COMMONWEALTH OF PENNSYLVANIA

PUBLIC UTILITY COMMISSION

Letter of Notification of Philadelphia Electric Company Relative to reconstructing and rebuilding of the existing 138 kV line to Operate as a Woodbourne-Heaton 230 kV line in Montgomery and Bucks Counties: Further Hearings.	:	Docket No. A-110550F055

Pages 1631 through 1696 Hearing Room No. 1
State Office Building
Philadelphia, Pennsylvania

Friday, May 28, 1993

Met, pursuant to notice, at 10:03 a.m.

BEFORE:

HERBERT SMOLEN, Administrative Law Judge

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C O N T E N T S

E X H I B I T S

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P R O C E E D I N G S

ADMINISTRATIVE LAW JUDGE HERBERT SMOLEN: Back on the record.

Are we ready to proceed with the calling of the next witness?

MR. SMITH: Yes, Your Honor.

JUDGE SMOLEN: Please proceed.

MR. SMITH: The company calls Dr. Philip Cole.

JUDGE SMOLEN: Dr. Cole, you previously testified in this proceeding. But because it is remanded I will ask you to be sworn in again.

Whereupon,

PHILIP COLE

having been duly sworn, testified as follows:

JUDGE SMOLEN: Please have a seat. State your full name and address.

THE WITNESS: My name is Philip Cole. My home address is 4855 Shady Water Lane, Birmingham, Alabama.

JUDGE SMOLEN: Counsel.

MR. SMITH: Your Honor, we have previously distributed to Counsel, the court reporter and yourself copies of a document labeled PECo Direct on Remand No. 4, the direct testimony on remand of Philip Cole on behalf of Philadelphia Electric Company. I ask that that be marked for identification.

JUDGE SMOLEN: So marked.

(Whereupon, the document was marked as PECo Direct on Remand No. 4 for identification.)

DIRECT EXAMINATION

BY MR. SMITH:

Q. Dr. Cole, do you have before you a copy of the document that has just been marked for identification as PECo Direct on Remand No. 4?

A. Yes, I do.

Q. Was the document prepared by you or under your direct supervision?

A. Yes, it was.

Q. If I were to ask you today the questions set forth in that document would your answers be the same as those contained therein?

A. Yes, they would.

Q. And would those answers be true and correct to the best of your knowledge, information and belief?

A. Yes, they would be.

MR. SMITH: Your Honor, I ask that PECo Direct on Remand No. 4 be admitted into evidence pending timely objections and cross-examination.

JUDGE SMOLEN: It is received with that qualification.

(Whereupon, the document marked as PECo Direct on Remand No. 4

was received in evidence.)

MR. SMITH: The witness is available for cross-examination.

JUDGE SMOLEN: All right.

Law Bureau.

MS. BURKET: No questions, Your Honor.

JUDGE SMOLEN: PP&L.

MR. DILLON: No questions, Your Honor.

JUDGE SMOLEN: OCA.

MS. McCLOSKEY: Thank you, Your Honor.

CROSS-EXAMINATION

BY MS. McCLOSKEY:

Q. Good morning, Dr. Cole.

A. Good morning.

Q. My name is Tanya McCloskey. I represent the Office of Consumer Advocate.

I would like to start at page two of your testimony, where you begin a discussion of the Swedish residential study by Feychting and Ahlbom. Would you agree with me that this study was a large and complex study?

A. Yes, indeed, I would.

Q. And would you agree with me that this study gave impressive attention to detail?

A. Yes, I would. In most respects.

Q. And would you also agree with me that the study improves the methodology that was used in early epidemiological research regarding EMF?

A. In some respects it does, yes.

Q. Now, at the bottom of page two, lines 23 through 33, you begin a discussion of how magnetic fields were estimated in the -- I am going to refer to it as the Swedish residential study so that the record does not get confused. Because there is a second Swedish occupational study.

A. Yes, true.

Q. Am I correct that the researchers used three methods to assess exposure?

A. Well, I would have described it as two, but maybe you are splitting one.

Q. Okay. Let me go through my three --

A. All right.

Q. -- and we will see where we may be differing.

Is one of the measures that they have used the calculated historic fields that you have described --

A. Yes.

Q. -- on those lines 23 to 33?

And is another measure distance from powerlines?

A. Yes.

Q. And would a third measure be contemporary spot

measurements?

A. Yes.

Q. Those are the three I am referring to.

A. Okay. I say two because I would have merged the first two because distance from the line is one of the components in the calculated or estimated field.

Q. Okay.

A. But it can certainly be described three different ways.

Q. And do these measures contribute an additional approach to assessing exposure to EMF that have not previously been used in EMF research?

A. Only the first does. The spot measurements have been used in the past, and distance from structures have been used in the past. The calculated field, or the calculation integrated over a period of time, is new in residential studies.

Q. And does the calculated historic field provide a more detailed estimate of exposure?

A. I think we could say that it provides a more detailed estimate. But I don't mean to imply by that that it provides a more accurate estimate. It may or may not be a more accurate estimate of the actual exposure experienced by the individuals. It is quite conceivable that the best measure is distance.

Q. And would the calculated historic field be more relevant to the etiologic period for the disease?

A. I think that is very difficult to say because we don't know where in the causal web magnetic fields would introduce their effect, if indeed they introduce any effect at all.

Q. Now, the sample for the Swedish residential study was selected from the Swedish population registry, correct?

A. No, not exactly.

Q. Could you explain?

A. The sample for the study was defined as opposed to selected from a larger roster. It was defined as all of those people who had lived within 300 meters of transmission lines of a certain size within that country over a certain period of time. These people in addition had to qualify as being in the Swedish population register.

Q. So a more accurate way of saying that is that it was defined from the Swedish population registry based on the criteria you have described?

A. I'm not sure if you are implying which one was primary or not. Both criteria had to be met.

Q. Yes. I did not intend which was primary. But essentially both criteria --

A. I don't know which was primary.

Q. Okay.

And is this methodology that they used an accepted procedure for avoiding bias in sample selections?

A. Yes. It is one accepted procedure. And I wouldn't say avoid bias. But it is one approach to try to minimize bias.

Q. Now, if you could turn to page four of your testimony, lines five and six, where you note that the study is weakly positive for childhood leukemia. And do you have a copy of the Swedish residential study with you?

A. No, I don't.

Q. I have a copy for you. I will show it first to your Counsel.

(Document handed to Mr. Smith.)

(Document handed to witness.)

BY MS. McCLOSKEY:

Q. I would like you to reference Table 4.3.

A. I have it now.

Q. And this table is entitled Cancer Risk in Children in Relation to Calculated Magnetic Fields Closest in Time to Diagnosis, Cutoff Points at 0.1 and 0.25 Microtesla.

And first, so we are all talking in inconsistent

terms, is 0.1 microtesla the same as one milligauss?

A. Yes, Ma'am. Approximately the same.

Q. And 0.25 microtesla would be approximately the same, then, as 2.5 milligauss?

A. Approximately, yes.

Q. Now, I would like to look at the portion of the table for leukemia and the column entitled the RR. Just so the record is clear in this phase of the proceeding, could you tell us what the RR is?

A. RR stands for relative risk and it is the risk for people in that particular category compared to the people in the less than .1 microtesla category.

Q. And for the 0.25 microtesla category the RR is 3.3, correct?

A. That's correct.

Q. And is that a statistically significant RR?

A. It is marginally so, but it is significant.

Q. Now, if you could -- you may need to hang on to that reference of Table 4.3, but if you could also now reference Table 4.38?

A. All right.

Q. And looking at that table is the increased risk for childhood leukemia that we just discussed from Table 4.3 for the category 0.25 microtesla, does that remain statistically significant when the RR is adjusted for

socio-economic status?

MR. SMITH: Your Honor, just a point of clarification here. This table does not have any data on 0.25 milligauss.

MS. McCLOSKEY: I'm sorry. In the 0.2 category.

A. Yes, I think we --

MR. SMITH: Then I would object to the form of the question, because the question was does the 0.25 from the previous table still hold in this table. And if we have clarified that they have different types of data, I object to the question or would at least ask her to clarify.

JUDGE SMOLEN: Let's go back. Maybe you can clarify.

BY MS. McCLOSKEY:

Q. Let me just go back and ask you a broader question and I will use Table 4.38 for a reference point.

Is the increased risk for childhood leukemia statistically significant when the RR is adjusted for socio-economic status?

A. And I understand you to be asking that question with regard to this 0.2-plus microtesla.

Q. Yes.

A. I understand that 0.2-plus.

Q. Yes.

A. The answer to your question is yes.

Q. And I will just direct your attention at this point to the Table 4.39. Is the increased risk for childhood leukemia in the 0.2 microtesla-plus category statistically significant when the RR is adjusted for car exhaust?

A. It is marginally statistically significant but it is moderately weaker than it has previously been.

Q. Now, if you could reference Tables 4.31 and 4.32?

A. All right.

Q. Did the authors report a statistically significant increase in risk for childhood leukemia associated with the distance of child's home from a powerline of zero to 50 meters?

