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COMMONWEALTH OF PENNSYLVANIA

PUBLIC UTILITY COMMISSION

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| 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 hearing. | Docket No. A-110550F055 |

Pages 1069 through 1135

Hearing Room No. 1
State Office Building
Broad and Spring Garden Streets
Philadelphia, Pennsylvania

Wednesday, November 20, 1991

Met, pursuant to adjournment, at 10:00 a.m.

BEFORE:

HERBERT SMOLEN, Administrative Law Judge

APPEARANCES:

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9th

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C O N T E N T S



E X H I B I T S



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PROCEEDINGS

ADMINISTRATIVE LAW JUDGE HERBERT SMOLEN: This is a further hearing in the matter of the Philadelphia Electric Company Woodbourne-Heaton line, Docket A-110550, Folder 055.

Mr. Watson, or Mr. Bonney, are you prepared for your next witness?

MR. WATSON: Yes, Your Honor.

I call Dr. Philip Cole.

Whereupon,

PHILIP COLE

having been duly sworn, testified as follows:

JUDGE SMOLEN: Please have a seat. Keep your voice up. Welcome to the PUC. State your full name and business address for the record.

THE WITNESS: My business address is 203 Tidwell Hall at the University of Alabama at Birmingham.

JUDGE SMOLEN: Mr. Watson.

DIRECT EXAMINATION

BY MR. WATSON:

Q. Dr. Cole, do you have in front of you a copy of a document entitled Rebuttal Testimony of Dr. Philip Cole and also labeled Philadelphia Electric Company Rebuttal Statement No. 2?

A. I do.

Q. Do you have any typographical additions or corrections to make to this document?

A. Yes. There are two typographical errors.

JUDGE SMOLEN: Before you start, is there an extra copy of the testimony? What I want to do is for Mr. Sugarman's sake I am going to make an extra copy of the corrections and when he comes in I will give it to him. We can save some time that way.

Go ahead.

BY MR. WATSON:

Q. The corrections?

A. Yes. There are two typographical errors in the testimony that I would like to correct now. The first one is on page 19, line 42. The name spelled "Branford" should be changed to "Bradford", with a "d".

Page 20, line ten, the word "the" should be deleted.

Q. Dr. Cole, are there any other additions or corrections you would like to make to your testimony to bring it completely up to date?

A. Yes. The November 1 issue of the American Journal of Epidemiology was published yesterday and it contains the final version of a report that is commonly referred to as the Peters study. I have had a chance to review that final report and as a result of that there

are a number of changes which convert the preliminary nature of my comments regarding that study to much more firm comments. So there are several changes that I would like to make for that reason.

Q. Let me just ask you, were there any material changes between the final report of Peters and the information that you have in your testimony about the Peters study?

A. No. The changes are not material. They basically represent a firming up of results with which I was already somewhat familiar.

Q. And which are referred to in your testimony?

A. Yes, sir.

Q. Could you give us the specific page and line numbers for those changes?

A. All right. There are some ten or so of these.

Page 12, line ten is the first one. The word "five" should be changed to the word "six".

Page 12 --

JUDGE SMOLEN: Go a little slower here.

THE WITNESS: Certainly.

(Pause.)

JUDGE SMOLEN: Go ahead.

THE WITNESS: Page 12, line 12, I would add to the end of the line the words "and London (1991)."

BY MR. WATSON:

Q. Could you tell us what London has to do with Peters?

A. The final report of what is referred to as the Peters study actually appears under the name of Stephanie London as the senior author. Peters is also included in the authorship.

Q. Am I correct in understanding that is one and the same study?

A. That's the same study, yes, indeed.

Q. Please continue.

A. Page 16, line six, I would ask that we insert the words "one of" before the expression "the best". So it now says "should be considered one of the best".

Page 17, beginning on line 35 with the word "since", I would strike the rest of that response from the word "since" to the very end. And in that same location add the sentence "I have also reviewed the final report of the Peters study."

JUDGE SMOLEN: Read the whole thing, then.

THE WITNESS: Okay. The whole response now reads as follows: "I was a member of the advisory committee which sponsored the Peters study and was present at the advisory committee meeting at which Dr. Peters presented his preliminary results," deleting the remainder now and

adding, "I have also reviewed the final report of the Peters study.

JUDGE SMOLEN: Following the word study --

THE WITNESS: That is the end.

JUDGE SMOLEN: What would the next thing be? "At present", or is that also stricken?

MR. WATSON: Your Honor, everything is stricken from the word "since" through "findings".

JUDGE SMOLEN: So the final two sentences are also stricken?

THE WITNESS: From the word "since" to the end of the original response is stricken, and then I add in a sentence.

JUDGE SMOLEN: All right. What is the next one?

THE WITNESS: Going now to page 18, line one, perhaps it would be easiest if I simply read this sentence now. There are two changes in it.

JUDGE SMOLEN: All right.

THE WITNESS: "Not enough design information and data were available." So we change "are" to "were". "Not enough design information and data were available from the Peters study at the time I originally prepared this testimony." And then it continues as is, that is, "to conduct a detailed epidemiologic evaluation."

I'm sorry. There are two more changes in that

response. The next one is 18-three. The word "cannot" should be changed to "could not". And at 18-four, change "is" to "was".

Page 18, line nine, delete, please, the first clause "with the limitations just expressed". Delete that and add the word "now" after the word "can". So that it says, "I can now state my..." and delete the word "preliminary". "I can now state my opinions."

Eighteen-16, delete the word "preliminary".

Eighteen-19, delete the word "preliminary".

BY MR. WATSON:

Q. Are there any more additions, Dr. Cole?

A. I just want to check here to make sure I have not omitted any.

(Witness perusing document.)

Yes, there is one more addition that I would like to make, and that is on page 31. After the reference to the work of Linet I would like to add in a reference to the work that represents the final report of the Peters study. So that reference which would go in there would read as follows: "London, S.J., Thomas, D.C., Bowman, J.D., et al., Exposure to Residential Electric and Magnetic Fields" -- that was Exposure to Residential Electric and Magnetic Fields -- "and Risk of Childhood Leukemia."

Next, the reference to the journal itself is as follows: first is the abreviation "Am." That is for American. Next "J", for Journal, and finally "Epidemiol" for Epidemiology. That is underlined, "American Journal of Epidemiology".

Next is the year, 1991,

Next, the volume number, 134:923-37.

That concludes the changes that I would like to make to my testimony.

JUDGE SMOLEN: Just give me a second here.

(Pause.)

JUDGE SMOLEN: We are ready to go?

BY MR. WATSON:

Q. Dr. Cole, if I were to ask you now while you are on the stand under oath each of the questions set forth in this document entitled Rebuttal Testimony of Dr. Philip Cole, would your answers be the same as set forth therein with the corrections you have noted today?

A. Yes, they would be.

MR. WATSON: Your Honor, we move that the testimony of Dr. Cole be admitted into evidence subject to timely objections.

JUDGE SMOLEN: All right. It is received subject to that qualification.

(Whereupon, the document was marked as PECo Rebuttal Statement No. 2 for identification, and was received in evidence.)

MR. WATSON: I pass the witness, Your Honor.

JUDGE SMOLEN: All right, Ms. Khanwalkar. No questions?

MS. BURKET: No, Ms. Burket has no questions.

JUDGE SMOLEN: I'm sorry. I'm reading from a list that I have. It's Mr. Dillon who is from PP&L.

MR. DILLON: That's correct, Your Honor.

JUDGE SMOLEN: Do you have any questions?

MR. DILLON: I do have two quick questions.

JUDGE SMOLEN: Go ahead.

CROSS-EXAMINATION

BY MR. DILLON:

Q. Good morning, Dr. Cole. My name is Jesse Dillon and I represent the Pennsylvania Power & Light Company, an electric utility here in Pennsylvania.

Could you please turn to page 26 of your testimony? I believe at lines 12 through 20 you are discussing a study by Dr. Matanoski involving linemen, telephone linemen, is that correct?

A. Yes.

Q. And you state at line 15 that preliminary results indicate two to three more cases than expected of

male breast cancer. And my question for you is is that two or three times more cases than expected two or three out of how many? Could you expand on that statement?

A. Yes. First I would like to point out that the Matanoski study was an offshoot of a larger investigation that she had conducted on leukemia morality, and she did this study, which is a follow-up study of line and non-line workers in the state of New York.

The results of that study are still in a preliminary form. However, we now have more specific information, and it is as follows:

In the entire group that she followed up there were approximately 205,000 man-years of follow-up experience, a man-year being one man followed up for one year. And she observed two cases, not two or three, but two cases of breast cancer among the men that generated these 200,000 or more man-years.

There is no formal expected number presented in the report of this, which I have seen, which is published as a letter and not in final form. However, it is contended by Matanoski that these two cases represent an excess.

It's not clear to me that it would, the reason being that the incidence rate of male breast cancer is about one per 100,000 man-years, and as I say, she has 200,000 man-years. So the expected number for that

series should be in the vicinity of two, a little more or less depending upon the age structure of the men that she actually followed up. So at the present time I would have to say that until there is clarification of that I really don't know that we can interpret the Matanoski study. It is clear that the expected number would be in the vicinity of the observed number but just how close they are, I really don't know.

MR. DILLON: No further questions, Your Honor.

JUDGE SMOLEN: All right. Ms. McCloskey.

MS. McCLOSKEY: Ms. Dusman will be conducting the cross-examination.

JUDGE SMOLEN: All right.

MS. DUSMAN: Your Honor, I have no objection to the changes made in Dr. Cole's testimony. However, although we were aware of the article in the issue of the Journal of Epidemiology that he referred to, of the Peters study, we do not have a copy of it and I would ask whether he could produce a copy of it today.

MR. WATSON: We have a copy. We just got it faxed to us. We would be happy to share one with you.

MS. DUSMAN: In addition to that, we noted that there was a page missing from a transcript which was provided us in response to our discovery. It is a minor detail but Mr. Smith has offered to check to see whether

they have that with them. And since we are anticipating a break anyway, I would reserve the right to cross-examine on either the missing page of the transcript or the Peters study following the break.

JUDGE SMOLEN: There is no objection?

MR. WATSON: No objection.

JUDGE SMOLEN: All right. Then we will proceed in that fashion.

MS. DUSMAN: Thank you.

CROSS-EXAMINATION

BY MS. DUSMAN:

Q. Good morning, Dr. Cole.

A. Good morning.

Q. My name is Dianne Dusman and I am here on behalf of the Office of Consumer Advocate.

MR. WATSON: Excuse me once again. I just noticed one other change that might be consistent with the changes Dr. Cole made on the Peters study that change basically the words from preliminary to now. Could I just bring that to everybody's attention for what it is worth?

JUDGE SMOLEN: Page 26?

MR. WATSON: Yes, page 26 line 12. It says "As with the Peters study not enough information is available," and I think that has now changed based on

what Dr. Cole said.

JUDGE SMOLEN: Why don't we let the witness say that.

MR. WATSON: Is that an appropriate correction?

THE WITNESS: Yes, that is entirely appropriate and apparently I omitted to mention that when I went through my list.

MR. WATSON: So how would you change that?

THE WITNESS: So page 26, line 12, reads as follows: just delete the expression "as with the Peters study," capitalize the word "not", so it simply says now, "Not enough information is available about the data and design of the Matanoski study."

MR. WATSON: Thank you.

I'm sorry for the interruption but I thought we ought to straighten that out.

BY MS. DUSMAN:

Q. Dr. Cole, with respect to the Matanoski study, do you know when to expect that final results will be available?

A. No, I don't.

Q. You state at page five of your direct testimony that the purpose of your analysis is to offer a professional opinion as to whether the E/MFs associated with the Woodbourne-Heaton 230 kV transmission line will

cause cancer. Is it accurate to state, then, that your testimony is immaterial to whether any other adverse health effects might be caused by E/MFs surrounding the line?

A. I would like to focus on the question of cancer as a potential adverse effect of E/MF.

Q. In other words, you are making no statement as to whether E/MF has a causal connection to other adverse health effects?

A. I would restrict my testimony to the question of E/MF and cancer.

Q. Would you turn to your testimony at page six, lines three to eight, where you address the meaning of the word cause. Is it accurate to state that when you use the term cause you also intend to include the meaning increased the risk of disease?

A. I'm sorry. I don't understand. It appears that the response already includes those very words. Maybe -- I must be missing something.

Q. Is it true that throughout your testimony when you use the word cause you also mean to express your opinion as to whether cancer is -- the risk of cancer is increased?

