Appendix E

Observer Comments Provided at the Peer Consultation Workshop

Note: The peer consultation workshop included three observer comment periods, one on the first day of the workshop and two on the second day of the workshop. This appendix includes verbatim transcripts (to the extent that specific remarks were audible from recordings) of the observer comments, in the order the comments were given.

Appendix E
Observer Comments Provided at the Peer Consultation Workshop

Day 1, Comment 1: Jenny Bard, American Lung Association of California

I actually signed up today as a private citizen, but since I am listed with the American Lung Association, I would just say that we are keenly interested in the work you are doing for obvious reasons. Any time you look at cancer risks, and indeed lung-disease risk, from exposure to asbestos, our organization has been intimately involved with providing resources and assistance to people with lung disease and asbestos-related lung disease. So, we want to thank you for all the time and effort you are doing on this very important issue.

In fact, the American Lung Association of California and the California Thoracic Society have actually requested, just for the record, that due to public health concerns from naturally occurring asbestos (and, in particular, tremolite asbestos) in California; and, in fact, maybe I could just help you orient a little bit. This map shows all the locations of asbestos deposits in the state of California. We can post this. I'll leave it here for the three days. I will only be attending for today. The green on that map is considered asbestos, but it includes all the forms of asbestos. There is a yellow marker indicating the areas of specifically tremolite asbestos, and it is a very localized tiny little area, but you will see it up there.

We remain concerned that a public health threat from naturally occurring asbestos may exist to residents who live in these areas, especially in areas where tremolite out-croppings have been identified. In order to fully understand the public health impacts from naturally occurring asbestos, and to better characterize areas of potential concern for naturally occurring asbestos exposure, we support additional research, including air monitoring, soil sampling, and exposure studies in these areas. We are particularly concerned about tremolite and other types of amphibole asbestos fibers, because recent research has demonstrated that amphiboles pose the greatest public health threat, and indeed that is somewhat supported by the methodology that you are reviewing. We have asked for expanded and aggressive air monitoring and soil sampling for amphibole asbestos fibers, especially tremolite, in areas where soil has been disturbed due to construction or where out-croppings of tremolite asbestos have been identified, such as the Sierra foothills. We support additional research on exposure to naturally occurring asbestos to fill information gaps on naturally occurring asbestos exposure in non-occupational settings and to better characterize the risks to the general population in areas with this mineral fiber. And we support conducting epidemiological investigations of the health effects of tremolite and other amphibole exposure in order to identify the unique health impacts potentially associated with such exposures, including low-level.

So, that's my American Lung Association of California hat. Now I'm going to put on my private citizen hat. The proposed risk assessment recommends two additional studies to fill in the gaps in the findings. I believe it is an amphibole-only cohort and a chrysotile-only cohort. These are mostly occupational studies that you have been reviewing, but I'm hoping that you can begin to

think about environmental exposures as they are taking place in California. The Lung Association gets so many questions about exposure: is it harmful? is it harmful to be around a serpentine rock? should I have it covered? The same kinds of questions that we used to get inside the homes in terms of insulation, we are now getting regarding outside exposures: how much is in the dirt? when you disturb it, how many fibers are going to get into the air?

As an example of the types of potential exposures I am describing, I would like to hand out some pictures. To describe one scenario, I'm not trying to be site-specific, I'm bringing this up so perhaps you can visualize a real-life scenario where environmental exposures are taking place. You are looking at a dirt parking lot. The dirt parking lot is where the students park. This school and adjoining neighborhood have been built on top of tremolite asbestos veins. The soil testing at this school and in the neighborhood: school results have routinely tested positive for tremolite asbestos in every soil sample. There was dirt from a pile of dirt cut out for a road that was 5–95% tremolite asbestos. Many generations of students have had potentially ongoing episodic exposures during human and natural activities on these soils, due to construction, vehicles driving, wind and weather, running, sports, and riding bicycles.

I am here to tell you there has never been a single breathing zone exposure study to determine how many fibers are airborne during these activities. This is not acceptable. Based on your methodology, this is something we would like to see changed. The soil samples collected from the school grounds, including dirt from the parking lot and the soccer field, have had long, thin fibers with aspect ratios up to 1000:1. In the methodology, this would no doubt be considered the most lethal form of asbestos. We have a situation with daily exposures. I guess to summarize, what I am trying to bring to your attention, is that we need the science. We need to know what these exposures are in the environmental situations to know if we are indeed producing a mesothelioma epidemic as we speak, if there is one already under way. The buildings in these areas are 20 to 30 years old; some of the people in the audience may correct me. If there is a mesothelioma epidemic that's going to show up, it will probably be another 20 years. I urge you to use your scientific expertise, your resources, to help get the human exposure studies that are so needed. Thank you.

Day 1, Comment 2: Eric Chatfield, Chatfield Technical Consulting, Ltd.