A. No.

Q. And why do you say no?

A. Well, I am basing, without going into the text, but basing it on the table that you have asked me to look at. You see that the category zero to 50 meters shows a relative risk of 2.9 with a lower bound of 1.0. 1.0 is itself the criterion of statistical significance. Most people would require that that lower bound be greater than 1 to be called statistically significant. This is not greater than 1; it is 1.

Q. So if the lower bound is equivalent to 1 --

A. That would ordinarily be considered not statistically significant. However, we are splitting hairs here. This is right on the threshold of statistical significance.

Q. If you could look at Table 4.32 then.

A. Yes.

Q. This table, for clarity, we should probably read the title, which is Cancer Risk in Children in Relation to Distance to Powerline Restricted to One Family Houses.

A. Right.

Q. And again looking at the zero to 50 meter category for distance from powerline, do the authors report a statistically significant increase in risk for childhood leukemia?

A. Yes, marginally so.

Q. Now, at page four of your testimony --

A. Yes Ma'am.

Q. -- lines 19 to 26, you note that the positive association with childhood leukemia is restricted to single family homes and not apartments. Did the authors present data that suggests a possible explanation for this difference in results between the two groups?

A. I don't recall their presenting data. I do

recall their offering an explanation.

Q. And it may be easier if you reference page 25 of the study.

A. Yes.

Q. And about the middle of the page there is a sentence that begins "furthermore".

A. Is it in the first paragraph or the second?

Q. It is in the first block.

A. I see it. Yes.

Q. Is that where you are referencing that the authors indicate a possible explanation for the difference?

A. Yes, Ma'am.

Q. Is that possible explanation the precision for the calculated fields may be lower for apartment houses than for single family dwellings?

A. Let me be sure I understand your question. Is it is their argument plausible?

Q. My first question is is that their argument?

A. Yes, that is their argument.

Q. And also in that paragraph do the authors indicate that it would take the shifting of one case from the lowest to highest exposure category among those in apartment houses to produce a relative risk of 1.8 for apartment dwellers?

A. They do.

Q. And if you could just reference page 11 of the study, the paragraph beginning, "For people living in apartment houses," am I correct that in that paragraph the authors set out that in about one-third or 200 out of 626 apartments actual measurements were not able to be taken inside the apartments?

A. That's correct.

Q. Have you had an opportunity to evaluate the authors' arguments in regard to the difference between single family houses and apartment dwellings and the data they present on Tables 3.8 and 3.10 in relation to the precision of measurement?

A. Yes, I have.

Q. Do you agree with the authors' explanation, or one of the possible explanations, for the difference?

A. The explanation that they put forward is acceptable but it represents only one-half of the full explanation. As you have made clear, their statement relates to the precision of the estimate. And I would agree that they have supported the idea that the precision of the estimates for apartment houses is poorer than it is for single family homes.

What is not represented but what is in fact true is that the precision -- the imprecision or relative lack of

precision can lead to an error in either direction. What they are proposing is that the absence of the finding in the apartment houses is attributable to the imprecision, but the fact of the matter is that the imprecision could go either way.

So that imprecision is not an explanation for a result. It is only an explanation for the variability associated with the result. So this does not explain the absence of association in apartment buildings. It is a possible explanation, but it is not the explanation.

Q. Now, back on page three of your testimony, lines 20 through 28, you reference information and data from the Swedish residential study that is still becoming available. In the preparation of your testimony did you utilize data that is not available yet in the published literature?

A. No. What I meant was that it has appeared in published form subsequent to the appearance of the original report. But it is published. I do not have access to any information on the Swedish study that is not in the public domain.

Q. Now, I would like to discuss with you the Swedish occupational study, which we referred to as the Floderus study. And if you could turn to page eight of your testimony, beginning at line 28 and then onto -- it

continues onto the top of page nine, you mention that there are three positive studies and one that is negative?

A. Uh-huh.

Q. Is the Swedish occupational study one of the three positive studies that you were referencing there?

A. Its data are positive, yes.

Q. Its data. All right.

And I believe that you state that none of these studies provide a significant improvement in method or design, correct?

A. That is my statement and that is -- yes, that is what I believe.

Q. Now, am I correct, Dr. Cole, that in the Swedish occupational study or the Floderus study job histories and dosimeters were used to do exposure assessment on the work site to EMF?

A. The major source of information is the job history. Some dosimetry was done for some of the people for some of their jobs. A very small sample of the total employment experience was actually dose-assessed, probably less than five percent, although we don't really know.

Q. And for the percentage that was dose-assessed using dosimeters, were those attached to the workers?

The dosimeters were attached to the workers in the workplace?

A. They were attached to the workers. But let's be clear that these are not the workers in the study. These are people who now occupy the jobs that these people might have occupied sometime in the past. The people in the study may or may not still be employed in the positions that are being measured. We have yet another problem there with trying to assume that we know the actual exposures of the people in the study.

Q. Perhaps the easiest way to ask this is then how was the information that they obtained from the actual dosimeters utilized by the researchers?

A. By the power of assumption. That is, they assumed that their study subject who was said to hold job 27 ten years ago had 27 years ago experienced the exposure that the man today on the job was experiencing.

Q. So they took the levels from the dosimeter readings at the work sites today and then assessed them to the same jobs?

A. They imputed them to the same job some period of time in the past, which could have been as much as 30 years ago.

Q. Now, I also would like to reference the Sahl study that you discuss beginning on page nine of your

testimony. First of all, do you categorize the data in this study as positive or negative?

A. This is a categorically negative study.

Q. Now, am I correct that there were two approaches to exposure assessment that were used in the Sahl study?

A. Yes.

Q. And one was based on job title in which a distinction was made between jobs related to energized equipment and other unrelated jobs, correct?

A. That's correct.

Q. And in the second approach current workers wore dosimeters?

A. That's correct.

Q. Could you explain for us what was then done with the information that was obtained from the current workers wearing dosimeters in the process?

A. First I would point out that the Sahl study uses dosimeters on current workers but the period of time of the retrospective nature of the study is somewhat shorter than that in the Swedish study. So the exposure information is more likely to be, all other things being equal, more accurate.

It is also a much more thorough assessment of the exposure in that a very substantial proportion of the

work history is covered by the imputed dose assessment.

It is also much more complete in the sense that all of the workers in the study are workers within the utility industry as opposed to the Floderus study, which has a cross-section of exposures throughout the Swedish population.

So the Sahl study is a much more focused study. It is focused right on utility workers, unlike the Floderus study. So information is available for a very substantial proportion of the work history of all of the people in the Sahl study, unlike the Floderus study. The information was taken and once again imputed to the study subjects.

Q. So with those caveats they followed the same basic procedure yet they have more data and information and were using it in a more recent time than in the Swedish occupational studies? Is that what you are saying?

A. I could agree that it is the same basic procedure only in the most general sense that people wore dosimeters. It is otherwise not in any sense comparable. The depth, quantity and quality of information between the two studies is just not comparable.

Q. Now, were the exposure assessments in the Sahl study dependent upon the length of time that a subject

worked for Southern California Edison, the utility that was used in the study?

A. If I may ask a question of clarification?

Q. Certainly.

A. When you say the exposure assessment do you mean the amount of exposure imputed to each subject?

Q. Why don't we start there. I believe that's what I am --

A. Then the answer is yes. That is, for each subject in the study the exposure imputed to him was a combination of the measurements for the positions that he had held -- and some subjects were still there themselves; the measurements were actually made on the people -- multiplied by the period of time that they had been in those particular positions.

Q. And does the article that was authored by Sahl tell us if the cases and controls worked for a comparable period of time as the workers whose exposure was being assessed?

A. I don't know the answer to that question. I'm sorry.

Q. Dr. Cole, is the incidence of cancer related to age?

A. Absolutely.

Q. Now, in the Sahl study did the study that was

published provide any age-specific relative risks?

A. No.

Q. And did the study provide any standard mortality ratios that compare age-adjusted cancer rates in the study population to the population at large?

A. No.

Q. Now, do you have a copy of the Sahl study with you?

A. No, I don't.

Q. I have a copy for you. I will show it to your Counsel first.

MR. SMITH: Thank you.

(Document handed to witness.)

BY MS. McCLOSKEY:

Q. I am just going to reference page 104, which is the first page of the study. This was faxed so I realize it is a little difficult to read.

A. Okay.

Q. I would like to reference the sentence in the second column near the end of the first block. The sentence begins "Nevertheless". Do you see that?

A. Yes.

Q. If you could read that sentence into the record for us?

A. All right. It says, "Nevertheless, the

available data provide some support for the theory that field intensities found in the work environment may play a role in cancer promotion or progression."

Q. And do you agree with that sentence?

A. Well, firstly I have to place this in the context.

Q. Sure.

A. This is the authors' interpretation of the existing literature. It does not relate to their study. This is a statement of background.

