BILLY B. SMITH, ET UX	:	14TH JUDICIAL DISTRICT COURT
VS. NO. 92-2915	:	PARISH OF CALCASIEU
HARTFORD ACCIDENT AND INDEMNITY COMPANY, ET AL	:	STATE OF LOUISIANA
JOHN W. CRAWFORD, JR., ET UX	:	14TH JUDICIAL DISTRICT COURT
VS. NO. 92-3098	:	PARISH OF CALCASIEU
HARTFORD ACCIDENT AND INDEMNITY COMPANY, ET AL	:	STATE OF LOUISIANA

DEPOSITION OF OTTO WONG, Sc.D., F.A.C.E.

PURSUANT TO SUBPOENA and the Louisiana Code of Civil Procedure, the above-entitled deposition was taken on behalf of Plaintiffs at 55 Madison Street, Suite 510, Denver, Colorado, on Wednesday, July 28, 1993, at 9:55 a.m., before Rebecca L. Semeyn, Registered Professional Reporter and Notary Public within Colorado.

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APPEARANCES:

For the Plaintiffs:
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I N D E X

EXAMINATION PAGE

July 28, 1993

P R O C E E D I N G S

OTTO WONG, Sc.D., F.A.C.E.,

having been first duly sworn, was examined and testified as follows:

EXAMINATION

BY MR. HOBSON:

Q. Would you introduce yourself please, sir.

A. Otto Wong.

Q. And how are you employed now, Dr. Wong?

A. I'm the chief epidemiologist at Applied Health Sciences.

Q. And what is Applied Health Sciences, please?

A. It's my company set up to do research and consulting mainly in epidemiology.

Q. And what kind of a company is it? Is it a corporation?

A. Yes, it is.

Q. And are you the only shareholder?

A. Yes.

Q. And where is the corporation located?

A. It's located at 181 Second Avenue, Suite 628, San Mateo, California 94401.

Q. Are there any other professionals who work with you at Applied Health Sciences on a full-time basis, Dr. Wong?

A. Not on a full-time basis.

Q. Are there any full-time employees of Applied Health Sciences?

A. I'm the only full-time employee.

Q. Who are any part-time scientists who are associated with Applied Health Sciences?

A. I have a number of people who work with me on an as-needed basis. I have a Ph.D. in biostatistics, his name is David Ragland; and a master level biostatistician, her name is Nancy Gordon.

Q. And could you spell her name, please.

A. G-o-r-d-o-n. I have a nurse/programmer, her name is Linda Hebbar, H-e-b-b-a-r.

Q. Is it H-a-p, as in Paul?

A. H-e-b, as in boy.

Q. Anyone else who has associated with you at Applied Health Sciences in the past?

A. And I have some data entry research assistants.

Q. Would it be fair to characterize the data entry people as nonprofessionals?

A. Technical people. Nonprofessional.

Q. How long has Applied Health Sciences been in business?

A. Since January, 1991.

Q. Did you have any professional activities since

leaving Environmental Health Associates besides Applied Health Sciences?

A. I assume you're talking about "professional" relating to epidemiology or health?

Q. Correct.

A. No.

Q. So basically you left EHA and started Applied Health Sciences and that covers all your professional work?

A. That's correct.

Q. What kinds of work do you have ongoing at Applied Health Sciences?

A. We have a number of long-term projects ongoing. I can give you some examples.

Q. Yes, I would like those.

A. We are finishing up a long-term epidemiologic study of gasoline workers for the American Petroleum Institute. I am doing a cohort mortality study for USG of people exposed to man-made mineral fibers.

I'm working on a cohort mortality study for the Styrene Information Research Center for people exposed to styrene. And I also have some ongoing activities--consulting activities with Mobil and Chevron. These are the projects that I can recall right now.

Q. Have there been any projects that you have

completed while you've been at Applied Health Sciences that are not in the group you just described for me?

A. We completed a project for HIMA, H-I-M-A, Health Industry Manufacturers Association. It was a project that carried over from the old EHA days. It was a study of workers exposed to ethylene oxide.

Q. Was that published?

A. Yes, it was published last year in the British Journal of Industrial Medicine.

Q. I see you have your CV with you. Does that include your publication list?

A. Yes.

Q. And it would include the ethylene oxide publication, then?

A. Yes.

Q. Any other projects that you've completed since you got to Applied Health Sciences?

A. Well, I'm drawing a blank. I work very hard, but I'm drawing a blank. What else did we finish?

Q. Lawyers know you can make more money if you continue to work on projects than if you finish them.

A. I guess some epidemiologists think the same too.

I didn't have any major projects that we finished during the last couple of years at this point.

Q. When you do consulting for Mobil and Chevron as

an employee of Applied Health Sciences, which you assume it is, what rate do you charge them?

A. 230.

Q. Dollars per hour?

A. Right.

Q. And when you give deposition work like you're doing today, how much do you charge for that?

A. 320.

Q. Why the difference?

A. In general, work related to litigation--they are pretty short-term. It doesn't really provide a long-term stability to the business.

Q. Any other reason?

A. That's the major reason.

Q. Do you charge $320 an hour to prepare for a deposition also?

A. Yes.

Q. Can you give me some idea of what percentage of your time you spend working on litigation-related activities? And by that I mean not including your epidemiological studies that you work on.

A. I would say over the last few years it would average to about 30 percent.

Q. By "the last few years," is that since you've

been at Applied Health Sciences?

A. Yes.

Q. Has the trend been more or less in the last few months--say the last six months?

A. I would say it stays about the same.

Q. So if you charge more per hour for litigation work than you do for your other kinds of activities, the amount of income you would generate from litigation would be more than 30 percent of your total income?

A. No. Let me clarify this. I think you asked me how much I charge for consulting work for Mobil and Chevron. I think that was the specific question.

Q. Yes, sir.

A. When we do a major study, sometimes we work on a fixed fee basis--a fixed budget. So if we do an efficient job, we can naturally come out ahead. So it's

very difficult to just look at the hourly wage to determine how much we make.

Q. To make sure I understand what you're saying, if you work on an epidemiology project that has a fixed payment for that project, you can actually make more than $320 an hour?

A. If we are efficient.

Q. Now, you say that USG was the sponsor of one of the studies. Is that United States Gypsum or what used to

be United States Gypsum?

A. I really don't know. Since I have known them it has always been USG.

Q. Where are they located?

A. In Chicago.

Q. And the American Petroleum Institute, you've done work for them in the past besides the gasoline workers project, correct?

A. Yes.

Q. And who is the styrene project being sponsored by?

A. The Styrene Information Research Center.

Q. And who is the Styrene Information Research Center, as you know them?

A. It's an organization, I guess, sponsored by the industry.

Q. And do you know who the member companies are in that organization?

A. I know some.

Q. Who do you know?

A. Dow Chemical, Chevron, some smaller companies, boat manufacturers and so on.

Q. Does your study look primarily at styrene manufacturing or does it include styrene consumption?

A. Can you clarify "consumption."

Q. Yes. Converting styrene into another product away from the manufacturing side.

A. We look at mainly industrial user's styrene.

Q. For instance, would it include a styrene butadiene rubber plant?

A. No.

Q. Or a polystyrene manufacturing facility at a different location than the styrene manufacturing facility?

A. No.

Q. And what kinds of users come to mind that you've been looking at for styrene?

A. Boat makers--manufacturers of boats. Any reenforced plastic manufacturers.

Q. You say that is a cohort mortality study?

A. Yes.

Q. What makes up the cohort?

A. Workers in the reenforced plastic industry who are exposed for a minimum of three months during a specific period of time, and I just don't recall the time.

Q. Where are you in that project? How close to completion?

A. Toward the end of this year, knock on wood.

Q. And the cohort mortality for USG, how far along

25 are you on that?

1 A. We just started.

2 Q. And the gasoline workers study for the API?

3 A. In fact, I'm writing the report now. Let me

4 back up a little bit. We issued a report on the cohort

5 analysis part two years ago, and subsequent to that we did

6 a case control study and now I'm working on the case

7 control study reports.

8 Q. What are the cases that are going into the case

9 control study?

10 A. All the leukemia cases, kidney cancer cases and

11 multiple myeloma cases.

12 Q. And you say the final report of the case

13 control study is being written now?

14 A. Yes, sir.

15 Q. And when do you expect to have that final

16 report generated?

17 A. I would say within two or three months.

18 Q. Who's the API coordinator for that project?

A. Dave Monogillo, M-o--you put me in a very difficult situation. I better know how to spell his name. M-o-n-o-g-i-l-l-o.

Q. Where is he located? Is he an API employee?

A. He's at API in Washington, D.C.

Q. Do you have a company coordinator for one of the member companies?

A. The chairman of the task force is Will Bailey at Chevron in San Francisco.

Q. How do you as an epidemiologist go about estimating exposures for cohorts that you study?

A. Well, take the API study--the gasoline study. We--"we" meaning myself and also the EHA, because that was started at EHA about nine years ago. We have the epidemiologic portion of the study and another group of investigators handled the exposure assessment part, although we worked together with them.

The person who was in charge of the components

is Dr. Tom Smith. He used to be at the University of Massachusetts. Now he's at Harvard University.

Q. That's a little more specific than I wanted to know.

I'm interested in knowing how you, as an epidemiologist, can go about doing these mortality studies or morbidity studies and assessing exposure as a part of them as a general proposition. Can you tell me how that's done.

If you were going to teach a class--a bunch of college students how to assess exposures and look at those responses for a mortality or morbidity study, how would you begin that lecture?

A. What we do is we collect all the employment

records of the cohort members in the study. We computerise the records. We print out a list of departments, job titles, and then we would go to people who are knowledgeable about historical exposure, and that would be company industrial hygienists, engineers and also

personnel managers--personnel employees, and work with them to determine what exposures were entailed in each department for each job title.

They would, in turn, rely on whatever industrial hygiene data that are available, and the historical records of engineering changes, production records and so forth.

Q. Can you tell me, from your experience, if you know of any instance where in a project that you've been involved with, the historical exposure information has been validated by talking to the workers who were actually exposed?

A. I don't know whether we actually have gone back and talked to some workers and validated that, but in some of our own projects we have input from employees, especially long-term employees, who remember the physical setup of the process and the job description, and so on.

Q. I'm trying to ask a very narrow question and I'd appreciate your best recollection.

The hourly workers who are the subject of the

study who would have had the historical exposure you're trying to evaluate, can you give me any instance where you have attempted to validate historical exposures with those workers who actually received the exposures?

A. We have input from those employees.

Q. How?

A. By talking to them in terms of--for example, we see a job title that's no longer used anymore. Let's say the job title was used in the '50s or '40s even. Nobody remembers what the job title was. We would talk to some old, long-time employees. We even pay some retirees to come back and work with us.

Q. And so I'm clear, are these hourly workers who have remained hourly workers or are they supervisors who at one time might have been hourly workers or might have some knowledge?

A. Both.

Q. And the "we" you use, is that Dr. Otto Wong or is that somebody else?

A. "We" meaning myself, plus the company

industrial hygienist.

Q. And so you yourself have actually sat down with hourly workers and validated historical exposures?

A. I hate to use the word "validate" because we have input from them in the process. As I said, I don't

remember--after we complete our assessment, I don't remember that we have actually gone back to some other employees to validate it independently. I don't think we have done that.

Q. That's the question I'm trying to ask. I do mean to use the word "validate."

"Validate," I think, has the conception with it of something you do after the fact to make sure that your assumptions were correct. Would that be how you would use "validate," or that your information was correct?

A. I guess in a sense we have not gone back to the employees for validation. What I was trying to do is point out that we do have input from them to start with.

Q. Why don't you do that? Why don't you go to employees who are the actual subjects of the exposure categorizations and validate that the exposure category you placed their work in measures up with what they tell you?

A. I think it's a practical consideration. The area that we have concerns about would be exposures a long time ago that we don't have industrial hygiene records, and most people--active people don't know anything about those jobs. So we have to identify some long-term employees who actually were exposed.

There aren't that many people that would fall

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into that category, and they're very difficult to find. So whoever we could contact, we make use of the information in an input process. There aren't that many people that we can go to, that's all. I think that's the major reason why we don't have the luxury of validating it again.

Q. Have you looked?

A. Oh, yes, we did.

Q. You've looked actually and considered validating the historical exposures?

A. Right.

Q. Have you published that?

A. For the API study--the gasoline study publication is coming out under Tom Smith, and I'm one of the coauthors in that publication.

Q. And that publication will address the validation of historical exposures or the inability to do it?

A. As I said, we did not have validation. We have input from long-term employees.

Q. That's what I'm trying to get to, Dr. Wong, is the validation of these historical exposures.

A. Well, I told you we don't have validation and I gave you the reason why we could not have the luxury of validating those data.

Q. And then I asked you, Have you ever documented your inability to validate historical exposures, and you mentioned Dr. Smith's study, which then I think you said does not address that.

So are there any written documents, published or otherwise, that you can direct me to where you have considered the validation of the historical exposures?

A. I don't know whether we have documentation of that or not. I can't give you an answer.

Q. In considering cause and effect relationships, the response which comes from the historical exposure data is very important, is it not?

A. Yes.

Q. Would you agree with me that in exposure assessments historically that is the weakest part of the epidemiological study?

MR. SPEARS: That being what, Herschel?

MR. HOBSON: The analysis of the historical exposures.

A. It's one of the potential limitations. I don't know if I would label it as the weakest part. Weak as compared to what?

Q. (BY MR. HOBSON) Well, death certificates, for instance. They're hard and fast pieces of paper that you can get and they say what they say and that's what you

have to go by, right?

A. Well, yes and no. If you get the right death certificate, yes. It's a piece of paper and you have to go by that. But a lot of times the information on the death certificate may be slightly different from what you have on the employee.

The social security number may not be a perfect match, the birthday may not be a perfect match. So sometimes you have to use your judgment to determine whether that is the right death certificate or not.

So when you get to the subtleties of every study, there are always some limitations.

Q. But the limitations on historical exposures are much greater than deciding whether or not you're going to use a particular death certificate, aren't they?

A. I would say they are equally important. I don't want to give the impression that historical exposure assessment is not important, but at the same time, death certificates are very important too.

Sometimes the difference of two or three deaths due to a specific cause can change the finding of a study. So I would say they're both very important.

Q. When you design an epidemiological study, you come up with a protocol and study design that you're going to follow; is that right?