A. Well, without reviewing each place, my response to that would be probably yes, it is conceivable to me.

I don't recall that there would have been a place where perhaps by example I might have used cause to mean a causal reduction. But I don't specifically recall any such thing. So generally yes, to cause is to increase the risk in general unless it is specifically clear otherwise.

Q. Since you have just stated, then, that your testimony is intended to be limited to whether cancer is caused by exposure to E/MF, you are also expressing no opinion, then, as to whether E/MF exposure increases the risk of other adverse health effects?

A. That's correct.

Q. Does your review, Dr. Cole, make any distinction between electromagnetic fields as a potential contributing factor to the development of cancer and E/MF as a possible contributing factor to the growth of cancer cells?

A. No, it does not. I consider both of those to be in the definition of a cause.

Q. At page 16 you direct your attention to lines 26 through 30. You state at that place in your testimony, quote, a few irregular statistically significant associations in a case-control study are to be expected even when nature is in a null state. Is that an accurate paraphrase of your testimony?

A. Yes, Ma'am.

Q. What do you mean by when nature is in a null state?

A. I mean by that that there is in fact and in truth no relationship between the factor under study and the disease that is under study.

Q. What would you use to draw a conclusion that there is no causal connection in fact between a particular factor and a human disease?

A. The ability to defend the proposition that there is not a causal relationship between a factor under study and a disease rests upon a number of different kinds of information. One would be a series of studies that are perceived to be of good quality and valid which failed to show any such association. And second is the absence of information either from animal studies or from other kinds of investigations to the contrary, that is, supporting the idea that there is a causal relationship.

Q. Can you give me an example of an instance in which, as you have stated here, nature is in a null state?

A. And in which the data in the study appeared to be contrary to that? That is, positive?

Q. No. I just want an example of an instance in which you would consider that nature is in a null state.

A. Okay. Saccharin does not cause cancer of the bladder in human beings. That is a null state.

Q. You have drawn that conclusion from the items you listed in your earlier answer?

A. That is correct.

Q. So in this particular circumstance you have only the materials that you have referred to within your testimony to determine whether nature is in a null state, is that correct?

A. When you say this particular circumstance are you speaking about this material contained on lines 26 through 30?

Q. I am talking about in general when you want to discern whether nature is in a null state.

MR. WATSON: Your Honor, I think we have at least a very confusing question at best. We have a question and then -- a question by the witness and a follow-up question.

JUDGE SMOLEN: Well, this is a sophisticated witness and if he does not understand the question I think he can say he does not understand the question.

Do you understand the question?

THE WITNESS: Your Honor, I think I understand the question but I don't understand the context of it, whether I am discussing E/MF or the nature of

establishing the absence of causality in general or what. That is where I am a little confused.

MS. DUSMAN: I will go forward, Your Honor.

BY MS. DUSMAN:

Q. The particular reference on this page, are you making this statement in terms of the Savitz study?

A. Yes.

Q. Does the Savitz study concern a connection between childhood cancer and electromagnetic fields?

A. Well, it -- yes, it does.

Q. Now, other than the materials that you have referred to within this rebuttal statement, do you have any others to use in order to determine when nature is in a null state or not?

A. May I ask, when you say how do I determine whether nature is in a null state or not with regard to the question of electromagnetic fields and childhood cancer?

Q. Yes, sir.

A. Okay. My perception that nature is probably in a null state in this regard is based primarily on my familiarity with the epidemiologic literature but I also have some degree of familiarity with the other literature as well.

Q. When you say other literature, what are you

referring to?

A. I am referring to animal studies and cellular studies.

Q. Later in your testimony at page 18 you discuss your preliminary opinions -- well, now you discuss your opinions -- about the Peters study, do you not?

A. Yes.

Q. And Peters also found an association between wiring code and childhood cancers did he not?

A. He found a weak association between wire code and not childhood cancers, no. Leukemia. That study was restricted to leukemia.

Q. If one more study were completed as the Peters study was just completed of that same caliber which showed an association between wiring code and leukemia, would that additional study cause you to reconsider your opinion as to this issue?

A. I would reconsider my opinion any time that I am exposed to any new information. But I would like to point out that an opinion such as I hold on this issue, which is now many years old and based on more than 50 studies, is not likely to change a great deal on the basis of any one study. It is much more important to look into the studies and to perceive the patterns of consistency or inconsistency among them.

For example, we have may have left the impression that the Peters study is clearly positive with regard to wire code. It most assuredly is not. There are inconsistencies in the data. There are changes in the data that reflect correction for other factors. And also the association is somewhat weaker than that which was seen in the Savitz study, which in turn is weaker than that which was seen in the original paper, that is, the Wertheimer 1979 paper.

So to me the question is not whether or not one more study might be positive, but rather, whether or not it can alter my perception of the long-term time trend in the results of these study, which is that as the studies are improving in methodology and size and focus they are getting weaker and weaker. To me that is the issue, not whether or not there is one more positive study.

Q. Would you agree, then, that consistently weak associations can lead you to draw an inference as to causation?

A. Oh, I would -- if I understand this question correctly, I would agree with you. It is possible that a large body of consistent information to the effect that an association is weak would permit that association to be accepted as causal. However, I don't know of any example of that but one, and even that one is somewhat

dubious. So in general weak associations just don't muster up. They tend to fade away.

Q. Dr. Cole, are there epidemiological studies which identify groups of individuals which are more or less susceptible to cancerous conditions?

A. I think it would be fair to say that most epidemiologic studies do not focus on the relationship between susceptibility and disease but, rather, exposure to external agents. However there some studies, very few, at least very few that I am aware, that do try to isolate what we might refer to as susceptibility, usually implying that it's a genetic trait or a consequence of some very early environmental exposure.

Q. Is it possible, then, that variations in susceptibility to the development of cancerous conditions among population subgroups can interfere with the consistency in epidemiological study results?

A. It's possible but it is not considered a major distorting factor.

Q. Dr. Cole, if further research were performed which in your opinion resulted in a demonstration that a biologically plausible mechanism existed connecting E/MF exposure and cancer, would that cause you also to reconsider your opinion as to whether the epidemiological studies demonstrate a causal connection between the two

things?

A. I don't think it would cause me to reconsider my opinion as to whether or not the epidemiologic studies demonstrate the relationship. But it would cause me to reconsider my overall opinion as to the credibility of the idea that magnetic fields cause cancer.

Q. Well, you stated in your testimony that you interpret the epidemiological studies and consider at the same time whether there is evidence of a biologically plausible mechanism. Are you now stating that you would not reconsider your opinion as to those studies in the event that the state of the science changed?

MR. WATSON: Objection, Your Honor. Argumentative. And I don't think it accurately states what the witness said.

THE WITNESS: I don't --

JUDGE SMOLEN: Wait a minute. We have an objection.

Do you want to be heard on that? The objection is that it is argumentative. It's asking why.

MS. DUSMAN: I don't think asking this witness a why question is argumentative at all. He has stated his rebuttal statement in terms of looking at the studies concomitantly. I asked him whether he would change his opinion about the epidemiological studies if further

evidence as to a biologically plausible mechanism were available. And then he said --

JUDGE SMOLEN: And now you are asking him for the reason why he wouldn't change his opinion.

MS. DUSMAN: He said no, he would not.

JUDGE SMOLEN: And now you are asking for what reason would you not change the opinion?

MS. DUSMAN: That's correct, Your Honor.

MR. WATSON: I think he said, no, he would not change his opinion if there was a biological plausibility theme brought to the forefront, but wouldn't change his opinion on the epidemiological studies themselves. But it might change his opinion overall. And I was concerned with the misstatement of what he previously said, not intentional, obviously.

JUDGE SMOLEN: Under those circumstances let's try the question again and see if we can get it straightened out.

BY MS. DUSMAN:

Q. Would the existence of a study which in your opinion showed a valid biologically plausible mechanism cause you to change your opinion as to this subject overall?

A. It is not likely that the appearance of a study such as you have described would cause me to change my

opinion if we are speaking about my opinion being the response to the question is there or is there not an established cause-effect relationship between E/MF exposure and cancer in human beings.

On the other hand, such a piece of information would certainly enter into my overall consideration of the issue.

Q. Dr. Cole, I have a few questions for you pertaining to ecological comparison, which you testify to at page 20, lines seven to 28.

Would you direct your attention to the final sentence of the first full answer where you state, "This type of ecological comparison, while subject to limitations, can provide important information as to a suggested relationship." Would you tell us what limitations you are referring to in that sentence?

A. The major difficulty in trying to interpret ecological comparisons as causal is their non-specificity. That is, they basically represent either time or place or time and place comparisons. And when one makes comparisons over, as in this example, over time it is not just thing that changes, not even one relevant thing that changes.

For example, while cigarette smoking was going up, so was air pollution. So this could be a detraction from

this idea that this particular ecological comparison is valid.

So that is the problem with ecological comparisons, non-specificity of association.

Q. Directing your attention to lines 19 to 20, where you state that you use power consumption as a surrogate for E/MF exposure, are you stating that a ten-fold increase in power consumption results in a ten-fold increase in exposure to electromagnetic fields?

A. No.

Q. What do you mean, then, by this?

A. Nobody knows how much E/MF exposure a human being has even today, unless that individual human being were to wear and use personal dosimetry for an extended period of time. We have such information for a few days for a few people. We certainly had no such information back in 1950. So I would not assert that, for example, there has been a ten-fold increase in E/MF exposure between 1950 and 1960 or 1980.

What I mean to assert here is that during this period of time there was a ten-fold increase actually in per capita power consumption, not just power consumption. And it seems reasonable to me to believe that E/MF exposure would, if not parallel the increase in power consumption, it would at least follow the same trend in

time, that is, upwards.

There are a number of observations that would support that. For example, the average American home built in 1950 had a 100 amp or 125 ampere power service put into it. The average American home built in the 1980s has a 250 to 300 ampere power service. It is also, I think, fair to say as a general observation that we use more electricity today than we did 30 years or 40 years ago.

Q. I believe initially in your answer you stated that no one can possibly know whether the population overall has experienced an increased exposure to electromagnetic fields.

A. Well, I don't know if I used the actual words "no one can possibly know". What I can see is this: I don't know and I have not been able to identify anyone who does know.

Q. In doing your comparison, then, is it accurate to state that you didn't consider the societal changes that have occurred since 1950, for example?

A. Perhaps I could ask for some clarification of what you mean by the societal changes.

Q. Well, I will rephrase the question. Did you factor in any societal changes in doing this ecological comparison?

A. No.

Q. You referred earlier to increased ampere service. Does that necessarily increase exposure to electromagnetic fields?

A. It could be contended that it does not necessarily increase exposure. But it would be very difficult for me to believe that it does not increase exposure. The purpose of upgrading the service is to allow the residents of the home to use more electric power. That is the purpose of it.

Q. Doesn't that comparison assume that a person is in their home consistently day in and day out for a long period of time?

A. There are many factors that can be brought to bear on the ecological comparison. You speak about the time that people spend in the home. But in addition we could point to the fact that more people work in offices today than they did in 1950. A lot fewer people work out of doors. These are facts.

Q. In doing your ecological comparison --

JUDGE SMOLEN: Wait. Let's see if the witness --

THE WITNESS: I am finished.

JUDGE SMOLEN: Okay. Go ahead.

BY MS. DUSMAN:

Q. Why did you limit your comparison in your study

to the 20 percent increase in childhood leukemia from 1950 to 1960?

A. Well, actually I -- maybe I don't understand. It was not an effort to limit it to 1960. The remainder of the response deals with the period of time subsequent to 1960 or at least I intended that it would. I tried to make a distinction between 1950 through '60 and 1960 and after for what I thought was a good reason, namely there was a rise between 1950 and 1960.

There was a rise in most forms of cancer in most age groups. And this was almost certainly attributable to improvements in the quality of diagnosis and certification of cause of death. From 1960 on, most of the modern diagnostic techniques -- not all, but most -- were in place and any rise that would have occurred since 1960 is more likely to be genuine than artifactual. But there has in fact not been any further rise in childhood leukemia.

Q. Do you agree or disagree with the statement that overall the evidence as to association between electromagnetic fields and cancer now available is too weak to allow firm conclusions either way?

A. I would agree with that. It is not my own position but I can understand how other people would hold that position.

Q. You agree with that statement but it is not your position in this case?

A. It is not my own position, that's correct.

(Pause.)

A. Perhaps I should just add one thing to my previous answer. My problem with the answer is the idea of firm conclusions, implying that one will never be able to change. This is the difficulty that I have with it.

Q. Do you agree or disagree with the statement that only in rare circumstances would it be justifiable to infer a cause-effect relationship from a small number of epidemiological studies?