Q. All right.

A. Now, the question is would I sign this?

Q. Yes. That's one way of putting it.

A. As it stands I would not sign it. Not because it is wrong but because it is a half-truth. The statement could be modified to say the available data provide evidence against theory. So there is some evidence for; some evidence against. And of course the question really is what is the balance of that evidence.

Q. Now, do you know whether the study by Sahl was part of a doctoral dissertation?

A. I don't know that for a fact. I understand that that may be true, yes, but I don't know it.

Q. And do you know whether a second article by Sahl with more details on worker exposure has been

published at this time?

A. I am not aware of it.

Q. Dr. Cole, do you know who Sahl, or Dr. Sahl, works for?

A. Yes. He works for Southern California Edison Company.

MS. McCLOSKEY: Thank you, Dr. Cole.

I have no further questions, Your Honor.

JUDGE SMOLEN: Mr. Sugarman.

MR. SUGARMAN: Yes, Your Honor. Thank you.

(Pause.)

MR. SUGARMAN: You caught me by surprise. I thought she was just warming up.

CROSS-EXAMINATION

BY MR. SUGARMAN:

Q. Doctor, on page 11 of your testimony you state that the epidemiological reports fail to demonstrate -- this is at the bottom of page 11 -- any strong or consistent pattern of association between EMF and cancer in human beings. The summation can only be that, to date, there is no demonstrated relationship between EMF and cancer in human beings.

What is the difference between those two statements?

A. Well, I think one is there is very little

difference. One is intended to be descriptive and the other one is intended to draw a conclusion.

Q. Well, does it follow according to established epidemiological standards that -- well, first of all, what does the word demonstrate mean as used in those two sentences?

A. To show.

Q. What?

A. To show.

Q. Well, is there a difference between to demonstrate or to show one the one hand and indicate on the other? Or suggest?

A. I would accept the word indicate as a synonym for demonstrate but I would not accept suggest.

Q. So would it be your testimony, then, that the epidemiological reports fail to indicate any strong or consistent pattern?

A. Yes, sir.

Q. Would it be your testimony that they fail to suggest any strong or consistent pattern?

A. In that sentence I will accept suggest as well. But not in the next one.

Q. So in the next sentence you would not testify today that the summation can only be that to date there is no suggested relationship?

A. Correct.

Q. And what difference are you making between the word demonstrate and the word suggest? What is the connotation that you want to distinguish them?

A. The distinction comes not from that but from the use of the word summation. That is, in my use of these words the concept of summation has to focus on the overlying idea as opposed to entertaining the various possibilities. So in sum they don't suggest.

If you want to point to one particular study that has a finding, fine. But my summary is that the available evidence now, and particularly in the last year, run counter. That is, we are not only showing that electromagnetic fields are not shown to cause cancer, but the available evidence now shows that they do not. That is, the contrary evidence is now in the affirmative.

Q. Can you state whether there is a difference between your conclusions with regard to childhood cancer and your conclusion with regard to adult cancer? And I call your attention to your language at the bottom of page seven where you say, "I conclude that the epidemiological evidence on EMF when viewed as a whole provides no persuasive scientific support for the hypothesis," and then compare that to your language over on page 11 with respect to adult cancer, line 14, where

you say, "I conclude that this body of studies does not support the hypothesis," and then you go on to say, "and that the best designed and conducted studies provide strong support against that hypothesis."

Now, will you agree with me that the conclusion that you reached on adult cancer is much stronger against the association than the conclusion you reached with respect to childhood cancer?

A. Mr. Sugarman, I think there has been a misunderstanding here.

Q. Okay.

A. On page 11, beginning at line 19 --

Q. Right?

A. -- this is not a conclusion regarding adults. This is a conclusion regarding the entire body. It is inclusive of adults and children.

Q. I understand that. I understand that. I was working back to the components. And you have divided at page seven, line 20, the conclusions with regarding childhood cancer from page 11, line six, conclusions regarding adult cancer studies. So I'm taking those as being the two components of your final opinion which you start at line 19 on page 11.

A. I just wanted to clarify that we are comparing information to which you had not previously made

reference. That is, the earlier information on page 11, with the information on page seven. Not the bottom of page seven with the bottom of page 11. It's the bottom of seven with the middle of page 11 that you want to compare, is that correct?

Q. I want to compare your conclusions regarding the childhood cancer studies with your conclusions regarding the adult cancer studies. And the conclusions regarding the childhood studies start at page seven, line 20, and the conclusions regarding the adult cancer studies start at page 11, line six. And I want to compare the two.

I find that there is a sharp difference between the conclusions and I am asking if you agree with me.

Your conclusion with regard to -- I will start again. Your conclusion with regard to childhood cancer studies is stated at page seven, line 29. You say -- I will start at 30, leave out the introductory words. You say, "The epidemiologic evidence on EMF when viewed as a whole provides no persuasive scientific support for the hypothesis that EMF causes cancer in children."

Now, the question I am asking you is do you agree that that is very different and a much more modified statement as compared with the statement that you make about adult cancer on page 11, starting at line 15 where

you say, "This body of studies does not support the hypothesis that EMF is associated with adult cancer and the best designed and conducted studies provide strong support against that hypothesis."

Do you agree with me that that statement on page 11 is much stronger than the statement you make on page seven?

A. Not at all. The very reverse.

Q. Well, by stronger I mean stronger against association.

A. The very reverse.

Q. Okay. Let me break it down somewhat and then I will give you a chance to explain.

Why do you put the word persuasive before the words scientific support on page seven, line 31? Are you saying there is scientific support but it is not persuasive?

A. Yes.

Q. Over on page 11, with respect to adult cancer, you don't acknowledge that there is any support at all, whereas with respect to childhood cancer you say there is support but you don't consider it persuasive. Do you agree with me there is that difference --

MR. SMITH: Objection.

MR. SUGARMAN: I haven't finished the question.

BY MR. SUGARMAN:

Q. Do you agree with me that you are making that difference?

MR. SMITH: Objection, Your Honor. Mr. Sugarman is now getting to the point where he is characterizing the testimony very strongly as he describes it rather than reading from it, and I object to, given the length of these questions, that kind of characterization. If he cuts it down and says is this a correct characterization that is one thing. But the questions are simply so long --

MR. SUGARMAN: Well, I want to compare the two. You are not saying I can't compare the two?

JUDGE SMOLEN: Does the witness understand the question?

THE WITNESS: Yes, sir.

JUDGE SMOLEN: Then I am going to overrule the objection. Let him answer it.

A. Mr. Sugarman, I would like to begin by saying that I have chosen my words in both of these instances as carefully as I could.

BY MR. SUGARMAN:

Q. I was sure you had.

A. Nonetheless, I find that your focusing on them and comparing them by abutting side-by-side causes

contrasts to appear that are greater than those that were intended by me when I wrote them.

From my perspective the essence of the first argument is one of causation and the essence of the second one is association. That is why the first one is in my mind and was intended to be stronger. That is, I am excluding the causal argument, which is in effect the whole argument.

In the second case, on page 11, the issue is one of association. And there is a call there for more studies because the information is of a lesser quality and quantity for adult studies than it is for childhood, now, with the exception of the Sahl study, which of course is quite a milestone in this area.

So the answer to your question is that as far as I am concerned the weight of evidence is strongly against the causal idea in both areas, but the residue of possible association remains in the adult studies. And I don't know that I can clarify it beyond that.

Q. Now, why did you say in regard to the adult studies that the best designed and conducted studies provide strong support against the hypothesis and not say the same thing with respect to the childhood studies?

A. Mr. Sugarman, I guess I was simply trying to reflect the idea that in the area of adult studies there

are a couple that stand out as clearly superior to the others and they are negative and in fact argue against the argument as opposed to simply being null or neutral, whereas in the childhood studies I think we have got only one fairly decent study and it is still not of the caliber of the two studies that we have, but only two, in the adult area.

Q. Okay. Just so I understand your earlier answer, would you agree that if the word association, or is associated, if that phrase were substituted for causes in your opinion on children on line 32, page seven, so that the sentence would read provides no persuasive scientific support for the hypothesis that EMF is associated with cancer in children, would you still sign on to the statement? Would that still be your opinion?

A. Oh, sure. But I've made an even stronger one. So it subsumes the associative hypothesis. This is a stronger statement.

JUDGE SMOLEN: You mean the word causes assumes associated?

THE WITNESS: Yes, sir. In order for something to cause something it must first be associated.

BY MR. SUGARMAN:

Q. If we were to drop the word persuasive, would you give as your opinion that the epidemiological

evidence on EMF when viewed as a whole provides no scientific support for the hypothesis that EMF is associated with cancer in children?

A. Well, may I just take a moment to think about that?

Q. Sure.

A. We have made two changes, now? We want to drop persuasive and substitute association for causation?

Q. Right. Exactly.

(Pause.)

A. I would not subscribe to the statement, then. That is, I will concede that there is some evidence of some association.