A. Yes.

Q. Is it important not to go back and amend the protocol after you begin the study and begin to actually analyze data?

A. Unless there are some major unanticipated changes in the data that requires some modification.

Q. What would be some considerations that you have used in the past to modify protocol under those circumstances?

A. If the sample size turned out to be quite different, then you have to modify analyses. I can't think of any instances that I modified a protocol in a major way; not right at this moment.

Q. If exposure categorization is part of the original protocol, do you also define what exposure categorizations are going to be in the protocol initially?

A. I would think so.

Q. Are there times when you change how that protocol for exposure standpoint will be dealt with after you begin to collect data?

A. I don't know how to answer these kind of general questions. If you give me a specific sample, I can probably give you a more specific answer.

Q. I'm trying to find out about your experiences.

How important is it to determine what protocol

will be utilized for analyzing exposure categories in the beginning before data is collected as far as how it

affects the outcome of the study?

A. I think every investigator would try to follow the protocol as closely as possible.

In your protocol you plan certain analyses, but if the numbers turn out to be very small in certain categories, then you may have to lump things together and that may be a deviation from the protocol. It may not address certain questions, but still that may be the only way you can do some analysis based on the data that you actually have.

Q. Will a carefully thought-out protocol consider those possibilities in the beginning to know what lumping together might be necessary if the numbers are small?

A. Sometimes we can anticipate some, I guess, unforeseen changes, but I guess nobody can predict the future.

Q. You can't get it always right the first time, but will you agree with me you can anticipate many of these data collection problems, and those that can be reasonably anticipated should be addressed in the initial protocol?

A. Sometimes, yes.

Q. Are there times when you can reasonably foresee

a problem and you would not address it in the protocol?

A. You mean--you're asking me very general questions. I just don't know how to answer them. Again, if you give me some specific samples, I would be happy to come up with a specific answer.

Q. So that's a question that's too general for you to give me an answer to?

A. It's very broad.

Of course everybody would follow the protocol. Everybody is trying to anticipate any unforeseen changes and try to deal with that, but it's not a black and white kind of decision sometimes.

Q. As a person who's skilled in statistics, what kinds of problems can you get into by changing protocol after you start collecting data?

A. Depends on what kind of changes we are talking about.

Q. Give me some examples that you have in your mind.

A. Well, for example, if you go and do a study and you anticipate a much larger cohort than you actually have, you may have to modify your conclusion because the sample size would not give you as much confidence as you would like. It may not even answer the question that you start with. That's a very common scenario.

You go into industry, you do a study, you thought you would have a large cohort and you end up with a much smaller cohort.

Q. I guess maybe I'm having a difficult time phrasing a question so we can communicate.

What I'm trying to find out about is if we talk about data collection, how can changing the protocol in the data collection process after data collection has begun affect the outcome of a study?

A. Depends on what the change is. I don't know how to answer your question more specifically than this

unless you give me a more specific example.

Q. Okay, let's try a couple, then. Let's talk about in evaluating exposure categories and you begin to gather your death certificate information--assuming we are talking about a mortality study--and partway through the study you change your criteria for exposure categorization.

A. In terms of what? In terms of duration of exposure? In terms of chemicals? In terms of departments, job titles?

Q. All of those?

A. All of those.

Q. Or any one of them.

What kinds of questions does that call to your

mind, then, about any results that you can gain from such a study?

A. Well, I assume that if we modify the definition of exposure--I'm talking about when you go out and

assemble a cohort. Regarding exposure, you have to rely on job titles or departments to determine whether someone was exposed or not. If you modify that, then you better have some reason why you're modifying that.

The previous definition of exposure must be incorrect or incomplete based on incomplete information. So to me if you modify the definition of exposure in terms of job titles or departments, then I would assume you have better information now as opposed to before.

If you modify the duration of exposure--a lot of times we would say we would only study people with at least three months of exposure, six months of exposure or a year of exposure. If you modify that, that may or may not change your study hypothesis at all.

Sometimes you have to relax the requirement for minimum exposure so that you have a larger sample size. Sometimes you have to decrease that to make it smaller to make it more manageable.

Q. Let's take a hypothetical and see if I can work this through in my mind.

Let's say that I'm a large industry and I come

to Dr. Wong to do a study based on a chemical exposure in my industry and I want there to be--this will be a mortality study--a historical perspective mortality study, and it will have, as a component, exposure categories so that we can get a measure of dose response. Does that make sense so far?

A. Yes.

Q. You design a protocol for such a study which includes exposure categorization. Data is begun to be collected. You start collecting the death certificates and you begin to do analyses and you do a preliminary analysis of what data you have and you give it to me because I'm the industry, I'm doing the exposure categorization. Is there a problem with that?

A. Can I interrupt?

Q. Sure.

A. When you say, do a preliminary analysis, do you mean mortality analysis actually with results such as standardized mortality ratios?

Q. Maybe not that sophisticated. Maybe I collect 30 percent, 40 percent of the death certificates you expect to find and you start to see what could be a trend.

A. Okay.

Q. And you come to me, the person who's paying for the study, and say, Well, here's something I see.

Is that a problem? Can it be a problem?

A. I think that's a problem.

Q. Why?

A. I can speak for myself.

Q. Yes.

A. I don't think I would ever want to present preliminary analyses based on 30 percent data to the sponsor.

Q. Why?

A. Number one, I don't think they should be that intimately knowledgeable about some ongoing research in terms of results. They should be aware of the problems and so on, but they should not know preliminary analyses

based on 30 percent of the data.

And besides, I would never analyze a study based on 30 percent data. It's just not--it's not efficient. It doesn't tell you anything, to start with.

Q. I guess there is some level, though, at which you would begin to do some preliminary analysis--some level of data collection.

A. The only scenario I can think of is we have about 90 plus percent of the data. When I say that, mostly I mean death certificates; that we would start running some preliminary analysis, make sure the computer programs work and so on.

Now, that doesn't mean we are not going to modify the data as we get more death certificates coming in during that period of, so-called, preliminary analysis, but I don't think the results would change that much based on some small percentage of additional death certificates.

Q. Would you give that kind of an analysis to the

sponsor?

A. We may present that, yes.

Q. What if then the sponsor said, Well, we want to change the definition of the exposure protocol?

A. I would ask why.

Q. Why would that be of any concern?

A. Certainly you would not change your criteria of exposure or any other classification scheme of your cohorts after you look at the results. That doesn't make a whole lot of sense.

Q. Why?

A. If a study has been going on for six, seven years, the exposure scheme should be worked out completely by then. That really raises some suspicion, why somebody would change the exposure categorization, especially if by doing so you've also changed the result.

Q. Let me come at the problem that I'm trying to get your opinion on a little bit differently.

Let's assume for a moment that I'm going to

hire you to do a study for me--I'm the sponsor--a large industry-specific chemical historical perspective study with exposure categories, and I am interested in whether or not there is a cause and effect relationship between the chemical and a particular disease category. And I know, because I've got a little bit of knowledge about epidemiology, that the more clearcut a dose response is, then the stronger the study results are likely to be. Does that make sense?

A. Right.

Q. And I want you to go out and collect the death certificate information, give me, then, an idea of where these particular kinds of suspicious diseases lie, and I want to be able then to manipulate the exposure data to soften the dose response curves.

How do you guard against that?

MR. SPEARS: As I understand the question, you're asking him in a hypothetical situation that this industry wants to commit a fraud, basically. Is that what you're trying to say, Herschel?

MR. HOBSON: Sure.

MR. SPEARS: How would he respond to that if he knew you were?

MR. HOBSON: No. I'm asking how he, as a scientist who's going to be dealing with companies who

28

might want to commit a fraud--

Q. (BY MR. HOBSON) --how do you design in your protocols ways to prevent that when the fraud is playing with the exposure data, because now the industries typically do provide you with a lot of the exposure information, correct?

A. Yes, but they--let me go back and tell you how we retrieve exposure information from the industry.

Normally we don't get exposure data on a specific individual. We get exposure information on departments, jobs, locations, and also calendar years, so it's like a multidimensional matrix. And we ask them for whatever industrial hygiene data they have. Not estimates; real data for those jobs for the specific time period, and so on.

Then, based on that, you should be the one who knows--we don't, usually. If you go back to the '60s or '50s, you don't have that much data or none at all, so you do have to do some kind of estimates, and that's the part we have to talk to old timers. We talk to the company industrial hygienist and we don't rely on the company alone. We rely on workers as well, long-term employees.

And the estimate--historical estimate's got to be consistent with the real data too that we have in the latter part of the study. And we do all this independent

of the mortality data, and we do all the classification before--even before we analyze the data.

Q. How do you know that the exposure categorization is done independently of the mortality data?

A. As far as we are concerned, it's done independently.

Q. How do you know that?

A. Because we have the mortality data. We never give the mortality data to the companies until the study is done.

Q. But isn't it true, Dr. Wong, that many of these industries that you do work for are the kind we have been describing?

They already have a big piece of the mortality data. They may not have as many death certificates as you do, but they have a lot of them through the benefit plans and they have some other epidemiology studies that have been done internally or through other consultants or studies done earlier. They have a piece of that already, don't they, many times?

A. They have part of the death certificates. Most likely they would have death certificates for their active employees who die on the job and also death certificates for annuitants who are vested in the retirement plan at

least for the latter part of the study.

Q. So while you keep your mortality data from the

sponsor, you know going into it that most of these sponsors, if not--let's say most of the sponsors already have a piece of the mortality information in-house, right?

A. Right.

Q. So that's what I'm asking you now, is how do you guard against that sponsor who wants to, to use Counsel's term, commit a fraud by manipulating the exposure data to make dose responses mushy? What do you do as a researcher to stop that?

A. Again, we would look at the consistency of the data.

Q. But there is no data. You said for most of these exposures there is no exposure data.

A. We have real exposure data for certain parts of the study period.

Q. And usually in the last 15 years?

A. Starting in the '70s, yes, we do have that.

Q. And even the information for exposure in the '70s is somewhat limited in most instances, is it not?

A. We always want to have more data than we actually have, but I don't know whether you'd call it

limited or not.

Q. If you were going to do a statistical analysis

of the exposure data that most of the sponsoring companies have, say, up to 1980, isn't it true that they don't have enough data to do really meaningful statistical interpretations by job for exposure?

MR. SPEARS: Can you--for the record, I want to object unless you identify what companies we are talking about. I think you asked him specifically there.

Q. (BY MR. HOBSON) Pick any of them you would like.

What I'm trying to find out, Dr. Wong--take a pipe fitter at a refinery.

A. Okay.

Q. I will pick Mobil in Beaumont. I don't know if you know anything about that or not. I know you studied that group. But in looking at a pipe fitter at Mobil in Beaumont, Texas for the exposure information for 1977--

A. Right.

Q. --do you think that there is adequate benzene exposure data to define any given pipe fitter's exposure, or even a group of pipe fitters at that refinery?

A. For that specific study, if we are talking about--

MR. SPEARS: Counsel--

Q. (BY MR. HOBSON) Counsel said give you a specific. I got one for you.

32

A. In that study we did not have any quantitative exposure estimates, as you are well aware of. We didn't use that in our study for the reason that we just don't have all the data that we need.

As opposed to--let's take another example. The API gasoline study that I am doing now. In that study API spent a lot of resources. In fact, they contracted the University of Massachusetts to come up with exposure assessment and that's a major component.

Q. How much of that was based on hard data as

opposed to estimates in 1975, '76, '77?

A. I don't recall. I'd have to go back and look at the report.

As I said before, that study was under the direction of Dr. Tom Smith. I work with him. I provide him with employment records and so on, but I'd have to go back and take a look.

Q. Let's talk about the CMA benzene study. That had exposure categories, did it not?

A. Yes, sir.

Q. How much of that information is based on hard data?

A. I recall we have industrial hygiene data in the '70s. We are talking about benzene, and benzene is one of the things that people measure for a long time relative to

other substances.

Q. Tell me how you mean that, as your perception?

A. Starting in the '70s.

Q. That was a long time?

A. Relative to other substances.

And our studies start in the mid '40s. Some company's studies started in the '40s and '50s. So I would say about anywhere between 1/3 to 1/2, depending on which company you're talking about, we have real data.

Q. For some part of the time?

A. Right.

Q. If it came down to a location and a person, how much information on benzene is it your perception existed for the CMA study?

A. The way we estimated exposure was based on jobs.

Q. Right.

A. We divide up the job into a number of what we call uniform tasks.

Q. You and I have had a chance to talk about the study many times, but I am trying to get more specific this time.

Give my your perception, if you would, sir, of how much information on benzene exposure on an individual cohort member for the exposed group in the CMA benzene

study there was.

A. It depends on the period of employment.

For someone who was hired since the '70s, I think we probably have pretty good data on that person, or at least on jobs similar to that person.

Q. That's a different question.

Jobs similar to that person I will ask you about next, but right now I'm talking about the individuals. What is your perception of how much exposure data you had for the individuals in the cohort? And I realize it varied by time, and that's what I'm trying to find out.

A. The way we estimate exposure is based on jobs and not based on specific individuals.

It may happen that some of the data on that specific individual went into the exposure estimates for that general category of jobs. So to that extent, we may have some specific information on that individual, but more likely than not we have information on jobs in

general categories.

Q. Did the information on jobs in the general categories--are those categories across different plants or are they specific for each plant that was studied?

We can talk about the CMA benzene study.

A. It's a little bit of both.

What they do is--"they," meaning the industrial hygienists.

Q. Of the industry?

A. Of the industry--and I would participate in those discussions as well--is to look at how comparable the job titles are across the company and also over time, and we reduce those job titles to some uniform comparable job categories.

Q. What happens to the confidence of what you're trying to learn about exposure as you combine and combine again and then extrapolate across, as you've just described?

A. I mean, in general terms, if we are not confident, we would not use it in our analysis.

Q. But your confidence would go down, right, as you continue to combine and average across?

A. Well, yes and no.

Q. The best of all possible worlds would be to know the exposures of each individual every day they worked, agreed?

A. That's the ideal world that the epidemiologist would like to have, but that's not going to happen.

Q. Doesn't exist?

A. No.

Q. And I guess a fallback to that would be if you

knew one day a week of exposure for every individual that was in the cohort, that would not be as good as knowing every day, but better than what you had, right?

A. No. I don't think a day a week is necessarily the second best. That one day a week may or may not be representative. It may be better to look at the overall

and come up with an average for that category, for that job title.