A. Absolutely.

Q. At page 12 of your testimony -- you absolutely agree with that statement?

A. Absolutely.

Q. At page 12 of your testimony, line ten, this reflects that you have looked at six published residential case-control studies regarding E/MF and childhood cancer?

A. That's correct.

Q. Would you consider that to be a small number of studies or a reasonably large number of studies?

A. Madam, I don't consider that particular group of studies to be a set in and off themselves. They are

part of a much larger set. There are, for example, adult residential studies and a number of other different kinds of studies of the question of E/MF and cancer in human beings. So even if we were to speak about just residential studies, this would not comprise the whole set.

Q. Well, let's turn for a moment to the conclusions that you express at the end of your testimony. Would you agree or disagree that you can hold opinions as to particular conclusions with varying degrees of certainty?

A. Yes.

Q. Have you ever said I am 100 percent certain of a particular statement of fact?

A. Are we speaking about cause-effect relationships or empiric facts like there is a table in front of you?

Q. I am asking you in general whether you have ever said that you are 100 percent certain of a particular fact.

A. Yes, I have.

Q. With regard to your opinion on the relationship between power frequency electric and/or magnetic fields and cancer in humans, can you assess your degree of certainty as to that opinion?

A. Is there some particular statement in here that you are asking me about? Or are you asking a more general statement?

Q. I am asking as to the opinion that you express on this page of your testimony.

A. Which page is that?

Q. Page 28.

(Witness perusing document.)

MR. WATSON: Is that line 40, Counsel, which you are referring to?

MS. DUSMAN: Yes.

MR. WATSON: Or wherever.

MS. DUSMAN: Yes, referring to line 40.

BY MS. DUSMAN:

Q. Can you assess your own degree of certainty with respect to your opinion that E/MFs have not been demonstrated to be carcinogenic?

A. Yes. I think I will try to be brief on this. We ordinarily look at the spectrum of credibility of carcinogenesis or causality to exist on a spectrum between, let's say, zero and 100 percent where zero would mean the relationship has not been demonstrated and 100 percent meaning that I am absolutely sure that it has been demonstrated. So that is the spectrum in which we ordinarily think of things, between zero and 100 percent.

But I point out that in causal thinking that is only half the spectrum. There is another half to the left which is between zero and minus 100, which means between not demonstrated and demonstrated not to be causal.

So the three points on the spectrum are minus 100: E/MF has been demonstrated not to cause cancer in human beings; zero: we are relatively uncertain about whether it has been demonstrated not or demonstrated to cause cancer; and to the extreme right we have plus 100, which is E/MF has been certainly demonstrated to cause cancer in human beings.

Now, on this full spectrum of the credibility of ideas, my position is minus 70. I believe that it is much more likely to be true than not that E/MF will be demonstrated not to cause cancer in human beings.

Q. Okay. Do you assess your degree of certainty as to that opinion at the same level for childhood cancers as you do for adult cancers?

A. Yes, I do.

Q. Do you agree with me that in this context you have drawn a causal inference -- I'm sorry -- you have drawn not a causal inference but you have drawn an inference as to absence of cause?

A. Well, I haven't actually drawn the inference by

which I would understand to mean once again this firm conclusion idea, that I am absolutely sure that E/MF does not cause cancer in human beings. I am 70 percent of the way there. I'm not 100 percent of the way there.

Q. I understand.

In your opinion do you need the same amount of data to draw an inference as to the absence of a causal relationship as you do to draw an inference as to the existence of a relationship?

A. Madam, I don't think it is a question of amounts of data, if I interpret your question literally. If I interpret your question a little more generally to mean is it as easy as or easier or more difficult to establish the minus 100 position, that is, the null state as opposed to the plus 100 position, then I would say that most people hold the opinion, and so do I, that it is more difficult to establish the null state with certainty.

Q. Is there a difference between holding an opinion and having a belief?

A. This is not an actual distinction that I have actually faced. When we speak about holding opinions with regard to cause-effect relationships I can only say that I like to perceive these opinions as existing on the spectrum that I have described to you and whether or not

that should be called a belief, I don't have any objection to that as long as it is understood that the belief, like the opinion, can change.

Q. I would like you to focus on lines 40 to 42 on page 28 of your testimony. Now, you have stated that you are of the opinion that E/MFs have not been demonstrated to be carcinogenics. And let's take these things one thing at a time. You have said that you are 70 percent certain of that opinion?

A. No. They are not exactly the same, and I don't --

Q. Would you clarify for me what you meant by, then, that you are 70 percent of the way towards feeling that there is a 100 percent certainty that that is the case?

A. It occurs to me that the best way I could do this would be to phrase two different questions, that is, am I of the belief or do I know of anyone who is of the belief that electromagnetic fields have been demonstrated to be carcinogenic in human beings. To my knowledge there is no one who would answer that question in the affirmative. There may be someone but I don't know of any such person.

MS. DUSMAN: Your Honor, I would ask that that be stricken and direct the witness to answer the question I

put to him.

JUDGE SMOLEN: Well --

MR. WATSON: Your Honor, I think it was a fair attempt to try to get at the point Counsel was asking.

JUDGE SMOLEN: I am going to overrule the objection. The witness says he does not know of anyone, which is something within his knowledge. So I will permit that to stand.

Let's go on with the rest of your answer.

A. In trying to be responsive I was simply pointing out that from my perception the issue here has related to two questions. One is where am I on the right side of this spectrum, and if that is the only spectrum we can talk about, if that is your wish, then I am at the zero position. I simply offered the opinion in response to your previous question, which was that in fact we perceive in causal thinking that the spectrum has a left side to it. And that is in fact where I am to be found these days.

BY MS. DUSMAN:

Q. I understand.

At line 41 you go on to state, "I believe that the 230 kV transmission lines poses no threat of cancer to persons in its vicinity. What I want to know is are you as certain of that belief as you are of your opinion that

E/MFs have not been demonstrated to be carcinogenic?

A. Yes. I believe it is much more likely than not that the line poses no threat of cancer.

MS. DUSMAN: Nothing further, Your Honor, except to reserve the right to cross-examine on the additional materials.

JUDGE SMOLEN: Do you want to redirect now if you have any, or do you want to wait?

MR. WATSON: I think I would rather wait.

JUDGE SMOLEN: All right. Let's take a break now. I don't know what time Mr. Sugarman is going to arrive. He said it was going to be before 11. He was second on the list. That was the message that he left in the office.

So let's take a general break. Let's report back in 15 minutes and see what happens at that time.

(Recess.)

JUDGE SMOLEN: Back on the record.

Mr. Sugarman, cross-examination.

MR. SUGARMAN: Thank you.

CROSS-EXAMINATION

BY MR. SUGARMAN:

Q. Dr. Cole, I am going to try and avoid repetition. I have some idea of your prior cross-examination so I will try to avoid repetition of

testimony you have already given.

As I understand it, you interpret -- and by interpret, I mean understand -- the Wertheimer, Leeper and Savitz and Matanoski and Peters work to show some larger or smaller amount of association between wiring codes and some disease or diseases, is that correct?

A. No, sir. I think it would be useful to exclude Matanoski from that series, since the Matanoski studies are of adults and don't relate to wire code whereas the other three relate to children and do relate to wire code.

Q. All right. And Matanoski shows some exposure from line work appears to result -- and this is without evaluating it, but based on its face -- result in different patterns of cancer risk?

(Pause.)

Q. Is that correct? That is what the report says?

A. I'm sorry. I would not agree with that statement as it was made and I don't know exactly what it is that you are reading.

Q. I am reading from the report, Cancer Incidence in New York Telephone Workers, Matanoski, et al. dated November, 1989. And it states, quote, these data suggest that different exposures from line work appear to result in different patterns of cancer risk."

MR. WATSON: Are you offering this to refresh his recollection as to what this study says?

MR. SUGARMAN: Well, he said he didn't know what I was referring to. So I'm offering it to him to explain what I was referring to.

I thought he testified that he was aware, in fact he was on the advisory committee of the Matanoski, page 26, lines three through 20. So I assume he knew about -- he characterized it so I assumed he knew about it.

MR. WATSON: And the pending question is whether this document says what you read?

MR. SUGARMAN: Yes. I want to make sure we all understand the study the same way. I am not asking him to agree with that conclusion but I am asking him if that is his understanding of what the conclusion says.

MR. WATSON: I am not sure that is a conclusion.

MR. SUGARMAN: Well, that is why I am asking him the question. He is the witness.

A. If I may simply make a comment or clarification, Matanoski has done two studies.

BY MR. SUGARMAN:

Q. Right. You so testified in your direct.

A. I just needed a moment to understand which one you are talking about and which particular comment.

Now, could you direct me to the comment that you

asked me about?

Q. Well, you are calling it a comment. Let's avoid argumentative characterization and call it a statement. It says here these data -- this is the next to last sentence of the report. It says these data suggest that different exposures from line work appear to result in different patterns of cancer risk. The last sentence, then, is the data are limited to New York because at this time New York was the only feasible population to study.

And I am asking you -- I wasn't showing you originally -- the original question to you was is it your understanding of Matanoski's report that it did purport to find some association between exposure to electromagnetic fields and cancer risk?

MR. WATSON: Your Honor, could we have at least an identification of which Matanoski study?

JUDGE SMOLEN: Which one?

MR. SUGARMAN: The one on New York telephone line workers.

JUDGE SMOLEN: Go ahead.

A. Mr. Sugarman, you have impressed upon me the importance of the words, and you asked me about a report. This is not a report. This is a representation of a poster session.

Q. Of what?

A. A poster session.

Q. Does the representation of the post-assessment by Matanoski as you understand it purport to find some association between exposure to E/MF and cancer risk?

A. Does it purport to find such a thing?

Q. Right.

A. Yes. It purports to.

Q. All right. Thank you. May I take that?

A. Yes.

Q. Thank you.

Now, we have Wertheimer, Leeper, we have Savitz, we have Peters, we have Matanoski, all of which purport to find some association between some electrical fields -- magnetic fields, I should say -- and cancer, albeit in different populations.

MR. WATSON: Objection, Your Honor. Counsel is testifying. That's not established, that's Counsel's characterization.

MR. SUGARMAN: That is what he said.

MR. WATSON: He only answered as to Matanoski. Counsel then proceeded in his question to list several others.

MR. SUGARMAN: Prior to that he testified as to the other three.

JUDGE SMOLEN: I will sustain the objection. Ask the next question.

BY MR. SUGARMAN:

Q. In the face of your testimony that you agree, as you testified a few minutes ago, with respect to Wertheimer, Savitz and Peters, and now as to Matanoski, that these studies purport to find some association, is it your testimony that all these studies should be totally disregarded?

MR. WATSON: Objection, Your Honor. Again it's the same thing you ruled on before. Counsel is characterizing the prior testimony. He's doing so incorrectly. This witness did not answer that question. The witness answered a question only as to Matanoski, and in those prior studies the purported relationship was only with wire code and not with measured magnetic fields.

MR. SUGARMAN: I didn't say measured.

MR. WATSON: I know you didn't.

MR. SUGARMAN: I said exposure to E/MF.

MR. WATSON: And it is not necessarily exposure to E/MF, Your Honor.

JUDGE SMOLEN: I sustain the objection. Ask a question --

MR. SUGARMAN: He can answer that. That is

cross-examination. He can explain.

JUDGE SMOLEN: I sustained the objection.

MR. SUGARMAN: What I am saying is he testified -- can we have his answer read back as to the other three studies?

JUDGE SMOLEN: The other three? Not Matanoski. Matanoski said it was a post-summary.

MR. SUGARMAN: Yes. But as to the other three.

JUDGE SMOLEN: You referred to it in your question as his testimony as to the study.

MR. SUGARMAN: Well, isn't this a post whatever it is document relating to a study?

JUDGE SMOLEN: Well, you have to ask that of the witness.

MR. WATSON: I might be able to help in one sense. I think there is a word misunderstanding. As I understand it, it was a poster, p-o-s-t-e-r, presentation --

JUDGE SMOLEN: That's what I thought the witness said.

MR. WATSON: -- rather than a post-presentation, which is if somebody puts a poster up on a board.

JUDGE SMOLEN: We are wasting time. Ask one question at a time rather than lumping all the studies together.

MR. SUGARMAN: But, Your Honor, my point is precisely that the studies all have to be lumped, and the question is when you lump all these studies can you just blithely say I see nothing that bothers me.

JUDGE SMOLEN: Ask the question again.

MR. SUGARMAN: Okay.

BY MR. SUGARMAN:

Q. You agreed with me a few minutes ago, or you corrected me and qualified your agreement with me a few minutes ago as to the three studies and said they were wire code and they were children, childhood cancer, and they did show an association.