Q. Now, if I remember your previous testimony correctly, and I could be wrong about it, but my recollection is that you testified -- well, let me ask you. Did you previously hold the opinion that there should be no additional studies in this area? Did you ever testify to that effect?

A. My recollection of my testimony along those lines was something like this: that I was aware of the fact that there were a relatively large number of so-called third generation studies under way -- by third generation, I mean studies using measurement-based exposure assessment such as the Sahl study -- and that it

was a reasonable thing in my judgement not to initiate any new studies until the existing crop of third generation studies had been harvested.

Q. And is the Sahl study such a study?

A. Oh, yes, indeed.

Q. And as a result of that, do you have an opinion as to whether additional studies should be done?

A. My position remains the same. There are a number of other third generation studies that will be coming to conclusion in the next 12 to 18 months and I think it is worthwhile to wait for those, unless somebody has some spectacular idea of how to do a study of a nature that is previously unthought of.

Q. Now, in regard to the Swedish study do you -- you indicated that you agreed with the ORAU panel on the Swedish study and their letter in Science magazine. The language there is that the evidence -- I am reading your testimony, page six -- "The evidence presented in these studies is not sufficiently compelling to alter the conclusions of the ORAU report."

What does the term not sufficiently compelling mean? Since you agree with it, what does it mean?

A. Perhaps we can just take a second so I can see it in context.

Q. Sure.

A. You say it is on page six?

Q. Page six. The quote appears at lines 24 through 29.

A. Yes. And you're asking me what I understand that to mean?

Q. Yes. The words not sufficiently compelling.

A. Well, I understood it to mean not sufficiently persuasive.

Q. Well, that means it was somewhat compelling or somewhat persuasive?

A. When I bought into this statement here by virtue of having incorporated it into my testimony I was using it to represent the position of what I believe is a well respected body. Exactly what they meant, I really don't know.

Q. Well, I am asking you what you mean by it.

A. Oh, I can tell you what I mean.

Q. Yes.

A. I mean it is inadequate to cause a change of position.

Q. Do you recognize any difference between the phrase not sufficiently compelling on the one hand and inadequate on the other?

A. Yes, I do.

Q. What is the difference between those two terms?

A. My term is intended, at least given my interpretation of both terms, is intended to be somewhat stronger than their's.

Q. Stronger in the negative sense?

A. Absolutely.

Q. Then you don't really agree with them?

A. Oh, I do agree with them.

Q. But you are saying by your understanding of the two phrases you would not go with the phrase not sufficiently compelling as you understand it, you would go with the phrase inadequate?

A. But, Mr. Sugarman, I am reasonably flexible human being. I understand that other people will choose differences of language to express their points of view. What I am saying is their position is perfectly consistent with mine. I go a little further, but their position is far more like mine than it is unlike mine.

Q. I understand your point and I wanted to clarify that. The question I would have is how strong would it have to be for you to regard it as sufficiently compelling?

A. Okay. Now, the antecedent of the word it in your question is the evidence.

Q. Right.

A. Okay.

Q. Yes, the evidence. Right.

A. Now, I have tried to make clear, and perhaps I have failed, that I have already gone beyond that. That is, the evidence in the two Swedish studies, and if I may I will add in the Sahl study. I hope that does not confuse the matter.

Q. That is the adult study, though.

A. Is in my judgement more than sufficiently compelling to cause me to say that in the last year or so, that is, during the period of time in which these studies have appeared, the evidence against the hypothesis has mounted.

And when I say against the hypothesis, I mean not in the direction of the null state but, rather, in the direction of an affirmation of the absence of an effect. Not merely that there is no evidence of an effect, but we now have evidence that there is no effect.

(Pause.)

A. These studies are, in my judgement, inconsistent with the possibility that EMF causes cancer in human beings.

Q. So you do not agree with the ORAU conclusion stated in lines 13 to 19 of your testimony, then?

MR. SMITH: Your Honor, this is argumentative and asked and answered.

JUDGE SMOLEN: No. He was referring to a different quotation.

MR. SMITH: I'm sorry. You are correct.

JUDGE SMOLEN: All right. The objection is withdrawn.

Go ahead.

A. No, I agree with it. I agree with it entirely. I believe these people are trying to express the scientific point of view of an open mind which, of course, I also ascribe to myself. But they make it very clear that while their mind is open on the subject they are extremely dubious of the proposition that a cause-effect relationship could exist.

Q. Where do you find any indication in their language that they are very dubious about the possibility of an association?

A. Beginning on line 17. "It is also clear that the available base of observation and theory" -- and it is a substantial base --

Q. Just so the record is clear, that is your interpolation?

A. Yes.

Q. Go ahead.

A. "Does not satisfy major criteria for causal inference."

Q. And you think that that is the same as saying they are very dubious?

A. Well, it is my interpretative statement, yes.

Q. Now, lastly, I think, you have expressed the conclusion at page 12, lines six to nine, that since EMFs have not been demonstrated to be carcinogenic in your opinion you believe that the Woodbourne-Heaton 230 kV transmission line poses no threat of cancer to persons in the vicinity.

Can you explain why the first half of that -- why the second half of that sentence follows from the first half. And by that I mean does the mere -- and this goes back to your interpretation of the ORAU language too -- does the mere absence of a demonstration enable you to form the belief that the EMFs pose no threat?

A. Yes. And I take it you are asking me to show how the second sentence --

Q. No. The second half of that one sentence follows from the first half. I am asking you --

A. I'm sorry. It is a comma. I'm sorry. I was reading it as a period. I'm sorry. I understand.

Q. Okay. We are on the same wavelength, then?

A. Yes, sir.

Q. I'm asking you how the second half follows from the first.

A. It follows from the first by virtue of the fact that the statement that EMFs have not been demonstrated to be carcinogenic is a statement of enormous power. And it is a statement of enormous power because the opportunity for EMF to have been shown to be carcinogenic is enormous. It is not one study. In other words, we now have a substantial body of evidence to the effect that EMFs are not carcinogenic.

Q. Didn't you conclude that the -- or didn't the NRPB conclude that the Swedish residential study -- and I am reading from page five, lines 28 through 31 of your testimony -- provides weak evidence to suggest the possibility exists? Didn't you report that the NRPB so concluded and didn't you state on page six state that you agreed with that?

(Pause.)

Q. Page six, lines one and two.

A. Mr. Sugarman, I place an interpretation on the language in quotations marks on page five, lines 28 through 31. My interpretation is based on the idea that the language used here is the weakest possible language consistent with not making a categorical statement as befits good scientists. They say, in a way that is uncharacteristic of the chief author of that report, suggests the possibility. That is in my judgement a

double qualification and is intended to imply the lowest level of credibility possible. And therefore I interpret it to mean unlikely in the extreme.

Q. Well, that is still a level of credibility.

A. Well, Mr. Sugarman --

Q. And you said you agree with it.

MR. SMITH: Your Honor, that is argumentative.

A. There are no absolutes in science. In my judgement the available evidence is to the effect that electromagnetic fields are now reasonably demonstrated not to cause cancer in human beings. If you are going to ask me can I stand up and prove to you that it is absolutely guaranteed beyond doubt at a level of knowledge given only to the Divine not to be true, no.

Q. Any data from your study yet in?

A. My study?

Q. Yes.

A. Another six months.

Q. And is your study a third generation study?

A. No.

Q. Second?

A. Yes.

Q. And how long before the third generation studies that are presently in progress are concluded, roughly?

A. Well, it is indeed roughly because I am not privy to the operations, but I think we will begin to see them after the first of the year.

MR. SUGARMAN: Thank you very much.

No further questions.

JUDGE SMOLEN: Mr. Hoffman.

MR. HOFFMAN: Your Honor, as you know, I represent the Office of Trial Staff. We phoned you yesterday morning and I trust you placed on the record a statement on our behalf.

JUDGE SMOLEN: I did.

MR. HOFFMAN: Since I am here today and have been here all day, I would like to make a brief statement if I may.

JUDGE SMOLEN: Please go ahead.

MR. SMITH: We have redirect for this witness.

JUDGE SMOLEN: Yes. Well, I wanted to give OTS an opportunity to --

MR. HOFFMAN: Is there an objection to my making a statement, Your Honor? I think I have the floor.

JUDGE SMOLEN: Wait just a moment.

I want to give the OTS an opportunity to make its statement and you will have an opportunity to proceed with redirect.

MR. SMITH: Okay.

MR. HOFFMAN: I would like to address the question of cross-examining this witness.

JUDGE SMOLEN: Go ahead.

MR. HOFFMAN: I would think that would be appropriate to make at this time.

JUDGE SMOLEN: Go ahead.

MR. HOFFMAN: OTS declines to cross-examine this witness as it declined to cross-examine the other witnesses because it is our position as we have explained in various documents filed in this proceeding that we have not been given an adequate opportunity to prepare and thus we have been denied due process vis-a-vis this case.