My name is Eric Chatfield. I am president of Chatfield Technical Consulting, Ltd., in Toronto, Canada. As Bruce said, coming here from Canada, I may be perceived as having a conflict of interest. I have often said that if you understood the relationship between Quebec and the rest of Canada, you wouldn't make remarks like that.

I've got two basic comments at this stage in the game. I am signed up for a comment tomorrow, but I'm pleased to see we have got far enough for me to make a comment now. One is that I've been vaguely uncomfortable for a long time about this protocol. Wayne and myself have discussed it a number of times. One of the principal problems I have is that the original animal

work, because there were not enough malignant tumors, the animal work was done on the basis of total tumors, including all of the benign tumors. The benign tumors were somewhat of the majority in the Davis work. So, we derived an exposure index based on total tumors. We then build on that to create an exposure index for humans and we build on it further to refer to only cancer. So, somewhere we have a shift here. To me, that scientifically doesn't seem acceptable to derive an index on one class of particles and then to extend it to another class of particles.

The second problem I have is the so-called change from 40 μm from the animal studies down to 10 μm. The absence of scientific data doesn't mean to say that should be an accepted group. If we don't have the data, we don't have the data. If we simply make a change from 40 μm down to 10 μm for this critical transition where we apply the increased weighting, and, by the way, there was also another change made, it was 0.4 μm in the animal studies that was increased to 0.5 μm for the index. These changes are both arbitrary and some folks might say capricious. We have a problem here that we don't have the basis to make that change, and I don't see how one can then push a thing like this through into legislation with this kind of departure from a scientific method incorporated in there. And that bothers me. So, I'm just throwing that out for discussion. I know that Wayne and Kenny probably disagree with me, but I think it should be addressed. Thank you.

Day 1, Comment 3: Chris Anaya, resident of El Dorado Hills, California

I've got some issues. There are so many questions, and it's too bad the audience can't participate with some of the questions. I know I'm supposed to make a statement and not ask questions, but there are so many questions that I have that unfortunately that I can't ask. Hopefully, I can put them in writing and one of the panelists can present them later on.

I noticed to look at chrysotile, one of the studies we looked at is the Quebec studies. It bothers me knowing that there is predominantly chrysotile there, but there are also traces of tremolite. Yet, we are assuming the mesotheliomas are from the chrysotile because it is the predominant fiber there, and that bothers me because there are studies showing that traces of amphibole is what is causing the mesothelioma, and not necessarily the chrysotile. The last I read, maybe I'm not followed up with the latest science, but that's what I understand.

Fiber length is an issue, just like what Eric said. My goodness, you can't just change these numbers to force that round peg fit into the square hole, and that's what we're doing, in my view. Before you start doing that, you need to come up with studies that support the scientific basis for doing that. Less than 5 μm is not going to be included, yet talc workers with fibers less than 5 μm are known to have GI cancer; but we are not talking about any kind of cancer, but lung cancer. Why is that? If this is for IRIS, we are supposed to be looking at all cancers, but, no, all I hear about is mesothelioma and lung cancer, and that's good. There's enough evidence showing that GI cancers do exist, do take place. Selikoff's name has been mentioned; he was the first to bring this up. Now, maybe somebody has turned around and said all his research was unfounded, I don't know. But I think we need to look at GI cancers as well. I know for a fact, based on what I

read, if the information is correct, talc workers in fact have received GI cancer from asbestos or talc less than 5 µm.

The Libby lung burden study show that the majority of fibers in the people that have been evaluated are less than 5 µm. So, basically, it wouldn't even meet the definition of asbestos, and yet, tell those people that. Look at the health problems they are having. I believe it's 60% of the fibers they found in the lung burden studies were all less than 5 µm. On the air sampling, I can't see how a body can determine these nice little formulas. I tell you what, formulas are great, but when I see numbers that the coefficient for chrysotile is a number of 3 and the coefficient for amphiboles is going to be 15. That's a 5-times difference. Well, according to Dr. Whitehouse from Spokane who studied Libby, Montana, residents as well as other people exposed to chrysotile and other fibers for the last 30 to 35 years, he said in testimony that, in fact, you have a 100 times greater chance of receiving mesothelioma with tremolite than you do with chrysotile. Yet, this formula here only shows 5-times more greater chance. Dr. Whitehouse is a pulmonary disease specialist, from what I understand, and I think we have a pulmonologist here. I don't know if you have ever seen his research or talked to Dr. Whitehouse, but that is what he has said in testimony based on his evaluation of all the patients and people he has had to deal with, and I'll be happy to provide you with his testimony. I have a copy of it.

