are replicated precisely in this thesis, precisely in his papers, Pinkerton, et al. 1984, 1996, 1990, these are precise replications of his very data as sent to me by him. These data

say that there is resolution, there is no persistent significant reaction to Coalinga fibre by 24 months.

Q. Yet Dr. Pinkerton refused to put his name on that paper, didn't he, as you have told us earlier today, correct?

A. He said he didn't want to be a coauthor.

Q. Now, let's move to table 4 in part 1 of your page?

A. O.K.

Q. Is this a table that you generated or did this come from somewhere else?

A. Well, these are data that I was sent by Dr. Pinkerton, is that your question?

Q. Right. That's what I am wondering.

A. Right, these are data.

Q. Do you draw any conclusions from these data? In other words, as I read this, these numbers are kind of all over the map, and I don't see any pattern or conclusion here. I am curious what you draw from it.

A. Well, there is no persistent induction or accumulation of noncellular

interstitial matrix induced by Coalinga at the end of the 24-month period when you compare Coalinga to controls.

Q. O.K. But I don't see any 24-month data in this table?

A. Well, it is 12 plus 12. The table reads 3-month exposure.

Q. O.K.

A. 12 months exposure, and then at 12 months, exposure ceases and so it is --

MR. WILL: O.K. I got you

A. Yeah, O.K.

Q. This is reporting noncellular what?

A. Interstitial matrix volume.

Q. Now, in the controls, in the males at 24 months, it is 178, but in the Coalinga it was 287. Are you saying those are the same?

A. On the basis of the statistical comparisons using the small number of animals, there is no statistical difference between the 178 and the 287.

Q. Well, doesn't that just illustrate that we don't have enough animals here to get the power of statistics to work?

A. I think what it indicates is what you have to do is you have to interpret the study as a whole and not rely solely on the morphometry. I think the morphometry are very telling. If you look at a bigger picture and compare in that same table 4 the two Canadian fibers, there is a massive increase by 24 months in the interstitial matrix volume or what Pinkerton terms the morphometric equivalent of fibrosis. Each were well over 400 as compared with the controls, as compared with the Coalinga fibre. I think the study, the morphometrical analysis is indeed limited by the fact that there were only four of each sex taken out --

Q. O.K.

A. -- for each group, that's absolutely certain. And there is also limitation posed by the fact that the electron microscopical analysis relied on 1 millimeter cubes of tissue, whereas in the light microscopical analysis, you look at basically a section through the whole lump.

So I think each method adds information, and to some extent one method gives you information you can't get from another, but I

think you have got to look at the whole thing together. When you look at the whole thing together, it all comes together to say there is no fibrogenic potential to Coalinga. That is absolutely clear from these data.

Q. It is clear to you, but it is not at all clear to me, so let's return to table 4.

A. All right then.

Q. You are saying at 24 months all you had was 8 Coalinga-exposed rats, 8 controlled rats, 8 UICC/B rats, and 8 Jeffrey rats. And you had this one small cube of tissue, each looked at microscopically?

A. Right.

Q. You are saying based on such a small number of rats, you cannot say that the Coalinga is greater than the control, right?

A. Well, I am simply saying that the statistical analysis will not differentiate between the control and the exposed.

Q. And that's because there aren't enough rats to do that?

A. Not necessarily.

Q. Well ---

A. It is --

Q. A difference in a volume between 178 and 287 you have to agree is significant. That's over an 80 percent increase in volume, you are saying that's insignificant?

A. It is also a question of the standard deviation, and the variation in confidence here, you know it is more than just the absolute numbers, and this is why one uses a multiple comparison analytical method which you know, but I mean this is why this is done.

Q. O.K. But to get back to the question, there is, you will agree with me, that among the 8 Coalinga-exposed rats versus the 8 controlled rats which are the Pinkerton data described by you at table 4, there is nearly 80 percent increase in the volume, in the Calidria-exposed rats, as opposed to the control. We can argue all day, I guess, whether that is significant or not, but that's a fact?

A. It is a fact in the analysis of the numbers, but if you go back to table 2 and you actually see that, you know, if you compare average fibrosis scores, I mean there is an

apparent difference of the Coalinga has a so-called slightly higher fibrosis score, and I think the difference that you are alluding to here is manifest perhaps in this what you are terming an 80 percent difference, but I mean if you look at the females, there is absolutely no difference whatsoever.

Q. But I wasn't looking at --

A. You are looking at the males.

What I am trying to say is, is there is -- there is some quote/unquote effect taking place, but it is not reflected in the fibrosis here.

Q. Yes, or by you?

A. As scored by me, as scored by Chris Wagner.

Q. Well, as scored by you in table 2 is what I am saying.

A. Yes. In fact, it is also scored in table 3, which is the other table used to look at parenchymal fibrosis.

Q. Now, I am turning my attention to table 7 which is at page 271.

MR. GERSON: Table what?

MR. BROWNSON: 7.

Q. This is a table entitled "Changes in number of the individual cellular components of the alveolar interstitium in rats sacrificed at 3, 12 and 24 months treated with Coalinga, Jeffrey and UICC/B chrysotile," right?

A. Well, it is right, but the UICC/B should obviously not be there.

Q. Why not?

A. Because the sections were according to Kent Pinkerton. I made a mistake in including it. That's why. But basically the full explanation is that Kent never analyzed for these particular changes in the individual cellular components, the UICC/B treated animals, so that's an error on my part.

Q. Now, let's turn on the same page 271 to your section which is headed "Survival Rates of Animals on Lifetime Test."

A. Yes.

Q. And again I am confused. You describe 53 controls, but going back to our early discussion today, there should be 80.

Are we saying you could only find

records of 53 of them?

MR. GERSON: Excuse me, where are you looking at?

MR. BROWNSON: Right at the top of the second column, page 271.

A. Well, I have in table 2 actually 54, so that should probably read on page 271, 4 out of 54 concurrent controls. I think that's what it should read, so that's correct, the record should read 54.

Q. So that's an error in the page 271, that should be 54 and not 53?

A. Yes.

Q. But again that indicates that when you reviewed these records in the archive, you could only find a record of 54 of the controls, and we don't know if that means that only 54 were done or if all 80 were done and you just couldn't find the records --

A. No. All 60 -- you meant all 60, it would have been -- oh, wait a minute. 56 actually. Remember it would be 28 at the end of the 24-month period, you are going all the way back.

Q. You're right O.K. 56. So we don't know what happened to the other two controls, if they were just not done or if the records didn't survive, correct?

A. That's correct, yes.

Q. Now, the next section on that same page is entitled "Parenchymal Fibrosis and Survival"?

A. Yes.

Q. And basically what we are saying here is that, or what you are concluding is that the Canadian asbestos reduced the life span of the surviving rats but the Coalinga asbestos did not, correct?

A. Yes.

Q. And again this is based upon your review of that same number of rats we talked about earlier?

A. Yes.

Q. But you then say, "Cases with pulmonary tumors, severe leukaemic infiltration, or marked uraemic involvement of the lung were excluded from the analysis," right?

A. Yes.

Q. Why were they excluded?

A. Because in those particular instances you couldn't with absolute certainty, as I have indicated before, this is -- these were confounding features which have made it difficult to analyze fibrosis in particular -- well, there were virtually -- very few cases in which the pulmonary tumor obscured the level of fibrosis. I think there was one, but as I have indicated, I believe in paper number 2, there were cases, and this is at the end of tables, I think number 2 -- there were cases that just could not be read.

If you look -- if you look at the bottom of table, for example, 2-A and 2-B, it says "cases from lifetime test that could not be read due to autolysis or cases on lifetime tests that could not be read due to leukaemia." So it is part of this issue of age-related lesion again.

Q. Now, going to the bottom of the page, you say, "A small percentage of slides were missing, 2 percent (5/208)."

What are we talking about there?

A. Well, there were just five -- there were autopsy reports for all the animals, but when

I went -- and on each autopsy report, there was an animal number and a histology number, but when I went to go to the actual cardboard boxes where the histology slides were kept, I couldn't find the slides for that particular animal, so in five cases that was so.

Q. Now, let's go to table 8, which is on the next page, page 272.

A. Yes.

Q. And this is a table which shows a number of things, but it lists the different types of asbestos.

A. Yes.

Q. And it shows various scores of fibrosis, and then it also shows tumor response, correct?

A. Yes.

Q. Now, if you look at the Coalinga number 6, which were the slides that you looked at, right --

A. Yes.

Q. -- these were the surviving rats that died, and you looked at their slides later?

A. Yes.

Q. And if you go to tumor response, you say 2 of 90. Where does that number come from?

A. That would come from paper 2.

Q. I don't want to jump ahead of ourself, but in paper 2, it says tumor response for the same surviving Coalinga rats is 2 of 51?

A. I think what I did there was I just took 90 as the original number of animals in the entire so-called Coalinga group before any was taken out, which may not be the absolutely correct way to present that. It should probably be 2 of 80.

Q. So 2 of 90 is incorrect, it should be 2 of 80?

A. I believe so. Unless one wants to consider the absolute number of animals at risk as 56, which might also be -- which might also be appropriate. Does that make sense to you, because 28 animals run lifetime test?

Q. We are jumping ahead to page 2, but just to do it at this point, you report finding two tumors in the Coalinga-exposed lifetime animals, right?

A. Yes.

Q. And in terms of figures out -- two out of how many, it could be either 90, 80 or 56 or is 90 just an error, it could be either 80 or 56?

A. It could be 80, 56.

MR. WILL: Or 90 if you count before you took the first ones out.

THE WITNESS: Yes.

A. There is one other thing about this table. I am going to jump ahead, but the 3.3 average concentration for Coalinga COF25, that should probably have been 8.8.

Q. Because are we talking about the overall concentration?

A. Yes, I believe that's the overall concentration and that would indicate that it is probably at least several hundred fibers per cc in that order.