Yesterday we got approval of the consulting contract that this whole controversy has been based upon. That was, I believe, May 27 just to put this in perspective. So as of now, we have a consultant under contract who could begin to review the record and the evidence in this case and to begin to prepare testimony and to begin to advise us on the necessary medical-technical aspect of cross-examining witnesses.

Ironically we are now in a process since the proceedings has passed us by that we cannot ask him to perform any work because we will just be expending the Commission's budget in a wasted effort. So our position

has to be to wait until the Commission rules on the document we filed to determine whether or not we are going to be permitted to participate in this case in any meaningful way.

Yesterday we had various conflicts among the Counsel assigned to this case. Had this been a case where we were actively participating as we are in every other case that I have ever known of where we have entered an appearance we would have had a lawyer here one way or the other. Given the fact that we are monitoring this and this is just one step above reading the transcript when it comes through, we felt it was more important to keep our attorneys assigned to the other case work that they had been assigned to.

We will endeavor to continue to monitor this matter, just so you understand what we are doing here. But it will be to sit here in the hearing room to hear the live testimony and cross-examination but to take no part in the case.

JUDGE SMOLEN: I think I spread some of that on the record yesterday in accordance with our telephone conversation. I think I put two paragraphs on similar to or encompassing what you have said, summarizing what you have said.

Have you concluded now your statement?

MR. HOFFMAN: I have, Your Honor. Thank you very much for your indulgence.

JUDGE SMOLEN: That's quite all right.

Let's go now to redirect.

REDIRECT EXAMINATION

BY MR. SMITH:

Q. Dr. Cole, a few moments ago when Mr. Sugarman was cross-examining you, you were asked some questions about association and causation and the relationship between them. Do you recall that testimony?

A. Yes, sir.

Q. At one point it was unclear to me, anyway, whether you were saying that one of those concepts assumes the other or subsumes the other, and I wanted to make sure the record was clear. So could you describe the relationship between association and causation?

A. Certainly. Causation is the more general statement and it subsumes or assumes the presence of association. That is, two factors have to be associated with each other in order to be causal. But two factors that are associated with each other may not be causal.

So it is true that in some studies, some of the studies, there is association between electromagnetic field surrogate exposures and cancer. But it is not true that there is a cause-effect relationship demonstrated

even in those studies in which there is an association.

I hope that clarifies it.

Q. Thank you.

When Ms. McCloskey was cross-examining you about the Swedish residential study by Feychting and Ahlbom, you were asked whether you agreed that the study is a large well done study. Do you recall that?

A. Yes.

Q. If you could take a look at page four, line 11, of your written testimony?

A. Yes.

Q. Where you state that, "The study is actually small -- very small -- with regard to childhood leukemia," do you see any inconsistency between that statement and the statement you agreed to in your answers to Ms. McCloskey?

A. No, not at all. I understood her question when she asked it to actually be a restatement of a statement of mine that she has probably seen someplace in which I say that the study is large. But that is a collective statement regarding the study as a whole, including the population base from which the study is drawn and the total number of cancer cases.

This statement -- and this is in fact the more important statement -- is that with respect to that one

category of disease for which -- and the only category of disease -- for which the study is meaningfully positive, the study is small. People have lost sight of the fact that there are only 38 cases of leukemia in this study. So from the perspective of leukemia, it is in fact quite a small study.

Of course, there were other forms of cancer among children and cancers among adults. Essentially all of that was negative.

Q. Thank you.

When Ms. McCloskey was cross-examining you she asked you a number of questions about whether data end points in the Swedish study are statistically significant. Do you recall that?

A. Yes, sir.

Q. Just so the record is clear, could you please describe to us the role that statistical significance plays in your analysis of the meaning of epidemiologic data?

A. Statistical significance is ordinarily taken as a necessary but not sufficient criterion of meaningful association. And generally speaking I would buy into that interpretation.

However, in a study such the Swedish study where dozens, perhaps hundreds, of comparisons have been made,

and let's not forget that there are ten forms of cancer evaluated in that study, there were two time periods, there are three exposure assessments. Let's see, ten times two times three is 60. There are 60 primary comparisons; hundreds of secondary. One is going to see statistical significance occurring to which you cannot attach the normal meaning of statistical significance. That is, in our language we would say that the statistical significance is only nominal. That is not meaningful because so many comparisons have been made that one would expect some of them to appear to be not due to chance when in fact they are consistent with chance.

In the Swedish study statistical significance has a very special and very limited meaning. The study is in fact positive only for childhood leukemia and even for that one category of disease there are major flaws that detract from any causal interpretation.

Q. Do you also recall when Ms. McCloskey was cross-examining you that she asked you with regard to the Sahl study where Dr. Sahl is employed?

A. Yes.

Q. Are you familiar with the other authors or do you know anything about the other authors of this study?

A. I'm not familiar at all with the second author,

Kelsh. I know the third author, Dr. Sander Greenland, very well.

Q. Where does he work?

A. Dr. Greenland is a professor of epidemiology and biostatistics at UCLA.

Q. Now, when you evaluated this study did the employment position of its authors affect your analysis?

A. No. Of course not. It is something that I pay no attention to. I pay much more attention to the individual reputations of the people.

Q. And do you know anything about the individual reputations of these authors?

A. I don't know the first two. But Dr. Sander Greenland is one of the most well known and most well respected epidemiologic analysts in the world.

Q. Thank you.

Finally, during Ms. McCloskey's cross-examination she asked you whether in the Sahl study any data on age-adjusted risk ratios was reported. Do you recall those questions?

A. I think she might have asked about age-adjusted SMRs.

Q. Of course.

A. There are in fact age-adjusted risk ratios.

Q. Does the absence of the type of data described

by Ms. McCloskey in her question from the Sahl study -- let me back up and reask that question. What effect on your analysis of the Sahl study does the absence of that data have?

A. Well, this is a rather intriguing hypothetical criticism that has been made of this study. That is, it has been suggested by others as well as Ms. McCloskey that the absence of --

MS. McCLOSKEY: Your Honor, I would object. I did not make any suggestion that this was a criticism. I just asked him if they appeared in the study.

JUDGE SMOLEN: We will sustain your objection.

THE WITNESS: Pardon me. I'm sorry. I spoke out of turn.

It has been suggested by others that the absence of SMRs in the study is a serious limitation. In fact this is a reflection of a misunderstanding of the nature of this study.

This study is a nested case-control study. It is not a follow-up study or cohort study of the usual variety. This study is superb in its design. Is a double study. It is a study within a study. And the focus of the analysis is on the case-control components for lymphoma, brain cancer and leukemia in adults within a context of heavily exposed people, that is, utility

workers. So our focus on the analysis and the analytic tools is on those of the case-control study not those of the follow-up study. And of course it is categorically negative for all forms of cancer in that context. So the absence of a SMR is irrelevant.

MR. SMITH: No further questions.

JUDGE SMOLEN: Is there any recross based upon this redirect? Let me ask Law Bureau.

MS. BURKET: No, Your Honor. I have no questions.

JUDGE SMOLEN: PP&L.

MR. DILLON: No, Your Honor.

MS. McCLOSKEY: Can I --

JUDGE SMOLEN: Mr. Sugarman. I'll give you a shot.

MS. McCLOSKEY: I just want a few minute before I answer yes or no.

JUDGE SMOLEN: Mr. Sugarman.

MR. SUGARMAN: No, Your Honor. Thank you.

JUDGE SMOLEN: Let's give the OCA a few moments.

(Pause.)

MR. SUGARMAN: Your Honor, there is one question I would like to ask based on redirect.

RECROSS-EXAMINATION

BY MR. SUGARMAN:

Q. In talking about statistical significance, Dr. Cole, it reminded me of your testimony at the earlier

hearing that statistically you would expect to see a -- that more exposure would be consistent with more leukemia if there were an association. Do you remember your testimony in that regard?

A. I don't specifically recall it, but that is certainly a statement that I would accept and make again.

Q. And that is based upon statistical theory, isn't it?

A. Oh, no. Not at all. It is based on an understanding of biology and carcinogenesis.

Q. So if you didn't see it why would it disturb you?

A. There is no known human carcinogen, neither chemical, physical nor biological that fails to show some type of monotonic ascending dose-response relationship. Or in plain English, the more of it you are exposed to the greater the risk of cancer. There is no exception to that. Therefore, if EMF were to be a human carcinogen I would expect it to display the same kind of relationship.

Q. Well, did you notice that -- do you agree that in the Swedish study the dose-response relationship was consistent, that the higher the dose the more the incidence of cancer?

A. The Swedish study is represented to be positive for leukemia by its authors. It is in fact true that

part of their argument rests on a perception that there is a dose-response relationship. But in fact there are a number of major detractions from a positive interpretation of the leukemia association in that study.

Number one, the dose-response relationship is seen only with calculated magnetic fields and distance. As I have already indicated in my response to one question, those are in fact the same measure. One is incorporated in the other. There is no dose-response when the information is looked at in terms of spot measurements.

Secondly, there is no association in apartment homes.