Q. What about for plant to plant? How does that affect your confidence in exposure categories if you're talking about two different manufacturing facilities where the job exposures are being combined?

A. We would combine them only if they're comparable.

Sometimes the same job title for different companies may involve different job functions and may not be comparable at all.

On the other hand, due to different reasons--employment reasons, union contract reasons--they may call the same job by different names. That's why we need input from the industry, from personnel, from industrial hygienists.

Q. You were, I think, personally involved in the exposure category determination in the CMA benzene study, correct--at least part of it?

A. Yes.

Q. How did you address a question like this, if you did: One plant has benzene workers who wash their hands and wash their clothes with benzene.

What does that do to your consideration of exposure categories in the CMA benzene study?

A. Are you still talking about differences between plants?

Q. However it figures in. I'm not sure that I know how you did consider it and that's what I'm trying to find out.

A. Are we talking about the CMA benzene study?

Q. We are.

A. In that study--I think I mentioned this before--for each job and for each time period we try to divide a job into different components.

In fact, we had a list of over 30, what we call, uniform tasks such as taking samples from the stream, analyzing samples, fixing up a leak, washing tools in solvents, benzene.

In fact, I remember using solvents or benzene

to wash tools as being a specific task, because that is one event that would entail some exposure, so we had that. And we put that into our equation in considering an eight-hour time-weighted average.

For certain jobs we would say, How often--based on talking to employees, how often would the employee wash his tools with benzene or solvents and what kind of exposure would be associated with that, and put that into an eight-hour time-weighted average calculation.

Q. Where would we go now to find which locations told you that their employees washed their tools in benzene in assessing job exposures for the CMA benzene study?

A. That study has been completed several years ago. Many years ago. I don't have the data anymore. I don't know whether CMA still kept the data or not--whether my former employer turned the data back to CMA or not.

So I don't know where the raw data are at this

point or whether they still exist, but I assume you have a copy of my report to CMA--not the publication, but the report itself to CMA--and attached to that report there is an example of our calculation of our time-weighted average, and we have a list of uniform tasks. I think it's in one of the appendix.

Q. Now, let me make sure I understand, Dr. Wong. Did you at one time have the information yourself as to which facility had workers who washed their tools in benzene?

A. Yes.

Q. And where did you get that?

A. From the companies.

There was a committee formed to come up with exposure assessments. In the CMA study we have estimates for every job and estimates for an eight-hour time-weighted average, and also a time for short-term peak exposure, and that was reported to us on a form.

In terms of the 30 or so uniform tasks, for

each task we had the percent of time that job title was spent on, the exposure in terms of ppm associated with that task, and then we calculated the eight-hour time-weighted average for that job title. So at one time we had all that data.

Q. And you no longer have that data?

A. No, I don't.

Q. And then you relied again on the companies to tell you whether or not their employees, by job, by year, actually utilized the different tasks that were on the list, and one of those tasks included washing their tools in benzene?

A. That information was based on industrial hygienist input as well as input from long-term employees.

Q. But it all came from the companies; not from Dr. Wong?

A. From companies. I would have no exposure

information on the employees.

Q. But what I'm trying to find out, sir, is all the information that you had about these exposure categories in creating the job profiles--the job exposure profiles, came from the companies themselves?

A. When you say "companies," you mean company industrial hygienists or you also include employees when you say "company"?

Q. I'm including both.

A. In that case the answer would be yes.

Q. And that was done by year, by job; is that right?

A. It would be more correct to say by calendar period.

We don't consider single years. Only when there are major changes in terms of exposure we would change to a different time period. So you may be--sometimes it can be a couple years. Sometimes it can be 10, 15 years if there were no major changes during that period of time.

Q. So tell me approximately what year these exposure categories were reviewed by the company employees, industrial hygienist employees that were

creating this information for CMA?

A. It would cover the entire study period.

Q. So it began about what year?

A. The observation period started in the mid or late '40s.

Q. I'm sorry. When did the company industrial hygienists and the company employees actually sit down to start working on getting the exposure profiles that were used in the CMA study?

A. I don't recall. That's one of the things that we did early on.

Q. And about what year was that?

A. I just don't recall. I don't remember. Again, it's a long-term study and I just don't recall the exact years.

Q. But it was done toward the beginning of that study?

A. Absolutely, yes.

Q. And so whatever time that was, then as of that date there were individuals within the companies that participated in the CMA benzene study who went back and determined what years it was the practice for various employees at various locations to wash their tools in benzene?

A. Yes, sir.

THE DEPONENT: Can we take a break? Is this a good time?

MR. HOBSON: Absolutely. Anytime is a good time if you want one.

(Break was taken from 11 a.m. to 11:05 a.m.)

Q. (BY MR. HOBSON) Dr. Wong, how can you assure yourself, as a scientist who is going to take this information on exposure categories and be the one who's going to publish based on some of this information, how can you take that information and make sure, as best you can, that it's correct?

A. Well, to some extent we have to use our

judgment. We may not be able to verify on a quantitative basis, but certainly by knowing the job description of certain jobs, certain departments, the process and so on, we have some idea of what kind of exposure you would entail, relatively speaking, to other jobs.

So on the relative scale, we should have certain confidence or comfort in exposure assessment.

Q. Let's take a specific, and, as best you can, give me the most specific exposure from the CMA study for benzene that you can give me.

MR. SPEARS: Are you asking for any particular company or just what he can remember?

Q. (BY MR. HOBSON) If you need to rely on the published studies, that's fine, or whatever else you might have, but, in other words, I don't think you can give me

the exposure of an individual. You might be able to give me the exposure of a craft, maybe you can only give me the exposure of a category of worker, like maintenance, but

can you give me, for instance, like a craft at a plant, what exposure--specific exposure categorization?

A. We had that information.

Q. You had it that specific?

A. Certain job title, certain period of time, yes, we have that.

MR. SPEARS: I think he said he doesn't have that data.

A. We had that information when we did the study, but I don't have the documents themselves now, today.

Q. (BY MR. HOBSON) Can you tell me--for instance, in the CMA benzene study, would the exposure category for a pipe fitter at Mobil have been different than a pipe fitter at Monsanto?

MR. SPEARS: Dr. Wong, don't guess. He's asking you if you can recall this.

MR. HOBSON: No. I'm asking if he had it.

A. We had that information.

Q. (BY MR. HOBSON) Can you tell me if it would have been different?

A. That, I don't know. Whether Mobil's pipe fitter was comparable to Monsanto's pipe fitter?

Q. And even if we are not calling a pipe fitter a pipe fitter at the different plants, I'm talking about the people that did that work.

A. That's right.

Q. So there was a time when you had in your possession a document that you could have looked at where you could have looked up the job of a pipe fitter--whether it was called a pipe fitter or some other name, but you could have looked up that job that was done at Monsanto and found out if that pipe fitter's job for a particular time period included washing tools with benzene, and you could have also gone to the Mobil category of employee listed as pipe fitter work and found out for another time period, or perhaps the same time period, if that exposure included washing tools with benzene?

A. Yes, sir.

Q. And all that information came to you from the employees of the sponsoring companies?

A. Yes, sir.

Q. It's correct, is it not, that you never went back to validate that information?

MR. SPEARS: Let me object to the word "validation" again, except that I think Dr. Wong did say he did talk to some people himself. Did he not already say that?

MR. HOBSON: Let's start with your objection.

Q. (BY MR. HOBSON) Dr. Wong, I want to make sure we are using words that you and I agree their meaning of, so let's start with "validate." Give me a definition of validate that you and I can use.

A. Validate means different things to different people and you may have different levels of validation.

The most complete, comprehensive validation would be--I think would be that you would come up with whatever estimates based on certain criteria, certain models. You use those estimates and compare them to some real data, independent data, data that would not include

it in the input process--in the estimating process. It has to be independent.

That would be the most complete, independent validation. That is the true meaning of the word. But in the real world, sometimes it's very difficult to do that.

Another validation may simply mean that you look at your exposure estimates and see whether they're consistent or not. See whether it makes sense in terms of--not on a quantitative basis, but in terms of jobs, the functions, whether it makes common sense. That's another level of validation. It's not as good as having independent data to do that.

Q. So if we want to talk about validation, we can

talk about two levels of validation. One is a validation based on real, independent data, and then a second, I guess, step down in validation is a comparison of nonindependent, but consistent--looking for consistency in data?

A. Right. And I wouldn't say there are only two ways to do the validation. There are probably many intermediate levels between those two.

Q. So we can communicate, let's stick with those two right now, and if we need others, you tell me that we are off base.

In looking at the exposures--the exposure categories in the CMA benzene study, did you ever do any validation in the sense that you took real, independent data and validated those exposure categories?

A. Not for the entire time period, because that was impossible to do because we simply did not have data for the early part of the study. So there was no data--independent, real data--industrial hygiene measurements, let's say--in the '40s or '50s for us to validate estimates for that time period.

Q. Then tell me what you did in later time periods that would be a real independent validation of the data?

A. If I remember correctly, we have set aside a small set of data that we did not put into the estimate

process so that we can verify our estimate process, but then, that period of time really did not need that much validation because we had real data to start with.

And that's the irony of the whole validation process, because you can only validate your data if you have real data, and if you have real data, your estimates are pretty good.

Q. Let's talk about the real data for a minute. The real data came from the companies too, right? That's the air sampling for employee exposures that the companies did?

A. Mostly air sampling data plus some engineering modification data and so on.

Q. But all that came from the companies themselves?

A. Yes, sir.

Q. When we talk about statistics and data collection for statistical purposes, you recognize that in the sense of doing exposure assessments there is a difference between random sampling and nonrandom sampling,

do you not?

A. Yes.

Q. Tell me why that's important, to know whether you're doing random sampling or nonrandom sampling?

A. Random sampling is a scientific procedure that

would give you a, quote, end quote, representative sample.

A nonrandom sample may not give you a representative picture of whatever you want to look at.

Q. Wouldn't it be true that one of the problems with nonrandom sampling is that you induce bias by the judgment of the person actually collecting the sample?

A. Yeah, that's possible.

Q. So an example of that would be if you have two manufacturing plants where benzene is being made and you have two different industrial hygienists doing exposure assessments at those plants, neither one of with which is doing random sampling.

One industrial hygienist at one plant can say, Well, if I see something that's really obviously wrong,

like benzene pouring out of a pump seal and men working in the area, there is no need to take an air sample because I know it's bad. I'm going to stop the flow of benzene, get the area cleaned up and then take a sample to make sure they really got it cleaned up.

That would be a nonrandom type of exposure analysis, correct?

A. Yes.

Q. And I've got another plant where the philosophy is, Anytime I see a problem, I want to go out and document that problem, but I'm not as concerned about what's left

after I've cleaned up the problem.

If the two exposures occurred identically, you would get two different data, would you not, out of those two sampling strategies?

A. Two different data for what?

Q. You've got two separate plants, two different industrial hygienists deciding what samples are going to

be collected. You have the same kind of leaking pump, same circumstances involved with employees being in the area doing their work.

One industrial hygienist says, I'm not going to take a sample until I have it cleaned up, and the other one says, The only sample I'm going to take is when I have a leak.

That's their sampling strategy. One plant has data that shows low exposure and the other plant has got data that shows high exposure when the same incident occurred, correct?

A. Right.

Q. How do you, as a scientist, correct for that?

A. When we look at data that are distinctly different, then we don't combine them into a group. That's why we need to look at the data, look at the jobs, and see whether they're comparable. Look at the real data, whether we can combine them at all.

Q. But how would you know what's real if it's not

random?

A. If they're very different, that tells us. Although by the description of the job title itself it may mean the same thing, in reality they are not and we would not call them the same job.

Q. But isn't that just exactly the opposite, Dr. Wong?

In other words, if you've got the same pump leaking the same way at two different locations and the only difference is how the person decides to collect their exposure data, you've really got the same job with the same exposure, but yet when you look at the data that goes with them you say, No, these are two different jobs. They're inconsistent because I have high data here and low data here.

So you really can't do that, can you, unless you know the sampling strategies being employed at the different plants?

A. Okay. I think we have a major confusion here.

When you talk about quantitative exposure estimates, all along I have in mind the ultimate goal is

to use that quantitative exposure estimate, regardless of job titles now, but quantitative exposure estimates to do a quantitative exposure analysis based on mortality data.

51

So it really doesn't matter what job titles we are talking about as long as we rely on the exposure estimates--the numerical exposure estimate itself.

Now, your q}es|ion would be appropriate if we look at a certain craft, certain job, and what you're describing to me is maybe at different locations we have different exposure scenarios, and if we lump those jobs--the same jobs, at least in terms of the title in the employment records--lump those jobs together as a group

ftually sat down with

hourly workers and validated historical exposures?

A. I hate to use the word "validate" because we have input from them in the process. As I said, I don't

remember--after we complete our assessment, I don't remember that we have actually gone back to some other employees to validate it independently. I don't think we have done that.

Q. That's the question I'm trying to ask. I do mean to use the word "validate."

"Validate," I think, has the conception with it of something you do after the fact to make sure that your assumptions were correct. Would that be how you would use "validate," or that your information was correct?

A. I guess in a sense we have not gone back to the employees for validation. What I was trying to do is point out that we do have input from them to start with.

Q. Why don't you do that? Why don't you go to employees who are the actual subjects of the exposure categorizations and validate that the exposure category you placed their work in measures up with what they tell you?

A. I think it's a practical consideration. The area that we have concerns about would be exposures a long time ago that we don't have industrial hygiene records,

and most people--active people don't know anything about those jobs. So we have to identify some long-term employees who actually were exposed.

There aren't that many people that would fall

into that category, and they're very difficult to find. So whoever we could contact, we make use of the information in an input process. There aren't that many people that we can go to, that's all. I think that's the major reason why we don't have the luxury of validating it again.

Q. Have you looked?

A. Oh, yes, we did.

Q. You've looked actually and considered validating the historical exposures?

A. Right.

Q. Have you published that?

A. For the API study--the gasoline study publication is coming out under Tom Smith, and I'm one of the coauthors in that publication.

Q. And that publication will address the validation of historical exposures or the inability to do it?

A. As I said, we did not have validation. We have input from long-term employees.

Q. That's what I'm trying to get to, Dr. Wong, is the validation of these historical exposures.

A. Well, I told you we don't have validation and I gave you the reason why we could not have the luxury of validating those data.

Q. And then I asked you, Have you ever documented your inability to validate historical exposures, and you mentioned Dr. Smith's study, which then I think you said does not address that.