Q. So is this another error in your table when you say 3.3?

A. Well, if one is talking about average concentrations all being total dust masses.

Q. I don't know what you are talking about. Your table reads "Average Concentration,"

and it reports 3.3, and you are now saying that is wrong --

A. Well, all I am saying is that the other data, to my recollection, were expressed as total mass doses, and the 3.3 is the respirable concentration. Does that make sense to you?

Q. Well, I don't know if it does or doesn't --

A. Well, O.K. it is not.

Q. You are saying the 3.3 you report in is wrong?

A. I don't say it is necessarily wrong, but to make it perhaps comparable to the others, it should probably be 8.8, which is a very high dose consistent with some of the higher levels reported in the other treatment groups, that's all.

Q. Well, when you say it is a very high dose, again if we go back, the average concentration as actually reported in the Pinkerton paper was 7.9, it wasn't 8.8.

A. Sorry, 7.9.

Q. The other two Canadians were a little over 10 and a little under 11, right?

A. That's correct.

Q. So up at the top, for example, where you say "Short 2" and "Long 2," where do you get that 10? Should that be the 11? If we are going to change these numbers?

A. No. "Short 2" "Long 2" is from Davis and Jones reference 24. That's their 1988 paper where they exposed the rats to short and long fiber preparations.

What I am trying to say here is that the COF25 administered at 7.9, as you pointed out, the correct figure would generate a fiber equivalent of well over 100 fibers per cc, which is listed. You see that in the third column of table 8? Do you?

Q. Of greater than 5 microns, correct?

A. Yes, which is a very high dose.

Q. But, Dr. Ilgren, didn't you state earlier in this same paper and in fact you said it right in the abstract that the Coalinga asbestos was, to use your words, virtually all less than 5 microns in length and now here you are saying we have in this table -- in this table now you are saying we have 100 fibers per cc over 5 microns in

length?

A. We are talking about air and water. That's what we are getting involved?

Q. Well, no. We are not talking about air and water. We are talking about this Pinkerton data which you told us before was air data?

A. Right.

Q. That was the 7.9 concentration by mass?

A. Right.

MR. GERSON: What is your question?

Q. And you are saying that works out to over 100 fibers per cc greater than 5 microns?

A. As a fiber concentration which he never calculated.

Q. Well, apparently you or somebody else has, because you have it listed here in the third column.

A. Correct. Chatfield and I, more specifically Chatfield, has calculated that it is over 100, which is thoroughly consistent with the calculation that you've made which was a fiber equivalent of 131 greater than 5.

Q. It is 100 fibers greater than 5 microns of Coalinga asbestos, right?

A. Right.

Q. Now, I am looking at the asbestos-exposed rats, which is the third from the bottom on your table which you get out of a paper reference number 52, right?

A. Right.

Q. And on that particular chrysotile, you report that there is 111 fibers, I guess, per cubic centimeter greater than 5 microns, is that right?

A. That's correct.

Q. And that produced 18 and 40 tumors?

A. That's correct.

Q. And that paper that Davis published that -- that's not your data?

A. That's Davis et al., 1986.

Q. Now, if 111 fibers per cc greater than 5 microns can produce and that's short chrysotile again?

A. Say that again.

Q. That is short chrysotil in this Davis paper?

A. No, that's very long. You can see 90 percent over 10 in the second column. That's the wet disperse chrysotil preparation.

Q. So 90 percent of that is over 10 microns in length?

A. Yes.

Q. Whereas in the Coalinga, 23 percent is over 10, right?

A. Well, you can see that superscript 5.

Q. I do see that.

A. Within the air that was the measurement that was found.

Q. And you claim that they were artificially made longer by a spinning process. Where do you get that from?

A. I don't think that's -- should be a spinning process. I think the correct word is a clustering. That was an editorial edition that John who -- we had a discussion about spinning. It is really a clustering, that particular word.

Q. So it says spinning, and if the reader like me reads it, I read spinning, but it shouldn't be that, it should be clustering?

A. In fact it should be "produced by

clustering."

MR. WILL: Who was the person who put in that word?

THE WITNESS: It is not a mistake. The chief editor of the journal and I discussed this, and he wanted to put in spinning, and I said it should be clustering, and he said it should be spinning.

Q. Is there any evidence anywhere there that this is spinning fiber?

A. It is not spinning in the sense of spinning a yard of textile, it is clustering, and I differed with John about that.

MR. GERSON: Is that a difference in semantics? By spinning, did he mean the same thing that you meant by clustering?

THE WITNESS: Yes.

Q. Of course as we know in the asbestos toxicology field, textile-grade long fiber carries certain connotations, doesn't it?

A. Yes.

Q. And the word "spinning" is something synonymous with a textile process?

A. Yes.

Q. So the reader could fairly conclude that this is some sort of textile-grade long fiber, when in fact it is nothing but Coalinga?

MR. WILL: I object to the hypothetical.

Q. Isn't that a fair --

A. I don't like the word "spinning." I think it should be clustering.

Q. Let's now move down. Now we are in the discussion on page 272, below table 8.

A. Yes.

Q. And you talk about in discussion other studies with Coalinga chrysotile, and then you say, "The investigation by Muhle, et al. is the only other inhalation study of Coalinga chrysotile and it too failed to find fibrosis."

That's what you say, right?

A. Yes.

Q. Then you cite your reference 51 to the Muhle study, but in checking it, I note that it is really 52?

A. Yes.

Q. So that's a little error there,

A. Yes.

Q. So the reader could fairly conclude that this is some sort of textile-grade long fiber, when in fact it is nothing but Coalinga?

MR. WILL: I object to the hypothetical.

Q. Isn't that a fair --

A. I don't like the word "spinning." I think it should be clustering.

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That's what you say, right?

A. Yes.

Q. Then you cite your reference 51 to the Muhle study, but in checking it, I note that it is really 52?

A. Yes.

Q. So that's a little error there,

correct?

A. Yes.

Q. What you are saying is your analysis of the surviving Coalinga-exposed rats, at least those you were able to grade for fibrosis, you didn't find any, and now you are saying you find support for that in Dr. Muhle's study, correct?

A. Yes.

Q. I am looking at the Muhle study, which is your reference 52 -- you say 51, but it is 52 -- and I don't see where he reports anything about fibrosis. Do you know where that is in his paper?

A. Do you want to give me the paper?

Table 7, "Histopathological changes in the lungs of rats exposed to various fibers (inhalation study column heading No. 3 septal thickening) interstitial fibrosis interstitial inflammation."

Q. So is that the fibrosis that you are talking about?

A. Well, that's what I am making reference to.

Q. And that's in table 7?

correct?

A. Yes.

Q. What you are saying is your analysis of the surviving Coalinga-exposed rats, at least those you were able to grade for fibrosis, you didn't find any, and now you are saying you find support for that in Dr. Muhle's study, correct?

A. Yes.

Q. I am looking at the Muhle study, which is your reference 52 -- you say 51, but it is 52 -- and I don't see where he reports anything about fibrosis. Do you know where that is in his paper?

A. Do you want to give me the paper?

Table 7, "Histopathological changes in the lungs of rats exposed to various fibers (inhalation study column heading No. 3 septal thickening) interstitial fibrosis interstitial inflammation."

Q. So is that the fibrosis that you are talking about?

A. Well, that's what I am making reference to.

Q. And that's in table 7?

A. Yes.

Q. Of Muhle's paper?

A. Yes.

Q. And where is the Coalinga or Calidria chrysotile asbestos reported on that table? It just says chrysotile. Is that the Coalinga?

A. Yes.

Q. Now, what that table reports is that the septal thickening, which is described as interstitial fibrosis interstitial inflammation, is 42 percent in the Coalinga-exposed rats and 24 percent in the controls, right?

A. Yes.

Q. So it is almost doubled?

A. There is two controls.

Q. Well, one control is by the nose?

A. There is actually a sham, so-called sham control at 11 percent and an untreated control 24 percent, and I think the more appropriate comparison is the 11 percent plus the 24 percent, and then there is also another control missing from that which would be a nonfibrous dust control. There is two controls at the bottom.

Q. Two different controls. One is what

is called the sham control where they actually put the nose tube on the rat but didn't make him breathe any dust?

A. That's right.

Q. And one is the control without treatment, where they just let the rats run around in cages?

A. That's right.

Q. So there is two different kinds of controls?

A. Yes.

Q. And they are called in this table -- well, for purposes of our questions, can we call them sham control and untreated control?

A. Right, that's fine.

Q. So Dr. Muhle in his paper, reports that the Coalinga -- Dr. Muhle in his published paper reports that the Coalinga chrysotile exposed rats at 42 percent fibrosis?

A. Septal thickening.

Q. Well, septal thickening, but you called that fibrosis in your paper, right?

A. No, I don't believe so.

Q. Well, you said it too failed to find

fibrosis?

A. I said in this investigation, fibrosis was assessed as septal thickening, but Muhle didn't mark it or didn't distinguish the thickening, whether it was due to proper scar tissue or whether it was due to cells, and as I think I also indicated in this or another paper, that we tried to get the slides to discriminate between those, and they were no longer available. But go on.

Q. Whatever it was and whatever he called it, he called it septal thickening?

A. Right.

Q. You saw that as a result -- consistent with results you found in the lifetime rats?

A. But if you look at page 272, paragraph 2, I have said that the level 42 percent is not different than a combined control of 36 percent.

Q. Yes, you did say that, and I am just trying to establish, Dr. Ingren, what you wrote.

You wrote that Dr. Muhle's results in his rat inhalation study is consistent with these

conclusions you derive from the lifetime rats, correct?