Number three, the study was divided in two time periods. It was negative in the larger of those time periods.

And number four, and something that has as yet been unappreciated, is the study does not show dose-response when the most valid measure of dose is used, namely duration of residence within the corridors. You will recall that that study had as a condition of inclusion of subjects that they lived within 300 meters of powerlines. It stands to reason that if there is a cause-effect relationship the longer a child has lived in the corridor, the higher the risk. In fact the available evidence are contrary. The children who have lived in

the corridors for the longest period of time have the lowest risk of leukemia.

And finally let's keep in mind that although we don't know the actual numbers for leukemia, we can estimate them. And there are in fact some 39 cases of leukemia in this study observed and about 35 expected. So there is no meaningful -- from an epidemiologic perspective 39 and 35 are the same number when you're dealing with large populations over long periods of time. There is no meaningful excess of leukemia among children living within 300 meters of powerlines. Thirty-nine observed, 35 expected.

Q. Well, Doctor, but if you break that down -- you didn't answer my question. You gave us very interesting information but my question was didn't the Swedish study show -- doesn't the data in the Swedish study show that the more the exposure within the subgroups that were studied, the more the exposure, the more the cancer.

MR. SMITH: I think that was asked and answered.

JUDGE SMOLEN: I will let him answer again.

A. My answer in short is what when two measures were used, yes. When two other measures were used, no.

BY MR. SUGARMAN:

Q. All right. And isn't it true that, when you say something --

A. Well, wait a minute. It isn't and isn't it true. This thing wasn't true.

Q. Isn't it true that when you used the phrase stand to reason, when you say that length of exposure, it stands to reason will increase incidence if there is a cause-effect relationship, that the term stands to reason is an unexamined hypothesis?

A. Not at all. It is a fully examined hypothesis. I have already indicated to you there is no example, at least none of which I am aware, of a cause of cancer which fails to show some type of ascending relationship between risk and exposure. So this is a pretty thoroughly examined proposition.

Q. But aren't there many different measures of exposure besides temporal longevity?

A. I assume you mean temporal duration of residence.

Q. Yes.

A. Yes. There are four that we can look at in this study and two are positive and two are not. I am just submitting that the ones that are not positive are as valid, possibly more, because they are objective and single measurements than the two that aren't.

Q. How is it not circular when you said the following two things: all known carcinogens have

dose-response relationships, and then you said, as I understood it, an agent is not carcinogenic unless there is a dose-response relationship. Aren't those circular statement?

A. As presented by you, yes. You are suggesting that there is a qualification necessary to be met.

What I was trying to say was this: all known examples, all recognized human carcinogens, show a dose-response relationship. If a new factor is to be put forward then my test is that it should meet that criteria. But if not, if not, then we have got to have an understanding of why not.

So, yes, I can conceive of a carcinogen that does not show a relationship of one of the more typical types. But let's face it, we are skeptical of that. We have to have an understanding, why would it be that this thing would display a physical reality that has never been seen before despite the fact that we have examples from the very same area, physical carcinogenesis and we have chemical and biological. So what is being proposed here is that EMF is unique in some way.

Q. Isn't it true that the method of cancer by which doses lead to cancer in general is very poorly understood? That there is almost no demonstrated, to use your earlier words, there is almost no demonstrated

method by which any agent causes cancer?

A. I disagree with that entirely. The mechanism of action of many carcinogens is well known. That is not to say all of them.

Q. How many -- what proportion of the body of possible agents -- as to what proportion of the body are the mechanisms known?

MR. SMITH: Your Honor, I just want to note that we are quite a ways beyond the scope of the cross-examination.

JUDGE SMOLEN: Well, I am going to let it go for a little while because it is in response to one of the Doctor's prior answers, or this question is.

Go ahead.

A. Mr. Sugarman, I cannot give you a number that I would then be prepared to defend. But I would say that some substantial proportion, perhaps 20 percent or so, of agents can be classified according to mechanism of action provided that the mechanism of action category is not -- does not have to be specified at the molecular level but, rather, at the cellular level.

BY MR. SUGARMAN:

Q. That being the case, isn't it likely based on your analysis that if all the work that has been done has failed to provide an answer to the cause of cancer,

despite all the work that has been done, the categories of dose-response relationship that are being used are not sufficiently complex to explain or to allow an explanation and that finding something other than those four is the name of the game in terms of finding the causes of all cancer, or 80 percent?

A. I'm sorry. Finding other than what four?

Q. Despite all the scientific investment that has been made into finding the causes for cancer, the framework of the four common dose-response relationships has been inadequate to enable the researchers to come up with a solution, that the causes are more complex and there is more of an interrelationship between them and other possible causes than existing research has demonstrated, and the same thing is true of EMF.

A. I hope I don't distract us, but you said something about there being four established cause-effect relationships? Dose-response relationships?

Q. Four common dose-response relationships.

A. You are not attributing that to me, are you?

Q. I was.

A. No, no. I didn't say that.

Q. I misheard you, then.

A. I probably misspoke or --

Q. Probably the word four, you probably said

something else and I --

JUDGE SMOLEN: Well, instead of dealing in probabilities, let's get a question and an answer.

BY MR. SUGARMAN:

Q. Whatever the number is, isn't it the case that the existing framework of commonly hypothesized dose-response relationships has been inadequate to explain the cause of cancer?

A. The idea that electromagnetic fields will be found to be a human carcinogen and will do this as essentially an universal promoter and will have a very special type of dose-response relationship previously unseen is a set of possibilities that some people may choose to embrace. I don't.

Q. But that was not my question.

A. I understood that to be your question.

Q. No. I didn't say previously unseen. I said that involved something more complex than what has been studied up to now. In other words, in most cases I mean the potential interrelationships between the dose-response relationships that exist and are treated as separate ones by you in your testimony, but that they interrelate with each other, as well as the possibility of others that are not normally considered.

For example, the relationship between the

dose-response -- the longevity of exposure and the variation in intensity, or the difference between the longevity of exposure and the time exposed in terms of apartment houses versus single family residences and their proximity to the line and their proximity to other confounding variations in the exposure. Isn't it possible that all this is a lot more complex?

MR. SMITH: Objection. That has got to count as a compound question, Your Honor.

MR. SUGARMAN: I will withdraw the question. I think we have gone as far as we are going to go with this.

JUDGE SMOLEN: All right.

OCA.

MS. McCLOSKEY: Thank you, Your Honor.

RECROSS-EXAMINATION

BY MS. McCLOSKEY:

Q. Dr. Cole, I think you clarified on redirect that your statement on page four, lines 11 to 12, where you referred to the study being small in regard to childhood leukemia. Was the study also small in regard to to children with brain tumors?

A. Ms. McCloskey, I'm sorry. I have just forgotten the number.

Q. Okay. If you could look at Table 2.2. I think

you still have the study in front of you.

A. 2.2?

Q. Yes.

A. I've got it.

Q. Am I correct there that there were 33 cases of children with brain tumors?

A. Yes. So it is small for brain tumors as well, certainly.

Q. Is it also, then, small with 19 cases for children with lymphoma?

A. Yes.

Q. And is it also small with ten cases for children with kidney tumors or Wilms tumors?

A. Yes, of course.

Q. And as to the 41 children who fell in the other category, you consider the number 41 to also be small?

A. Yes, I do. Not because it is so very small in itself but it has to be subdivided among a number of cancers. Presumably there is more than one in that category. We have a pretty good idea of what is in that other category. It is at least three entities.

Q. Now, you also --

A. May I just say by way of comparison that if we look at the adults, we see case series that are typically 50, 60 or 70 or so, that is double or triple the size of

the case series that's in the same table, the case series for children.

Q. But also in some of the adults we see, for example, for some subcategories we see only 14 adults with acute lymphatic leukemia, correct?

A. Correct.

Q. And that would be the smallest category for the adults?

A. Correct.

Q. Now, you also in redirect mentioned that -- let me just back up. In my cross-examination I had asked you the question as to whether the Sahl study provided any age-specific relative risks and I believe you answered no. And in redirect you mentioned that it had presented some age-adjusted data, is that correct?

A. Yes.

Q. And those are two different types of data?

A. Absolutely. Very different.

Q. Just so the record is clear, if you could tell us the different between the two types of data?

A. Age-specific relative risk is a relative risk that pertains to a narrow segment of age. For example, 40 to 59, or 60 and over, or under 20, something of that sort. So age-specific means restricted as to age.

Age-adjusted means unrestricted as to age but

manipulated statistically in such a way that comparisons are devoid of any effect of age.

MS. McCLOSKEY: Thank you, Your Honor. I have no further redirect.

JUDGE SMOLEN: Anything further?

(No audible response.)

JUDGE SMOLEN: By any Counsel of this witness?

(No audible response.)

JUDGE SMOLEN: Hearing no response, the witness is excused. Thank you very much, sir, for appearing and testifying.

(Witness excused.)

JUDGE SMOLEN: Does that conclude today's witnesses?