There is lack of information regarding asbestiform tremolite. Very few studies that you mentioned up here even address it. And yet, where I'm from, in El Dorado County, we have a significant problem, and yet it is not being addressed because there aren't any studies that show it's a hazard: a little trace of tremolite here in this study, a little trace of tremolite in this study. But all the other amphiboles are studied. But yet nothing there, and so it doesn't bring comfort to me to know that we're going to plug these numbers in and it's not going to be representative of what the environment is that I'm placing my children in.

Another thing is air sampling; that's what I was starting to get to. We have a formula also for converting the exposures based on how air sampling is conducted. Well, if somebody places a monitor in an improper place, where they get zero detects, this nice little neat formula you have is going to have zero exposures. Yet, that is exactly what happened in Libby: you had zero detects on all these people and yet they had many people dying from exposures to tremolite or a form of tremolite asbestos. And they couldn't figure it out; it was because the methodology for sampling was wrong, faulty, and it gave them the wrong data. So, my question is: how do you know every study that we are comparing, these 100s of studies, how exactly did they perfect their monitoring to determine that everyone of them had the same quality of exposure. In my neighborhood, where I'm from, the state, supported by EPA, places their air monitors for tremolite asbestos on top of rooftops; yet, the children, who are 3 feet off the ground, that's supposed to be representative of people on the ground. They are nicely getting zero detects, yet our soil content is 1 to 3%, 5%, or even higher in some cases, depending on the grab sample you take. You know there's exposure, but, when you place a monitor on a rooftop at 2 liters a minute, you're going to get no detects every time. So, I have a problem with plugging in these numbers without having a consistent way of monitoring the air to see exactly what those exposures are.

I don't believe any of these studies that you are using from 20 to 30 years ago really do anybody any justice. The animal studies, for one, are flawed in many cases; this is guesswork. And you are using PCM in many cases, when we should be using TEM. I know that is another thing is that we are finding out now that these ultra-fine fibers are not detected with the methods we used to use. Some of these bulky fibers, you can see them under the older type of methods, but the fibers that I'm dealing with are ultra-fine. You cannot see the fibers in bulk sampling. The same thing occurred in Libby. They could not find any sampling. I think somehow, to sum it up, we need more data. We need to be able to make sure that these methods are not just plugging in numbers to make a nice linear graph, because that's not going to get it for me. I hope it doesn't get it for you. We need scientific information to support what we have. And I'll just close with this: how can this study only claim mesothelioma to be 5 times greater risk than chrysotile when evidence looking at bodies essentially shows different?

Day 1, Comment 4: Stan Dawson, CalEPA

I would like to present an alternative view that gives some perspective on the studies that have been done on mesothelioma and lung cancer and just concentrate on the potencies from the published data. Basically, this is going back before the Crump/Berman report, using data that was available in about 2000, mainly in an article by Hodgson and Darnton, which is a review article that is mentioned in the Berman/Crump report, and also some data by [authors' names inaudible]. Anyway, what I want to emphasize here is another way of looking at this rather than just thinking about averages and standard deviations of distributions of potencies. I wanted to look at the full spectrum of potencies for each mineral. In particular [referring to a graph shown to the panelists], we have crocidolite here, we have amosite here, the mixtures are here, chrysotile is here, and the one Libby study of tremolite. What's plotted on the horizontal access is the lifetime unit risk, which is just the factor K_L multiplied by a few things to get it into the usual regulatory units for lifetime unit risk.

You can see that one of the advantages of this is that, instead of just looking at the average value or the central tendency, which you can talk about the median here, you can look at the 90^{th} percentile here and see the somewhat convergence of the amphiboles and chrysotile. Of course, each one of these is a study, for example, this one is South Carolina (Dement et al.) and that one is, of course, the Quebec study. And, of course, I'm using the previous designation of South Carolina as a chrysotile study, which may be a little simplistic.

OK, well I'll have to use my powers of description to tell you about the mesothelioma result. There are, I believe, copies being made of this, so at least you can see it on the copy. Anyway, the basic result is that, in the case of mesothelioma, the studies of the various minerals are much more separate than they were in the case of lung cancer, but you still can see the perspective you get from looking at the 90^{th} percentile, because these curves are bent over so much, that it's quite a ways away from the central tendency. So, it gives you this spectrum. You know, one of the points I wanted to emphasize, we hear about the differences between the Quebec and the South Carolina study, which is great, but what I'm trying to do is place this into perspective that, for

one reason or another, there is one result in Quebec and there is another result in South Carolina and there is a spectrum of stuff in between. And that's the way it is. So, if we are going to go on to risk assessment, we need to take that into account, that, sure enough, chrysotile can be pretty potent. So, that's it.

Day 1, Comment 5: Jay Turim, Sciences International, Inc.

My name is Jay Turim, and I'm with the consulting company called Sciences International, Inc., in Alexandria, Virginia. I wanted to make one point, and it turns out to be an elaboration of a point made by Dr. Chatfield, and that is a question of the exposure index. I know it is one of the charge questions to the panel. It was spoken about by Wayne this morning, and there was some questions asked by the panel. I'd just like to elaborate and not take too much of the 8 minutes allotted to me.