So are there any written documents, published or otherwise, that you can direct me to where you have considered the validation of the historical exposures?

A. I don't know whether we have documentation of

that or not. I can't give you an answer.

Q. In considering cause and effect relationships, the response which comes from the historical exposure data is very important, is it not?

A. Yes.

Q. Would you agree with me that in exposure assessments historically that is the weakest part of the epidemiological study?

MR. SPEARS: That being what, Herschel?

MR. HOBSON: The analysis of the historical exposures.

A. It's one of the potential limitations. I don't know if I would label it as the weakest part. Weak as compared to what?

Q. (BY MR. HOBSON) Well, death certificates, for instance. They're hard and fast pieces of paper that you can get and they say what they say and that's what you

have to go by, right?

A. Well, yes and no. If you get the right death

certificate, yes. It's a piece of paper and you have to go by that. But a lot of times the information on the death certificate may be slightly different from what you have on the employee.

The social security number may not be a perfect match, the birthday may not be a perfect match. So sometimes you have to use your judgment to determine whether that is the right death certificate or not.

So when you get to the subtleties of every study, there are always some limitations.

Q. But the limitations on historical exposures are much greater than deciding whether or not you're going to use a particular death certificate, aren't they?

A. I would say they are equally important. I don't want to give the impression that historical exposure assessment is not important, but at the same time, death certificates are very important too.

Sometimes the difference of two or three deaths due to a specific cause can change the finding of a study. So I would say they're both very important.

Q. When you design an epidemiological study, you

come up with a protocol and study design that you're going to follow; is that right?

A. Yes.

Q. Is it important not to go back and amend the protocol after you begin the study and begin to actually analyze data?

A. Unless there are some major unanticipated changes in the data that requires some modification.

Q. What would be some considerations that you have used in the past to modify protocol under those circumstances?

A. If the sample size turned out to be quite different, then you have to modify analyses. I can't think of any instances that I modified a protocol in a major way; not right at this moment.

Q. If exposure categorization is part of the original protocol, do you also define what exposure categorizations are going to be in the protocol initially?

A. I would think so.

Q. Are there times when you change how that protocol for exposure standpoint will be dealt with after you begin to collect data?

A. I don't know how to answer these kind of general questions. If you give me a specific sample, I can probably give you a more specific answer.

Q. I'm trying to find out about your experiences.

How important is it to determine what protocol

will be utilized for analyzing exposure categories in the beginning before data is collected as far as how it affects the outcome of the study?

A. I think every investigator would try to follow the protocol as closely as possible.

In your protocol you plan certain analyses, but if the numbers turn out to be very small in certain categories, then you may have to lump things together and that may be a deviation from the protocol. It may not address certain questions, but still that may be the only

way you can do some analysis based on the data that you actually have.

Q. Will a carefully thought-out protocol consider those possibilities in the beginning to know what lumping together might be necessary if the numbers are small?

A. Sometimes we can anticipate some, I guess, unforeseen changes, but I guess nobody can predict the future.

Q. You can't get it always right the first time, but will you agree with me you can anticipate many of these data collection problems, and those that can be reasonably anticipated should be addressed in the initial protocol?

A. Sometimes, yes.

Q. Are there times when you can reasonably foresee

a problem and you would not address it in the protocol?

A. You mean--you're asking me very general questions. I just don't know how to answer them. Again, if you give me some specific samples, I would be happy to

come up with a specific answer.

Q. So that's a question that's too general for you to give me an answer to?

A. It's very broad.

Of course everybody would follow the protocol. Everybody is trying to anticipate any unforeseen changes and try to deal with that, but it's not a black and white kind of decision sometimes.

Q. As a person who's skilled in statistics, what kinds of problems can you get into by changing protocol after you start collecting data?

A. Depends on what kind of changes we are talking about.

Q. Give me some examples that you have in your mind.

A. Well, for example, if you go and do a study and you anticipate a much larger cohort than you actually have, you may have to modify your conclusion because the sample size would not give you as much confidence as you would like. It may not even answer the question that you start with. That's a very common scenario.

You go into industry, you do a study, you thought you would have a large cohort and you end up with a much smaller cohort.

Q. I guess maybe I'm having a difficult time phrasing a question so we can communicate.

What I'm trying to find out about is if we talk about data collection, how can changing the protocol in the data collection process after data collection has begun affect the outcome of a study?

A. Depends on what the change is. I don't know how to answer your question more specifically than this unless you give me a more specific example.

Q. Okay, let's try a couple, then. Let's talk about in evaluating exposure categories and you begin to gather your death certificate information--assuming we are talking about a mortality study--and partway through the study you change your criteria for exposure categorization.

A. In terms of what? In terms of duration of

exposure? In terms of chemicals? In terms of departments, job titles?

Q. All of those?

A. All of those.

Q. Or any one of them.

What kinds of questions does that call to your

mind, then, about any results that you can gain from such a study?

A. Well, I assume that if we modify the definition of exposure--I'm talking about when you go out and assemble a cohort. Regarding exposure, you have to rely on job titles or departments to determine whether someone was exposed or not. If you modify that, then you better have some reason why you're modifying that.

The previous definition of exposure must be incorrect or incomplete based on incomplete information. So to me if you modify the definition of exposure in terms of job titles or departments, then I would assume you have

better information now as opposed to before.

If you modify the duration of exposure--a lot of times we would say we would only study people with at least three months of exposure, six months of exposure or a year of exposure. If you modify that, that may or may not change your study hypothesis at all.

Sometimes you have to relax the requirement for minimum exposure so that you have a larger sample size. Sometimes you have to decrease that to make it smaller to make it more manageable.

Q. Let's take a hypothetical and see if I can work this through in my mind.

Let's say that I'm a large industry and I come

to Dr. Wong to do a study based on a chemical exposure in my industry and I want there to be--this will be a mortality study--a historical perspective mortality study, and it will have, as a component, exposure categories so that we can get a measure of dose response. Does that make sense so far?

A. Yes.

Q. You design a protocol for such a study which includes exposure categorization. Data is begun to be collected. You start collecting the death certificates and you begin to do analyses and you do a preliminary analysis of what data you have and you give it to me because I'm the industry, I'm doing the exposure categorization. Is there a problem with that?

A. Can I interrupt?

Q. Sure.

A. When you say, do a preliminary analysis, do you mean mortality analysis actually with results such as standardized mortality ratios?

Q. Maybe not that sophisticated. Maybe I collect 30 percent, 40 percent of the death certificates you expect to find and you start to see what could be a trend.

A. Okay.

Q. And you come to me, the person who's paying for the study, and say, Well, here's something I see.

Is that a problem? Can it be a problem?

A. I think that's a problem.

Q. Why?

A. I can speak for myself.

Q. Yes.

A. I don't think I would ever want to present preliminary analyses based on 30 percent data to the sponsor.

Q. Why?

A. Number one, I don't think they should be that intimately knowledgeable about some ongoing research in terms of results. They should be aware of the problems and so on, but they should not know preliminary analyses based on 30 percent of the data.

And besides, I would never analyze a study based on 30 percent data. It's just not--it's not efficient. It doesn't tell you anything, to start with.

Q. I guess there is some level, though, at which you would begin to do some preliminary analysis--some level of data collection.

A. The only scenario I can think of is we have

about 90 plus percent of the data. When I say that, mostly I mean death certificates; that we would start running some preliminary analysis, make sure the computer programs work and so on.

Now, that doesn't mean we are not going to modify the data as we get more death certificates coming in during that period of, so-called, preliminary analysis, but I don't think the results would change that much based on some small percentage of additional death certificates.

Q. Would you give that kind of an analysis to the sponsor?

A. We may present that, yes.

Q. What if then the sponsor said, Well, we want to change the definition of the exposure protocol?

A. I would ask why.

Q. Why would that be of any concern?

A. Certainly you would not change your criteria of exposure or any other classification scheme of your

cohorts after you look at the results. That doesn't make a whole lot of sense.

Q. Why?

A. If a study has been going on for six, seven years, the exposure scheme should be worked out completely by then. That really raises some suspicion, why somebody would change the exposure categorization, especially if by doing so you've also changed the result.

Q. Let me come at the problem that I'm trying to get your opinion on a little bit differently.

Let's assume for a moment that I'm going to

hire you to do a study for me--I'm the sponsor--a large industry-specific chemical historical perspective study with exposure categories, and I am interested in whether or not there is a cause and effect relationship between the chemical and a particular disease category. And I know, because I've got a little bit of knowledge about epidemiology, that the more clearcut a dose response is, then the stronger the study results are likely to be.

Does that make sense?

A. Right.

Q. And I want you to go out and collect the death certificate information, give me, then, an idea of where these particular kinds of suspicious diseases lie, and I want to be able then to manipulate the exposure data to soften the dose response curves.

How do you guard against that?

MR. SPEARS: As I understand the question, you're asking him in a hypothetical situation that this industry wants to commit a fraud, basically. Is that what you're trying to say, Herschel?

MR. HOBSON: Sure.

MR. SPEARS: How would he respond to that if he knew you were?

MR. HOBSON: No. I'm asking how he, as a scientist who's going to be dealing with companies who

might want to commit a fraud--

Q. (BY MR. HOBSON) --how do you design in your protocols ways to prevent that when the fraud is playing with the exposure data, because now the industries typically do provide you with a lot of the exposure information, correct?

A. Yes, but they--let me go back and tell you how we retrieve exposure information from the industry.

Normally we don't get exposure data on a specific individual. We get exposure information on departments, jobs, locations, and also calendar years, so it's like a multidimensional matrix. And we ask them for whatever industrial hygiene data they have. Not estimates; real data for those jobs for the specific time period, and so on.

Then, based on that, you should be the one who knows--we don't, usually. If you go back to the '60s or '50s, you don't have that much data or none at all, so you do have to do some kind of estimates, and that's the part we have to talk to old timers. We talk to the company industrial hygienist and we don't rely on the company alone. We rely on workers as well, long-term employees.

And the estimate--historical estimate's got to

be consistent with the real data too that we have in the latter part of the study. And we do all this independent

of the mortality data, and we do all the classification before--even before we analyze the data.

Q. How do you know that the exposure categorization is done independently of the mortality data?

A. As far as we are concerned, it's done independently.

Q. How do you know that?

A. Because we have the mortality data. We never give the mortality data to the companies until the study is done.

Q. But isn't it true, Dr. Wong, that many of these industries that you do work for are the kind we have been describing?

They already have a big piece of the mortality data. They may not have as many death certificates as you

do, but they have a lot of them through the benefit plans and they have some other epidemiology studies that have been done internally or through other consultants or studies done earlier. They have a piece of that already, don't they, many times?

A. They have part of the death certificates. Most likely they would have death certificates for their active employees who die on the job and also death certificates for annuitants who are vested in the retirement plan at

least for the latter part of the study.

Q. So while you keep your mortality data from the sponsor, you know going into it that most of these sponsors, if not--let's say most of the sponsors already have a piece of the mortality information in-house, right?

A. Right.

Q. So that's what I'm asking you now, is how do you guard against that sponsor who wants to, to use Counsel's term, commit a fraud by manipulating the exposure data to make dose responses mushy? What do you

do as a researcher to stop that?

A. Again, we would look at the consistency of the data.

Q. But there is no data. You said for most of these exposures there is no exposure data.

A. We have real exposure data for certain parts of the study period.

Q. And usually in the last 15 years?

A. Starting in the '70s, yes, we do have that.

Q. And even the information for exposure in the '70s is somewhat limited in most instances, is it not?

A. We always want to have more data than we actually have, but I don't know whether you'd call it limited or not.

Q. If you were going to do a statistical analysis

of the exposure data that most of the sponsoring companies have, say, up to 1980, isn't it true that they don't have enough data to do really meaningful statistical

interpretations by job for exposure?

MR. SPEARS: Can you--for the record, I want to object unless you identify what companies we are talking about. I think you asked him specifically there.

Q. (BY MR. HOBSON) Pick any of them you would like.

What I'm trying to find out, Dr. Wong--take a pipe fitter at a refinery.

A. Okay.

Q. I will pick Mobil in Beaumont. I don't know if you know anything about that or not. I know you studied that group. But in looking at a pipe fitter at Mobil in Beaumont, Texas for the exposure information for 1977--

A. Right.

Q. --do you think that there is adequate benzene exposure data to define any given pipe fitter's exposure, or even a group of pipe fitters at that refinery?

A. For that specific study, if we are talking about--

MR. SPEARS: Counsel--

Q. (BY MR. HOBSON) Counsel said give you a specific. I got one for you.

A. In that study we did not have any quantitative exposure estimates, as you are well aware of. We didn't use that in our study for the reason that we just don't have all the data that we need.

As opposed to--let's take another example. The API gasoline study that I am doing now. In that study API spent a lot of resources. In fact, they contracted the University of Massachusetts to come up with exposure assessment and that's a major component.

Q. How much of that was based on hard data as opposed to estimates in 1975, '76, '77?

A. I don't recall. I'd have to go back and look at the report.

As I said before, that study was under the direction of Dr. Tom Smith. I work with him. I provide him with employment records and so on, but I'd have to go back and take a look.

Q. Let's talk about the CMA benzene study. That

had exposure categories, did it not?

A. Yes, sir.

Q. How much of that information is based on hard data?

A. I recall we have industrial hygiene data in the '70s. We are talking about benzene, and benzene is one of the things that people measure for a long time relative to

other substances.

Q. Tell me how you mean that, as your perception?

A. Starting in the '70s.

Q. That was a long time?

A. Relative to other substances.

And our studies start in the mid '40s. Some company's studies started in the '40s and '50s. So I would say about anywhere between 1/3 to 1/2, depending on which company you're talking about, we have real data.

Q. For some part of the time?

A. Right.

Q. If it came down to a location and a person, how

much information on benzene is it your perception existed for the CMA study?

A. The way we estimated exposure was based on jobs.

Q. Right.

A. We divide up the job into a number of what we call uniform tasks.

Q. You and I have had a chance to talk about the study many times, but I am trying to get more specific this time.

Give my your perception, if you would, sir, of how much information on benzene exposure on an individual cohort member for the exposed group in the CMA benzene

study there was.

A. It depends on the period of employment.

For someone who was hired since the '70s, I think we probably have pretty good data on that person, or at least on jobs similar to that person.

Q. That's a different question.

Jobs similar to that person I will ask you about next, but right now I'm talking about the individuals. What is your perception of how much exposure data you had for the individuals in the cohort? And I realize it varied by time, and that's what I'm trying to find out.