JUDGE SMOLEN: Wait a minute. Let him answer that question. Let him answer that question.

Make that a question. Was that your prior testimony?

THE WITNESS: I don't know if it was my prior testimony or not but I will assert now that the three other studies, that is, the original Wertheimer study, the Savitz study and the Peters study, both contain information relevant to a link between wire code and either childhood cancer or more specifically leukemia, and in each of those three studies there is some positive result. There are also some negative results.

BY MR. SUGARMAN:

Q. Now, there are some positive results there. There are some positive results in the Matanoski whatever you call it. And my question to you is is it your testimony that all four of those can be safely disregarded?

A. Mr. Sugarman, in attempting to form an opinion and continually update my opinion as to whether or not electromagnetic fields cause cancer in human beings, I have tried to rely on the totality of the epidemiologic literature, which now numbers more than 50 studies. It is not to me a question of whether or not we pick out four studies and say we can ignore them or interpret them one way or the other. The most important characteristic of epidemiologic data is it is subject to error and chance and the need to interpret it in a pattern. So I neither dismiss these studies nor do I form my opinion solely on the basis of them.

I want to point out, too, that every one of those studies includes substantial negative information with the exception of the original Wertheimer study. They are not clear-cut positive. As a matter of fact, the Peters study is in my judgement an overwelmingly negative study.

Q. By overwhelmingly negative, you mean very few of the exposed population show a manifestation?

A. No. I mean that the Peters study used

measurements of magnetic fields in the homes of the subjects. It's the first study to have done so in any substantial proportion of people. With regard to the measurements, the Peters study is negative. There is no link whatever between childhood leukemia and in-home measurements of magnetic fields.

Furthermore, with regard to the wire code, the study is substantially weaker than the Savitz study and also much less consistent. A more subtle point in the Savitz study -- in the Peters study -- with regard to wire code is that all of the effect of the wire code is restricted to the highest exposure category. And even is there it undergoes a major reduction when it is corrected for other factors that could influence risk.

Both of these things, that is, the fact that the long-term time trend is down and the fact that correction for extraneous factors reduces the effect, strongly suggest that you are not dealing here with a true causal factor. So it is negative as far as I am concerned with regard to the wire code, at least at the interpretative stage if not the literal data stage, and it is clearly and unequivocally negative with regard to the magnetic field measurements.

Q. The magnetic field measurements, were they of proximity to a transmission line?

A. The measurements were made in the homes of the children.

Q. Right. But was there a proximity to a transmission line?

A. There may have been in some homes. In others I would assume there was not.

Q. It's not reported, is that right?

A. The data are not provided for the location of the measuring device and the location of all facilities that might influence it for everyone of the nearly 500 children in the study. Statements are made to the effect that the measurements were taken under some conditions and those conditions are specified. For example, one of the conditions was in the middle of the child's bedroom, if at all possible.

Q. But you are saying that as far as you know there were no transmission lines involved?

A. No, I didn't say that.

Q. Oh, okay.

A. What I did say was I don't know whether or not there were any transmission lines involved.

Q. What were the magnetic field measurements, what was the range of them?

A. The magnetic field measurement range is given in the actual paper. May I consult it?

Q. Sure. I sorry. I thought you had a copy of it?

A. I have it.

Q. Was it Table 3?

(Witness perusing document.)

A. Table 3 gives some measures of central tendency, averages and the like, of the measurements made in the home of the children with leukemia and the children who don't have leukemia, and you see, for example --

Q. But none of them are of a level sufficient to qualify as being affected by transmission lines, is that correct?

A. Well, if I could, I would like to finish my answer to the previous question since you asked me to characterize --

Q. No, I didn't. All I asked you was whether --

JUDGE SMOLEN: The witness can finish the answer to the previous question.

MR. SUGARMAN: He wants to talk about the results of the study and I am not into that at this point. I just want to know if they show exposure to transmission line milligauss --

JUDGE SMOLEN: I understand.

You can finish your previous answer.

A. Well, I will simply state, rather than reading any numbers, that the information in Table 3 shows that the cases and controls have essentially identical readings with regard to their magnetic field exposures.

BY MR. SUGARMAN:

Q. And there is nothing in there that shows any elevations that you would expect near a transmission line, is that right?

A. Mr. Sugarman, I cannot answer that question.

Q. Okay. Fine. You can't answer it.

A. I don't know what level one would expect to see in a home that was relatively close to a line of what relative size operating under what relative load conditions.

Q. So you don't know.

Then in your testimony at lines 36 to 42 on page 28 you discuss the exposure of people proximate to this transmission line. Did you have any information as to the elevated levels of milligauss associated with this transmission line?

A. I don't have any information as to what the readings would be at the edge of the right-of-way or within the right-of-way of this line under various load conditions, no.

Q. Do you think that might have anything to do

with an increased risk of cancer?

A. No, I don't.

Q. So it wouldn't matter to you?

A. It's not that it wouldn't matter. It's that the 230 kV line is not going to produce enormous fields. The fields are going to be similar to those produced by other lines. And in --

Q. What kind of lines?

A. And in any case, if I may complete the answer, there has been no link established between magnetic field exposure and cancer. So unless you were going to talk about something that was of a completely different nature I will have to stay with my response.

Q. You say there has been no link established but you acknowledge that studies have purported to find such a link?

A. That's correct.

Q. But in your view they don't establish it?

A. Mr. Sugarman, I will contend that not only is it true in my view that there is no established cause-effect relationship between magnetic field exposure and cancer, but I will further contend that it is not true in anybody's view. At least I know of no one who holds that view, and I think I know the opinions of quite a few people.

Q. Your testimony is limited to the effect of magnetic fields on cancer, is that correct?

A. Yes.

Q. It does not deal with any other health conditions?

A. That's correct.

Q. Have you seen the studies -- I say studies -- the letters in Lancet -- are you familiar with Lancet?

A. I read The Lancet, yes.

Q. Have you seen the two letters in Lancet in December, 1990, and March, 1991, relating to findings of health effects of electromagnetic fields in Sweden?

A. Yes, I have.

Q. And do those relate to cancer?

A. Yes, they do.

Q. And do you regard them as epidemiological information of value?

A. No, I don't.

Q. Why not?

A. Mr. Sugarman, it is my policy not to incorporate into my overall assessment of a cause-effect relationship information which is published in letter form or which appears as posters at scientific meetings unless there is something most exceptional about the letter.

Q. What do you mean by most exceptional?

A. Something that is to the effect that it is so lengthy and detailed that it represents what is in effect a published study. That is quite rare.

Q. Are you aware that Einstein's theory of relativity was first published as a letter?

A. I have heard it said that Einstein's theory of relativity was published in every form conceivable to man but I have never actually seen the document. So no, I don't.

Q. Hawking's book has a picture of it.

MR. WATSON: Objection, Your Honor. Move to strike. Counsel is testifying.

JUDGE SMOLEN: Sustained.

BY MR. SUGARMAN:

Q. Dr. Cole, have you done any epidemiological studies related to electromagnetic fields?

A. I am just beginning a study at this time. I have not completed any study in this area.

Q. What exposure are you going to use?

A. The study that I am going to be doing will be based on a detailed occupational history.

Q. And how are you going to characterize exposure?

A. It will be described in terms of probable, possible and likely, based on the occupational history.

Q. But to what? Exposure to -- I mean, we all have exposure to E/MF in every house as the control in the Peters study shows. How are you going to characterize the exposure. Exposure to what?

A. It is to magnetic fields.

Q. You are not going to distinquish between people who are exposed to it in every home in the country versus people who are exposed to a high power transmission line?

A. My study is a case-control study which focuses on the workplace.

Q. How are you going to characterize the workplace?

A. By going through the records of each individual human being who is incorporated into the study.

Q. But are you going to characterize the amount of milligauss to which the person is exposed?

A. No. That information will not be available.

Q. You don't think it has any relevance?

A. I think it would be very relevant. I would be delighted to have it. But I know of no way to obtain it.

Q. Didn't Matanoski obtain it for his New York telephone study?

A. She obtained some information for some of the workers through an extremely expensive approach, that is, she tried to simulate circumstances that had existed 20

or 30 years ago. But I would like to point out that Matanoski's study and mine are fundamentally different. Her study is a follow-up study and my study is a case-control study. It is as a practical matter infeasible, maybe even impossible, to get exposure assessments for my people.

Q. What kind of study was Peters' study?

A. The Peters study is a case-control study.

Q. What is the meaning of the exposure, Table 3? You say it is almost impossible to get exposure levels. Table 3, exposure, arithmetic mean, median, geometric mean. Isn't that exposure?

A. This is exposure and it is a case-control study.

Q. And it is?

A. It is indeed.

Q. So why are you saying it is impossible for you to get it?

A. It's a completely different setting. These are children and these are residential studies. Even so, he failed to get measurements on the vast proportion, I think 30 percent or so.

In my study it relates to the workplace some 10 to 30 years ago. It is just not practical. The workplace is gone now, it is changed. In Peters' study he went to

the home in the present time and made the assumption that it would be similar to the home of five years ago. It is a somewhat different circumstance.

Q. You say you would like to have the information as to the exposure levels. You would be delighted to have it, I think was your phrase. How can you form an opinion as to the dangers of this transmission line without having the exposures?

A. I'm sorry, Mr. Sugarman. What transmission line?

Q. The proposed Woodbourne-Heaton 230 kV transmission line.

A. Oh. I have tried to arrive at an overall position with regard to the likelihood that E/MF causes cancer. I have arrived at the position that this is not an established cause of human cancer. As a matter of fact, I went a little bit further and said it is very unlikely that it will ever been shown but that is perhaps neither here nor there.

Given that I hold the position that E/MF is not an established cause of cancer in human beings, the question of this particular line, the Woodbourne-Heaton line, fits right into the overall pattern that this line, like other line, is not a cause of cancer.

Q. So let me ask you this, then. Do you have any

position of agreement or disagreement with PECo's recommendation that people consider prudent avoidance of exposure to E/MF?

A. The question of prudent avoidance, as I have understood it, is either a question of individual behavior or perhaps could be extended to public policy. And this is an area where I just don't have any expertise. I do restrict myself to the study of the causes of cancer among human beings.

Q. I'm afraid I don't understand. If you were giving this Commission an opinion as to whether they should advise people to forget about prudent avoidance you would say you have no opinion?

A. If I were asked by the Commission to say whether or not people should consider or not consider prudent avoidance -- is that the question?

Q. Yes.

A. My first response to them would be exactly what I responded to you, that I am a cancer epidemiologist, not a public health policymaker.

Q. Let's say we go one step further and we say we are concerned about the -- we don't want people to be exposed to any health risks. From your vantage point should we encourage prudent avoidance or would we be wasting our time and money?

MR. WATSON: Objection, Your Honor. He has already testified it is beyond his field. It's not covered in his testimony.

JUDGE SMOLEN: It's the third time, the same question.

MR. SUGARMAN: Sustained?

JUDGE SMOLEN: Sustained, yes.

MR. SUGARMAN: No further questions.

I just wanted to make sure that I had not missed a chance to get an answer by not asking the question another way.

JUDGE SMOLEN: Well, you are practicing law the way it is supposed to be practiced.

MR. WATSON: Your Honor, we have the pages of the transcript that the OCA has asked for this morning.

(Document handed to Ms. Dusman.)

MS. DUSMAN: Thank you.

JUDGE SMOLEN: Do you want to break now to take a look at those pages?

MS. DUSMAN: I would appreciate that, Your Honor.

JUDGE SMOLEN: Let's take a ten minute break. When we come back on the record, without looking at that, do you have any other questions based on your reservation this morning?

MS. DUSMAN: I may, Your Honor, but it will be

brief if at all.

JUDGE SMOLEN: How about redirect? Are you going to have any or not?

MR. WATSON: Unlikely, Your Honor.

JUDGE SMOLEN: All right. Ten minute break.

(Recess.)

JUDGE SMOLEN: Back on the record.

Further cross?

MS. DUSMAN: Just a couple of questions, Your Honor.

JUDGE SMOLEN: Go ahead. Cross-examination.

FURTHER CROSS-EXAMINATION

BY MS. DUSMAN:

Q. Dr. Cole, having looked at what we have been referring to as the Peters study, which was authored primarily by London, is it accurate that you stated in response to a question by Mr. Sugarman that there is no information in this study as to distance to transmission lines?

A. I think what I tried to say was that information is not provided with regard to specific individuals about the distance between, say, their home and the transmission lines. It is true that if a home was located near a transmission line that that would influence the wire code that was assigned to the

particular residence.