In the 1995 paper by Berman/Crump/et al. in Risk Analysis, a very important paper and a very good paper, they re-analyzed animal data and came up with an exposure index showing most of the risk is in fibers greater than 40 μm. Wayne, in his comments this morning, made the point well, that although 40 μm turns out to be the number that the statistical analysis of the re-analysis of the animal data showed, we can't expect it to be a step function, and, if anything, probably the potency increases from a number less than 40 μm and it goes up. But, because of limitations and the availability of certain data, the analysis showed 40 μm to be the break point used. It's been known, Stanton and others showed, that fiber length is an important determinant of cancer. 40 μm was the number shown in the 1995 paper. When Wayne was giving his comments this morning, he said he believes that maybe 20 μm is a better figure than 40 μm, and I think other people believe 20 μm might be an interesting break point for where most of the potency of long fibers comes into play. And yet the Berman/Crump report, the report that this panel is examining, has a break point of 10 μm.

The difference between 10 and 20 and 40 μm can be enormous in a practical sense, depending on the characteristics of a dust cloud in a practical situation. If you have fibers between 10 and 15 μm, using a 20 μm break point and a 10 μm break point can dwarf the difference in K_L and K_M. That exposure index, to me, is an extraordinarily important aspect of what this panel is to deliberate. Going from the 40 μm number that the 1995 paper developed to the 10 μm break point in the Berman/Crump report, the only explanation in that very voluminous document was a couple of paragraphs that said, for *ad hoc* reasons and for risk conservatism reasons, we are going to use 10 μm as a break point. It appears to me, IRIS is supposed to be a scientific document; this is supposed to be a scientific document. Questions about using policy decisions of conservatism to base a break point is not what this report is supposed to be about. This, I understood, is to be a scientific report. As far as I can tell, the data shows 40 μm. There might be some questions about 40 μm, but I ask the panel to consider very, very carefully whether 10 μm is the important break point. Thank you.

Day 1, Comment 6: Eileen Kempel, NIOSH

I'm Eileen Kempel from NIOSH, and I have a couple of comments for the panel to consider. The first one concerns the estimation of the risk coefficient for chrysotile versus the amphiboles. I noticed that, based on the statistical tests, the hypothesis that the risk coefficient for chrysotile and the amphiboles could not be rejected as being equal, which suggests that the risk coefficients should be equal for chrysotile and amphiboles, and it seems like there is not a good justification for having the risk coefficient for the amphiboles as being five times greater than chrysotile when the statistical test could not rule out the possibility that they are, in fact, equal. That's the first comment.

The second one concerns the proposed revised exposure index, and I think it's very important to keep in mind that the basis for this is from animal studies that were performed with exposures for 12 months, which is half the standard chronic bioassay according to the criteria that are used in cancer bioassay studies. So, it is uncertain whether the relative potency by fiber size that was seen after 12 months exposure, and they were followed for an additional 12 months without exposure, but whether that would be consistent with what would be seen after a full 2-year chronic bioassay. I think it is very important to keep that in mind. There has been a more recent study by Hesterberg et al. in 1998 in which they used chrysotile with a geometric mean length of 1.6 μm. That was a full 2-year bioassay. In that study, they found statistically significant increases in both lung cancer and mesothelioma. And, again, the mean length of that chrysotile was 1.6 μm. So, I think it will be very important to include this more recent study that was a full 2-year chronic bioassay and see what influence inclusion of those data may have on the proposed revised exposure index. And it's also important to keep in mind that the human data do not include any information on exposures to fibers less than 5 μm, so there is no way to test the hypothesis in the human studies as to whether there is a risk of exposure to the short fibers. So there's a lot of uncertainty in the assumption of the proposed revised exposure index of zero potency for the shorter fibers. And, in fact, the vast majority of fibers in airborne exposures, both in terms of mass and number, are the shorter fibers. So the risk index is being based on a very small proportion of the fibers that people would be exposed to and there are no human data that we can use to evaluate that. And the rodent study, the more recent one that is based on the 2-year study, that used the shorter fibers has not been included in that proposed revised exposure index.

There's also a number of mechanistic studies in rodents showing that there are adverse health effects from exposure to the shorter particles or fibers, including pulmonary inflammation and lung cancer. So, therefore, I think that there is considerable uncertainty and there should be a lot of concern about assuming a zero potency for the shorter fibers. Thank you.

Day 1, Comment 7: John Budrow, California Office of Environmental Health Hazard Assessment

My colleague from NIOSH over here stole some of my comments about both the length of exposure in the Davis studies and the Hesterberg study. And one point to note with the

Hesterberg study is that there were exactly zero chrysotile fibers in that study that the animals were exposed to that were longer than 20 µm, so you are looking at pretty much exclusively a short chrysotile exposure that caused both lung cancer and mesotheliomas.