A. Yes.

Q. O.K. And you then say, you write that Dr. Muhle failed to find fibrosis, right? That's what you say?

A. Yes.

Q. And in Dr. Muhle's study, the fibrosis which he reported was this septal thickening, right?

A. Yes.

MR. WILL: Well, object to the form of the question. He is using the term "septal thickening" as a proxy for fibrosis, but that doesn't mean he's --

MR. BROWNSON: Well, I guess I am just reading what Dr. Ingren writes.

MR. WILL: He didn't quote it accurately.

Q. Fibrosis -- Muhle identified fibrosis as septal thickening, right?

A. Quote/unquote.

Q. Right?

And as we just said a minute ago, in

Muhle's rats, out of 50 rats exposed to Coalinga chrysotile, 21 or 42 percent had septal thickening or this type of fibrosis, right?

A. Well, I wouldn't call it a type of fibrosis. They had septal thickening, and it doesn't differ from the combined controls, absent another needed control.

Q. Yes. Before we get to that, though, I am trying to take this a piece at a time. I am just trying to use your language.

You are the one, Dr. Ilgren, that says that Muhle's study did not find fibrosis, that's your exact quote, right?

A. Yes.

Q. And what Muhle was reporting was septal thickening, which as you say is -- that's the thing that you call fibrosis here on page 272, right?

MR. WILL: I object to the form. No, that's not what he says. Read the next sentence, Bob. It explains it.

MR. BROWNSON: Let me please start over.

Q. You say that Muhle's study finds no

fibrosis caused by Coalinga asbestos, correct?

A. It too failed to find fibrosis.

Q. Pretty clear?

A. But it is clear.

Q. And the thing that Muhle found was septal thickening, right?

A. Right.

Q. That's the so-called fibrosis that you are talking about here, right?

A. That's what I am saying. "Fibrosis was assessed as 'septal thickening.'"

Q. By Muhle?

A. By Muhle.

Q. Now, of Muhle's 50 Coalinga chrysotile-exposed rats, 21 of those, or 42 percent had the septal thickening, correct?

A. Correct.

Q. And you say, well, that's the same as the control rats because it is pretty close to the amount found in the control rats, right?

A. Right.

Q. And, ergo, the Coalinga doesn't cause increased septal thickening, is that a fair conclusion?

A. Right.

Q. O.K. Now, if we look at Muhle's control rats, he's got two different types of controls. He's got what we called earlier the sham controls and he's got the without treatment controls, correct?

A. Correct.

Q. And the sham controls he had 55 of those rats, correct? I am just looking at this --

A. O.K., sure.

MR. WILL: Why don't you give him the paper.

MR. BROWNSON: Take a look at it. We only got one there.

A. Right, he's got 55.

Q. And how many septal thickenings did those 55 sham control rats get, 6, right?

A. 6.

Q. 11 percent. Now, you say 12 percent in your paper, but you really mean 11 percent, right?

A. I mean 11 percent.

Q. And it is not 6 of 50, as you say in your paper, it is 6 of 55?

A. 6 of 55.

Q. Now, then he had another 50 rats, 50 control rats that were the untreated control rats, correct?

A. Correct.

Q. And of those 50 untreated control rats, 12 had septal thickening, which is 24 percent, O.K.?

A. O.K.

Q. And what you then say in your paper, Dr. Ilgren, is if you put the two groups of control rats together, the 50 which -- what you call 50 but what is really 55, and the other 50 which is 105 --

A. Yes.

Q. -- and you add those two percentages, what you call 12 but what is really 11 and 24, you get 36, but it should really be 35, right?

A. Right.

Q. And you are saying that 35 is essentially the same -- 35 percent is essentially the same as 42 percent?

A. Right.

Q. Now, let me ask you this question,

Dr. Ilgren.

We have got two different groups of control rats. One of them has 11 percent septal thickening and one has 24 percent septal thickening. You add those and say the control rats have 36 percent septal thickening, correct?

A. Right.

Q. But there is no group of control rats here that has 36 percent septal thickening, is there?

A. You just add them together.

Q. Well, Dr. Ilgren, if I have two bank accounts and one earns me 24 percent and one earns me 12 percent, I didn't make 36 percent. I made 18 percent, didn't I?

A. I imagine so.

MR. WILL: They are equivalent.

A. But the manipulation -- the reason why it is added we -- to my mind, the manipulation in itself would seem to have been able to induce some kind of change. That's why I added them together, and I think what we are seeing here is that there is some kind of age-related increase in septal thickening which is consistent, and I think

what we are not seeing at all in the Muhle data is the nonspecific nonfibrous dust control which is totally absent. I mean he has included that. He has included that in other studies, things which attempt to account for the specificity of the observation, and that's not here at all.

Q. But that's not what you are talking about.

Let's look at Muhle's data because that's what you are talking about, O.K. Muhle's controls are 105 total rats?

A. Right.

Q. 55 shams and 55 untreated, right?

A. Right.

Q. Those 105 rats had 18 septal thickenings, right?

A. Right.

MR. GERSON: Can we go off the record for a second?

MR. WILL: Why don't we take a break. We have been going for an hour already.

MR. GOLDMAN: Let's finish up on this calculation here.

Q. Muhle has 105 control rats, 18 of them got septal thickening, right?

A. Right.

Q. 18 percent of his control rats, even a little less, got septal thickening, isn't that true?

A. Based on that calculation, that's correct.

Q. That's just straight math?

A. That's straight math.

Q. And 42 percent of his rats exposed to Coalinga chrysotile asbestos got septal thickening, correct?

A. That's correct.

Q. So when you said 36 percent of the control rats had septal thickening, that's a mistake, isn't it?

A. It may be a mistake.

Q. So what we are really comparing is 18 percent control rats with septal thickening versus 42 percent Calidria chrysotile rats with septal thickening?

A. That on the basis of those data adding them together that would appear to be

correct.

Q. And that's over a 100 percent increase, it is over twice as much, isn't it?

A. For whatever that means in terms of the composition of the thickening, it is over 100 percent.

Q. Well, but you --

MR. GERSON: Bob, if you are going to question further on this report, we need to make a copy of it.

MR. BROWNSON: We can do that. I will stop. We can do that.

(Recess taken)

BY MR. BROWNSON:

Q. Now, I am backtracking to page 271 of part 1 of your paper.

MR. WILL: You can't do that. You can only go forward.

Q. I am moving forward to 271 and down to "Age-Related Lesions" in the right-hand column toward the bottom. We talked about this earlier. There were the 22 percent of slides that could not be read. Do you see that?

And you state there that it was 11 of

50, but I note over on table 2 it indicates it is 11 of 51.

Do you know which of those is the correct number?

A. I believe it is 51.

Q. So the notation 11 of 50 on page 271 is an error that should be 11 of 51?

A. I believe so.

Q. Now, I would like to move on to part 2 of the paper, which is entitled "Coalinga Fibre - a Short Amphibole-Free Chrysotile," part 2 "Evidence for lack of tumourigenic activity," right?

A. Yes.

Q. And again this is your review of the slides and other surviving materials from the archive of the old Pinkerton rat experiments, right?

A. Yes.

Q. The same data really we had in part 1 -- part 1, 2 and 3, we are working off the same data, so I don't have to keep repeating this all the time.

A. That's right.

Q. And again if I can -- I will do this at my peril, but if I can summarize your conclusion here in a nutshell, what you are saying is that the two types of Canadian chrysotile, the UICC/B and the Jeffrey caused tumourigenic responses in the rats, in these lifetime rats, but the Coalinga chrysotile did not. Is that fair to say?

A. Above controls, that's correct.

Q. So again what you did is you looked at the lifetime control rats, the lifetime Coalinga-exposed rats and the two groups of lifetime Canadian-exposed rats and just kind of compared them, if I can put it in layman's terms?

A. Correct.

Q. And your conclusion was again in layman's terms, is that the control rats at the end of their life and the Coalinga control rats at the end of their life had about the same number of tumors, and the two Canadian chrysotile-exposed groups of rats at the end of their life had more tumors, is that right?

A. That's correct.

Q. Now, the basis of these conclusions,

as I understand your paper, was again from the archive material, you looked at the slides and the autopsy reports and such documentation and determined which of those rats died of tumors or which had tumors and which didn't, is that right?

A. Yes, that's basically correct.

Q. Now, again, was this an analysis or an examination by you or by Dr. Wagner or by you and Dr. Wagner or by others?

A. It is the same discussion as before. I examined all of them and then I also examined the same subset as the ones we looked at for fibrosis with Dr. Wagner.

There had also been for the untreated controls, concurrent controls, life span controls. The concurrent controls and the life span controls and also for the UICC/B, those materials had been reviewed before, and in table 1 it is indicated as such as control 1984 NIEHS or UICC 1984 NIEHS. They are the same animals I looked at.

Q. Can we -- let's turn our attention to table 1 of part 2 of your paper. In this table it is a table that you put together, as I understand it?

A. Yes.

Q. This summarizes the tumors, if you will, on these different groups of rats?

A. Yes.

Q. Now, let's start then with the controls because, as you just noted, you actually list five different kinds of controls, right?

A. Yes.

Q. And if the 1, 2, 3, 4 one -- 1984 NIEHS, that looks like a big group of controls of the 5,400 and some -- 5,740?

A. Yes.

Q. Or whatever it was. Over 5,000?

A. Yes.

Q. The control 1998, is that those -- that group of lifetime rats that died but were the control group that you then looked at the slides? That's what they are?

A. Yes, they are the same animals as the third one down Control 1984 NIEHS, that's the -- they are the identical animals. Except they found three tumors and I found two.

Q. Now, I guess this is something new to me. Let me ask you this.

I wasn't aware that back in 1984 the NIEHS actually analyzed those lifetime rats, control rats, did they do that?

A. That's published in McConnell et al. 1984 in the Euro report symposium. It is referenced in this particular paper.

Q. Maybe I missed it, but it looks like what you are saying is that the lifetime rats, both the controls and the UICC/B-exposed rats, were examined back in '84?