So the people who made the wire code assignment would use the fact of presence of a transmission line or a distribution line to influence the wire code. But we are not provided with that information in any way that we can disentangle it from the wire code collectively.

MS. McCLOSKEY: May we have just a minute, Your Honor?

JUDGE SMOLEN: Yes.

(Pause.)

BY MS. DUSMAN:

Q. Just so I understand your answer, Dr. Cole, would you explain your understanding of what wire code is?

A. Yes. First I will say that there are several different wire codes that are in use. This particular study used both the so-called Wertheimer-Leeper wire code, which they refer to as the Denver-Wertheimer-Leeper wire code, and they also the Caune code but they don't present results for that. They say that the results were less positive. So perhaps I should talk about the Wertheimer-Leeper wire code.

As originally used by Wertheimer it was a four point scale from very low current, to low current, to high current, to very high current configuration, and she

would assign a home to one of these four categories depending upon what she perceived as the service drop to the home and any structures in the immediate area of the home.

In the Savitz study this was refined to a five category scale, and I believe that is what they have used in this paper too, a five point scale.

Q. Is it accurate to state that this study supports -- the study that we have been referring to, which is the Peters study, primarily authored by London, just published -- that it supports an association between childhood leukemia risk and wiring configuration or wiring code as you just described it?

A. If I don't --.

MS. DUSMAN: Your Honor, if the witness would please answer yes or no first and then explain if necessary.

JUDGE SMOLEN: Well, if he can answer yes or no.

THE WITNESS: This particular question I cannot answer yes or no. I think both of those answers are wrong.

BY MS. DUSMAN:

Q. Would you direct your attention, then, to the -- you have the study before you?

A. Yes, Ma'am.

Q. Would you direct your attention to the final sentence in the abstract on the first page of the article that you provided us and read it into the record?

A. "Our results support an association between childhood leukemia risk and wiring configuration but not direct measurements of electric and magnetic fields."

MS. DUSMAN: Nothing further, Your Honor.

JUDGE SMOLEN: All right.

MR. SUGARMAN: I just have one question.

FURTHER CROSS-EXAMINATION

BY MR. SUGARMAN:

Q. As you said, they have a category or several categories, one of which is very high. Page 929, the first paragraph, and then Table 7. Is that right?

(Pause.)

JUDGE SMOLEN: Do we have a question?

MR. SUGARMAN: Yes.

BY MR. SUGARMAN:

Q. Is that right? Am I reading it right?

A. Well, Table 7 does include reference to the wire code and I think you said 929, first paragraph?

Q. Right.

A. It makes a reference to the wire code, yes.

Q. On page 928 they say they sketched overhead electric transmission facilities. The last paragraph.

A. Yes, that's correct.

Q. On 929, in the right-hand column, the second paragraph, it says, "Three highly influential 24 hour magnetic field measurements (two cases and one control) were excluded from all analyses involving continuous scale data but were included in analyses involving ordered categories." Do you understand what that means?

A. Yes, sir.

Q. What does it mean?

A. It means this: there were three readings, as it says, two for cases and one for control, which were extremely high. And that is what they mean by highly influential. That is, the inclusion of those individual measurements would pull up the average in and of themselves.

So they they excluded those measurements when they made individual assessments of means and averages and the like so that the measure of central tendency would not be pulled inordinately by any one or two measurements. However, when they use categorization of the field strength like low, medium and high, then they put them into the high category.

That's what it means.

Q. Isn't that apples and oranges, putting them in and taking them out?

A. No.

Q. Shouldn't they have been consistent and excluded them altogether or included them altogether?

A. What they have done is commonly done and widely accepted as long as it is pointed out that it is done, and it is a common practice in what they call continuous scale or what I might call ratio scale variables to exclude what are sometimes referred to as outliers at either end. It is a very common practice.

Q. And is it true that in Table 7, if you believe it, that the higher the exposure category the more -- the exposure category in terms of exposure based on the wire configuration classification -- that the higher the cancer risk or leukemia risk?

A. Again, Mr. Sugarman, it is true and it is not true. That is, there is a trend in the direction that you have just said.

Q. Right.

A. There is a trend towards increased risk with higher wire code.

Q. Right.

A. But as I mentioned in response to a prior question, there are two important factors that have to be borne in mind before one attempts to interpret that and come to the interpretation that there is such a

relationship.

I think that that interpretation is best shown in Table 9, in the bottom half of it, where we have now a much more sophisticated presentation of the same information. That is, Table 7 represents what we might term the crude data and Table 9 the refined data. And what we see as we look at Table 9 is that in the second category of wire code the risk is actually estimated to be 20 percent lower than it is in the lowest. So we don't have what we could call here a consistent dose response.

The second thing is the comparison of the 2.15 with the 1.73. This is quite important because it is only in that category, that is, the very high category, that there is really any meaningful association. So the question becomes what is the interpretation to be placed on that change, that is, from 2.15 to 1.73. The answer is that when they have corrected for other factors that could influence leukemia risk the apparent association with very high current configuration wiring went down by 37 percent.

I point out --

Q. In each category --

JUDGE SMOLEN: Let him finish the answer.

MR. SUGARMAN: I'm sorry.

A. Please let me finish. I am speaking only of the category very high. And I want to point out that the change from 2.15 to 1.73, while it may look trivial, has to be appreciated to be a 37 percent reduction, the reason being that you subtract out the one, which is baseline.

Now, the control that they have influenced to bring about this change is imperfect. It is limited by the data that they have and statistical devices. It implies, though, such a large change, that if they had better data the reduction would be even greater.

In my judgement, then, while these data set could be described as saying that they do show some relationship between wire code and leukemia risk, they are very unimpressive to that effect.

BY MR. SUGARMAN:

Q. I understand.

Just so we are clear, the category very high relates to what level of exposure?

A. I believe that comparison is actually given in the paper.

Q. I think so.

A. There is a table somewhere in here that shows the average milligauss reading in the homes in any particular wire configuration category. At least that is

my recollection.

(Witness perusing document.)

A. Yes, here we are. Table 4 says that the mean reading, that is the average, an arithmetic average, in the very high current configuration homes is 1.15 milligauss.

MR. SUGARMAN: Thank you.

I don't have any further questions. Thank you very much.

JUDGE SMOLEN: Anything?

MR. WATSON: No, Your Honor.

JUDGE SMOLEN: No further questions, no redirect.

The witness is excused. Thank you very much for appearing and testifying, sir.

(Witness excused.)

JUDGE SMOLEN: That apparently concludes today's session and we will reconvene on Friday morning.

The hearing is adjourned.

(Whereupon, at 12:06 p.m., the hearing was adjourned, to be reconvened at 10:00 a.m., on Friday, November 22, 1991, 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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By: Robert J. Stonaker

Robert J. Stonaker

Philadelphia Electric Company Rebuttal Statement No. 2

A-110550F055 11/20/91
Phila, PA ROS

**BEFORE THE**
**PENNSYLVANIA PUBLIC UTILITY COMMISSION**

# REBUTTAL TESTIMONY OF DR. PHILIP COLE



**ON BEHALF OF**
**PHILADELPHIA ELECTRIC COMPANY**



**November 1991**

# REBUTTAL TESTIMONY OF DR. PHILIP COLE

## CONTENTS

## I. Background and Qualifications

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, AL 35294.

Q. What is your occupation?

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

Q. Dr. Cole, what is epidemiology?

A. Epidemiology is the scientific observation of human beings for the purpose of identifying the environmental causes of their diseases.

Q. Where are you employed?

A. I am Professor of Epidemiology and Chairman of the Department of Epidemiology at the School of Public Health of the University of Alabama in Birmingham.

Q. Do you hold any other positions at the University of Alabama?

A. Yes, I am the Associate Director for Epidemiology at the Comprehensive Cancer Center at the University.

Q Are you testifying today on behalf of the University of Alabama or any other medical or scientific institution?

A. No. I am testifying in my personal capacity as a researcher in epidemiology and a medical doctor.

Q. Dr. Cole, would you describe your educational background for us?

A. I received a Bachelor's degree from Michigan State University in 1960. My medical degree, which I received in 1965, is from the University of Vermont. After receiving my medical degree, I entered Harvard University, where I received a Master's degree and a Doctor of Public Health (Dr. P.H.) in Epidemiology.

Q. As a medical doctor, are you board certified in any medical specialty?

A. Yes, I am board certified in Preventive Medicine.

Q. Are you also licensed to practice medicine?

A. Yes, in the state of Alabama.

Q. Dr. Cole, could you give us a brief description of your professional career after you received your Dr. P.H. from Harvard?

A. After receiving my Dr. P.H. from Harvard, I taught in the Department of Epidemiology at that institution and conducted epidemiologic research. I remained on the faculty at Harvard for nearly a decade and became a full professor. I then left Harvard to chair the Department of Epidemiology at the University of Alabama's School of Public Health, where I have remained as Chairman and Professor.

Q. Could you give us some examples of epidemiologic courses you taught at Harvard and the University of Alabama?

A. Examples of epidemiologic courses I have taught include the Epidemiology of Cancer, Advanced Epidemiologic Methods, Principles of Epidemiologic Research, and Epidemiology of Neoplastic Diseases.

Q. In addition to Harvard and the University of Alabama, have you taught in any other programs?

A. Yes, I have taught in several other graduate programs, as well as at the International Agency for Research on Cancer ("IARC").

Q. What is the International Agency for Research on Cancer?

A. IARC is an arm of the World Health Organization dedicated to research on cancer issues. I taught a course in Cancer Epidemiology at IARC.

Q. Are you currently involved in any epidemiologic research?

A. Yes, I am currently conducting several research projects in cancer epidemiology. Two of these research projects are funded by the National Cancer Institute. One is a study of the occupational causes of cancer, including breast cancer. The other is a study of the causes of cancer of esophagus in mainland China.

Q. Does your epidemiologic research have a particular area of emphasis?

A. Yes, my research for the past 25 years has focused on the epidemiology of cancer and on the relevant methodology for conducting such research.

Q. Have you published any of your research in scientific journals?

A. Yes, I have published over 130 articles on epidemiology, nearly all of which involved cancer epidemiology and the methodology of cancer epidemiology.

Q. What are some of the scientific journals in which your epidemiologic research has been published?

A. The scientific journals in which my work has been published include the *American Journal of Epidemiology*, the *New England Journal of Medicine*, the *Journal of the National Cancer Institute*, the *American Journal of Public Health*, and *Cancer*.

Q. Have you served as a scientific reviewer or editor for any scientific journals?

A. Yes. I have been an editor or on the editorial boards of several journals, including the *American Journal of Epidemiology* and *Cancer Research*.

Q. Have you been involved with any other professional activities that involve the review of epidemiologic or other research?

A. Yes. For example, I served on the Board of Scientific Counselors for the Division of Cancer Cause and Prevention at the National Cancer Institute. I also served as Subcommittee Chairman in connection with the National Cancer Institute's National Planning Effort for Cancer Prevention and Control, the purpose of which was to identify all known causes of cancer as the first step to reducing cancer by 50 percent by the year 2000.

Q. Have you received any honors or awards in your professional career?

A. Yes. I was the John Rankin Visiting Professor of Occupational and Preventive Medicine at the University of Wisconsin at Madison, and the Eleanor Leader Memorial Lecturer at the University of Toronto. Among the awards I have received are the American Cancer Society Faculty Research award, the Kammer Merit in Authorship award from the American Occupational Medical Association, and the French Cancer Society's Grand Prix Lacassagne for cancer research.

Q. What is the Grand Prix Lacassagne?

A. The Grand Prix Lacassagne is an award given by the French Cancer Society for excellence in cancer research. I was jointly given this award with co-researchers In Boston, Greece, and Australia for research on the causes of breast cancer.

Q. Have you ever been asked to provide advice on cancer epidemiology matters to a governmental body or institution?

A. In addition to my work with the National Cancer Institute, I testified before a Committee of the United States House of Representatives in 1987 on electric and magnetic field ("EMF") health issues, focusing particularly on the EMF cancer epidemiology research. I have also advised the State of Maine and the State of Vermont on electric and magnetic field health issues.

Q. Dr. Cole, what were you asked to do in connection with this case?

A. I was asked to review the epidemiologic literature concerning power frequency electric and/or magnetic fields and to prepare testimony evaluating this literature. I was also asked to offer my professional opinion as to whether the electric and/or magnetic fields associated with the Woodbourne-Heaton 230 kV line will cause cancer.

## II. Background on the Science of Epidemiology

Q. Before discussing the epidemiological studies on EMF and cancer, please describe the major strength and limitation of epidemiology as a science.