There is a couple of other recent human lung burden studies also that I would like to call the panel's attention to. [Author name inaudible] 1994 and Suzuki and Yuen 2001. [Author name inaudible] looked at 5 or 6 cases of mesotheliomas; looked at lung burden in parenchymal lung tissue specimens. In one of the American cases, found primarily short chrysotile fibers. And Suzuki and Yuen did a study with, I think, 114 American mesothelioma cases and found that the predominant fiber type in the mesothelial tissue was short chrysotile. I think only something like 4% of the fibers were 8 µm in length or longer. So, this collection of more recent data suggests two things: one is that maybe the half-life of chrysotile in humans is not that short and that it may not necessarily be a good idea to establish a very small potency for chrysotile fibers in the 5 to 10 µm range and to assign a zero potency for chrysotile fibers shorter than 5 µm.

Day 1, Comment 8: Suresh Moolgavkar, Fred Hutchinson Cancer Research Center

First of all, I'd like to commend Drs. Berman and Crump for taking on this formidable task of trying to synthesize this huge literature on asbestos and cancer. And I think, by and large, they have done an excellent job. I've got to say that I understood their approach to this problem much better today, after their presentations, than from the draft document that I was able to get off the Web. I think it is unfortunate that it was posted there in the first place, because today's talks were just so much clearer than that document.

They've already done a lot of work, and I'd hate to ask them to do any more, but I see this as an opportunity. It's been almost 20 years since EPA reviewed the asbestos literature in 1986, and I see this as a real opportunity for detailed epidemiological understanding and analysis of the asbestos data, not only to setting risk but also to understanding some of the mechanisms by which asbestos might be causing lung cancer and mesothelioma. So, I think there is a real opportunity for a detailed exploration of exposure-response relationships. Note, I say exposure here and not dose-response. And a real opportunity to look at the temporal evolution of risks, particularly after exposure stops, and to try and understand if there is any difference between the chrysotiles and amphiboles in this regard and, if there is a difference, what it might be attributable to. And also an opportunity to study the interaction with other carcinogens, particularly tobacco smoke. I think in large part the report misses the opportunity to examine these issues detail. As Dr. Crump said, the main goal of the report was simply to see if the 1986 EPA model did a good job of describing the data. So, much of the epidemiological analysis were restricted to minor extensions of the methods used in 1986. It began in Chapter 6—the exploration of temporal evolution of risk—but abandoned this exploration because of lack of time. There was no real exploration of interaction with other carcinogens. There was some discussion of interaction with tobacco smoke with the panel this morning, but it is not at all clear that this interaction results in a multiplicative relative risk. It seems to me that, despite this opportunity for a thorough epidemiological analysis, the main thrust of the report is a proposal

for a new index of asbestos exposure based on TEM measurements and the defense of this exposure measure.

As to specific comments for the epidemiology study, they used mainly minor extensions of the 1986 EPA models. There is a linear excess relative risk model with a multiplicative constant to adjust for background rates. But even with this limited linear ERR formulation, there are various possibilities. One could do linear regression, or weighted linear regression, which is the way that apparently Nicholson did the analyses in 1986; or one could use generalized linear models with Poisson variance and the offsets are the expected numbers, and this is I think what Dr. Crump did. This is what you will get if you explicitly take into account the Poisson variance. Now one would imagine that, looking at one of two, there would be very small differences in the results, but this is not true. With small numbers of cases, there can be substantial differences in the results using other linear regression or Poisson regression. Or one can use generalized linear models with Poisson variance, but a log link, so that you will have log-linear excess relative risk. And this is in fact what Kyle Steenland was talking about this morning; he asked why this process was not adopted here. And if this is done, one can ask the question: is the multiplicative factor α necessary with this formulation? And all the above models could be done repeated with exposure-response formulations that are not linear, for example, linear quadratic exposure response relationship. It would add only one more parameter, but you would get rid of the α.

In addition to the group-level data, they also had individual-level data in two cohorts, in South Carolina and Wittenoom. Here, there was a real opportunity to investigate the separate contributions made by the intensity of exposure and duration of exposure, rather than just cumulative exposure. Now, if as is generally believed, asbestos is a promoter, then you would expect duration of exposure—and you see this for mesothelioma anyway—to be a much stronger fact than intensity of exposure, and you should see this for lung cancer as well if you do the analyses. So the temporal evolution of risk, including the risk after exposure stops, could also have been examined. [Sentences not recorded at end of tape.] And this hypothesis could be examined, albeit crudely, in the epidemiology data sets that they have, had they pursued the ideas based on multi-stage carcinogenesis.