A. The way we estimate exposure is based on jobs and not based on specific individuals.

It may happen that some of the data on that specific individual went into the exposure estimates for that general category of jobs. So to that extent, we may have some specific information on that individual, but more likely than not we have information on jobs in general categories.

Q. Did the information on jobs in the general categories--are those categories across different plants or are they specific for each plant that was studied?

We can talk about the CMA benzene study.

A. It's a little bit of both.

What they do is--"they," meaning the industrial hygienists.

Q. Of the industry?

A. Of the industry--and I would participate in those discussions as well--is to look at how comparable the job titles are across the company and also over time, and we reduce those job titles to some uniform comparable job categories.

Q. What happens to the confidence of what you're trying to learn about exposure as you combine and combine again and then extrapolate across, as you've just described?

A. I mean, in general terms, if we are not confident, we would not use it in our analysis.

Q. But your confidence would go down, right, as you continue to combine and average across?

A. Well, yes and no.

Q. The best of all possible worlds would be to know the exposures of each individual every day they worked, agreed?

A. That's the ideal world that the epidemiologist would like to have, but that's not going to happen.

Q. Doesn't exist?

A. No.

Q. And I guess a fallback to that would be if you

knew one day a week of exposure for every individual that was in the cohort, that would not be as good as knowing every day, but better than what you had, right?

A. No. I don't think a day a week is necessarily the second best. That one day a week may or may not be representative. It may be better to look at the overall and come up with an average for that category, for that job title.

Q. What about for plant to plant? How does that affect your confidence in exposure categories if you're talking about two different manufacturing facilities where the job exposures are being combined?

A. We would combine them only if they're comparable.

Sometimes the same job title for different companies may involve different job functions and may not be comparable at all.

On the other hand, due to different reasons--employment reasons, union contract reasons--they may call the same job by different names. That's why we need input from the industry, from personnel, from industrial hygienists.

Q. You were, I think, personally involved in the exposure category determination in the CMA benzene study, correct--at least part of it?

A. Yes.

Q. How did you address a question like this, if you did: One plant has benzene workers who wash their hands and wash their clothes with benzene.

What does that do to your consideration of exposure categories in the CMA benzene study?

A. Are you still talking about differences between

plants?

Q. However it figures in. I'm not sure that I know how you did consider it and that's what I'm trying to find out.

A. Are we talking about the CMA benzene study?

Q. We are.

A. In that study--I think I mentioned this before--for each job and for each time period we try to divide a job into different components.

In fact, we had a list of over 30, what we call, uniform tasks such as taking samples from the stream, analyzing samples, fixing up a leak, washing tools in solvents, benzene.

In fact, I remember using solvents or benzene to wash tools as being a specific task, because that is one event that would entail some exposure, so we had that. And we put that into our equation in considering an

andpoint.

A. That's the kind of detail I was not involved in. I don't know whether they regard it the same or not. "They" meaning the industrial hygienists.

Q. (BY MR. HOBSON) Coming back to where I started

off, I think, at least somewhat, Dr. Wong--how could you as the outside scientist involved in the project assure yourself that these industrial hygiene employees, the employees of the companies sponsoring the studies, didn't obscure or alter exposure patterns by the way they did their work?

A. The only thing we can do is to check for internal consistency, see whether there are any big gaps or any changes, jumps in the data, gaps in the data, that are not explainable. We have to start from somewhere in order to do occupational epidemiology.

Q. And all of those things that you just told me that you would look at, they still go back to the company

industrial hygienist or company engineers or the company supervisors, don't they?

A. Yes.

Q. And I guess I would come back, then, to ask you, to your knowledge, no one who was not an employee of

one of the sponsoring companies would take one of these job profiles of the uniform tasks and independently go to the union or go to a union member or an hourly worker and say, Here's what I understand your job to be or to have been. What is your input?

You've never attempted, and, to your knowledge, no one else ever attempted that kind of a validation with real independent data for the job tasks?

A. I didn't do that in the CMA benzene study, but something similar to that was done in the API gasoline study. I believe Dr. Tom Smith did go back and talk to some long-term employees in terms of his exposure estimates.

Q. But in the CMA study, to your knowledge, no one went back and talked to the workers themselves with the uniform task profile for their job and said, Does this match up with your recollection, correct?

A. We had input from the long-term employees in the process itself, but I don't remember and I don't know for sure whether the company industrial hygienist went

back and talked to some other long-term employees again after we ccme up with the estimates.

Q. But if I'm a company and I don't want to play fair and I want to cheat on the data, I can do it then that way, can't I? I can shade the uniform task. I can go to people that I either expect to get a certain answer from or I can take their answer and I can alter it and I can give you incorrect information about exposures and no one would be the wiser, would they?

A. Theoretically they can try, but remember too they do not have the mortality data. We kept that separate.

Q. But didn't you tell me earlier that the companies already have a large portion of the mortality data because they have the death certificates on active employees and annuitants?

A. I didn't say "large." I said they have death certificates for active employees and annuitants, but I don't know--I would not think that's a large portion of a cohort study over a period of 40 some years.

Q. What percentage would you say that would typically make up, the total cohort?

A. The death certificates--the proportion of death certificates that we are taking from companies would be very small.

What we do is when we do our follow-up, so to speak, vital statistic determination--the first thing we do is ask the companies, Whatever death certificates you have on the employees, give them to us so that we don't have to repeat the process. And based on my experience, that percentage is very low.

Q. The number?

A. A don't have a number.

Q. 10 percent, 15 percent?

A. Probably in that range. 10, 15 percent.

Q. But now I wouldn't have to know the mortality information to be able to confuse exposure, would I?

MR. SPEARS: Object to the form of the question.

You ask an obvious question, Herschel. If someone wants to cheat, they can cheat. That's obvious, I guess. If somebody wants to give you false information, the answer is, yes, they can give you false information, but that's an obvious answer, if that's what you're asking him. Is it possible that someone can cheat on giving information?

Q. (BY MR. HOBSON) Well, I guess that's the beginning point, sure, but what I guess I'm asking, Dr. Wong, is isn't it true that you don't really need to know the mortality information to confuse those responses

if you want to?

In other words, if I'm sitting here and I want to create havoc in the interpretation of an epidemiological study and I know that one major component of assessing cause and effect is dose response, really all I have to do is confuse dose. I don't have to know anything about the response to affect the outcome, do I?

A. No. I think if what you're implying is that you can introduce some random error into the exposure estimate and then the whole thing would disappear--the dose response--that may or may not be so.

If you really want to cheat, I think you have to underestimate the exposure of people who die from a certain disease and overestimate people who didn't. So you really have to know some mortality data before you can be successful in confusing the picture.

Q. But really, it's not random error I would want to introduce, is it? Isn't it nonrandom error I would want to introduce, because random error would tend to balance out, would it not?

A. That's exactly my point. So you do have to know the health outcome of the people you want to change the exposure.

Q. Isn't it really true that all you need to know is what the true exposure picture is?

A. I think you're confusing me now.

You're talking about how to cheat, and I gave you one scenario. If you really want to cheat, the only way you can do that is by overestimating noncases--people didn't die from a certain disease--and underestimate exposure of those people who die from that disease. That is the only way to get rid of the dose response relationship.

Q. Or make it so soft and wishy-washy that it can't be interpreted?

A. Well, there is always the scenario in between, but I'm giving you the specific example.

Q. You've told me that you don't think it's possible to know the outcome in advance because there is not enough of the mortality information available to the companies, and I'm saying, okay, let's talk about that intermediate step where you just make the exposure data so mushy that dose response can't be interpreted. That can be done, can it not?

A. It's a very broad generalization. Anything is possible.

Q. But wouldn't it be fairly simple to take some

of the exposure profiles and go back and validate those after the fact with the real independent data? Just go talk to the workers themselves and say, We have for this

job, this job and this job that you didn't wash your tools with benzene, and we have for this job, this job and this job that you did.

A. I think we are going back into this question over and over again, and let me make it clear that we have input from long-term employees, and I mentioned to you earlier this morning that it's very difficult to identify those employees. And basically we use all of them that we can identify in the input process and we really have no one else to go to for validation.

You're talking about validating exposures in the 30's, '40s and '50s. There aren't that many people around that a company knows of still here for us to talk to.

Q. Let me make sure I understand. You say that some of the long-term employees were questioned and I

thought you told me earlier that it's your belief that those were supervisory people?

A. No.

MR. SPEARS: Object to to that.

Q. (BY MR. HOBSON) What is your understanding, then?

A. He can be a pipe fitter and retires as a pipe fitter.

Q. Tell me if you know that was done.

A. I told you that was done.

Q. You're telling me that you know in the CMA benzene study that long-term pipe fitters who were not part of management were requested to validate the exposure of profiles from a uniform task standpoint?

A. Herschel, we are getting nowhere with this.

I did say that long-term employees, nonsupervisors, nonmanagement, but again, I said we did not validate because we didn't know that many long-term

employees to start with.

So the small number that we could identify, we used their information in the input process and there was no one left for us to validate.

MR. SPEARS: I think he's describing a form of validation, Herschel. I guess it's the question, do you think it's valid in your definition?

MR. HOBSON: I think I understand what you're saying now, which is different. My understanding is different than it was earlier.

MR. SPEARS: The record will reflect it.

MR. HOBSON: Obviously.

Q. (BY MR. HOBSON) I'm not saying that you didn't say that earlier, Dr. Wong, I'm saying I didn't appreciate what you said earlier.

I take it that there are from time to time

conflicts in people's memory as to what particular tasks are incorporated into a given job. Would that be fair?

A. I'm sure that happened from time to time.

Q. And so, again, that's where I come back to the needing to go out and after the fact validate this information that if you're--because what I think you told me is at the beginning of the study when you were considering setting up the exposure had classification and finding what uniform tasks were involved in which job, that's when these long-term employees were sought out and questioned and their input sought. Am I right?

A. Yes, sir.

Q. And so they may have said, Well, yes, these crafts or these jobs or these activities, however the proper wordage was, did or did not involve washing your tools with benzene. And then other people say, Wait a minute. I have a different memory. I don't think it did or I do think it included these tasks.

And you can't have it both ways, right? Either that task of washing your tools with benzene is included or it's not and someone has to make a decision.

A. That's right.

Q. And so after that decision is made, that's what you stick with in categorizing the exposures of the

different jobs?

A. Yes.

Q. And you're telling me, then, that because the companies included everyone who was possible to include, the long-term employees that were actually there on the job, there was no way to go out and do a real independent data validation. Have I got your testimony correct now?

A. In the CMA benzene study?

Q. Yes, sir. We have been talking about the CMA benzene study.

MR. SPEARS: And he would say yes, if you are asking him did he go out and validate it to each and every person. I guess you're asking him, As to every person who was studied, subject to that study were they questioned independently by Dr. Wong?

MR. HOBSON: No. Not at all. I'm sure a lot of the people in that study were dead.

MR. SPEARS: But he had to rely on information given to him by long-term employees who based it on their

personal knowledge. So that's validation.

MR. HOBSON: I think we have been through what's validation and what's not. I don't want to beat that to death again.

Q. (BY MR. HOBSON) But it is correct, Dr. Wong, that the information you had for the exposure categories is all information that came through these

company-sponsored employees, industrial hygienists that were on the committee? All that information filtered through those individuals on the committee, did it not?

A. Yes, sir.

Q. And so you, Dr. Otto Wong, or no one who was independent of the corporation took this information from the long-term employees when it was gathered. You relied on the company industrial hygienist and the others to gather that information for you. Is that right or wrong?

A. Yes, sir, that's correct.

Q. And so trying to move on, because I think we

have got it now, there was no one outside the company people, either at your direction or somebody else's direction--some other consultant to the project that was not a company sponsored employee--who validated the exposure tasks for each of the jobs. Would that be correct?

A. That's correct.

Q. And the reason all this becomes important, Dr. Wong, is that I want to make sure that if I represent someone who was one of of these people who did a job like the jobs that are in your CMA study, and they're alive and they were there in the '40s and '50s and '60s and they come in and say, Nobody ever asked me about any of these uniform tasks and what a pipe fitter did or no one ever

asked me about these uniform tasks and what a boiler maker did or any other job that comes along, you have no way of telling me that that's so or not?

A. That's correct.

MR. HOBSON: Off the record.

(Break was taken from 12:20 p.m. to 12:35 p.m.)

Q. (BY MR. HOBSON) Dr. Wong, when you do epidemiological studies for industry, do you consider the fact that there might be some in industries who would want to shade the truth?

A. Well, it depends on what kind of data we're talking about.

To the extent possible, we always try to have some measure to assess that possibility, either knowingly or unknowingly, whether we have complete records or not.

Q. You know that with many of the studies that you're involved with, depending on the outcome, millions of dollars can change hands, whether it's litigation or regulation. Your studies are done for a purpose and those purposes involve a lot of money at risk. You realize that, don't you?

A. I'm aware of that.

Q. Tell me some of the things that you do in your epidemiological study protocols that are designed into them to make sure that your clients doen't shade the

truth.

A. Well, one of the major considerations we have is how complete the cohort is. If we go out and assemble a cohort based on the employment records made available to us, we want to know that we have all the records. That is something we call a cohort verification, using independent documents to verify whether we have a complete cohort or not.

Q. How do you do that?

A. There are different ways of doing that. It depends on what kind of independent documents we have.

A lot of times we go to the Social Security Administration and get copies of 941 forms, quarterly report earnings, so that we know we have the names of all the individuals who worked there, and compare it to what we have.

Q. And how far back in time are 941s kept by the Social Security Administration?

A. It depends on how the company is structured. It can go back all the way to the beginning of our study,

which sometimes can be the '40s, depending on the employer identification number, which number they use, whether they use one number for all the divisions in a single state or what.

If that was the case, it would be very

difficult to use that. If they use one single number for a specific operation it is much easier to get information from the Social Security Administration.

Q. What other things are built into your study protocols to keep companies from shading the truth?

A. In addition to social security 941 forms we also request, to the extent possible, union seniority lists as another data source for verification.

Q. And this all goes to identifying the cohort and how complete it is?

A. Yes, sir.

Q. What else is built into your studies to keep companies from shading the truth? What other areas

besides just cohort identification?

A. Well, there are two types of data that a company provides to us in general. One would be employment records, and that's the one we just talked about, how to verify the completeness of the data they provide to us in terms of employment records.