A. Epidemiology is the study of humans in their everyday environments, and the major strength of epidemiology is that it pertains directly to human beings. As such, facts established through epidemiologic research have direct implications for public health strategies, unlike most laboratory research. The great limitation of epidemiology is that it is a non-experimental science and, consequently, is highly susceptible to error. Both the strengths and limitations of epidemiology must be recognized when conducting an evaluation of epidemiologic studies.

Q. What is the importance of the limitation on epidemiology you described?

A. The history of cancer epidemiology has taught us that, with rare exception, only epidemiologic investigations of the highest quality have proven to be of value.

Q. What kinds of conclusions can be reached from a body of epidemiologic literature?

A. In recognition of the imprecise and error-prone nature of the science, epidemiologists generally draw causal inferences only when there is a reasonably large number of independent studies with consistent findings. Even then, epidemiologists do not place substantial confidence in findings of weak associations where the magnitude of the reported "risk" may be less than epidemiologic studies can correctly identify.

Q. What is meant by the term "causal inference"?

A. Generally, the question asked in an epidemiological study is whether exposure to some agent causes disease. "Cause" is used in a very practical sense in epidemiology. The term causal inference means that the epidemiologic data is sufficiently strong and consistent that it is widely believed to be true that exposure to the agent of interest increases the risk of disease.

Q. Can epidemiologic data establish a cause-effect relationship?

A. Yes. Epidemiology is a non-experimental science, and the researcher cannot exercise strict controls over the study population. It is thus sometimes perceived that epidemiology can never establish the existence of a cause-effect relationship. As a practical matter, however, epidemiological research is a powerful -- perhaps the most powerful -- approach that we have for establishing the causes of disease, including cancer, in man.

Q. Can you give an example of a cause-effect relationship that was established with epidemiological data?

A. Two commonly known cause-effect relationships that were established with epidemiologic data are the relationship between cigarette smoking and lung cancer, and the relationship between certain types of asbestos and mesothelioma. These relationships were eventually confirmed by laboratory evidence.

Q. What kind of epidemiological data is necessary to establish a cause-effect relationship?

A. Unlike laboratory research, it is the nature of epidemiologic research to produce a pattern of mixed results (positive and negative results) even where there is no cause and effect relationship between the disease at issue and the agent under investigation. In fact, in the absence of a cause-effect relationship, epidemiologists would expect to find a large proportion of reported positive findings due to random chance alone. This is due to the non-experimental nature of epidemiologic research, and it underscores the need for consistent findings in drawing causal inferences.

This need for consistency has been well documented in cancer epidemiology. Agents that have been accepted as genuine causes of human cancer share one feature in common -- from the early epidemiologic research forward there was a high degree of consistency of results among independent studies and evidence of relatively strong associations between exposure and cancer risk. In contrast, epidemiologic studies of agents which have not been and may never be accepted as causal have been characterized by inconsistent findings both within and among studies and, at best, weak statistical associations.

Q. How do epidemiologists evaluate a body of cancer epidemiologic research?

A. Application of the principles of epidemiology to assess the carcinogenic potential of an agent involves a three-step process. First, individual studies are examined to assess the quality, reliability and validity of the research results. Second, the studies are examined collectively, giving appropriate weight to individual studies, to assess consistency and other important indicia of causality. Third, the epidemiologic studies are considered in context with basic science research and other available information to assess the coherence or biologic credibility of causal inferences.

Q. You have used the term "exposure" to an agent -- what does exposure mean in epidemiology?

A. Exposure means contact with the environmental agent in question.

Q. How is exposure determined?

A. The ideal method is direct measurement, but that is often difficult to achieve.

Q. When direct measures of exposure are not available, how is exposure determined?

A. If exposure cannot be measured directly, then epidemiologists use some substitute (sometimes called a surrogate or a proxy) as an indicator of exposure.

Q. Could you give us some examples of surrogate measures of exposure?

A. Some commonly used surrogates are information from questionnaires, from employment records, and from death certificates.

Q. What exposure data can be derived from surrogate sources?

A. Using surrogate data sources, inferences about the population's actual exposure are made based upon some supposed exposure.

Q. What would be an example of this type of inference?

A. For example, using occupational information from a death certificate, one might infer that an individual who was listed as having worked as a service station employee had been exposed to gasoline fumes.

Q. Are there any problems with surrogate measures of exposure?

A. Yes, there can be. With surrogate measures it is often difficult to know if the surrogate is a reliable predictor of actual exposure. In the example above, the service station employee may only have worked at the cash register, and may never have been exposed to gasoline fumes. This is one of the typical problems of surrogate measures of exposure.

Q. Is it important to have accurate measures of exposure in epidemiologic studies?

A. I think its fair to say that epidemiologic studies are only as good as their assessment of exposure.

Q. What is the consequence of not having reliable exposure data?

A. A lack of good data on exposure can throw the results of a study into question. If we are not confident about the actual exposure, it's unlikely we can have confidence in the results of the study.

Q. Is evaluation of the surrogate a factor that you consider when analyzing individual studies?

A. Evaluation of the surrogate is an important factor in the evaluation of epidemiological studies, both individually and as a group.

Q. After completing an evaluation of one individual epidemiologic study, is an epidemiologist in a position to say whether or not a particular agent has an adverse effect on health?

A. No. Conclusions concerning the relationship of a agent to a disease rarely if ever can be based on a single study.

Q. What else is necessary to reach a conclusion from epidemiologic research?

A. Conclusions need to take into account the results of all the relevant studies. It is thus necessary to evaluate not only the strengths and weaknesses of each study on its own merits, but also the strengths and weaknesses of each individual study in the context of the entire body of literature.

Q. Do you ever completely dismiss the results of an epidemiologic study when conducting this evaluation?

A. Occassionally, a study is so deficient that that it provides no useful information and can be completely dismissed. More commonly, the analysis of the individual study reveals that it has both strengths and limitations. Some studies are so limited, either in design or in conduct, that they are properly termed "hypothesis-generating" -- that is, they raise a question without providing useful or persuasive information about the answer to that question. Such studies are given relatively little weight in the overall epidemiologic analysis.

This is far different from dismissal of the study. An epidemiologic evaluation that did not include analysis of substantially all of the available data would be of questionable utility, and the utility of the evaluation would decrease as larger amounts of data were dismissed or otherwise excluded.

## III. Epidemiologic Research on EMF and Cancer

Q. Dr. Cole, how many epidemiologic studies have been published regarding EMF and cancer?

A. Approximately 50 epidemiologic studies have been published which contain data on EMF and cancer. These studies are known as the "primary" literature.

Q. Have you personally reviewed each of these studies as part of your analysis of the epidemiologic literature?

A. Yes. As I stated earlier, review of the strengths and limitations of each individual study is a necessary step in analysis of a body of epidemiologic literature. I have therefore carefully reviewed the data, design, and conclusions of each of the epidemiological studies in the primary literature.

Q. Did you review any literature other than the primary literature?

A. Yes. The epidemiologic literature in this area also includes commentaries on the literature, review articles, and abstracts. I have reviewed all of this secondary literature of which I am aware, including the Office of Technology Assessment ("OTA") review, and the draft Environmental Protection Agency ("EPA") report. It should be emphasized, however, that these secondary sources do not contain original data, and are by and large interpretive in nature. Thus, while the secondary literature can be helpful in understanding the state of the scientific debate, they should not be given the same weight as the primary literature.

Q. What kinds of epidemiological studies have been conducted regarding EMF and cancer?

A. Several designs and methods have been used to study a variety of cancers and measures of exposure to magnetic fields. For childhood cancer, the most important studies are the residential case-control studies. Adult cancers also have been examined in residential studies, using both the case-control design and the retrospective follow-up ("RFU") design. There have also been numerous studies of adult cancers in the occupational setting. The

occupational studies are case case-control studies, proportionate mortality ratio ("PMR") studies, and retrospective follow-up studies.

### A. The Research on EMF and Childhood Cancer

Q. Dr. Cole, please describe the "case-control" design of epidemiology studies.

A. In the case-control method, one identifies a group of people who have developed a disease, such as cancer. These people are known as the "cases." A second group of people, known as the "controls," is then identified. This group consists of people who have not developed the disease of interest, but who in other ways are similar to the cases. For example, the cases and controls should be of the same age group.

Having identified these two groups, one then seeks to describe the pattern of exposure to the agent of interest in each group, looking for similarities and differences in that exposure.

Q. Does the case-control method have any particular strengths?

A. Yes. For example, case-control studies generally allow for good control over confounding. Not every case-control study, however, controls adequately for confounding.

Q. What is confounding?

A. "Confounding" means that the factor under study is not an actual cause of the disease but rather is only a marker of some true cause. For example, some early studies seemed to show an association between coffee drinking and bladder cancer. It is likely that coffee drinking does not cause bladder cancer. Rather, these studies were probably reflecting the the fact that heavy coffee drinkers are, on average, heavy smokers. Smoking is a well established cause of bladder cancer.

Q. Does the case-control study have any particular weaknesses?

A. Yes, the design is difficult to conduct correctly and is prone to bias.

Q. What is bias?

A. In epidemiology the word "bias" is not used in its everyday sense of "prejudice." Instead, it means "in error."

Q. Please describe the residential case-control childhood cancer studies related to EMF.

A. To date there are five published residential case-control studies regarding EMF and childhood cancer: the studies by Wertheimer and Leeper (1979), Savitz (1987, 1988), Fulton (1980) , Tomenius (1986), and Myers (1988). Generally, these studies identified childhood cancer victims in a specific geographic area and time frame, as well as a group of controls in the same geographic area and time frame. The residences of these cases and controls were identified, and data was collected for each residence related to magnetic fields or some surrogate for magnetic fields. It would be worthwhile to separately discuss each of these childhood cancer studies to help provide an understanding of some of the epidemiologic issues involved in this area.

**Wertheimer and Leeper (1979)**

Q. What was the first epidemiologic study on EMF and childhood cancer?

A. The Wertheimer and Leeper study, which was conducted in Denver, was published in 1979. It was the first published epidemiology study in this area and initially raised the question of whether exposure to magnetic fields is associated with an increased cancer risk.

Q. Did Wertheimer and Leeper study actual magnetic field levels, or did they use a surrogate?

Q. Wertheimer and Leeper used only a surrogate for magnetic fields known as the "wiring code." The "wiring code" is a a surrogate measure of exposure in which the size, number, arrangement, and distance of electric lines are used to predict residential exposure to magnetic fields. These data are gathered visually. For example, the relative width of the electric lines might be estimated by simply looking at the wires from the street.

Q. What results did Wertheimer and Leeper report in their 1979 study?

A. They reported that the wiring code was associated with childhood cancers, including childhood leukemia.

Q. What is your evaluation of the Wertheimer and Leeper study?

A. As I stated earlier, the Wertheimer and Leeper study was the first epidemiological study conducted regarding EMF and cancer. As is often the case with such exploratory studies, there were numerous problems in the design, conduct, and analyses of the study.

Q. Could you give an example of a limitation of the Wertheimer and Leeper study?

A. Yes, I can give several examples of major shortcomings with this study. First, the cases and controls are of dubious comparability. If the case and controls are not truly comparable, the results of any comparisons between the two groups are also questionable.

Second, the data results show a pattern that I can only term as "inordinately consistent." The paper contains about 30 comparisons between the wiring code surrogate and various types of cancer. The results of all but one of these comparisons fall within a narrow range of positive associations. This pattern has not been observed in subsequent studies and, based upon my experience reviewing epidemiologic data on other agents, the inordinate consistency of this pattern stretches credulity. It is more likely that this inordinate consistency resulted from a systematic bias in the study design or conduct.

Third, the researchers did not use "blind" procedures when they assigned wire code classifications to the residences involved in the study. In other words, the person who assigned the wiring code for each residence knew whether or not it was the residence of a cancer case. Unblinded procedures such as this can introduce bias into a study. Additional shortcomings could be enumerated. These limitations of the Wertheimer and Leeper study render it non-persuasive, and I view it merely as an early exploratory study that raised the question of whether exposure to EMF is associated to cancer, but that contributed little if any to answering that question.

## Fulton (1980)

Q. Please describe the Fulton study.

A. Fulton attempted to replicate the findings of Wertheimer & Leeper concerning childhood leukemia by using the wiring code surrogate in a study of childhood leukemia in Rhode Island.

Q. What results did Fulton report?

A. The conclusions of Fulton were in conflict with those of Wertheimer & Leeper. Fulton found no relationship between childhood leukemia and wiring codes. The Fulton study also is an early study that has been subject to criticism. The findings and interpretation of the study are unclear, and I consider it to be a negative, but non-persuasive, study.