A. Yes, they were.

Q. And then you looked at them again?

A. Yes, that's right.

Q. And in addition to that, you looked at the Jeffrey Canadian chrysotile-exposed rats, you didn't look at the rats, you looked at these slides --

A. Right.

Q. -- in 1998, and you looked at the Coalinga chrysotile-exposed rat slides in 1998?

A. That's correct.

Q. And then you summarized in terms of which rats among all these various groups had tumors, you summarized that all in table 1?

A. That's correct.

Q. It is this data from which you derive your conclusion that the tumors amongst the control rats and the Calidria-exposed rats were about the same and then the tumors among the Canadian chrysotile-exposed rats were higher?

A. That's correct.

Q. Now, let's look at the table -- first of all, if we go through the columns on the left-hand side, is the type of exposure, whether they are control or which type of asbestos they were exposed to, right?

A. Yes.

Q. Then next we have the sex, male or female?

A. Yes.

Q. Then the next column is marked "N1," right?

A. Yes.

Q. Now, I was looking at the column marked "N1" back in table 2 of part 1 of your paper.

A. They should probably read 28 instead of 30.

Q. Right. That's what I am getting to. There is different numbers in the two, and that's what I am wondering about.

A. Yes, yes.

Q. So table 1 then in part 2 of the paper for this top group, the 1998 controls should be, you say, 28 instead of 30 for males?

A. Yes, the problem was as exemplified in this table 1, for example, if you look at UICC 1998 in the data table itself there were 29 animals found in the archives so again there is a discrepancy between what one would calculate to be on lifetime test as opposed to what one finds in some of the data tables. But generally N-1 should be 28 in the table 1.

Q. Actually what you called N-1 in table 2 of part 1 really looks to me to correspond more to what you call N-2 in table 1 part 2 if that makes any sense although there is still a couple different numbers, but is that right?

A. Would you just say that again? N-1 in --

Q. Let me put it a different way in table 2 of part 1 the column you marked N-1 as I

understood it were the archival rat data that you were able to find?

A. That's correct.

Q. As opposed to what might have been you know back when?

A. That's right.

Q. And then if you go to table 1 in part 2 it looks to me like what your column marked "N2" is -- that same data that you were able to find and "N1" looks like it is more of the data that should have been, is that a fair --

A. That's a fair statement.

Q. Is that a fair statement?

A. Yes.

Q. O.K.

A. But you see with respect to determining the presence or absence of tumors as opposed to determining the presence or absence of fibrosis, it is easier to tell whether there is a tumor there; in other words, the leukemia and the autolysis and the other -- the confounders that might make fibrosis difficult are not playing the same sort of effect when you are diagnosing tumors. Is that --

Q. Well, you anticipated my next question because if we go down and look at the Coalinga data, you examine 27 slides of male rats and 24 of females and it looks like you didn't have that reading problem you had with the fibrosis, that's what you are just saying?

A. Right, that's exactly right.

Q. So in terms of your part 2 paper where you are determining whether the different types of asbestos were tumourigenic in these rats, you were able to actually read more slides, at least for the Coalinga rats, than you were trying to determine if it was fibrogenic?

A. Yes.

Q. Now, let's look at the Coalinga-exposed rat slides that you read that are reported in table 1 of part 2, and as we said before, there are 27 males and 24 females, correct?

A. Yes.

Q. Now, again were these the slides that you looked under the microscope at down at the archive?

A. Yes.

Q. And this determination then of which of them had tumors and which didn't was made upon what data or what basis? Looking at the slides or autopsy reports or what?

A. The criteria? Well, looking at the slides, applying the criteria of McConnell et al., which would be hyperplasia versus adenoma versus carcinoma in conjunction with the gross description that was principle in the autopsy reports. That would be the basis. Just the traditional way of diagnosing a tumor.

Q. Now, with respect then to the Calidria-exposed rat slides, you found two tumors among the 51 Calidria-exposed rat slides you were able to examine, correct?

A. Yes.

Q. That works out to 7.4 percent tumor rate, right?

A. Yes.

Q. That's what you report here in table 1. Now, if we go up and look --

MR. WILL: Well, he reports 2 out of 27 is 7.4 percent, 2 out of 51 is not --

MR. BROWNSON: You're right.

Q. Among the males --

A. Among the males.

Q. -- it is 7.4 percent.

Among the females, it is zero?

A. It is zero.

Q. I must have been adding it.

Now, then if we go up and look at the control 1998, we see that among the males it is --

A. Well, there is an error. There is again under "Total" there is "2(7.4)" but the exact tumors, again I don't know why there is a zero there, but there should be either 2 adenomas or 2 carcinomas or 1 adenoma or 1 carcinoma. There was no mesothelioma.

Q. Before we talk about that, I want to focus on the right-hand column, which is "Total."

A. Sure.

Q. What we see among the slides of the control lifetime rats that you examined in 1998 was that there were two tumors among the males for 7.4 percent and none among the females for zero percent?

A. Yes.

Q. Am I right in saying then that when

you conclude the Coalinga-exposed rats at about the same rate of tumors as the control rats, you were comparing those two numbers and they are the same?

A. Yes.

Q. 7.4 percent for the males and zero for the females, right?

A. Yes.

Q. Now, I wanted to move on to what you just mentioned, but when I go back and look at the other columns that you got there for the control rats, I don't see where those two tumors are. That's my question. Where were those two tumors?

A. There is an error here. I have to go back and check the data.

MR. WILL: I think if you look at the 1984 NIEHS control, does that tell you where --

MR. BROWNSON: That one has three tumors.

MR. WILL: Right.

A. Well, that has three tumors, but with reference to that specific data set, I have to go and check my -- I have to check my notebook.

Q. What I would like to focus on now are the slides that you looked at, O.K. So we can't tell from looking at table 1 which of the control slides had tumors other than you just say there were two male tumors someplace?

A. We can't tell whether they were both adenomas or whether they were both carcinomas or whether there was one of each. We can't.

Q. Do we know in fact that there were two, however?

A. Yes.

Q. So you are saying that one of those zeroes are. Maybe more than one is an error?

A. Yes.

Q. There were two tumors, and at 7.4 percent that's not the error?

A. That's not the error.

Q. So the open question then, part 2 of the paper doesn't tell us and your data reported at table 1 doesn't tell us is where were these two control tumors?

A. Benign versus malignant, right.

Q. Would you admit that that's a fairly important question since we need those two tumors

to make the controls be the same as the Coalingas?

A. In what sense?

Q. Well, if, for example, there were no control tumors, then there would be an increase of tumors among the Coalinga, wouldn't there?

A. Well, there are two tumors. I don't know whether they were both benign or both malignant.

Q. But at least your paper creates some doubt as to where those tumors might be?

A. You mean in the class are the ones benign or malignant?

Q. Well, what they are at all.

A. Like I said, it is either going to be adenoma or carcinoma, and for the purpose of assessing so-called risk in this instance, it is the same, you count them the same.

Q. But you will agree with me that your paper provides us no specifics on those tumors?

A. On the malignant potential of those two tumors.

Q. Right. Then if we look at this column marked BAH, is that what you called the --

A. Hyperplasia.

Q. You had another term you used a minute ago, pretumors or something, what did you call it?

A. No, I said hyperplasia versus adenoma versus carcinoma.

Q. So BAH, why did you include that column in your reporting here?

A. Because that's what was included in other pages in a standard way.

Q. And that's because that's considered a significant finding. It's not a tumor, but it is a hyperplasia which can be induced by asbestos exposure, for example, correct?

A. As I say, some people would like to see that particular lesion to understand what the level is, right.

Q. And in fact some scientists, many scientists consider that a significant finding in asbestos-exposed animals because it is a marker of exposure and a marker of potential tumors if there is more exposure along the way, is that fair to say?

MR. WILL: I object to the form of the question. I think it is fair to ask

him if he thinks it is. It is not fair to ask him what a lot of scientists may or may not think without naming them and being specific.

A. I don't think it is a tumor, necessarily a tumourigenic lesion, and I think the lesions of concern are the adenomas and the carcinomas. Others may disagree with that.

Q. Well, for example, I am looking at this Muhle paper we looked at a moment ago that he published in 1987, and remember, we were talking about the septal thickening a moment ago, but he also has -- in that same table, for example, he reports on the BAH, does he not?

A. Yes.

Q. And I note that in your references you cite this paper by Oberdorster.

A. What about Oberdorster?

Q. Actually I am jumping ahead of myself in citing Oberdorster. Forget that paper for a moment. I am going to cite that in a minute.

Here is what -- Mr. Goldman has reminded me if we turn to page 24 of your paper in the second column called "Other Studies with

Coalinga Chrysotile," about halfway down or two-thirds of the way down in the column, you have the sentence, "However, Muhle et al. have countered" -- do you see that?

A. Yes.

Q. And I will read it, "However, Muhle, et al. have countered this by stating that 'the high rate of bronchiolar alveolar hyperplasia of 74 percent, one case of squamous metaplasia and one adenocarcinoma may indicate a tendency of the crocidolite fibres used to induce neoplasms.'"

Do you see that?

A. Yes.

Q. Again at my peril, I will try to summarize this, but in Muhle's paper there was a finding where not only did the Coalinga asbestos not produce tumors but neither did crocidolite that he dosed these rats with?

A. Right.

Q. And Muhle in that paper talks about, and you talk about in your paper the fact that -- the fact that there was BAH present among the crocidolite-exposed rats, indicates a tendency of the crocidolite fibers to induce neoplasms; right?

A. Right.

MR. WILL: Just to correct something, you stated earlier, Mr. Brownson, Muhle's paper does report one tumor with a crocidolite. You said it didn't produce any.

MR. BROWNSON: O.K. I stand corrected.