Another set of data or another set of information that we rely on the company for is industrial hygiene or exposure data and we discussed in length this morning how we verify that.

Q. Anything else that comes to mind about what you could put in your protocols to keep a company from shading

their data?

A. That would be the two major sources of data that we have to rely on the companies.

In terms of mortality data, we don't rely on companies that much. Whatever death certificates they provide to us is fine, but then we would send the rest of the cohort to Social Security Administration, the National

Death Index, all those external sources.

Q. And can you think of any other ways than what we have talked about this morning already that you can keep the companies from shading the truth about the exposure part of the information you use in your studies?

A. The only thing I can think of is going again back to exposure data. After we have exposure estimates we look at internal consistency and at the same time also look at the literature--what has been published for similar populations. And if our estimates are very different from theirs, different from what has been reported in the literature, then again that would raise some questions.

Q. And is the comparison with the consistency of the data, is that done by the industrial hygienist on the committee or is that something that you in your research activities do independently of the committee?

A. We would do that. When we look at the exposure

estimates, let's say for a certain job over the years, we would expect some kind of continuum instead of some gaps or jumps up and down.

If we do see some unexpected pattern of exposure, then we would go back and talk to them and ask them to explain to us why that was the case.

Q. And the "we" you use is someone other than the company industrial hygienist on this committee?

A. "We" meaning myself and--myself.

Q. And your team?

A. Meaning the people that work for me, not employed by the companies.

Q. So then when you do these studies you--independently of the companies that are sponsoring the research, you get all their exposure data; is that right?

A. That's the assumption. We get all the data, yes.

Q. You did an epidemiological study for Mobil in Beaumont?

A. Yes.

Q. Did you, independently of the company

personnel, get all their exposure data to benzene that they had?

A. That's a bad example because we didn't do any

exposure quantification in that study.

Q. Well, you studied the Mobil chemical benzene workers in part of the CMA study, did you not?

A. Right. I'm confused. I thought you were referring to the Beaumont refinery study we did for Mobil itself. Are you referring to the Mobil plant in the CMA study?

Q. Yes.

A. Okay. And the question was?

Q. Did you get Mobil's benzene monitoring data for the CMA benzene study independent of the company personnel?

A. No. The exposure data were provided to us through that uniform task assessment process.

Q. I misunderstood you and I was trying to find

out if you, as the independent consultant--

A. Right.

Q. --if you actually got the exposure data so that you could independently look for these consistencies between the periods of time when you had no data to when you had data to see that there were no gaps, that that was the consistency you would expect.

A. We checked that mainly through looking at the computerized data we have subsequent to the exposure assessment.

In other words, based on the committee's assessment we put in the data in our computer data base and we would call up a certain job and look at the exposure pattern over the years.

And if--as I said, if there were some gaps or jumps, then we would go back and say, Explain to us why that was the case.

Q. But as far as getting industrial hygiene monitoring data, if there was any from Mobil in 1967 or

1968, you independently never got that air sampling data and then went to the exposure categories to see if there was consistency?

A. No, we did not look at that. We might have the data in terms of data entry, but we did not make that assessment.

Q. You're familiar with the term "statistical significance" as used in epidemiological studies?

A. I would think so.

Q. Would you agree with me that the statistical significance level that is used by epidemiologists is an arbitrary number?

A. It's arbitrary, but it's also a convention.

Q. Will you agree with me it's a convention that not everyone uses in epidemiology?

A. I can't speak for everyone. All I can say is

it's a common practice.

Q. Could the statistical significance be

calculated at this 51 percent level?

A. Yes, but it's not very meaningful.

Q. Well, it certainly would answer the question as to whether or not something was more likely than not to occur, wouldn't it?

A. No. These are two different concepts, statistical significance and probability of causation or more likely than not. They are two independent concepts.

Q. Maybe we--

A. I have yet to meet a lawyer who is not confused with this.

Q. I don't think I intended to ask that question. Maybe I asked it, but I didn't intend to.

A statistical significance level of .49 would tell us that an event was more than 50 percent likely to be real than chance, would it not?

A. The 49 percent refers to the probability that we are willing to accept a false negative.

Q. Yes.

A. But that is not the same as, given that someone has a disease, what is the likelihood of that person getting the disease through certain exposure?

25 Q. I agree. Two different questions.

1 A. Very good.

2 Q. But you will agree with me that a statistical

3 significance level of .49 does have to do with whether or

4 not the finding, whatever it is that the study is looking

5 at, occurred from chance alone or not?

6 A. Yes.

7 Q. And you could easily calculate the 49 percent

8 level with your statistics, could you not?

9 A. Yes.

10 Q. Have you ever had anyone that you've contracted

11 to do a study with instruct you not to have contact with

12 the study members in follow-up?

13 A. I'm not so sure I understand your question.

14 Q. In doing a study for mortality, for instance,

15 in determining vital status, have you ever had any of your

16 clients instruct you or contract with you not to contact

17 the families direct to determine vital status?

A. We don't go to the family to determine whether somebody is alive or not. It would be just too expensive to do that on a cohort of tens of thousands of people.

We use a centralized data base such as the Social Security Administration, the National Death Index.

Q. But even after you go through those organizations you still end up with some percentage that are at that point lost to follow-up?

A. Right.

Q. And what I'm asking you, then, is for that percentage that is lost to follow-up, you want the number to be as low as possible?

A. Yes, sir.

Q. Has anyone ever contracted with you specifically that you will not go to family members in pursuit of the vital status of these people that have been lost to follow-up?

A. We would not propose that either simply because I don't even know where the family is for those cases.

You don't have an address. You don't have a phone number to call. Those are the difficult ones to locate.

In order to reduce that number of lost to follow-up, what we do is we usually go to some kind of commercial credit bureau to determine whether they have information on those people in terms of applications for a loan or some credit history and so on.

Sometimes we also go to--if we know the state where they lived last, we go to those states and see whether they still have a driver's license, that kind of thing, but we don't contact families. It's too expensive. It's not very promising anyway.

Q. Yes, sir. I understand you're telling me about the different indirect methods you can use to determine

vital status of people in follow-up with a cohort, but what I'm trying to find out is if you can remember if anyone has contracted with you to prevent you from making direct contacts in determining vital status?

A. No, because that's never in a proposal. That's never in the protocol to do that.

Q. Would you know if anyone who has contracted you to do a study has intended to keep that study secret from the subjects of the study?

A. That, I have no idea.

Q. As far as you know, there has never been expressed to you an intent by the sponsor to keep the study a secret from the subjects. Would that be accurate?

A. That would be correct.

Q. Have you ever, that you can recall, been dissatisfied with exposure categories established for cohort in a study that you've been responsible for, but yet gone ahead and used the exposure categories despite your reservations?

A. This is not a yes or no kind of question. I guess it's a matter of degree of satisfaction, so to speak.

It really depends on what questions you want to ask based on the analysis, based on the data. Certain exposure would be quite adequate for some questions and

some maybe not.

Q. I guess really what I'm trying to find out more about is can you recall an instance where you've had a contract to do a study and you felt like there were improvements in exposure categorization that could reasonably be made and the sponsor said, No, we are not going to do it, and you went ahead and did the study anyway.

A. No, I don't recall the situation at all.

Q. One of the papers that you brought was the De Coufle study on benzene. Do you remember that one? It's the first one.

A. Yes.

Q. Mortality Among Chemical Workers Exposed to Benzene and Other Agents.

A. Yes.

Q. By De Coufle, et al.

A. Yes.

Q. Do you know whose employees those were that he

studied?

A. It may be enough--not 100 percent sure. I think it was a Conoco refinery, and then later on they changed it to a chemical plant in the Baltimore area.

Q. I think you're right.

Q. Have you ever done any work for Conoco?

A. I assume you mean outside of litigation cases?

Q. Even including litigation cases.

A. I know I have worked on some cases in which Cononco is a codefendant.

Q. Which ones do you recall?

A. I don't recall them. I know for sure--

MR. SPEARS: They are all the ones you have, Herschel.

MR. HOBSON: There may be a few I don't have.

A. In terms of projects, I don't remember doing any projects for Conoco itself. That doesn't mean we don't have projects through API and Conoco may be a participant in those projects.

Q. That was going to be my follow-up. Do you know?

A. I know you pretty well. Now I can anticipate your questions.

Q. There are only a certain number I know to ask, and once I get through those, I'm out.

You don't know one way or the other whether Conoco has been a participant in any of the trade association studies, then?

A. If I was the principal investigator of that project, I would know.

Q. Have there been any that you can recall that

Conoco has either begun as a participant and didn't finish the study or finished the study?

A. Conoco is not a participant in the ongoing API gasoline study. That, I know.

I was involved in some specific analysis of an update of the old Tabershaw, T-a-b-e-r-s-h-a-w, Cooper

studies, and I don't remember whether Conoco was part of that study or not. That would be the only study that I could think of that Conoco might have participated in that I have some association with.

Q. Do you know if Conoco has ever begun to participate in an epidemiologic study and withdrawn?

A. No, I don't know that.

There was another project that I worked on several years ago when I was at EHA and that was a mortality registry. It's not a study, per se, but a program set up to keep mortality data on a prospective basis.

Conoco was one of the companies included in the first phase of the project. In other words, to determine what companies would have adequate data to participate in such a registry if API decided to fund such a registry. Conoco was part of the first phase.

Q. Was data actually collected?

A. Not mortality data, but in terms of how many

records they have, how many years we can go back, yes, there was some data.

Q. You're familiar with the Sloan-Kettering work--the registry and perspective study that was done by API Sloan-Kettering?

A. I know of the project, yes.

Q. Is the Conoco project a different project than that?

A. It's almost like the second phase of the Sloan-Kettering project.

There were some problems in terms of data in the Sloan-Kettering registry and several years ago that project--I think in the early '80s the Sloan-Kettering project was terminated, but then after a few years there was some interest in the petroleum industry to do something like that again and that's why they sponsored EHA to go out and look at what we can do.

Q. But the study was never undertaken, then, following the EHA's work. Is that what you're saying?

A. That's correct.

Q. Do you recall if there was a final report from

22 EHA on what was found?

23 A. The first part of the study?

24 Q. Yes.

25 A. Yes, there was a report.

100

1 Q. And that was API activity?

2 A. Sponsored by API.

3 Q. Have you done any epidemiological studies on

4 vinyl chloride?

5 A. Yes.

6 Q. Was Conoco a participant in those?

7 A. I don't know for sure. I just don't remember.

8 There were a whole bunch of companies in that study. I

9 just don't recall today at this moment whether Conoco

10 was or was not in the study.

11 Q. What about Vista?

12 A. I don't recall that either.

13 Q. Who do you recall originally was contemplating

14 participating in the CMA benzene study but who ultimately

15 did not complete that project as a participant?

A. ARCO was one of the participants when we started the project, but later on because of some problems with the data, ARCO withdrew from the study.

Q. Anyone else that you can think of?

A. That's the only company.

Q. Can you recall if there were any companies who had--who appeared to have adequate personnel records, but you who elected not to participate in the CMA benzene study?

A. I have no knowledge of that.

101

Q. Was there ever a time that the API had any involvement with the CMA benzene project or what became the CMA benzene project, to your knowledge?

A. It wasn't exactly what you describe.

At one time when we were doing the CMA benzene study there was supposedly a separate but parallel study on the manufacturer's side.

The CMA study consists of users of benzene, and

supposedly we have an independent study on the manufacturing side that would be member companies of API, and, in fact, I was the principal investigator of that project when I worked for Dr. Tabershaw, but there was very limited company participation on the API side.

I remember there were only two companies on the API side: Gulf and also Ashland. By "Gulf" I mean the Philadelphia refinery--the Gulf refinery in Philadelphia. And their contribution to the study was like 90 some percent of the data, and Ashland contributed less than 10 percent of the data.

The decision was made that based on that limited number of companies in the study, we should not label that as an industrywide study and maybe API shouldn't pay for that study.

As far as I know, subsequently Gulf did an independent study of the Philadelphia refinery and I don't

know what happened to Ashland.

Q. Do you know how many companies originally had

data sufficient to participate in the API portion the Tabershaw group was involved in?

A. I don't know. But on the API side what we are talking about are refineries, and at that time during the '80s there were a lot of cohort mortality studies on refinery workers. So independently the companies are doing studies of their own refineries.

Q. But the refinery worker studies, none of those studies done by the API looked at benzene exposures, did they?

A. We did. There was a study that I did with Gulf in Port Arthur. We did look at the group of employees at the benzene units. We looked at that subgroup.

Q. That was with Dr. Wen at Gulf, right?

A. Right. That was a joint project. The Port Arthur refinery study was a joint project between Gulf and Tabershaw.

Q. But what I was trying to address was your comment that the API, and, I guess in essence the industry, was already doing studies of refinery workers and what I was trying to find out is whether or not it's

true that those API refinery worker historical perspective epidemiological studies did not address benzene exposure?

A. Well, I just gave you an example. The Port Arthur refinery study was one of those studies conducted in the '80s independent of the CMA or API benzene studies. And one of the analyses that we did in that study was to look at people exposed to benzene.

Q. But that, again, would not be an industrywide study?

A. No, that's not an industrywide study, that's correct.

Q. The Tabershaw-Cooper study of '74, the follow-ups to it that were done by other investigators, those were industrywide refinery worker studies, were they not?

A. Yes.

Q. And none of those industrywide refinery worker studies dealt with benzene exposure, did they?

A. It depends on what you mean by "dealing with

benzene exposure."

We can safely assume that the majority of refinery workers would have some exposure to benzene--the gasoline which would have benzene content in it, but it's true that none of those studies have sufficient employment history information to identify a separate group as benzene cohorts.

Q. So going back to where I think I started down

this path, can you recall if there were any API member companies who had sufficient record keeping or records in place which could have done a study of benzene exposures in refinery workers similar to what you ended up doing for Gulf in Philadelphia?

A. Quite a number of companies have sufficient records to do a mortality study.

Q. Of benzene exposures in refineries?

A. Right. And in fact, they did do cohort mortality studies of their refinery workers, but the

exposure information may not be sufficient for them to identify a separate group out of the entire refinery.

Q. Isn't it true that the same historical exposure techniques that were used by the CMA benzene study group are available to be used in refinery worker studies?