MR. BONNEY: Yes, Your Honor.

JUDGE SMOLEN: All right. Then we are going to adjourn today and resume Tuesday morning at 10:00 a.m. for...

MR. SMITH: For Dr. Gelmann.

JUDGE SMOLEN: Yes, Dr. Gelmann.

Then the hearing is adjourned today. And thank you very much for appearing and have a nice weekend.

Before we go off...

MR. SMITH: Your Honor, perhaps we ought to take a moment here to make sure whether we need to show up on

Tuesday. Because it is unclear to us whether the parties actually have cross-examination.

JUDGE SMOLEN: Let's find out.

Ms. McCloskey and Mr. Sugarman, Counsel for the company has asked whether or not there will be cross-examination of Dr. Gelmann on Tuesday. Because if there won't be, he is suggesting that the testimony be stipulated.

MR. SMITH: Yes.

JUDGE SMOLEN: I don't know whether there will be cross-examination.

MR. SUGARMAN: I have some.

JUDGE SMOLEN: All right. Then that's it. Mr. Sugarman has indicated that he has some cross-examination. And that is not to preclude others from cross-examining. But we will reassemble Tuesday at 10:00 a.m.

MR. SUGARMAN: I find myself having received notice today of a hearing in Delaware County on Tuesday morning and in the time I had this morning I could not see where I am on the list or whether I can arrange it. I am sort of between a rock and a hard place. I just don't know how long the list is out there and I don't want to ask that we defer things here and then find myself on a list out there that they don't get to me until the afternoon

anyway. I have a feeling that there is a list out there and I will be able to work something out out there.

MR. SMITH: A possible alternative suggestion, Your Honor, is cross-examination by interrogatory if it is limited. Ms. McCloskey has indicated that she might be able to do that with Dr. Gelmann.

MR. SUGARMAN: Well, I just find that doesn't work.

JUDGE SMOLEN: I think it might delay the proceeding, because if there is cross-examination you may have redirect, et cetera.

Let's reassemble Tuesday, 10:00 a.m., and we will see if Mr. Sugarman works his potential conflict to a satisfactory resolution.

(Whereupon, at 11:37 a.m., the hearing was adjourned, to be reconvened at 10:00 a.m. on Tuesday, June 1, 1993, in Philadelphia, Pennsylvania.)

C E R T I F I C A T E

I hereby certify, as the stenographic reporter, that the foregoing proceedings were taken stenographically by me and thereafter reduced to typewriting by me or under my direction; and that this transcript is a true and accurate record to the best of my ability.

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BEFORE THE

PENNSYLVANIA PUBLIC UTILITY COMMISSION

DIRECT TESTIMONY ON REMAND

OF

PHILIP COLE

ON BEHALF OF

PHILADELPHIA ELECTRIC COMPANY

May 12, 1993

DIRECT TESTIMONY OF PHILIP COLE

I. Introduction and Background

Q. Please state your name and business address.

A. Philip Cole, Department of Epidemiology, School of Public Health, University of Alabama, Room 203 Tidwell Hall, University Station, Birmingham, Alabama 35294.

Q. What is your occupation?

A. I am a professor and researcher in epidemiology and a medical doctor.

Q. Dr. Cole, have you previously testified in this proceeding?

A. Yes, I submitted written testimony (PECO Rebuttal Statement No. 2) in November 1991. My previous testimony addressed the epidemiologic research concerning power frequency electric and/or magnetic fields.

II. Scope of Testimony

Q. What were you asked to do for the remanded hearings in this proceeding?

A. I was asked to identify epidemiologic studies and materials concerning power frequency electric and/or magnetic fields that have become available to me since my previous testimony in this proceeding, to analyze those studies, and to provide my professional opinions and conclusions regarding those studies.

III. Recent Epidemiologic Research on EMF and Cancer

Q. Please provide a brief overview of the recent epidemiologic studies and materials concerning power frequency fields that you will address in your testimony.

A. I would like to discuss several new epidemiology studies, the most important of which are the occupational study by Sahl et al. (1993) and the Swedish residential study by Feychting and Ahlbom (1992). In addition, I would like to discuss two

reviews of the research issued respectively by the United Kingdom's National Radiological Protection Board ("NRPB") and the Oak Ridge Associated Universities ("ORAU"). As in my previous testimony, I would like to discuss the research on childhood cancers separately from the research on adult cancers.

A. Recent Research on EMF and Childhood Cancer

Q. Please describe the recent epidemiological research on EMF and childhood cancer.

A. Three recent reports are relevant to childhood cancer, Feychting & Ahlbom (1992), Olsen (1992), and Jones et al. (1993).

Feychting & Ahlbom

Q. Please describe the study by Feychting & Ahlbom.

A. The study by Feychting & Ahlbom (1992) is a residential case-control study of childhood and adult cancer, generally referred to as the "Swedish residential study." The study population was limited to people who, at some point in their life, had lived within 300 meters of high voltage transmission lines in Sweden.

Q. Please explain how magnetic fields were estimated in the Swedish study.

A. The researchers had access to data on past loading (amperage) of transmission lines in Sweden. For each transmission line, they calculated the average annual magnetic field that would have been associated with that loading. Using this information the researchers estimated, for each residence in the study, the average annual strength of the magnetic field that the transmission line would have created in that residence. Most of Feychting & Ahlbom's data based on this method of estimating exposure deal with the average annual magnetic field associated with the transmission line in the year immediately prior to a child's diagnosis with cancer.

For some residences, spot measurements of magnetic fields were made in the residences.

Q. What are the results of the Feychting & Ahlbom study for childhood cancer?

A. There are several important results. First, the researchers calculated that, if the children who lived in the transmission line corridor had cancer at the same rate as the general childhood population in Sweden, 138 cases of childhood cancer would be expected in the children who had lived in the transmission line corridor. In fact, 142 cases of childhood cancer were observed among those children.

This means that children in Sweden who live close to a transmission line develop cancer at virtually the identical rate as the general population of children in Sweden. This is very strong evidence that this is a negative or null study -- that is, that residence near high voltage transmission lines is not associated with increased risk of cancer in children.

Q. Was this information included in the original publication of the Swedish study in 1992?

A. No, it was not. The paper released in 1992 by Feychting & Ahlbom was not published in a peer-reviewed journal. Information about the study, and further data from the study, are still becoming available as the scientific community reviews the study and asks questions about it. The information on observed and expected cancers was compiled and released by the researchers in early 1993 in response to inquiries from members of the scientific community (Ahlbom & Feychting 1993).

Q. What are some of the other important childhood cancer results of the Feychting & Ahlbom study?

A. The data are null (no difference between cases and controls) or inverse (cancer risk decreases with higher exposures) for "all childhood cancers," and childhood brain cancers, whether exposure is estimated using calculated fields or with actual magnetic field measurements. The study is weakly positive for childhood leukemia.

Q. Please further describe the Feychting & Ahlbom data on childhood leukemia.

A. First, although the study base of this study was all children who had ever lived in the transmission line corridor from 1960-1985, the study is actually small -- very small -- with regard to childhood leukemia. Only 38 cases of childhood leukemia were identified in the entire study base.

Second, children who lived in the transmission corridor for the largest percentage of their lives had the smallest risk of cancer. For children who lived in the corridor from birth to diagnosis, residence in a higher-field home was associated with no increase in cancer risk.

Third, the data exhibit inconsistent patterns that would not be seen if EMF were a cause of cancer. For example, the positive association with childhood leukemia is restricted to certain types of residences (single-family homes but not apartments), and it does not exist for children who were diagnosed with cancer from 1960-74. A true cause of cancer would cause cancer in both types of residences, and during all time periods included in the study.

Finally, there is no association between childhood leukemia and actual magnetic field measurements in this study. In fact, the data on measured magnetic fields are inverse -- that is, they show a substantially lower incidence of cancer in children who resided in homes with higher field measurements.

Q. What do you conclude from the childhood cancer data in the Feychting & Ahlbom study?

A. The study is negative for "all cancers" and brain cancers. It is weakly positive for leukemia. However, because of the inconsistencies in the childhood leukemia data, it is my view that those data support a null or negative interpretation as easily as a positive interpretation.

Report of the National Radiological Protection Board

Q. Please describe the scientific review by the United Kingdom's National Radiological Protection Board ("NRPB") you mentioned earlier in your testimony.

A. In 1992, a Scientific Advisory Panel of the NRPB, chaired by the eminent epidemiologist Sir Richard Doll, released an extensive review of the EMF epidemiology and concluded that:

> "In summary, the epidemiological findings that have been reviewed provide no firm evidence of the existence of a carcinogenic hazard from exposure of paternal gonads, the fetus, children, or adults to extremely low frequency electromagnetic fields that might be associated with residence near major sources of electricity supply, the use of electrical appliances, or work in the electrical, electronics, and telecommunications industry."