Q. But in any event, what Muhle is saying there and from what you quote from him and say in your own paper is that the BAH in that instance indicates a tendency of the crocidolite fibers to produce neoplasm?

A. At 74 percent it is suggested.

Q. Now, again if we turn to table 1, part 2 of your paper, I note that none of the lifetime control rats whose slides you examined had BAH, right, neither male nor female, but of the Coalinga-exposed rats 3 males and 3 females for a total of 6 had BAH, correct?

A. Right.

Q. So would you agree with me that there is more BAH or bronchiolar adenomatous hyperplasia among the Coalinga-exposed rats than there are at

the control rats at lifetime?

A. Yes.

Q. So at least as to that thing, there is a measurable increase, there is 6 versus zero?

A. Right.

Q. Now, again I am in table 1 here of page 2 of your paper, and as you noted earlier, there are a number of different control rats described. And I am not looking at what I call the big control group which is this 1984 IEHS group of some 5,000 rats.

A. Right.

Q. I see that out of -- and these are rats who were never exposed to any asbestos, right? We talked about this earlier, right? That's the big control group?

A. Right.

Q. I see that among that large control group, out of 2,320 male rats, there were 60 tumors?

A. Right.

Q. And you have calculated that to be 2.7 percent, right?

A. Right.

Q. I also note that of the females, out of 2,320 rats, there were 28 tumors, right?

A. Right.

Q. You calculated that to be 1.3, although when I calculated, I only got 1.2?

A. Right.

Q. Be that as it may, there is 1.2 or 3 among the females, O.K.?

A. O.K.

Q. Total tumor rate then among all of those control rats in this big group of over 5,000 controls again would be the average of 2.7 and 1.3, and I didn't do that exact math but it is about 2 percent?

A. Right.

Q. And the tumor rate then among the Coalinga-exposed rats at lifetime, which was 7.4 of the males and zero among the females, and I didn't do the exact math but it is about 3.7 percent, correct?

A. Right.

Q. And you would agree with me that that's about a 50 percent increase or difference over a percentage in the big group of controls?

MR. WILL: No, it is a 25 percent decrease, not a 50 percent increase.

MR. BROWNSON: 2 to 3-1/2.

MR. WILL: It just depends on the way you do the math, whether you take the percentage --

A. I mean I think the analysis you have just done -- you're analyzing the number of tumors that appear in the life span controls and the historical controls, and you are working out percentages?

Q. Right.

A. And the former analysis of the very same concurrent control group done and kept under the same identical conditions at the same time, which is the third entry down on table 1, "Control 1984 NIEHS" found up to 10 percent -- they report 10 percent tumors in the males and none in the females.

And I think the more appropriate control is to compare the concurrent control from the same study as reviewed by McConnell and others with what we have here both for the control group and the Coalinga group, I mean the life span and

the historical groups are done at different times, different periods under different conditions and different lapse, and we know that they can have -- they can have an effect.

So I would say the more appropriate control comparison, if you are looking for another control is to compare the third group down with the findings that we made in this study for controls and to compare against Coalinga so -- does that make sense to you?

Q. Yes. The most important comparison of course are the ones you highlighted in boldface type when we are looking at the Coalinga are the identical lifetime rats, control rats, Coalinga rats, UICC and Jeffrey rats?

A. Well, the identical -- and again the identical group is also the third entry down, this one here I am indicating.

MR. WILL: The 1984 NIEHS control group.

A. That's the same group.

Q. Well, that came out of the same 400.

MR. WILL: No, it's the same rats.

A. No, it is the exact same animals, the

exact same slides. I am sorry it is so confusing. They are the exact same animals. They are the exact same slides. The only difference being for the control 1998, Chris Wagner and I looked at them.

Q. You saw two tumors and they saw three?

A. Exactly. And for the control 1984, Dr. McConnell and I think one other pathologist looked at them and they saw three tumors, but they are the exact same slides, same animals, exact same everything.

Q. Well, let me ask you this. What you are saying then is you take those same lifetime control rats and when you looked at the slides in 1998, you found two tumors, and back in 1984, Dr. McConnell and others found three, right?

A. Right.

Q. And when you then looked at the Coalinga slides in 1998, you found two tumors but we don't know what they would have found because they never looked at those slides?

A. Right.

Q. O.K. So in terms of the closest

comparison being the same rats, the same slides of those dead rats and the same reader, you, it would be the 1998 controls versus the 1998 Coalingas?

Do you agree with me on that? As luck would have it, both turn out to be 7.4 percent?

A. Without the variable readers, is that what you are saying?

Q. Yes.

A. Sure.

Q. What I understand you are telling me is the next closest comparison would be these 1984 controls because it is the same rats but somebody else is looking at the slides, right?

A. Correct.

Q. Then after that, would the next closest comparison be the big rat group that are different rats, bigger group, different people looking at them?

A. Correct.

Q. And you must have thought that to be of some significance because you include it here in your table?

A. Well, I mean I think it is of

importance that this is just a data set that was generated by the NIEHS at the same time, not necessarily in the same place, by the same people. I just thought it would be of interest to the readership to see that there.

Q. But again you had told us a little bit earlier today that it was in fact done in connection with this particular study because remember, I asked if that was a general thing that had nothing to do with this, and you said no, it was this big control group tied into this study and that study was in England?

A. But again one is historical controls and the other is concurrent controls.

Q. Now, I notice again if we go back to the BAH column that the Coalinga rats when you read their slides had the highest level BAH of any of the different asbestos types or the controls for that matter, is that correct?

A. Well, there is more as absolute numbers, but whether there is an actual higher percentage remains to be seen, if you understand what I mean.

Q. I do understand what you mean. There

were six among the Coalingas and five among the UICC, but the percentage may be a little different?

A. Right.

Q. But in any event, at least for that problem of the longs, the Coalinga is not or the Canadian chrysotiles are not worse than the Coalinga, are they?

A. No, it doesn't appear to be.

Q. How would we be able to find out, we or you or anybody, how would anyone be able to find out where these two control tumors are?

A. I have them in my notebook in the office. I can -- it is not -- I will check those. I have those data.

Q. You have those data somewhere in a notebook?

A. Yes.

Q. How about the slides themselves, are they sitting down in that archive?

A. Yes.

Q. And if a layman like me stumbled into that archive and looked for them, are they organized in such a fashion that you could find

them or would it take a skilled person to try to figure out which were which?

A. I think you have to apply, you know, for permission to review, but they will retrieve the slides for you.

Q. But assuming you applied for permission and they said yes, Brownson, we will let you look at them, would they be available to look at anyway, as far as you know anyway?

A. Yes, sure.

Q. As far as you know, they haven't thrown them away or anything since that time?

A. As far as my knowledge.

Q. As far as you know, are you the only person that ever looked at those archives or did you get the impression or find out that other people had been examining these?

A. They said they were lost.

Q. But then they found them in the archives, right?

A. Yes.

Q. And were you able to form any impression one way or another if you were the first person to, you know, come across these or

had other people examine them, do you know?

MR. WILL: You mean since whoever looked at them originally?

MR. BROWNSON: Sure.

A. I think originally Dr. McConnell and Dr. Boorman had reviewed those.

Q. Back at the time of the study?

A. Back in 1981.

Q. I guess my question isn't very clear.

Were you able to get any impression from the people down in the archives whether since the time those things -- you know the study was over and the slides were put away in an archive, did you get any impression if you were the first person then that had come down and looked at the archive or did you find out or seem to think that other people have done that over the years?

A. My impression is that subsequent to McConnell and Boorman looking at them that no one had ever looked, but I can't be sure.

Q. You don't know for sure?

A. I don't know for sure.

Q. Now, when you went to this archive, did they allow you to look at this material

voluntarily or did you have to pry it out of them, was it --

A. Pry it out of them?

Q. Well, when you asked to look at it, did they say, "Sure, go ahead and look at it," or did they give you a hard time? This is what I am wondering.

A. I don't understand. I mean, one person's hard time is another --

Q. Did you have to make a Freedom of Information Act request to get at this stuff or did they voluntarily let you look at it?

A. Once they found it, I was free to look at it.

Q. But when you made your first request to look at it before they found it, did they say, "O.K., we will go find this for you, Dr. Ilgren," or did you have to take forceful steps to make them look for it?

A. Well, in 1992, I had asked Dr. McConnell where they were, and he said the archives.

And then I said, "Would you check and see if they are in the archives?"

And he said he checked and they weren't in the archives.

And so after discussions with various other people who questioned the fact that they -- I mean -- they questioned this idea that they may not be in the archives. I called quite a number of people at the NTP and requested that they search again, and I suppose over a several-year period, they ultimately found these materials, but once they were found, it was just a question of writing them a letter and asking in a line or two would I have the permission to look at these materials.

Does that answer your question?

Q. Did they say yes, come on and look at them?

A. Sure.

Q. So you had to call a number of people and kind of get after them to get them to look for them and find them, but once they found them, you had no trouble going and looking at them?

A. No, not at all.

Q. Now, I am continuing in --

MR. WILL: They were back in the

warehouse next to the box for Raiders of the Lost Ark.

MR. BROWNSON: We don't have to be on the record here.

(Discussion off the record)

MR. BROWNSON: Let's go back.

BY MR. BROWNSON:

Q. Again in part 2 of your paper, I am now looking at tables 2-A and 2-B at pages 20 and 21. And it looks like what you have done here is you have set out some big tables for the UICC/B-treated lifetime rats and the Jeffrey-treated lifetime rats where you go through every single rat and tell what kind of tumor they have, et cetera, right?

A. Right.

Q. For both males and females?

A. Right.

Q. Now, my question is and I think this is of great interest to all of this, why isn't there a table like this for the Coalinga-treated rats?