## Myers (1988)

Q. Please describe the study by Myers.

A. The Myers study was conducted in the United Kingdom. Rather than using the wiring code surrogate, the Myers study used distance from power lines as a surrogate for magnetic field exposure.

Q. What were the results of the Myers study?

A. The Myers study, which was conducted in the United Kingdom, reported a slight increase in cancer risk for those living near a power line, but also reported a slight inverse association between estimated magnetic field strength and cancer risk. In other words, residences close to a power line were associated with a slightly smaller risk of cancer than those further away from a power line. The Myers study does not support the hypothesis that magnetic fields are related to childhood cancer. Because of certain limitations in the study, including its small size, the study also does not provide persuasive refutation of the hypothesis.

## Tomenius (1986)

Q. Please describe the residential childhood cancer study by Tomenius.

A. Tomenius conducted his study in Sweden. He also used a "distance" surrogate. He categorized a residence as "exposed" to magnetic fields if the residence was within 150 meters of an electrical installation, including power substations, transformers, electric railroads, high voltage wires, and subways. In addition, Tomenius collected data on measured magnetic fields.

Q. What were the results of the Tomenius study?

A. There were two major findings of importance to interpreting the Tomenius study. First, with regard to childhood leukemia, the Tomenius study found no association either for the distance surrogate or for measured magnetic fields. This was inconsistent with the Wertheimer and Leeper results, which had found an increased risk of childhood leukemia associated with the wiring code. Second, Tomenius' data shows an inverse trend between distance of a dwelling from the installation and cancer risk. Tomenius also found an inverse relationship between cancer risk and measured magnetic fields. In short, the Tomenius study reported that, among those exposed to magnetic fields (whether exposure was measured by the surrogate of distance or with actual magnetic field measurements), increased exposure resulted in lower cancer risk. Due in part to the inverse trend apparent in the Tomenius data, I conclude that the Tomenius study provides no support for the hypothesis that EMF's are related to an increased risk of childhood cancer. In fact, it provides some evidence against that hypothesis.

## Savitz (1988)

Q. Please describe the residential case-control study of childhood cancer conducted by Savitz.

A. The Savitz study, which was conducted in Denver, collected data on the wiring code surrogate and childhood cancer. Savitz also collected data on actual measured magnetic fields and childhood cancer. In addition, Savitz collected data unrelated to magnetic fields, such as parental smoking, breast feeding history, and traffic density on nearby streets.

Q. What is your overall evaluation of the Savitz study?

A. The Savitz study showed a marked improvement in design, conduct, and analysis over previous childhood cancer studies. This study also has limitations, several of which are pointed out by Dr. Savitz himself in his published paper. To date, however, this study should be considered the best done of the published studies on childhood cancer and EMF.

Q. What were the results of the Savitz study?

A. The Savitz group has reported data in several publications. In summary form, Savitz' data show no significant associations between cancer and measured magnetic fields, but they did show a few weak, irregular statistically significant associations between childhood cancer and the wiring code surrogate. Savitz also has reported data from this study population showing similar weak associations between childhood cancer and failure to breast feed (Davis and Savitz, 1988), between childhood cancer and paternal smoking (John and Savitz, 1991), and between childhood cancer and high traffic density on nearby streets (Savitz and Feingold,1989).

Q. Is it unusual to find a few irregular statistically significant associations in a case-control study?

A. No, in fact they are to be expected even when nature is in a null state -- that is, even when there is no true association between the exposure (or the surrogate) and cancer. Random chance alone would produce some statistically significant associations in a large group of calculations such as conducted in the Savitz study.

Q. What is your interpretation of the results of the Savitz study?

A. The Savitz study contains a large amount of data, and many comments on this study are possible. For this testimony, I would like to make three comments. The first deals with the data on measured magnetic fields and the wiring code surrogate. The pattern of data, with no statistically significant associations between measured fields and cancer, and a few weak irregular associations between the wiring code and cancer, is consistent with nullness -- that is, with a lack of effect of exposure to magnetic fields.

The second level of interpretation attempts to place the magnetic field data in the context of the larger body of data that has been reported from the Savitz study on breast feeding, traffic density, and exposures. The pattern of weakly positive results found in this larger body of data suggests that none of the exposures is itself a cause of childhood cancer. Rather, I interpret this pattern as suggesting a profile or risk factors that characterize children at greater risk of leukemia: parents who smoke, absence of breast feeding, residence near busy streets, etc. Viewed in this larger context, I would hesitate to accept any of the weak reported associations, including the weak irregular associations with wiring code, as descriptive of a cause-effect relationship.

Finally, I would note that the positive associations with the wiring code that Savitz did report are weaker than the associations reported by Wertheimer and Leeper. In epidemiology, we expect that if a reported statistical association in one study is truly descriptive of an association that is occuring in the real world, that association will become stronger, not weaker, as the design and conduct of subsequent studies improves. The fact that the reported association is weaker in the relatively well-done Savitz study suggests that there is no true association between EMF and cancer underlying these statistical reports.

**Peters**

Q. Are you familiar with the childhood cancer study in California by Peters?

A. Yes. The Peters study is residential case-control study of childhood cancer conducted in California.

Q. What is the basis of your familiarity with the Peters study?

A. I was a member of the advisory committee which sponsored the Peters study, and was present at the advisory committee meeting at which Dr. Peters presented his preliminary results. Since the results were preliminary, they are subject to change as analysis and review of the data continue. At present, the Peters' results have not appeared in the published literature. The only public announcement regarding the Peters' study of which I am aware is a press release summarizing some of his preliminary findings.

Q. What is your evaluation of the Peters study?

A. Not enough design information and data are available from the Peters study to conduct a detailed epidemiologic evaluation of the study. As an epidemiologist, I therefore cannot rely upon the Peters study until information is available to conduct such an evaluation.

Q. Are you able to reach any conclusions about the Peters study?

A. With the limitations just expressed, I can state my preliminary opinions about the Peters study. The Peters study examined spot measurements of magnetic fields, 24-hour measurements of magnetic fields, and the wiring code. Peters found no association between either of the measures of magnetic fields and childhood cancer. He found an association between the wiring code and childhood cancer that is weaker still than the association seen by Savitz. With regard to the findings on measured magnetic fields, my preliminary evaluation is that this study confirms the consistent findings in the published literature that there is no association between measured magnetic fields and cancer. With regard to the findings on wiring code, my preliminary conclusion is that the weaker association between the wiring code and cancers continues the pattern of better-done studies reporting smaller associations, which again suggests that there is no true association between EMF and cancer underlying these statistical reports.

**Additional Issues Related to the Childhood Cancer Studies**

Q. Dr. Cole, are there any further issues that you would like to address with regard to the childhood cancer studies?

A. Yes. I would like to mention three further issues: consistency of results, biological plausibility, and an ecologic comparison.

Q. What is the importance of consistency in the childhood studies?

A. As noted previously, consistency is an important factor in accepting a cause-effect relationship. If a cause-effect relationship truly existed between an exposure and a disease, one would expect that relationship to appear consistently in any one study, and also to appear consistently between studies.

Q. Do the childhood cancer studies show consistent results?

A. The data from the wiring code and other surrogates is inconsistent. Some studies report a weak association, while others fail to report an association. The data from the measured fields consistently shows no statistically significant association between measured fields and cancer risk.

Q. What is meant by "biological plausibility"?

A. Biological plausibility refers to the question of whether there is a credible biological mechanism that supports the hypothesis that exposure to the agent causes cancer. Biological plausibility can be established either through mechanistic research, or through animal research that shows that exposure to the agent causes cancer in experimental animals.

Q. What is the importance of biological plausibility?

A. The importance of biological plausibility varies depending upon the strength and consistency of the epidemiological evidence. If the epidemiological data consistently shows high risks of exposure, as is the case with cigarette smoking and lung cancer, we can accept the epidemiological data even without supporting evidence of biological plausibility. When the epidemiological data is weak and inconsistent, biological plausibility is more important. If the experimental data is supportive of biological plausibility in such a situation, we can place more confidence in the epidemiologic results; if biological plausibility is missing, we place less confidence in the epidemiological results.

Q. Does the experimental data on EMF support biological plausibility?

A. No. While biological effects have been reported by some researchers, those effects are not related to the processes of carcinogenesis and do not provide biologically plausible support for the epidemiological data.

Q. Do you consider it to be "non-scientific" to include biological plausibility in your evaluation of the epidemiologic data?

A. No. Biological plausibility is a standard factor that is considered in the evaluation of most, if not all, bodies of epidemiologic literature. Professor Branford Hill (1965), in his widely quoted paper published in the *Proceedings*

*of the Royal Society of Medicine,* listed biological plausibility as one the factors that should be considered in an epidemiological evaluation.

Q. What is an ecological comparison?

A. In an ecological comparison, one collects data on the general population, rather than on individuals within that population. For example, in this century cigarette smoking and lung cancer rose sharply during the same time frame, a fact that provides persuasive evidence that the there is a causal relationship between the two events. This type of ecological comparison, while subject to limitations, can provide important information as to a suggested relationship between an agent and a disease.

Q. Have you conducted an ecological comparison for EMF and childhood cancer?

A. Yes. Using power consumption as a surrogate for EMF exposure, data show that power consumption has increased approximately 10-fold since 1950. If exposure to EMF was associated with, for example, childhood leukemia, we would expect to see a similar sharp rise in childhood leukemia rates over that same time period. Yet childhood leukemia rates rose approximately 20% from 1950-1960, and it is probably true that most of that increase was not attributable to an actual increase in leukemia, but rather to improved diagnostic procedures. Childhood leukemia rates have remained essentially stable since 1960. This ecologic comparison further supports the conclusion that exposure to EMF is not causally associated with childhood cancer.

**Conclusion Regarding the Childhood Cancer Studies**

Q. Dr. Cole, what is your overall conclusion concerning the childhood cancer studies?

A. Looking at the childhood cancer studies as a whole and recognizing the degree of validity of individual studies within that group, I conclude that there is no demonstrated increased risk of childhood cancer when actual electric or magnetic fields were measured. When surrogates such as the wiring code have been used in the childhood studies, the data have shown a pattern of mixed results between weak positive and negative outcomes. This is a pattern of random association in a non-experimental science, not the

pattern of cause and effect. Long term time trends of EMF exposure and leukemia in children in this country are not consistent with a cause-effect relationship. Finally, neither theoretical considerations nor animal studies are available to support a cause-effect hypothesis. I conclude that the epidemiologic literature on EMF, when viewed as a whole, provides no persuasive scientific support for the hypothesis that EMF causes cancer in children.

**B. The Draft Report of the Environmental Protection Agency**

Q. Dr. Cole, are you familiar with the draft report of the Environmental Protection Agency on cancer and EMF?

A. Yes. As I stated earlier in my testimony, I have reviewed the draft EPA report.

Q. Does the EPA report discuss the epidemiologic literature on EMF and childhood cancer?

A. Yes, there is a separate chapter on the epidemiologic literature.

Q. Did you rely upon the draft EPA report in forming your opinion about the epidemiologic literature and childhood cancer?

A. As I stated previously, secondary sources such as the draft EPA report can provide useful context for the scientific debate, but they do not provide primary data or design information. I therefore do not give substantial weight to secondary sources. The draft EPA report also is a somewhat special case even amongst the secondary sources.

Q. Why do you refer to the draft EPA report as a "special case"?

A. Normally, literature reviews and other secondary sources are published in the peer-reviewed literature. These publications have thus been critiqued by expert reviewers before they are published. The draft EPA report, while it has been made public, is still undergoing its "peer-review" process. This fact is clearly indicated on the cover of the draft EPA report. For instance, it states: "Draft: Do Not Quote or Cite." In addition, the comments that have been generated as part of the review process suggest substantial revision of the draft EPA report may be forthcoming.

Q. Could you briefly describe the ongoing review process for the draft EPA report?

A. The draft EPA report was authored by internal staff at the EPA. The EPA has convened a panel of outside experts from academic and research institutions, known as the Non-Ionizing Electromagnetic Fields Subcommittee (the "NIEMFS" panel), to review and comment on the document. This panel held several public meetings to receive comment on the EPA report and conducted its own review of the draft report. Its final comments, which were recently forwarded to the Radiation Advisory Committee of the Science Advisory Board of the EPA, state that " currently available information is insufficient to conclude that the electric and magnetic fields are carcinogenic" and that "the EPA document has serious deficiencies and needs to be rewritten." The NIEMFS panel's recommendations have been adopted by the Radiation Advisory Committee and forwarded to an Executive Committee of the Science Advisory Board.