Q. Did the NRPB's Scientific Advisory Panel review the Swedish residential study?

A. They did not review it in their original report. However, in March 1993 they issued an addendum to their report (NRPB 1992), in which they conclude that the recent residential studies, including the Swedish residential study, ". . . do not establish that exposure to EMF is a cause of cancer, although they provide weak evidence to suggest the possibility exists." They also stated, after reviewing the Swedish study, that "[A]t present, epidemiological studies do not provide an effective basis for quantitative restrictions on exposure to electromagnetic fields."

Q. Do you agree with the conclusions of the NRPB Panel?

A. Yes.

Report of the Oak Ridge Associated Universities

Q. Please describe the scientific review by the Oak Ridge Associated Universities ("ORAU") you mentioned earlier in your testimony.

A. The Oak Ridge Associated Universities is a private, not-for-profit consortium of 62 universities. At the request of the federal government's Committee on Interagency Radiation Research and Policy Coordination ("CIRRPC"), ORAU formed a scientific panel to review the EMF research. With regard to the epidemiologic research, the panel concluded that:

> "The question whether ELF-EMF has a carcinogenic effect represents an intriguing scientific problem. ELF-EMF clearly cannot be exonerated, since very large and valid studies showing no association with cancer occurrence do not currently exist, but it is also clear that the available base of observation and theory does not satisfy major criteria for causal inference."

Q. Did the ORAU Panel review the Swedish residential study?

A. Again, they did not review it in the original report. In April 1993, however, the Panel published a letter in Science magazine in which they state:

> "Because the two Swedish studies were made public when the ORAU report was already in the printing process, we consider it necessary to indicate that, in our opinion, the evidence presented in these studies is not sufficiently compelling to alter the conclusions of the ORAU report."

Q. Do you agree with the conclusions of the ORAU panel?

A. Yes, I do. I would note, however, that since the ORAU report was issued, a "very large and valid stud[y] showing no association with cancer occurrence" has been published. This study, by Sahl et al. (1993), is discussed later in my testimony in the section on adult occupational studies.

Olsen et al.

Q. Please describe the study by Olsen et al.

A. The Olsen research is a residential case-control study of childhood cancer being conducted in Denmark. At the present time, only a short, preliminary report of partial results is available. Those preliminary results indicate that, in the Danish study population, living in a home near a high-voltage electric facility is not associated with increased risk of childhood leukemia or childhood brain tumors. The authors report an increased risk of childhood lymphoma based on three cases. I give the study little weight at this time since the reported data are both partial and preliminary.

Jones et al.

Q. Please discuss the study by Jones et al.

A. The Jones study is an evaluation of the method used in some of the previous EMF epidemiology research -- the wiring code. The Jones data suggests that the positive results of the wiring code studies may have been falsely created by "residential mobility bias."

Conclusion Regarding the Childhood Cancer Studies

Q. Dr. Cole, taking into account the recent research on childhood cancer, what is your overall conclusion concerning the childhood cancer studies?

A. There is no demonstrated increased risk of childhood cancer when actual electric or magnetic fields were measured. When surrogates such as the wiring code, or the estimated fields used in the Swedish study, were used in the childhood studies, the data have shown a pattern of mixed results between weak positive and negative outcomes. This is not the pattern of cause and effect. I conclude that the epidemiologic evidence on EMF, when viewed as a whole, provides no persuasive scientific support for the hypothesis that EMF causes cancer in children.

B. Recent Research on EMF and Adult Cancer

Q. Please describe the recent research on EMF and adult cancers.

A. With the exception of the study by Sahl et al. (1993), the recent research on adult cancer does not introduce significant changes in study design or method from the previous research in this area. I would thus like to briefly review this other research and then discuss the Sahl study in some detail.

Adult Residential Studies

Q. Please describe the recent adult residential studies.

A. The Swedish residential study by Feychting & Ahlbom (1992) is uniformly null or negative, for both measured fields and calculated fields, for all forms of adult brain cancer and for all forms of adult leukemia except chronic myeloid leukemia, which shows a small increase for calculated fields.

A residential study in the Netherlands (Schreiber et al. 1993), which used the retrospective follow-up method, was also released. This study is also negative, showing no association between residence near transmission facilities and cancer risk.

In the last year I became aware of a case-control study from Poland for which two separate reports have been published, one discussing residential data, the second discussing occupational data. (Gajewski et al. 1989 and Pachocki et al. 1991). The residential data from this study are negative, but non-persuasive due to limitations in the study.

Adult Occupational Studies

Q. Please describe the recent adult occupational studies.

A. There are four recent occupational case-control studies (Gajewski et al. 1989, Richardson et al. 1992, Floderus et al. 1992, Matanoski et al. 1993). Three are positive and one is

negative. None provides a significant improvement in method or design, and I do not believe that they add substantially to our knowledge.

In addition, Guenel et al. (1992) have released preliminary results of a retrospective follow-up study from Denmark. The preliminary results are negative or null for breast cancer, melanoma, and brain cancer in both men and women. For leukemia, it is weakly positive for men and negative for women.

Sahl et al.

Q. Please describe the occupational study by Sahl et al. (1993).

A. The Sahl study is by far the most comprehensive and best designed of the occupational studies. It represents a substantial improvement over all previous occupational studies and essentially redefines the quality level for research in this field.

First, unlike previous occupational studies, the Sahl study used the main approach to exposure assessment -- actual measurements of magnetic fields in the workplace -- rather than a peripheral approach such as use of job titles to categorize exposure. Workers were equipped with meters to record their magnetic field exposure during their full work shifts and this information was used to characterize magnetic field exposure in the jobs evaluated in the study. This is a valuable improvement over previous studies which utilized job titles to estimate EMF exposure because it provides a much more reliable basis to compare "exposed" versus "non-exposed" workers. Indeed, based on the workplace measurements, Sahl was able to identify categories of workers who had significantly higher magnetic field exposures. The significance of this is that Sahl was able to evaluate cancer

risks among truly exposed individuals, which strengthens the results of the study substantially.

Second, the study consists of four separate investigations: a cohort (retrospective follow-up) study and three case-control studies. This is a distinct improvement over previous occupational studies in that the Sahl study addresses the question of whether EMF occupational exposure is associated with cancer using a variety of methods.

Third, the size of the study is quite large: cancer deaths among more than 36,000 electric utility workers were examined for the period 1960-88. During that period, in the normal course of events more than 3,000 of the utility workers died, including more than 700 who died from cancer.

Fourth, the thoroughness of the analysis in the Sahl study is impressive. The study considered a number of different measures of workplace magnetic field exposure, including: (1) mean; (2) median; (3) 99th percentile; (4) fraction exceeding 10 mG; and (5) fraction exceeding 50 mG. For each of these measures of exposure, the study examined associations with leukemia, brain cancers, and lymphoma. In addition, Sahl evaluated a number of possible "latency period" assumptions by considering exposure windows of 10 and 20 years in combination with assumed latency periods of 2 and 5 years. No other EMF occupational epidemiology study has employed such a comprehensive analysis.

Q. What are the results of the Sahl study?

A. The results of the study are consistently negative. This is true of the cohort study and the three case-control studies. No consistent associations were found between any measure of workplace magnetic field exposure and leukemia, brain cancer, or lymphoma risk -- even among those workers who had

significantly higher exposures to magnetic fields. In addition, there is no evidence of a dose-response relationship in the study, and there is no indication that the negative results of the study were significantly influenced by confounding or other factors.

Conclusion Regarding the Adult Cancer Studies

Q. Dr. Cole, taking into account the recent research on adult cancer, what is your overall conclusion concerning the adult cancer studies?

A. These studies show weakly positive to zero to inverse associations. The studies that use the most acceptable design consistently have found no association between EMF and cancer. The recent Sahl study is a substantial improvement in study design and method and it is uniformly negative. I conclude that this body of studies does not support the hypothesis that EMF is associated with adult cancer, and that the best-designed and conducted studies provide strong support against that hypothesis.

IV. Conclusions Regarding the EMF Epidemiologic Research and Regarding Right-of-Way Width Standards

Q. Dr. Cole, taking into account the recent epidemiologic research, what is your opinion on the relationship between power frequency electric and/or magnetic fields and cancer in humans?

A. Taken together, the epidemiologic reports fail to demonstrate any strong or consistent pattern of association between EMF and cancer in human beings. The summation can only be that, to date, there is no demonstrated relationship between EMF and cancer in human beings.

Q. Dr. Cole, in your opinion does the epidemiologic research, taken as whole, provide a basis for setting a standard for right-of-way width or for a specific milligauss level at the edge of the right-of-way for the Woodbourne-Heaton 230 kV line?

A. No. Since I am of the opinion that EMFs have not been demonstrated to be carcinogenic, I believe that the Woodbourne-Heaton 230 kV transmission line poses no threat of cancer to persons in the vicinity. The epidemiologic research thus provides no reason to set any standards related to this line. Moreover, it does not provide any effective basis for setting such a standard.

Q. Does this conclude your testimony?

A. Yes.

Dr. Philip Cole

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