A. There was no significant fibrosis, and there was no significant tumor response.

Q. Well, there were two tumors among the males?

A. But I consider that to be significant increase over controls, so I didn't include it in a detailed table. I mean all you would have seen for fibrosis scores is a bunch of 1's and 2's and the odd 3, and for primary tumor you would see none straight down the page except for two entries, and then there was some extrapulmonary, there was some pancreatic -- I can't remember exactly what the extrapulmonary tumors, but there were a few extra.

Q. Would you agree with me that it would be helpful for the reader of this paper since the purpose of the paper is to compare the tumourigenesis of the three types of asbestos if you were to set out all of the rats and all of the tumors for the three types of asbestos?

A. No, because as I just said, the editor wouldn't have let it in.

Q. Is that why it is not in here because the editor wouldn't let you put it in?

A. I can't remember what the exact discussion was. He may have said to me, "For

space reasons unless there are positive findings, don't put it in," but I mean there just weren't a significant number of positive findings to include them in the paper.

Q. Well, O.K. It would have told us, for example, what those two particular tumors were that were found in those male rats, we would have had that data?

A. Well, that you could just say, I mean that should have been in table -- in table 1.

Q. But it is not in table 1. All it just says is two tumors. It doesn't give us all this detail that you give us in 2-A and 2-B?

A. But that's an error. But they should have been included in table 1 but there was no reason why if you could simply convey that information in table 1 why you had to generate, say, a table 2-C or a table 3 to indicate all of that.

Q. O.K. Well, you also have a column in both table 2-A and 2-B for the two Canadian asbestos for extrapulmonary tumors, and we have no data one way or another on the Coalinga-exposed rats or extrapulmonary tumors anywhere in the

paper, do we?

A. There might be a description in the -- in another part of the page, but I don't think so.

Q. But there isn't, though, is there?

A. I don't recall, but if you say so, I will take your word for it.

Q. So if I can summarize what 2-A and 2-B show us with respect to the rats dosed with the two types of Canadian chrysotile, you go through every single rat and you tell us what their fibrosis score is. If they have a primary tumor, what that is, if they have a secondary tumor, what that is, and if they have an extrapulmonary tumor, what that is, right?

A. Right.

Q. We don't get that sort of data for the Coalinga-exposed rats?

A. I didn't think it was necessary since the findings were very clearcut.

Q. Let me ask you this question. With respect, for example, to the male Coalinga-exposed rats, there is only 27 of them, right, in your table 1?

A. Right.

Q. And of those 27 rats, if you count the BAH and the tumors which we don't know what they are, that's 5, 5 out of 27 is almost 20 percent. Are you saying that's not significant?

A. Yes. Because I do not -- I am not counting the hyperplasias as tumors. I don't see anybody else who would count that.

Q. Yet in table 2-A you are reporting the hyperplasias?

A. In brackets but not as tumors.

Q. But it is data that is provided to the reader and it is data that you saw fit to provide to the reader with respect to the Canadian asbestos, right?

A. But it is also provided here for the Coalinga. You are saying basically that I need to put an extra table in for Coalinga to lay out all the extrapulmonary tumors, to lay out everything else.

Q. I am not saying anything what you have to do. I am saying you did it with the Canadian asbestos but you didn't do it with the Coalinga.

A. I have not attributed nearly as much significance to the BAH's as to the clearcut increase in the primary tumors in the Canadian-treated animals.

Q. But you have to admit, Dr. Ingren, if the careful reader here wanted to compare this, the tumors and the other problems in the Coalinga-exposed animals, as small as they might be, with the tumors and the other problems in the Canadian-exposed animals, that careful reader can't do it because you just give the data for the Canadian. You don't give the data for the Coalinga?

A. The careful reader --

MR. WILL: The answer is no, you didn't give the other data.

A. But I don't think that's the answer. I think the answer is that the careful reader has been asked if they wanted additional data to apply to the authors, it is not necessarily for this item but -- and I can't remember whether it is in part 1 or part 2, but there are certain places where we say data not included, the authors will provide the data, and I think if someone wants

that information, I am perfectly happy to tell them which animals --

Q. You have such a table?

A. With -- in my raw files?

Q. Yes.

A. Probably, sure.

Q. What lethal nonpulmonary tumor did female rat number 11 in table 2-A get?

A. Lethal -- oh, one couldn't tell because N/A means not available. They said in the autopsy description that the animal was largely cannibalized. That was the only one.

Q. Where does it say N/A?

A. Are you looking at table 2-A?

Q. 2-A females?

A. You are looking at female number 2.

Q. No, number 11.

A. Oh, 11, I'm sorry. And what's the question?

Q. What was that thing?

A. In terms of a primary tumor or in terms of what was what thing?

Q. Well, you have got a question mark in parenthesis and two exclamation points. I am

wondering what that is. It is either some horrible tumor that is too bad to mention or it is --

A. Oh, I see what that was. I just wanted to indicate that when you looked at the autopsy report, Mr. Brownson, there was an actual page for each animal. There was an autopsy report which gave the findings and all the details, and it said in the gross description that there was a lung tumor seen grossly, but when I looked at the actual slides, I couldn't see it, so that's that strange notation there.

Q. Going back to table 2-A and 2-B?

A. Yes.

Q. The heading of each of those two tables where we -- where you lay out all these tumors for the Canadian asbestos, it says "Nonpulmonary neoplasia."

Are there also pulmonary tumors reported someplace?

A. Hang on. I'm sorry, go back to --

Q. In both tables 2-A and 2-B, pages 20 and 21 --

A. Oh, what I am trying to say there is

that the table 1 is supposed to talk about just pulmonary neoplasia.

Table 2 is supposed to also indicate it should actually be after the Coalinga.

Do you see that on a "Lifetime Test:" And the heading, it should be "Pulmonary and Nonpulmonary Neoplasia."

Q. So that's just an error?

A. Yes.

Q. But again I don't want to beat a dead horse on this. We have no data here as to the nonpulmonary neoplasias anyplace, whether in table 1 or in table 2-A or 2-B with respect to the Coalinga-exposed rats.

A. Not in this paper, no.

Q. Now, I am looking at table 2-B. I note that, for example, with respect to the male rats, it says there are 8.3 percent probably lethal nonpulmonary tumors, do you see that?

A. I beg your pardon?

Q. Table 2-B?

A. I see that. 8.3 percent.

Q. Those are the nonpulmonary tumors?

A. 8.3 percent, 2/24 probably lethal

nonpulmonary tumors. What is your question on that?

Q. I am just saying that's what it says there.

A. That's what it says there.

Q. First of all, were these tables put together by you or were these again taken out of some --

A. No, I put these together.

Q. Why did you write that there? Why did you write probably lethal nonpulmonary tumors? I mean what significance does that have?

A. I have to go back to the text to refresh my memory on this point. I think that discussion has to do with competing causes of death in trying to understand a bit more fully why the lung tumor incidence of the females appeared to be much more lower than the males. Are you with me on that?

Q. O.K.

A. And the -- well, for example, look in table 2-B, if you look under female No. 13, female No. 14, female No. 18 or the female No. 25, you had these huge, often metastatic tumors which

clearly were lethal, and I was just trying to understand what, you know -- whether the females were developing these tumors earlier and that that was causing a reduction in survival as opposed to the asbestos.

Does that make any sense to you?

Q. Well, I guess it does, but again we don't have that sort of information with respect to the Coalinga-exposed rats, do we, anywhere in these papers?

A. No.

Q. Now, I am going to results, which is at page 22.

A. All right.

Q. And again we are talking about -- you're talking about the incidence of primary pulmonary tumors in the Coalinga-exposed rats whose slides you examined. You had 51 rats. You examined for tumors for -- slides of rats you examined for tumors, right?

A. Right.

Q. And you had two tumors and you calculated that out to be 3.9 percent?

A. Right.

Q. Now, earlier back in paper No. 1 at table 8 remember, we noted in that right-hand column, you had noted two tumors out of 90 Coalinga-exposed rats, and my question is did you examine the slides of 51 rats or 90 rats for tumors?

A. No, 51 rats.

Q. So again, going back to part 1, that is an error. That should be changed to 2 of 51 and 2 of 90?

A. No, the data presented -- you are talking about paper 1, table 8.

Q. Right, table 8.

A. It should be 80 as opposed to 90, just to standardize that with the rest of the studies that were under comparison. Table 8, "Summary of Selected Inhalation Bioassays," is that what you are talking about?

Q. Yes.

A. The studies which are respective to short, long, chrysotile, et cetera, are the studies of John Davis, and in those studies the denominator or 40 is the number of animals which he actually started out with in his original

group, and so I would say that the number, the comparable starting number, so to speak, in this instance, I guess would be 80.

Q. But the question is was it 80? In other words, you didn't examine 80, the slides of 80 dead rats to see if they had tumors. You only examined the slides of 51 dead rats to see if they had tumors?

A. I don't know what the comparable number would be for the denominators in the Davis study? In other words, he probably examined less than 40 as well. Do you understand?

Q. I guess I understand what you are saying, but you don't know what he examined or didn't examine?

A. I would have to go back to the original papers.

Q. But we do know what you actually examined and you examined the slides of 51 dead rats for tumors?

A. Right.

Q. Now, under your results again on page 22.

MR. WILL: Excuse me one second.

(Counsel confers with witness.)

MR. WILL: Go ahead.

Q. I am now looking at the results, page 22?

A. O.K.

Q. And you say there in the second paragraph, "The 2 Coalinga-treated tumor-bearing animals had fibrosis scores of 3.0 which were probably overestimates due to concomitant uraemic pneumonitis and leukaemic infiltration."

Do you see that?

A. Yes.

Q. Again, unless I am missing something, I don't see the data in a paper where we can see that. Is that presented somewhere?