So, with the individual-level data that they have, they can investigate the above questions with at least two approaches: either use the Cox proportional hazards regression or use hazard functions based on ideas of multi-stage carcinogenesis. I personally prefer the latter approach. I think it is better than the Cox proportional hazards for this problem, and Drs. Berman and Crump did try the latter approach, but there are a number of technical problems with the approach that I cannot go into now that are detailed in my written report, which I will send as an e-mail attachment to ERG. This attempt was abandoned for lack of time.

Now, consideration of other carcinogens: There was an opportunity to update interaction of asbestos and tobacco smoking causing lung cancer and possibly mesothelioma—that's a question out there. If smoking information, for example, was available on some sub-cohort, there was a possibility of doing a case cohort analysis of this data. And I think it very important, I think Stan

Dawson brought up the report by Hodgson and Darnton earlier today, that a comparison of results with those reported in other recent reviews, for example Hodgson and Darnton, would have been very useful.

What about the new exposure index? I'm not an expert on exposure, but it seems like to me a couple of points can be made here. Clearly to translate potencies from epi studies based on PCM measurements to the new exposure index, you need to set up a mapping from the old to new indices. When I saw the document, I could not understand what was being done. I must say that, after the talk today, I understood this conversion factor much better, but clearly any new index must be risk-neutral for existing data. What I'm trying to suggest is that this is a reality check. Dr. Berman presented a table in which he looked at ratios of K_Ls and so on and indicated that the new index is less likely to underestimate risk than the old index. That may be true, but I think a direct reality check might be the following: you use the new index in the existing cohorts that you already have to generate exposures and see whether the risks that you obtain are in the same ballpark, because clearly any new index must be risk-neutral. And, as I said, I cannot follow the chain of reasoning used, but I understood it much better today. Thank you. I'll stop there.

Day 2 (morning), Comment 1: Drew Van Orden, RJ Lee Group

My name is Drew Van Orden. I'm a senior scientist with RJ Lee Group. I'm here representing Rich Lee, who is a bit under the weather and sends his regards. A couple of short comments: PCM equivalent, as I understand it, is used in the model, refers only to asbestos concentrations. In a mixed-fiber environment, such as what you would find in the insulators or the shipyard workers, the PCM and the PCM-E concentrations are not equivalent, and I think you have got to account for that. I would like to see an appropriate reference to proper mineralogy in the model, such as the International Mineralogical Association. You know, the ones we use in the ASTM meetings. I think there should be a limitation on the upper size, in the analytical protocol, of matrices and clusters. As it stands now, we would count asbestos particles that are embedded in clearly non-respirable particles. And then, the epidemiology studies present good evidence that there is a difference between cleavage fragments and asbestiform fibers. I use cleavage fragments here as the non-asbestiform varieties; the protocol does not discriminate between the two of them, and I would like to see that added in. Thank you.

Day 2 (morning), Comment 2: Eric Chatfield, Chatfield Technical Consulting, Ltd.

Good morning. I did mean to make this remark yesterday, actually: the documents that are on the Web, which I downloaded, didn't have any tables in them. As a result, it is a bit difficult to review a document with none of the tables which are referred to frequently in the text. Is there any way that we can get those tables? That's number one question.

The main point I want to address this morning is the topic of cleavage fragments as it relates to the selection of this *ad hoc* break point, above which fibers are assigned the 300-times increased potency, in particular, the effect of the change from the 40 µm, predicted by the animal work, to either 20 or 10 µm, as it now stands. The population of cleavage fragments in all amphiboles, as far as I'm aware, will largely be excluded by this index, but the use of the lower 10 µm break point will have some consequences. In looking at populations of cleavage fragments derived from known fragments of obviously [inaudible] amphiboles, I have never seen a cleavage fragment longer than 30 µm, with a width of 0.5 µm. However, there is a small proportion that have widths close to 0.5 µm, and lengths between 10 and 20 µm. This means that, under the current proposal, some will be assigned this increased potency of 300 times potent. Now, given the analytical sensitivity considerations that we have with air sampling, the observation of even one cleavage fragment in the increased potency range could decide a significant risk, where there is no evidence that any risk exists. As an analyst, I'm called upon almost daily to discriminate between asbestos and non-asbestiform cleavage fragments, and I welcome the procedure that relieves me of that responsibility. At the moment, we have no guidance.

However, the current proposal doesn't meet the requirements of the U.S. courts, in some respects: in particular, the application of the new exposure index, derived from asbestos, which is one kind of material, and then applied to non-asbestiform amphibole, which is a different material; the *ad hoc* selection of the 10 µm value for the transition to the high potency range simply because there is insufficient data. Those are two points, and neither of them, neither of these actions, are likely to meet [inaudible] the rules of evidence in U.S. courts nowadays. This legal stuff won't likely arise if there is no reason to challenge the protocol. For this reason, I urge the panel to consider lowering the 40 µm transition predicted by the animal exposures to 20 µm, rather than 10 µm. In my experience, this would relieve a load of problems and minimize the possibility of legal problems. Make no mistake about this, it doesn't mean that we are excluding the cleavage fragments from counting; just that they are less likely that any will be assigned this increased potency of 300 times. We have got little evidence, if any, that cleavage fragments themselves in these size ranges are potent, and those studies of cleavage fragments were used in deriving the new exposure index.