A. Theroretically, yes, we can apply the same method to estimate historical exposure.

Q. So such a study could have been done had one wanted to do it?

A. On a theoretical basis I would say yes, but I really don't have much exposure information on the refinery side.

Q. Well, at the same time the chemical plants were taking benzene exposure samples in the late '70s and early '80s in response to OSHA regulations, the refineries were

doing the same thing, weren't they?

A. That's why I say on a theoretical basis we should be able to do that.

Q. It's more than just theoretical, isn't it?

Isn't it practical?

A. Unless they could not retrieve the data--the historical data. Unless the job description is so different that they cannot come up with some comparable groups. There are a lot of practical considerations in carrying out that process.

Q. Well, certainly Gulf in Philadelphia had that ability to do such a study, did they not?

A. No. I was referring to the Port Arthur refinery. Subsequently they identified--using the department, the location of work, they identified a group of workers who were employed at the benzene units. I didn't say that about the Philadelphia refinery.

Q. Was there a benzene study of the Philadelphia refinery workers or not?

A. Not that I'm aware of. There is an overall refinery study at Philadelphia. There is an overall refinery study at Port Arthur, but within that study we also did an analysis for people assigned to the benzene units.

Q. Would it be correct to say that as of today

there is not an epidemiological study of refinery workers that addresses benzene exposure?

A. Depends on what you mean by "address the issue of benzene exposure" because when you look at the entire refinery, certainly employees in a refinery are exposed to benzene. And by looking at the job titles, you can identify the groups with potential exposure to benzene relative to some other crafts or some other job titles.

Q. Is there an epidemiological study of a petroleum refinery that addresses benzene specifically by breaking down the job, task, however the exposure categories are created, looking for those response relationships?

A. Not in terms of quantitative exposure estimates.

Q. How about qualitative? Is there even a qualitative estimate of benzene exposure in a refinery population?

A. I think at one time in the old API industrywide

study they used some kind of ranking classification--high, medium and low. I was not involved in that study so I really don't know how they came up with that classification, but they do report mortality ratios by degree of exposure in terms of high, medium and low.

Q. And what do you recall they found?

A. I don't recall what they found.

Q. That's not one of the papers you've brought to the deposition today, is it?

A. No. And the reason being, subsequent to that we have individual company studies and they are all components within the old API study.

Q. And the individual company refinery workers studies address benzene exposures?

A. Not on a quantitative basis.

Q. How about qualitative?

A. Indirectly through job titles, also maybe through period of exposure.

Q. Which ones are those?

A. For example, the study I did for Chevron, we looked at those people exposed prior to 1947, '48 as opposed to people who were hired subsequent to that.

The rationale behind that was the exposure, at least at the recommended standard, dropped from 100 to 35 ppm over a period of two years around '47, '48. So that's an indication of level exposure.

Q. Is inherent in using that information that you just gave me about the Gulf study the change from 100 to 35?

A. No Chevron.

Q. I'm sorry. Chevron or Gulf?

A. Chevron. I said Chevron. You need lunch.

MR. SPEARS: There is no Gulf any more.

MR. HOBSON: I know.

Q. (BY MR. HOBSON) Well, let me go back.

When there is a change as you described in maximum allowable concentration from 100 to 35 participate

per million, do you assume, then, that all exposures are kept at or below those levels in a refinery?

A. I can't say all. I can say on an average basis certainly the exposure prior to 1947 or '48 would be much higher than exposure thereafter.

Q. What's the basis for being able to say that?

A. Because that was the recommended standard during that time. And by talking to company industrial hygienists, that's their impression.

Q. You're aware that the old Gulf Oil Company did not have an industrial hygienist before the mid-'60s?

A. We are talking about Chevron.

Q. But didn't--didn't the study you're talking about occur at the Gulf Port Arthur Refinery?

A. No. The Chevron refinery in California.

Q. I beg your pardon. I thought you told me earlier about the Gulf refinery at Port Arthur.

A. That was a separate study. But when you asked me about analysis by period of exposure, the example I

gave you was based on two refineries out in California that belonged to Chevron all this time. They are not from Gulf.

Q. I apologize. I didn't follow that.

Is it reasonable, in your view, to think that the exposures would be the same or similar at other refineries as the two Chevron refineries in California?

A. I would say there are some minor differences, but on an average basis it should be similar.

Q. Were the refinery workers in California washing their tools in benzene?

A. We have examples of that, sure.

Q. And were they washing their clothes in benzene?

A. I don't know about clothes, but their tools, certainly.

Q. They cleaned their hard hats with benzene?

A. I don't know specifically about hard hats.

Q. For the refineries in California, for what time period was the practice of washing their tools with benzene a practice?

A. I don't remember.

Q. Is this part of the pre-'47 time period or after?

A. I'm sure that practice was done prior to 1947, '48, but I don't know whether that was completely gone

after 1948 or if there were some individuals who were still doing that. I don't recall.

Q. Would it be your view that you could have met a 35 parts per million maximum allowable concentration and wash your tools with benzene?

A. I would think it's possible if the time involved in washing the equipment, or whatever, with benzene is relatively short and the rest of the day you don't have much exposure. Certainly the time--the eight-hour time-weighted average could be below 35 ppm.

Q. If you took--tell me, how long do you recall you attributed in a workday to washing your tools with benzene in your tasks for the CMA benzene study?

A. I don't have the data now so I really can't

answer that question. The time estimates involved in that depends on the location as well. I remember seeing different estimates from different companies.

Q. What ballpark are we talking about for those estimates? Are we talking about one minute, ten minutes, an hour?

A. Not an hour, for sure. Probably less than an hour.

Q. Would ten minutes be a reasonable length of time to wash your tools in benzene?

A. I don't recall.

Q. Well, let's talk about now, then, not the CMA study.

As we sit here right now, how long do you think would be reasonable to attribute to a worker who is going to wash his tools in benzene?

A. Reasonable as opposed to what I actually had in the study?

Q. Yes, sir.

A. I don't know what's reasonable. Depends on, I guess, how many pieces of equipment you want to wash.

If you're talking about personal equipment, maybe no more than 10, 15 minutes.

Q. Let's pick 10 minutes. And if we pick 10 minutes and we say that the eight-hour time-weighted average that we have of interest is 10 parts per million, what would be the maximum exposure for that 10-minute period that you could have and not exceed the eight-hour time-weighted average of 10 parts per million?

A. Assuming there was no exposure for the rest of the day?

Q. Yes, sir.

A. We can sit down and calculate that. That can be done.

Q. It would be 480, wouldn't it?

A. You are the industrial hygienist. I take your

word for it.

Q. Well, it's a matter of parts per million, minutes and working it out, isn't it?

A. Right.

Q. Would you have any idea what the breathing zone levels of benzene would be if you wash your tools in benzene?

MR. SPEARS: Herschel, we are getting theoretical here. Washing in a closed environment? Washing outside in the wind? Washing in a 55 gallon drum, in a sink? We could sit here all day and hypothesize about things.

Q. (BY MR. HOBSON) Okay.

A. We actually have some specific exposure data on that task in the CMA study. I just don't have the data now.

Q. For Mr. Crawford, do you have any estimate for his benzene exposure?

A. No, I don't.

Q. Have you read his coworker depositions?

A. Yes, I have.

Q. How about for Mr. Smith? Do you have any benzene estimates for exposure for Mr. Smith?

A. No.

Q. Have you read his deposition?

A. Yes, I have.

Q. How about his coworkers?

A. Yes, I have.

Q. Can you categorize their exposure qualitatively, either of those gentleman or both?

A. I would say they are typical of some of the studies that I have done.

Q. Typical for what time period?

A. For the same time period from the '50s until now.

Q. So it's your impression that these exposures that Mr. Crawford and Mr. Smith received at Conoco would be typical of refinery workers, say, working at Exxon in the same time period?

MR. SPEARS: I don't think he said Exxon, Herschel.

A. I never studied Exxon, come to think of it. I have done studies for Mobil, I have done studies for Chevron, I have done a study for Gulf, but I have never done an Exxon study.

So I would say based on my experience, it would be experience derived from Chevron, Mobil, and Gulf.

Q. (BY MR. HOBSON) And so you see no difference of any consequence in exposures between Conoco and those other three refining corporations?

A. Based on the job description that I get for the two plaintiffs from the depositions, I would say their exposure would be comparable to my studies, the studies that I have done.

Q. So would it be fair to say, then, you read that it was a practice to wash their hands, wash their tools with benzene at Conoco. You read that, didn't you?

A. Right.

Q. You accepted that as the truth?

A. I assume that's true, and that is not any

different from the common practice that I have observed when I did my study. The same practice occurred in my own study.

Q. And when you say "in my own study," you're now talking about at Mobil and at Chevron and at Gulf for comparable time periods?

A. Right.

Q. And, of course, Mobil and Chevron and Gulf employed you in your area of expertise to do those studies where you gained this knowledge; is that right?

A. Yes, sir.

Q. Will you acknowledge that there were numerous industry publications as early as the 1940s that said washing your tools with benzene was a bad practice?

A. I'm not aware of those publications or

documents that you refer to.

Q. You've never read the toxicological review for benzene put out by the American Petroleum Institute?

A. I don't believe so.

Q. Will you agree with me that as of 1948 there were reasonably well-documented cases of leukemia in workers exposed to benzene?

A. There are some case reports, if that's what you're referring to, but there weren't any studies.

Q. But would you agree that those could be described as reasonably well-documented cases of leukemia from workers exposed to benzene?

MR. SPEARS: I object to the form of the question as what you mean "reasonably well-documented." You could drive a truck through that definition.

MR. HOBSON: That's one the industry used. It's one Dr. Wong would agree with.

A. I guess we can use the word "documented" within the framework of case reports. To that extent, yeah, there are some case reports in the literature, but I still have to say that there weren't any studies--formal epidemiological studies.

Q. (BY MR. HOBSON) Could a reasonable person conclude in 1948 that benzene could cause leukemia in humans?

MR. SPEARS: I'm going to object to that question. That is really hypothesizing to the point of infinity now. Could a reasonable person?

A. I don't know what a reasonable person would think. All I can tell you, in trying to answer your question, is that as a scientist if I were given the case reports in the '40s or '50s, I would not be able to make the definitive conclusion that benzene had caused leukemia.

If that were the case, then I don't think NIOSH would spend the money to do a study on benzene and leukemia and CMA would not fund a study.

Q. (BY MR. HOBSON) Of course, NIOSH had to meet certain regulatory requirements before regulations could be passed. You realize that, don't you, Dr. Wong?

A. I realize that, but at the same time the regulatory requirements are actually less than the scientific criteria for causation for establishing an

association.

Q. How certain does someone need to be about benzene causing leukemia before they should warn workers, in your view?

MR. SPEARS: Object to the form of the question. That's beyond his expertise. You're asking him for a legal conclusion now.

MR. HOBSON: No, I'm not. I want to know his personal opinion.

A. I am not an expert in that area.

I can tell you what criteria were used in determining causation based on scientific data, but I don't know whether you should tell people what you can interpret from those data.

Q. (BY MR. HOBSON) If you had information available to you that there were reasonably well-documented cases of leukemia from exposure to benzene, is that something that you would pass on to your workers?

A. That's really outside my area of expertise.

Q. I'm not necessarily asking for an expert opinion. I'm just asking for what you would do as an employer.

A. I don't know. I have never been put in that position, so I really cannot answer your question.

Q. Do you know what the term "etiology" means?

A. As an epidemiologist I would interpret that as the causal agent of that disease.

Q. What would the term "etiology established" mean to you?

A. For an epidemiologist that would mean there are epidemiological data to support a causal relationship.

Q. Are you aware that as of 1949 that a representative of the Manufacturing Chemists Association and a representative of the American Petroleum Institute were involved in a publication of a document that said that the etiology had been established that benzene was a

human carcinogen?

MR. SPEARS: In all fairness to Dr. Wong, what's the name of the article, Herschel? Has he seen it?

MR. HOBSON: I don't know. That's what I'm trying to find out.

MR. SPEARS: What's the title? Is there a title?

MR. HOBSON: It's a real life document listed in our exhibit.

MR. SPEARS: Do you know what he's talking about, Dr. Wong?

THE DEPONENT: I understand the question, but I don't know of such a document, number one. Number two, whoever wrote that must not be an epidemiologist.

Q. (BY MR. HOBSON) Is that something that only an epidemiologist could conclude?

A. In terms of disease causation for chronic diseases. I would think epidemiology is the appropriate discipline to address that question.

Q. Now, remember my question goes back to 1949.

A. Right.

Q. In 1949 would it be so that this would be a questions that only an epidemiologist could address?

A. Well, as I said, epidemiology is the appropriate discipline to address causation issues. It doesn't change with time.

Q. So if someone were not an epidemiologist and came to such a conclusion about the etiology being established that benzene was a carcinogen--a human carcinogen, they would be acting beyond their abilities?

A. I would like to know what data they rely on in arriving at their conclusion.

We are talking about a document that I have never seen. I don't know what kind of data they relied on.

Q. Wouldn't you expect that if representatives of the American Petroleum Institute and the Manufacturing Chemists Association were going to put their names behind such a statement, that they would have had whatever they felt was sufficient to make those statements?

MR. SPEARS: I object to the form of the question. You're calling for speculation. I don't even know who the people were. You haven't identified who the people are or the article so how could he possibly know what was in their minds or the basis for their opinion?

MR. HOBSON: I'm not asking that question.

MR. SPEARS: You asked, Wouldn't you expect? He doesn't know who the people are. You haven't told who they are.

MR. HOBSON: Representatives of those two trade associations, both of whom he's done trade work for.

A. I have no opinion. I don't know those people and I don't know what the basis--

Q. (BY MR. HOBSON) You know the American Petroleum Institute, correct?

A. Yes.

Q. And the new Manufacturing Chemists Association which is now the CMA, Chemical Manufacturer's Association?

A. Yes. I worked with a very small number of

people within those organizations.

Q. And based on your work with those organizations do you think they are people that can be looked to to be relied upon for information about chemical hazards?

MR. SPEARS: You said--I object to the question. You are saying the organizations themselves are reliable?

MR. HOBSON: Yes, based on his experience with them.

A. I can't answer that question because the people that I deal with at CMA or API, they are administrative

people. They are project administrators. They don't participate in the scientific aspects of the studies.

I work with the committee formed within API or CMA, and the committee members by and large are company employees, industrial hygienists or epidemiologists or member companies.

Q. (BY MR. HOBSON) Can you give my some examples

of the CMA or API publications that you have found to be unreliable?