A. Just in my notes.

Q. If there had been a table 2-C showing the Coalinga-exposed rats, we would have seen that sort of data on the table?

A. No.

Q. Well, the table has the fibrosis scores?

A. Well, the fibrosis score can be put in text, but you wouldn't have seen the

information about uraemic pneumonitis, for example.

Q. But we could have determined what the fibrosis scores were of the two tumor-bearing animals and we could look at all the other fibrosis scores and we could see that, couldn't we?

A. Well --

Q. And we could see something about the leukemic infiltration since that is a nonpulmonary tumor of the sort of thing you described in tables 2-A and B, couldn't we?

A. If I felt it was important, you know, I would have put it in. I didn't feel it was a major confounder. I mean I just --

Q. Well, then why do you say it was a confounder? You say it was an overestimate?

A. Well, a confounder in the sense that it didn't prevent one from scoring. It wasn't the kind of extensive pneumonitis or infiltration that precluded scoring. I mean this was not the sort of thing that one would put down as prevent slides from being read. This was just something that was also there, and I just wanted to tell the reader

that the additional cellularity due to the pneumonia and due to the leukemia probably increased the so-called fibrosis score.

You see the fibrosis score of 3. Do you know the scoring system for the Wagner grade? Do you know the scoring system?

Q. I have read it?

A. Well, there is eight grades, but the first three are actually cellular grades. 4, 5, 6, 7 and 8 are so-called fibrosis, so a so-called fibrosis score of 3 really just denotes increased cellularity and the pneumonitis increases cells and the leukemia increases cells. It doesn't -- it doesn't causes fibrosis per se. It just adds additional cells so I am --

Q. O.K.

A. O.K. So I am saying that you have an additional level of cellularity on top of -- on top of this lung which was clearly due to leukemia and the uraemic edema and the pneumonitis.

Q. It is also true that some of the Canadian chrysotile asbestos-exposed rats had some of this additional cellularity caused by several things as well which could have raised their

fibrosis cells?

A. We are perhaps talking at cross purposes here. That's why I asked you if you knew the grading system.

Q. Let me ask that question.

Isn't it true that some of those rats also had these -- things that you describe here in these two Coalinga rats?

A. Yes. The point I am trying to make here, if you look at tables 2-A and 2-B, you are dealing with so-called scores -- let's put the word "fibrosis" aside for one moment -- so-called scores of 5 and above, and almost every single instance where a score can be assigned, and so it is not a question of whether additional cells is going to increase the score as such. It is a totally different situation. Does that -- is that clear?

Q. I am not sure it is clear.

MR. WILL: Additional cells doesn't affect 4, 5 and 6 scores, is that right?

A. You go from minimal to moderate to moderately severe cellularity with respect to Grades 1, 2 and 3.

For Grade 4, you get what is called cuboidalization of the alveolae.

For Grade 5, you get an increased collagen definition.

For Grade 6, you get a linking of the lovules.

And for 7 and 8, you get a massive laying down of fibrosis.

But Grades 4, 5, 6 and 7 don't entail in the definition of those scoring grades an increase in cells. O.K.

Q. O.K.

A. I know it is difficult conceptually to visualize this, but it is the clearest explanation I can give.

Q. Let's continue then with the data you then are presenting, is the tumor rates and the number of tumors in the surviving rats from the experiments whose slides you looked at, right, the lifetime surviving rats?

A. Right.

Q. Because then you go on to say in your results, and I am now reading over on the right-hand column on page 22, "The incidence of

tumors in the interim sacrifice animals could not be determined precisely since some of the records were missing."

A. Well, the 3 and 12-month animal lungs were not available for review. They were lost. We couldn't find them.

The 24-month animals for controls and for the UICC/B, if I have this right, had already been reported. Then there was some other statement, I believe, in Kent Pinkerton's thesis about tumors in the 24-month interim animals.

Q. How about the 3 and 12-month animals?

A. There has been no -- well, if you look at McConnell et al. 1984, he basically says that tumor incidence or neoplasia is very low before 24 months. But there is no -- there is no -- there is no -- the answer to your question is there is no data on the 3 and 12-month.

Q. So you're speculating that there is probably no more than one tumor in the Coalinga-exposed rats who were killed as part of the original experiment, but you don't know that for sure?

A. I am just reading my sentence here.

I just want to see what I am referring to here in reference 19 and 20.

MR. WILL: I just want to object to the form of the question, to the use of the word "speculate." His report says the available data suggests, and I think it strongly suggests and I think that is different than speculating.

A. I need to go back. I can't recall -- if you look on page 30 under "References," there is 19 and 20, I can tell you that from the thesis Kent Pinkerton said that no more than 0.1 percent of the Coalinga-treated animals had any tumor tissue in the lung among the ones he saw, and he wouldn't have been able to say whether they were primary or secondary, plus he included under the definition of tumor he had metaplasia, and neoplasia, do you understand?

So it is unclear what he was talking about.

As far as reference 20, I would have to go back and see. You know, I just don't recall what I am referring to when I say the available data strongly suggests an absence of tumor. I

just don't recall.

Q. But you are willing to put out in the text of your paper that it was a strong suggestion, even though you don't know exactly what it is?

A. I just can't remember sitting here today. I can't recall.

Q. And I guess you couldn't recall when you wrote the paper either because you said you couldn't tell precisely, but you didn't think it was more than one?

A. Yeah, but I can't establish sitting here today, or I can't recall what was in the NTP documents that made me, you know, come to that conclusion, so I just have to check that.

Q. Now, you make an interesting statement on page 24 under the heading "Other Studies with Coalinga Chrysotile."

You are talking about this Muhle paper again. You say, "These workers chose Coalinga as their 'positive' control since they were unable to obtain sufficient amounts of Canadian chrysotile for a bioassay"?

A. That's correct.

Q. Is that what -- and then you say that's from a personal communication in 1988?

A. That's correct.

Q. Is that what Muhle actually said, that he couldn't get Canadian chrysotile?

A. He said he couldn't at that time get the several-kilogram amount he needed to do the inhalation. He could do enough to do the injection, which is why he contacted NIOSH and asked them if he could get the other material.

Q. Now, I am now turning to page 25. Let's go back to page 23 of part 2 of the paper. At the top of 23 is table 4.

MR. GERSON: On top of what page?

MR. BROWNSON: 23.

A. I just lost 23 for some reason. O.K. I have got it.

Q. And without quoting that title in detail, basically what you are reporting is changes in cell volume among the different rats, exposed to the different types of asbestos, correct?

A. And number, yes.

Q. And this is a table you got out of

Pinkerton's data, this is not your data?

A. Right, and the other sources here. But that's correct.

Q. And if you look down in the text on page 23 where you are talking about that increases in cell volume, and I am looking now in the right-hand column, you are making these different comparisons with fferent types and you are talking about, among other things, the Jeffrey asbestos increasing cell volume, right?

A. Just remind me again where you are in the text, is it over on page --

Q. 23, in the second column.

A. O.K., right. I got it.

Q. When I look at table 4, I don't see anything above Jeffrey asbestos. Where is that?

A. That's another when they did the editorial -- well, there has been an editorial deletion of the Jeffrey data column here, and they are going to publish that as an erratum, so the Jeffrey had not been included in here.

MR. WILL: That was sent to the paper.

A. That was sent to the paper, and they

put the table vertical instead of horizontal and deleted it.

Q. Didn't you catch that when you read the galleys?

A. Well, it didn't come back that way. Where is page 22? You see page 22?

Q. So when it came back up to you, the Jeffrey was in there but when they published it, it got cut off?

A. Right. You see how they have arranged the header on table 3?

Q. Yes.

A. And you see how they have arranged it in table 4?

Q. Yes.

A. Well, they put the header to the left of the column.

Q. So in any event, that slipped through and nobody caught it and it got cut off and now we don't have it available and again if the careful reader looks for the Jeffrey column, he doesn't find it?

A. Right.

Q. Let me go back on table 1 in the same

paper, part 2 and you know we talked at some length about the controls where we had -- you report two tumors, but then when we look for the different categories "00." Is that something else that slipped through the galley --

MR. GERSON: I object to that form.

Q. Well, I am wondering is that just an error that slipped through or I mean did you --

A. I don't know. I don't know. I am as surprised to see it as you are.

Q. Now, with respect to table 4, which is at page 23, which is the increase in cell volume table, again, I don't want to go through this in detail because your paragraph is -- the text is almost a full page long, but essentially you say that this table supports your conclusion that the Coalinga-exposed rats weren't getting tumors, right?

A. Do I say that -- can you point out where I say that?

Q. Otherwise why would it be in the paper? It says that they don't get tumors, what's the point of having it here if it doesn't support your --

A. It is consistent with the idea that -- or it is consistent with the tumor data. I think that's what you are trying to say?

Q. Right.

A. O.K.

Q. Now, my question is this: As I read this again, these scores are kind of all over the map but, for example, at 12 months, the controls have 57 for the report of volume plus or minus 8, but the Coalinga has 118 plus or minus 50. That looks to me like a pretty big increase?

A. Which one are you on, table 4 or 3?

Q. I am on table 4.

A. O.K., and which are you on, cell volume?

Q. I am looking at the 24-month cell number.

A. Cell number, O.K.

Well, like I said before, you know, the statistical comparison based on the sample size, the standard deviation and the confidence interval suggests that there is no significant difference between these two.

Q. It also suggests that there is no

difference between the controls and the chrysotile and the UICC/B? That's even lower than the Coalinga?

A. But the confidence interval was very -- it is plus or minus 7.

Q. And of course we don't know what the Jeffrey is because the editors cut that one off. If we looked then at cell volume, which is higher up in the table again, for example, and I will look at some of these at 24 months, the control males are 28 plus or minus 9, and the Coalinga-exposed rats are 62 plus or minus 15. Wouldn't you call that a significant increase?