To resolve this issue in the future, EPA can do the community a great service by commissioning an animal inhalation study using elutriated cleavage fragments of several amphiboles. By elutriated, I mean prepared from some large amounts of material, those fractions that are less than 0.5 µm and longer than 5 µm, and do the animal inhalation work. These amphibole samples, however, have to be carefully characterized mineralogically to ensure the absence of true asbestos. I believe that such a study would resolve this issue once and for all. At the moment, you can't do an inhalation study of cleavage fragments, because you can't get enough of them in; they're too thick. So, you want to separate the thin ones, and try that.

I want to finish up with a few comments about chrysotile, and in particular, the Coalinga calidria chrysotile, which is the trade name for it. The mine is about 100 miles south of here. There are some remarks about this in the protocol. This chrysotile is quite unique, and it is a different

geological origin from the more traditional types. Unlike other chrysotiles, when this is dispersed in air, the fiber bundles are much thicker as the lengths increase. So, as you pick a long body, it is generally thick, and it is not very long before you get to a non-respirable diameter. That doesn't happen with the other kinds of chrysotile; they all stay thin. So, much of the material from Coalinga, in fact, ends up being non-respirable. On the other hand, it disperses very readily in water to single fibrils. I have never seen a calidria single fibril longer than about 30 µm. It just simply does not exist. I have been using this material as a reference standard since the 1970s to simulate water dispersion, and it is a very different material from other chrysotiles, and I think that should be recognized in the protocol as far as possible. Thank you.

Day 2 (morning), Comment 3: Chris Anaya, resident of El Dorado Hills, California

Good morning. The citizens of El Dorado County have become students, I guess, about asbestos and unfortunately that's nothing I really like doing. But myself, and a number of other people, have read a number of studies, researched this greatly, so that we have a firm grasp on if we have a problem on our hands where we are from. And I can tell you we do. I'm saying this because that's why I'm here. That's why a lot of people showed up. So, I think we have a fairly firm grasp on tremolite versus chrysotile, and matters such as that, but I think there needs to be a change in the current methods for determining exposures for determining risk assessment. I know the studies or the current methods are flawed, as proven in Libby, because, statistically, Libby residents should never have had the problems they faced. 25% of the people afflicted were non-occupational exposures. I believe 5%—and I'm sure some of you will correct me, maybe I'm wrong—they couldn't find any exposure pathway whatsoever. And this is pretty common with the tremolite fiber, which is a solid core fiber, versus the chrysotile, which is a hollow cord fiber; and it acts differently in the air.

Our concern, because I don't understand the modeling here, it may be good, but I would like this panel to address, maybe when the time is there: this current proposal, does it lessen chrysotile's risk on paper and keep the amphibole the same? Or, does it keep chrysotile risk assessment the same, and raises the bar on amphibole above where it is now? And that's unclear to me, what this study does, because I believe amphibole needs to be raised above where it is now, not remain the same; and lessen the severity of chrysotile. So, I want to leave that thought in your mind, to review that for me please.

All studies that you guys are faced with were animal studies—short term exposures and injections—and occupational workers—40 hours a week, 8 hours a day, 5 days a week. Well, we're in a situation, where I live, we have exposures 24/7. There are no studies that show what that's going to do. We have the proposed analytical method, and I know we touched on this a little bit, but we need to be consistent on how we are going to measure the air, how many fibers per cubic centimeter, how are we going to get that data to plug into this formula? Because, unless we do that, we are going to come up with information that is inconsistent between one study and another, and I'll give you an example. On my former address, there are a couple of air monitors;

monitoring real close to a point source came up with, the lowest was 0.2250 fibers per cubic centimeter; the highest level happened to be 100 feet away from the point source, 93.967 fibers per cubic centimeter. Now, this is in a residential area. There is no study that shows what 93.967 fibers per cubic centimeter for chrysotile will do. This is off the chart, but yet it is not supposed to exist, but yet it does. So, what does that tell us, the residents? What's going to happen to us or our children? We don't know. We have to come up with our own little formula.