A. I don't read their publications. I'm interested in studies. I'm not interested in policy statements.

MR. HOBSON: Off the record.

(Discussion off the record.)

Q. (BY MR. HOBSON) Do you think it's reasonable, Dr. Wong, for people in the general public to rely upon publications of the American Petroleum Institute or the Manufacturing Chemist's Association?

MR. SPEARS: I'm going to object to the form of the question. I don't think that's within his expertise and I don't think it's a very well-defined question. The general public?

A. I can't answer your question.

Q. (BY MR. HOBSON) Can benzene cause CLL, chronic lymphocytic leukemia?

A. No, not based on epidemiological studies.

Q. Is that something about which reasonable minds can differ?

A. Well, I would assume--by looking at all the important papers, all the pertinent literature I come to the conclusion that exposure to benzene would not increase the risk of CLL, and if somebody offered a different opinion, I certainly would like to see why he or she would arrive at a different opinion, what kind of studies they relied on, and to that extent it may or may not be reasonable.

Q. Well, are you so convinced that benzene is not a cause of CLL that you will tell us that anyone who has a contrary opinion is just flat wrong?

A. It would be incorrect based on the current knowledge we have.

Q. You know that Dr. Morgan has said in his opinion benzene is the cause of CLL, don't you?

A. I don't know that.

Q. You don't know that?

A. I don't know that. I have not read his deposition.

Q. Well, assume for the purpose of this question that Dr. Morgan has said that in his opinion benzene is the cause of CLL.

123

A. I would like to know what kinds of studies he relies on.

Q. Are you willing to say he's flat wrong?

A. Yeah, I would say he's wrong.

Q. Is there any kind of leukemia or other cancer besides acute myelogenous leukemia that you believe benzene is a probable cause of?

A. No.

Q. Do you have to have aplastic anemia before you can develop acute myelogenous leukemia from benzene exposure?

MR. SPEARS: I you object. Now I think you're asking a medical question. He's not a medical doctor; he's an epidemiologist.

MR. HOBSON: It may or may not be a medical question.

MR. SPEARS: It sounds like a medical question to me and I will defer to Dr. Wong.

MR. HOBSON: Some people might think medical causation is a medical question.

MR. SPEARS: You're asking whether one disease precedes another, and you're asking him can you have something before having another type of disease. That's a medical question.

Q. (BY MR. HOBSON) Do you have an opinion or not,

Dr. Wong?

A. I don't have an opinion. I have not looked into that issue and I don't know whether there are data that would tell us one way or another.

Q. Have you read--what depositions have you read in this case--or these cases, Dr. Wong?

A. Depositions of the plaintiffs and depositions of the coworkers.

Q. Which ones? Which coworkers?

A. In fact, there is a cover letter that was sent to me together with all the materials.

MR. SPEARS: I think it's all the coworkers that can be designated as being coworkers in this case.

A. Here's a list of coworkers.

Q. (BY MR. HOBSON) And you've read all the depositions listed on this sheet?

A. Yes, sir.

Q. I notice that most of these have "Summary" after the name. Did you read the deposition or did you read the deposition summary?

A. I read both.

Q. Okay if I mark this letter?

A. Go ahead.

Q. Were there any CLLs in the benzene CMA study?

A. There were two observed, and we expected about

1.6 something.

Q. And that was in the comparison with the exposed group to the nonexposed or exposed to the general

population?

A. With the general population.

Q. In which groups did the two CLLs appear?

A. I don't recall.

Q. Would it be specified in the article--the published article?

A. Yes.

Q. And there were no CLLs in the unexposed workers?

A. There was no leukemia at all in the nonexposed group.

Q. Let me show you, Dr. Wong, this document which is purported to be a statement submitted to the OSHA benzene hearing by you--

A. Okay.

Q. --dated March of 1986, I believe. Do you recognize that?

A. Seems to be my statement, yes.

Q. Anything about that statement that you now recognize was incorrect at the time you made it?

A. Well, I made that statement in 1986.

25 Q. Yes, sir.

126

1 A. Subsequent to that we have a number of studies
2 published after 1986. To that extent, my opinion today
3 might have been modified by those studies published after
4 that date.
5 Q. But really, the first question I was going to
6 ask you ask is, is there anything about that March 1986
7 statement that you now know was incorrect at the time you
8 made that statement?
9 A. I have to go--I have not read this for some
10 time. I would have to go back and take a careful look if
11 you want to be that specific.
12 Q. Nothing comes do mind as you sit here now?
13 A. Not really.
14 Q. Whether your opinions are the same or different
15 now, I guess in order to answer that we would have to go
16 through it line by line and statement by statement?
17 A. I have to go through and try to recall--refresh
18 my memory exactly what I said and whether--based on the

studies that I'm aware of published subsequent to that date, whether those studies would have modified my opinion expressed on that date.

Q. And is there anything that you know--off the top of your head without going through it line by line or item by item, that you know you've changed your opinion about?

A. I think in general the issue of cell-type specific leukemia may be somewhat different than the situation we had in 1986 because we have more data now today on that issue than we had before that time.

Q. Give me the summary of what your opinion was in 1986 and a summary of what it is today on that regard, if you can.

A. In 1986 I wasn't aware of some of the writings by hematologists talking about the facts that different cell-types of leukemia represent different diseases. I wasn't aware of that.

There are also some epidemiologic studies that use--take advantage of some techniques that we only have within the last few years, and that is the identification of specific onco genes associated with specific cell-type leukemia. That wasn't available in the early '80s.

On top of that, we also have a lot more epidemiologic studies now on refinery workers that we did not have--at least not published before 1986. So we have a better idea of cell-type leukemia today than back in 1986.

Q. And so I'm not sure I understand where your opinion has changed.

A. Well, my opinion in 1986, if I remember correctly, was that we just don't have enough data on

cell-type leukemia. There weren't enough studies to allow us to make that assessment. But today what I'm saying is we do have a lot more studies now that we can address that question, cell-type specific leukemia and exposure to benzene. That's on the epidemiologic side.

At the same time, we also have clinical data to support that indeed different cell-type leukemias are different diseases.

Q. And how many industry-sponsored studies on benzene have you done since 1986?

A. Certainly the Chevron study was published after 1986. The study was done around that time, but we didn't complete that until sometime after 1986.

Q. Was that cell specific? Was the Chevron study cell specific for leukemia?

A. Maybe I omitted one part in my answer.

Based on any single individual study you're not going to be able to come up with a definitive answer on cell-type leukemia simply because the number is too small based on any single study.

The assessment has to come from several studies. We have cell-type specific information in the Chevron study for example, but that itself doesn't tell us that there is an association or there isn't because the sample size was not big enough for CLL, for example.

But on top of the Chevron study we also have a number of studies published around that time or subsequent to that. When we combine all the studies together, we have a pretty good picture.

Q. And the studies that are combined together have exposure to benzene quantitatively assessed?

A. There are some benzene-specific studies such as the Rinsky study, such as the CMA study. On top of that, we also have refinery studies overall.

Q. The refinery studies do not address benzene exposure specifically, do they?

A. Not specifically, but it's pretty safe to make that assumption that refinery workers are exposed to benzene.

Q. Which ones?

A. The majority of refinery workers would be exposed to benzene.

Q. How about a worker who works in a boiler in a refinery in Port Arthur? Is he exposed to benzene?

A. If he walks through the refinery, there would

be some exposure to benzene.

Q. And so that would be enough to allow you to draw conclusions about cell-type and benzene exposure/causation information?

A. Not just based on the example that you gave.

We are also talking about operators who are constantly exposed to benzene.

Q. Which operators are constantly exposed to benzene?

A. There is one study that we did with Gulf. We identified a process unit that makes benzene and we studied the operators of those units.

Q. Can you accept that operators on a benzene unit can have lower exposure to benzene than employees on a coking unit?

A. Sure.

Q. How did that happen?

A. Depending on the process, the engineering,

whether there is any leak and how they sample the products and so on.

Q. What about benzene exposure being lower for operators on a benzene unit than operators on a catalytic reforming unit?

A. That's also possible.

Q. So I guess I go back to the refinery worker studies. Let's take the Mobil Beaumont refinery worker study that you did. Which of those workers would have exposure to benzene and how would you categorize who had higher exposures than lower?

A. You're talking about the Mobil study in the CMA

study.

Q. The refinery workers.

A. We did not have exposure quanitification in that study.

Q. Which refinery worker study are you aware of where there is benzene quantification?

A. I'm not aware of any. The only study that has

some qualitative benzene exposure data would be the Gulf study that I did with Dr. Wen.

Q. And there you did high, medium and low?

A. No. We identified a process that involved benzene exposure and studied only those people that worked at those units.

Q. So that's one study?

A. Right.

Q. And was there enough information from that one study of refinery workers that you could get cell-specific information with benzene exposure?

A. No, not based on one study.

Q. I guess, Dr. Wong, it seems to me like a great leap that you're willing to go from the refinery worker studies where benzene is not qualitatively or quantitatively expressed on the one hand when I question you about that at Mobil and Beaumont to then leap over to while looking at refinery worker studies in general saying

everyone is exposed to benzene while working in a refinery.

That's not logical and I want you to explain how you can do that.

A. It's more than looking at refinery studies in general. I told you we looked at--in forming my opinion, I looked at studies of benzene-exposed workers and that would include my CMA study. That would include the Rinsky--the Rinsky NIOSH study, and that would include the De Coufle study you mentioned earlier, and I also relied on case controlled studies as well.

Q. And there is enough information on those that you can do cell-specific analysis and have statistical significance?

A. I'm not so sure the question is meaningful in terms of statistical significance. What I can say is based on all the data combined, I think the data that I have--I'm quite confident about the data.

Q. Do you find in those same studies you just told me about that there is an increase in AML, acute myelogenous leukemia?

A. Yes.

Q. And there is no increase in the other forms of leukemia or lymphoma?

A. It may not be true for every study, but some

studies report an increase of AML.

Q. You said you're looking at them in total.

A. Right.

Q. And I'm saying, when you look at them in total is the AML level statistically increased and being caused by benzene exposure?

A. For some studies, yes.

Q. I'm talking about whether you looked at them all.

A. I did not combine them and look at the overall result for AMLs. I didn't do that.

Q. Did you do that for any of the cell-types?

A. Just for CLL.

Q. Have you written that up anywhere?

A. No.

Q. Have you reduced that to writing in any way?

A. Not yet.

MR. HOBSON: Let me get the reporter to mark some of this stuff.

(Deposition Exhibits 1, 2 and 3 were marked.)

Q. (BY MR. HOBSON) Dr. Wong, we have marked as Deposition Exhibit No. 3 your statement that you gave to OSHA on March the 4th, 1986, correct?

A. Yes.

Q. And Exhibit No. 1 is your most recent

curriculum vitae, and I take it that it's relatively up to date?

A. Yes.

Q. And then Exhibit No. 2 is the letter that sets out the records that you were sent with regard to this case, correct?

A. It's two cases, yes.

MR. HOBSON: That's all I've got. Thank you very much.

(The deposition concluded at 2 p.m.)

I, OTTO WONG, do hereby certify that I have read the foregoing transcript and that the same and

accompanying correction sheets, if any, constitute a true and complete record of my testimony.

_____
Deponent

( ) No changes ( ) Amendments attached

Subscribed and sworn to before me this

_____day of _____, 1993.

My commission expires _____

_____
Notary Public

Address _____

_____

BS

Smith vs. Hartford Accident and Indemnity

24

25

STATE OF COLORADO)

) ss. REPORTER'S CERTIFICATE

COUNTY OF DENVER)

I, Rebecca L. Semeyn, do hereby certify that I am a Registered Professional Reporter and Notary Public within the state of Colorado; that previous to the commencement of the examination, the deponent was duly sworn by me to testify to the truth.

I further certify that this deposition was taken in shorthand by me at the time and place herein set forth and was thereafter reduced to typewritten form, and that the foregoing constitutes a true and correct transcript.

I further certify that I am not related to, employed by, nor of counsel for any of the parties or attorneys herein, nor otherwise interested in the result

of the within action.

In witness whereof, I have affixed my signature this _____ day of _____, 1993.

_____

PATTERSON REPORTING SERVICE
Rebecca L. Semeyn
Registered Professional Reporter
and Notary Public

PATTERSON REPORTING SERVICE
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Otto Wong
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181 Second Avenue, Suite 628
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Re: Smith vs. Hartford Accident and Indemnity
VS Nos. 92-2915 and 92-3098
Deposition of OTTO WONG

The deposition in the above-entitled matter is ready for reading and signing. Please attend to this matter by complying with ALL blanks checked below:

____ arranging with us at 303-320-6628 to read and sign the deposition in our office

____ having deponent read your copy and sign amendment sheets, if any (orig. signature page enclosed)

_XX_ reading enclosed deposition, signing attached signature page and correction sheets, if any

_XX_ within 30 days of the date of this letter

____ by _____ due to a trial date of_____

____ Due to a trial date of _____ a telephone message was left with _____ on _____ advising the deposition is available for reading and signing until _____ at which time the original will be filed.

Please be sure that the signature page and accompanying amendment sheets, if any, are signed before a notary public and returned to our office at the above address.

If this matter has not been taken care of within said period of time the deposition will be filed unsigned pursuant to the Rules of Civil Procedure. Thank you.

PATTERSON REPORTING SERVICE

cc: Herschel L. Hobson, Esq.
William B. Baggett, Esq.
Kenneth R. Spears, Esq.

PATTERSON REPORTING SERVICE
55 Madison Street, Suite 510
Denver, Colorado 80206

William B. Baggett, Esq.

Baggett, McCall & Burgess
3006 Country Club Road
P.O. Drawer 7820
Lake Charles, Louisiana 70606

Re: Smith vs. Hartford Accident and Indemnity

Enclosed is the deposition of: OTTO WONG

_____ Previously filed. Forwarding signature page and amendment sheets.

_____ Signed, no changes.

_____ Signed, with changes, copy of which is enclosed.

_____ Unsigned, notice duly given_____ pursuant to the Rules of Civil Procedure.

_____ Not signed, notice duly given_____ since trial is set for _____.

_____ No signature required.

_____ Signature waived.

_____ To be signed in court.

_____ Signature pages/amendments to be returned to court on date of trial.

_____ Mailed by certified mail # _____.

_____ Hand-delivered _____.

Enclosures: (As above-noted)
cc: Herschel L. Hobson, Esq.
Kenneth R. Spears, Esq.