A. No, I mean the significant -- the numbers were put into, as it indicates in the legend of the table, the various statistical tests which are Dr. Duncan's multiple comparison tests, for example, and when the data were put into that statistical package, there was no difference found between the control and Coalinga.

Q. Who put the data into that statistical package?

A. Kent Pinkerton.

Q. But be that as it may, when we look

at these columns virtually -- in virtually every instance, the Coalinga cell volume and cell number is largely increased and the UICC/B cell volume and cell number is largely increased, but it is about the same as the Coalinga, it is not dramatically different, is it?

A. Sorry, I am getting a bit tired, just ask me again.

Q. Well, as we look at table 4, you say that this shows that Coalinga controls are about the same and UICC/B, to quote the text, as larger yet when you look at both cell volume and cell numbers put in table 4, it looks to me like Coalinga and UICC/B are increased and in about the same amount, isn't that correct?

A. Well, all I can tell you is what I said before. There are statistical differences here. There is trends here. The overall difference between control and Coalinga on the basis of the statistical analysis and in consideration of the size and in consideration of the confidence interval suggests that there is little or no difference between control and Coalinga. It is unfortunate we don't have the

Jeffrey data to compare, but with respect to control and Coalinga, that's what I would --

Q. So you are saying that if a competent statistician looked at data set out in table 4 that he would conclude that control and Coalinga are about the same and UICC/B is much greater?

A. It would be greater to the degree of significance as indicated in the superscripts using the various tests.

Q. Turning to page 25 --

A. Yes.

Q. -- you talk about various injection studies which in your mind "have convincingly demonstrated Coalinga's lack of carcinogenicity," correct?

A. Yes.

Q. Now, you then, however, criticize injection studies by Maltoni for a number of grounds, correct? You don't find that one to be persuasive?

A. On a number of grounds.

Q. Right, and of course you don't agree with the results either because he found that Coalinga was -- did cause tumors on injection,

didn't he, in his studies which were published twice, by the way, correct?

A. Correct.

MR. GERSON: What are you asking? Is it correct that they are published twice or --

A. Maltoni et al. in 1982, and Minardi 1984.

Q. You are not saying that Dr. Maltoni is not a competent experimental animal researcher, are you?

A. I am not saying anything, but what is in the text is indicated by these specific criticisms.

Q. And one of your criticism of course is you are not sure if this asbestos that Coalinga -- that Maltoni injected his rats with was Coalinga or not, right?

A. That's right.

Q. And putting aside the fact that, of course it was Coalinga, you state it could have come from one of the many serpentine ore bodies in California?

A. We don't agree to the fact when -- I

mean I wrote to Maltoni and I called Maltoni, and I never received a response as to what the source was.

Dr. Langer has given you some verbal information which I find very interesting, but I find it interesting also that the very man who did the studies wouldn't respond to my requests for information.

Q. Maybe it is because he is busy. I mean you don't know why he didn't respond?

MR. WILL: You mean the careful reader couldn't learn that from his paper?

THE WITNESS: No.

Q. Well, let me put it to you this way.

When you wrote this paper in about 1998, you wondered if this California chrysotile that Maltoni injected his rats with was Coalinga or not, didn't you?

A. Yes.

Q. And you said, and you quoted in the text of your paper, part 2, that it could have come from any of the many serpentine ore bodies in California, right?

A. Right.

Q. You know, Doctor, there is only one commercial serpentine ore body in the State of California, and that's Coalinga?

A. That's wrong.

Q. What are the others?

A. There is Copperopolis, number one. There is two or three others for sure.

Q. What are they?

A. I don't recall.

Q. So there are many, but you don't know what they are?

MR. WILL: Well, there are others.

A. There are others, and it is the state -- serpentine happens to be the state mineral.

Q. Well, assuming it was, you still would have other criticisms, don't you?

A. Yes.

Q. You don't level any criticisms, for example, at Muhle's injection studies that I can read here, do you?

A. No.

Q. And you don't level any criticisms at Pott's injection studies?

A. As laid out in the studies you are referring to in table 5?

Q. Right.

A. No.

Q. You would agree with me, Dr. Ilgren, that animal injection studies with Coalinga chrysotile, some show lack of tumors and some show tumors, right?

A. Correct.

Q. O.K. And what you have done here is criticize those that show tumors and you accept those that do not, haven't you?

A. Will you say that again.

Q. What you have done in this paper is criticize those injection studies that show tumors but you accept those that do not show tumors? Because I see no criticism of those studies.

A. I didn't find any -- I didn't find any major problems with these particular injection studies.

Q. Well, for example, you criticized the Suzuki studies on a number of grounds?

A. Yes.

Q. You described that, for example, as

New York/Japanese collaborative group?

A. That's right.

Q. Where do you get that from?

A. Suzuki is based in New York and Kohyama is based in Tokyo.

Q. But when that study was done, they were both in Mt. Sinai in New York, weren't they?

A. I don't know.

Q. Shouldn't you have checked if that is something you put in your text?

A. Correspondence with Kohyama and Tokyo, I assume he is based in Tokyo.

Q. Of course that study was published in '83, wasn't it?

A. Well, I have just assumed rightly or otherwise that he was in Tokyo.

Q. Then you also say that study is confounded, among other things, by "supra--maximum-tolerated-dose effects"?

A. Correct.

Q. And for that proposition, you cite reference 29 which is the Oberdorster paper I referred to earlier, correct?

A. Right.

Q. Would you agree with me, Dr. Ilgren, that you can read the Oberdorster paper from front to end and you will never see that scary term?

A. I don't know. I haven't read it in some time, but if you say so, then I would accept that.

MR. WILL: By "scary term," you mean "supra-maximum tolerated dose effects"?

MR. BROWNSON: Right.

Q. Now, I am looking at the appendix on page 29, and my first question is very simple. Why is this here?

A. Why is this here?

Q. Right. What is it? I can't figure out what it is.

A. O.K. In 1981, a panel of pathologists was convened in Wales to review the same slides from a part of the collaborative study.

Do you know what I mean when I say the collaborative study? It is the study where, for example, the MRC dusted animals with UICC/B and the NEIHS dusted animals, and they did them concurrently. And this table is a modified

version of a table I got from Chris Wagner from his, I guess, document repository or archive, and I just wanted to indicate that there was some difference in opinion when certain pathologists looked at the same slide in some cases.

Q. But I guess my question is the text of this paper doesn't talk about what this is, so I was confused because it just kind of sits there at the end?

A. All right.

Q. Is there anything in the text that explains anything about this appendix?

MR. GERSON: Other than the notations on the appendix.

A. I have to go through the paper and check.

Yeah, it is on page 26, column 2. It starts at the top of the page. Actually, if you look at the bottom of column 1, it says," Wagner et al. also examined untreated and UICC/B-treated animals maintained in a manner identical to those of McConnell et al. and observed tumors yields similar to ours."

Then I have, "Small discrepancies in

tumor yields between our own analysis and those of McConnell et al. and Wagner are potentially attributable to interobserver variation. This is well supported by the findings of 13 independent pathologists from 9 institutions who reviewed 20 tumor cases from the comparative study."

Q. Just so we are clear, the comparative study was not this review of the slides in 1995 through 1998, that was the --

A. Original 1984, right.

Q. And it also included that one over in England, the one here and the one in England?

A. Yes, it did.

MR. WILL: The next sentence of the article says, "Diagnostic variation was particularly notable (Appendix)." That's the reference.

Q. Then if we look at the appendix, we note that apparently after a meeting between these many eminent pathologists, many diagnoses reclassified after the meeting were BAH, so once again that term appears, correct?

A. Yes. Particularly in the direction of tumors being reclassified from tumor to BAH.

That seems to have been the major trend.

Q. Dr. Ilgren, you obviously have extensively reviewed the medical literature as apparent from your references. In your long and extensive review of the literature, have you ever seen a drinking parable in your scientific paper other than this one you threw in here?

A. A drinking parable?

Q. You talk about the man drinks whiskey, and then he is drinking gin, and that's at page 29 of part 2?

A. You don't like it? You don't happen to like that?

Q. Well, whether I like it or not, I am just curious because I have never seen a drinking parable in a peer-reviewed scientific paper. And I can ask that as a serious question.

Have you ever seen a drinking question in a peer-reviewed scientific paper?

A. I don't think I have ever seen a drinking parable in a scientific paper.

Q. And now I want to turn -- I want to follow up one final thing on part number 2.

Did you proofread the galleys before

this was published?

A. Yes.

Q. And is the same true with part number 1?

A. Yes.

Q. I now want to turn my attention to part 3 --

MR. WILL: Let's take a break.

(Document entitled "Coalinga Fibre-A Short, Amphibole-Free Chrysotile Part 1" marked Defendant's Exhibit 36 for identification, as of this date.)

(Document entitled "Coalinga Fibre-A Short, Amphibole-Free Chrysotile Part 2" marked Defendant's Exhibit 37 for identification, as of this date.)

(Continued on the next page)

Ilgren

(Document entitled "Coalinga Fibre-A Short, Amphibole-Free Chrysotile Part 3" marked Defendant's Exhibit 38 for identification, as of this date.)

(Time noted: 5:15 p.m.)

_____

Subscribed and sworn to before me
this _____ day of _____, 1998.

_____

C E R T I F I C A T E

STATE OF NEW YORK)
) ss.:
COUNTY OF NEW YORK)

I, NANCY R. SULLIVAN, a Shorthand Reporter and Notary Public within and for the State of New York, do hereby certify:

That I reported the proceedings in the within entitled matter, and that the within transcript is a true record of such proceedings.

I further certify that I am not related, by blood or marriage, to any of the parties in this matter and that I am in no way interested in the outcome of this matter.

IN WITNESS WHEREOF, I have hereunto set my hand this 2nd day of September, 1998.

Nancy R Sullivan
NANCY R. SULLIVAN

August 13, 1998

I N D E X

E X H I B I T S