In addition, the Committee on Interagency Radiation Research and Policy Coordination ("CIRRPC") has also submitted comments on the draft EPA report. CIRRPC is comprised of representatives from several agencies of the executive branch of the federal government. Many of those agencies have submitted comments on the draft EPA report. CIRRPC has compiled those comments and forwarded them to the EPA. Some of those comments have been supportive of the report. Others were not supportive. For instance, the National Cancer Institute stated in its comments that: "Four staff members, all well-versed in epidemiologic studies, reviewed the above document and written comments are provided. All reviewers agreed that the human epidemiologic data to date did not support the sweeping conclusions made in the EPA document. In our judgment the conclusions presented remain scientifically unsound and unnecessarily alarming."

It is not possible to predict what changes will ultimately be made to the draft EPA report at the conclusion of this review process. In my opinion, however, the draft EPA report should be treated as a secondary source that is still in the process of review, and I give it little if any weight in my evaluation of the epidemiologic data.

## C The Research on EMF and Adult Cancer

Q. Dr. Cole, have you also reviewed the epidemiologic data on adult cancers?

A. Yes. The adult cancer studies include residential studies similar in design to the childhood studies, as well as occupational studies.

### The Adult Residential Studies

Q. How many adult residential studies have been conducted?

A. Four adult residential studies have been conducted: Wertheimer and Leeper (1982), Severson (1988), Coleman (1985, 1989), and McDowall (1986). The first three of these are case-control studies, while the McDowall study is a retrospective follow-up study. (I will discuss the retrospective follow-up design in my discussion of occupational studies.)

Q. What were the results of the adult residential studies?

A. Wertheimer and Leeper conducted a residential case-control study of adult cancer in Denver very similar in design to their childhood study, and with many of the limitations of that study. They reported a strong and consistent relationship between the wiring code and adult cancers. The remaining adult residential studies, including the case-control study by Severson in Washington State, the Coleman study in London and the RFU study by McDowall in East Anglia, England, reported no association between wiring code, distance from power lines, or measured fields and adult cancer.

Q. What do you conclude with regard to the residential studies of adult cancer?

A. With the exception of the early, flawed report by Wertheimer and Leeper, the residential studies of adult cancer have been consistently negative for both actual measured fields and for magnetic field surrogates. I conclude that these studies provide no support for the hypothesis that EMF is associated with an increased cancer risk in adults.

## The Occupational Studies

Q. Please describe the occupational studies of adult cancer and EMF.

A. The occupational studies use some form of occupational information, such as a job title, as a surrogate for magnetic field exposure. For example, an occupational study might categorize all electricians as "exposed" to EMF.

Q. What types of study designs have been used in the occupational studies?

A. As I noted earlier in my testimony, the categories are: (1) case control studies, (2) proportionate mortality ratio ("PMR") studies, and (3) retrospective follow-up ("RFU") studies.

Q. Before discussing the individual study designs, are there any comments you would like to make generally about the occupational studies?

A. Yes. The approach of using occupational information such as job titles has limitations. As noted, these studies might categorize all electricians as "exposed " to EMF. However, not all electricians will have exposure to EMF. Those who do have exposure to EMF may also have exposure to industrial chemicals, welding solvents, or other potentially carcinogenic agents. This latter factor is an example of "confounding." Potential confounding is difficult to control in occupational studies, and is a major limitation of those studies.

Q. Does failure to control for confounders effect your evaluation of the studies?

A. Yes. it effects the evaluation of the individual studies, and the body of literature as a whole. If an individual study does not control for confounders, less confidence should be placed in any associations that are reported, because the researchers were not able to exclude other possible causes of the cancer.

The failure to control for confounders also effects the evaluation of the body of literature as a whole. The associations with cancers that have been reported in the occupational literature are not consistent. For example, one study might report an increase in leukemia, but not in brain cancer, while another study might report an increase in brain cancer, but not in leukemia.

It is possible that the workers in the first study had increased exposure to benzene, which is known to cause leukemia in adults, while the workers in the second study had exposure to some agent associated with brain cancer. The inconsistent pattern of results in these studies is thus consistent with multiple confounders operating in the various studies.

Q. Have any of the occupational studies controlled for confounders?

A. Yes. Despite the difficulty of controlling for confounders in an occupational setting, Thomas (1987), in a study of brain cancer, was able to distinguish electrical workers who were exposed to only electric and/or magnetic fields from electrical workers who had additional exposures. Thomas found no increased risk of brain cancer in workers exposed only to electric and/or magnetic fields, suggesting that exposure to EMF is not a cause of brain cancer. The Thomas results suggest that confounding should be considered a substantial factor in the evaluation of the occupational studies.

## The Occupational Case-Control Studies

Q. Please describe the occupational case-control studies.

A. Similar in design to the residential studies, the occupational case-control studies compare exposure histories of subjects with and without cancer to determine the relative risk associated with the surrogate for EMF exposure used in each study.

Q. What were the results of the occupational case-control studies?

A There have been 19 non-residential case-control studies which have examined various indices of possible EMF exposure. These studies are useful, but it must be borne in mind that the case-control design is a difficult type of investigation to conduct. All of these studies have limitations and many are flawed. One major limitation is that none of the studies included measurements of EMF exposure. In addition, the men in these studies most likely were exposed to known or suspected carcinogens. Nonetheless, fourteen of these studies have reported some positive association between a surrogate of EMF exposure and cancer. It should be noted that the Thomas study, which did control for confounders, was a case-control study.

Q. Are you familiar with an occupational study by Matanoski?

A. Yes. I was a member of the advisory panel for the Matanoski work. There are actually two studies by Matanoski, neither of which has appeared in the peer-reviewed literature. The first, and larger, of the studies is a nationwide case-control study of mortality in telephone line workers. The second is a retrospective follow-up study of cancer incidence in telephone line workers, limited to data from the state of New York.

Q. What is your evaluation of the Matanoski study?

A. As with the Peters study, not enough information is available about the data and design of the Matanoski to conduct an epidemiologic evaluation. The preliminary results of which I am aware, however, suggest that Dr. Matanoski observed 2-3 more cases than expected of male breast cancer amongst the New York state linemen. I should emphasize that an individual study must be evaluated in the context of the entire body of epidemiologic literature. When evaluated in the context of the larger body of epidemiologic literature, I view these preliminary results from the Matanoski study as positive, but non-persuasive.

**The Proportionate Mortality Ratio Studies**

Q. Please describe the proportionate mortality ratio studies.

A. The PMR occupational studies have evaluated the proportion of workers presumptively exposed to EMF who die from cancer. As noted above, proportionate mortality studies generally relate to occupational titles found on the death certificates of persons employed in occupations where the researchers assume (without testing) that there was more exposure to EMF than is experienced by the general population. A major limitation of PMR occupational studies is that spuriously high PMR's can emerge if the exposed workers have an overall mortality rate that is lower than that of the general population. This is typically observed in occupational settings (i.e., the "healthy worker" effect) and may yield the appearance of risk that is, in reality, artificially inflated.

Q. What were the results of the PMR studies?

A. There are eight such studies and they are evenly divided between positive and negative. The PMR studies, however, do not provide a reliable basis to infer causality, principally because of the inherent limitations of such studies. In this type of studies, slightly elevated cancer mortality rates are to be expected due to the deficit of mortality that is typically observed among active workers. The results of these studies are consistent with this expected phenomenon and, as such, I do not believe that the results of the proportionate mortality studies are helpful in answering the question of whether occupational EMF exposure is associated with cancer.

**The Retrospective Follow-Up Studies**

Q. Please describe the retrospective follow-up studies.

A. In the RFU occupational studies, a group of subjects who presumably sustained moderate to heavy exposure to EMF over a long period of time (e.g., 5 - 25 years) are traced up to a recent point in time. The number of deaths within the group from specific diseases of interest are then compared with the number to be expected if they had died at the same rate as the general population. In general, the principal advantage of RFU studies is that they usually are not biased to a large degree. The principal disadvantage is that they are typically prone to confounding and may be imprecise given the relatively few deaths from any one particular cause.

While the retrospective follow-up studies are not without their problems and limitations, they generally are considered to be the most valid form of epidemiological research that has been conducted to address the question of whether cancer is linked to EMF exposure. One of the limitations of the RFU studies is that they evaluate EMF exposure using job titles. In most other ways (i.e., absolute measures of disease risk; superior, less arbitrary control groups), however, they are more reliable than are proportionate mortality ratio or case-control studies.

Q. What were the results of the RFU studies?

A. There have been 10 RFU studies and only one has reported positive results. It is striking that virtually all of these studies are negative, a fact which provides strong support against the hypothesis that EMF exposure causes cancer.

### Conclusion Regarding the Adult Cancer Studies

Q. What do you conclude about the epidemiologic studies involving power frequency electric and/or magnetic fields and adult cancer?

A. The studies involving power frequency electric and/or magnetic fields and cancer show weakly positive to zero or negative associations. The studies that use the most acceptable design consistently have found no association between EMF and cancer. 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.

## D. Conclusions Regarding the Epidemiologic Research on EMF and Cancer

Q. Dr. Cole, do you have an opinion on the relationship between power frequency electric and/or magnetic fields and cancer in humans?

A. Yes, I do.

Q. What is that opinion?

A. Taken together, the epidemiologic reports fail to demonstrate any strong or consistent pattern of association between EMF and cancer among human beings. Indeed, even among the positive studies, findings are not supported by the internal characteristics of the data. The summation can only be that, to date, there is no demonstrated relationship between EMF and cancer in human beings.

Q. In your opinion, will persons residing near or otherwise proximate to the Woodbourne-Heaton 230 kV transmission line have an increased risk of cancer from the electric and/or magnetic fields associated with that line?

A. Since I am of the opinion that EMF's have not been demonstrated to be carcinogenic, I believe that the 230 kV transmission line poses no threat of cancer to persons in its vicinity.

Q. Does this conclude your testimony?

A. Yes.

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BEFORE THE
PENNSYLVANIA PUBLIC UTILITY COMMISSION

EXHIBIT PC-1
OF
DR. PHILIP COLE

ON BEHALF OF
PHILADELPHIA ELECTRIC COMPANY

November 1991

# CURRICULUM VITAE


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| Licensed, Alabama Medical Licensure Commission | 1981 |
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| Honors: | American Cancer Society, Faculty Research Award | 1973-78 |
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| | Grand Prix Lacassagne du La Ligue Nationale Francaise contre le Cancer (with B. MacMahon, J. Brown and D. Trichopoulos) | 1986 |
| Major Committees: | | |
| | Scientific Advisory Committee Division of Cancer Cause and Prevention National Cancer Institute | 1978-80 |
| | Epidemiology and Disease Control Study Section National Institutes of Health | 1973-77 |
| | Clinical and Epidemiological Research Advisory Group National Cancer Institute of Canada | 1973 |
| | Committee on Epidemiology and Prevention (Chairman) National Bladder Cancer Project | 1971-73 |
| | General Motors-United Auto Workers Occupational Health Advisory Board | 1982-87 |
| | Mott Prize Selection Committee General Motors Cancer Research Foundation | 1985 |
| | Prevention, Cancer Control (Chairman) Steering Committee for the National Planning Effort National Cancer Institute | 1984-85 |

Major Committees (continued):

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|---|---|
| Board of Scientific Counselors<br>Division of Cancer Prevention and Control<br>National Cancer Institute | 1986-1990 |
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Teaching:

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| Harvard School of Public Health | |
| The epidemiology of chronic diseases | 1969-72 |
| The epidemiology of neoplastic diseases | 1973-77 |
| Epidemiologic methods | 1976 |
| Principles of epidemiology | 1978-79 |
| University of Minnesota - Graduate Summer Session | |
| The epidemiology of cancer | 1971,74-80 |
| Principles of epidemiologic research | 1985 |
| Fundamentals of epidemiology | 1986, 87 |
| International Agency for Research on Cancer | |
| Cancer epidemiology | 1974,76,78,80 |
| University of Massachusetts-Graduate Summer Session | |
| Principles of epidemiology | 1981-84 |
| Cancer epidemiology | 1982 |
| Tufts University - Graduate Summer Session | |
| Epidemiologic bases of public health policy and law | 1986, 87 |
| University of Alabama at Birmingham | |
| Epidemiology of cancer | 1980 |
| Principles of epidemiologic research | 1980-83 |
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Research Interests:

The epidemiology of breast cancer, Hodgkin's disease and bladder cancer
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Editorships:

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| Associate Editor, Cancer Research | 1982-85 |
| Associate Editor, American Journal of Epidemiology | 1982-88 |
| Editorial Board, International Journal of Breast and Mammary Pathology | 1984- |
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