Likewise, the tremolite, I mentioned, is a different fiber: acts differently, behaves differently. New Caledonia, Turkey, Cypress, Libby, El Dorado County. We don't get detects in the air because the way the fiber behaves. You cannot put a monitor on top of the roof of a building, as you would for ozone measurement, and expect to find what kind of exposures are at ground level. Likewise with the chrysotile, you put the monitor at a different location from a point source, you're going to get different readings. And so, whatever method you folks come up with, there has to be a consistency with how you gather that data. I would probably guarantee you, and I don't know this, but those monitors placed on the workers in occupational exposures probably had them strapped to their waist, I'm guessing, or had the monitor close by to see exactly what the true exposure was at the breathing zone level. But yet, it's not required to perform it that way, here in California, at least. EPA accepts putting these monitors on the rooftops. To me, it's unacceptable. OSHA would require it—OSHA would cite somebody, if these children are workers; the people that hired these children, or workers in this case, would be cited. But yet, it's OK because there is nothing in the regulations that say you have to measure breathing zone levels where the children are being affected.

I want to close with: I'm a firefighter. When people call us for medical aid, we have to err on the side of public safety. Whether it's a fire, if we are not sure, if we think the fire is extinguished, we are going to take the extra step and open up a wall to make sure it hasn't extended in the wall. We don't want to assume that everything is OK. Likewise, you have a stomach pain, we're not going to assume it's indigestion. We are going to treat you for heart attack, if there is a chance for that, because it is not going to hurt anything. This formula is very important that you have before you. You have to make sure that you are going to err on the side of public safety. Because if you don't, somebody is going to pay dearly for it later on. So, this is very, very important. Make sure that you err on the side of public safety.

Day 2 (morning), Comment 4: Lance McMahan, private citizen

I'm a private citizen, registered civil engineer, though, with some experience with environmental issues. What I want to talk about this morning is background levels of mesothelioma, lung cancer, and asbestos-induced illnesses. It was Mary Jane over here who mentioned that, I believe, it was Australia had a risk of 8 in 1,000,000 currently in the background level; that's what they're measuring. Now, I don't know why that is. Maybe they are moving people onto asbestos deposits, and they are not keeping track of those people, so they are not accounting for them. Personally, I don't think that kind of risk is appropriate. It is extremely high. If you are getting

that much mesothelioma, imagine how much lung cancer you might getting along with that.

I lived in El Dorado Hills, and I have seen these veins of tremolite asbestos that are out there. Department of Toxics went out and found up to 96.5%, period, in a residential area, kids on their bikes in the area. This dirt, mixed in with this vein, was used to make the pads that the homes are built on. You know, the mass padding—take the top of the mountain, move the dam, build homes on top of it. I don't think I want my children to be part of that background level, because no one is looking at this. Having said that, that really doesn't bear directly on what you are looking at, necessarily, but it does provide an opportunity. There has only been in existence for 15 years since they started doing that grading in that particular area. Some of the residents are still there from the initial home purchase. There is a fair number of people that leave, of course; it's a very high turnover area generally. New people come in. So, exposures are 24/7 for the kids, for the school next door, for the community center across the street where the kids some of their summertime.

I believe you have an opportunity to do an epidemiological study that looks at this area, looks at the soil concentrations, gets breathing zone monitoring data. This has been going on for at least 7 years that I'm aware of. Of course, it's been going on since before the work, before they began the construction work out there. No one is doing anything about it. They are not doing breathing zone monitoring. I've asked EPA to do it. I've asked CalEPA to do it. I've asked the county to do it. They won't do it. So you may as well go ahead and let all the folks continue to live there, and take the opportunity over the next 10–15 years, to actually collect information on what people are being exposed to, to track the residents, to see how ill they become, and make use of this experiment that we're conducting. It's better than lab rats—actual human beings. So, you folks may as well get started, before people realize exactly what it is they are living on, and make use of lab rats. Is there any questions?

Note: At this point, one panelist (BC) addressed issues raised in the previous two comments. The panelist concurred that exposure assessment is a critical aspect of the proposed methodology, and he added that the expert panel did not have the expertise necessary to review thoroughly how occupational exposures were interpreted in the proposed protocol. Regarding the situation in El Dorado County, the panelist indicated that further research is needed to understand the health implications of exposures, and he encouraged regulatory agencies, government officials, and other entities to support such research.

Day 2 (morning), Comment 5: Terry Trent, El Dorado County resident

I'm Terry Trent. I'm a biologist. I'm from El Dorado County. Thank you, Bruce. I don't have to say very much now. I'm perhaps, on the topic of El Dorado again, Wayne Berman's most vociferous critic in the private sector. Me and my family, through asbestosis and lung cancer, have nearly paid the ultimate price—have lost most of what we own in the world—due to abuses of philosophy, measurement, and risk assessment in El Dorado County. At this point in time, it

remains to be seen what else we might loose. However, in reviewing Wayne's formula and the math last night, and in comparison to the literature and my own investigations in El Dorado County and elsewhere in California, I have to congratulate Wayne in his development of this risk formula. This is very eloquent, Wayne. My concern now becomes measurement techniques, how they are to be plugged into this formula, the mineralogy and how it is to be plugged into this formula. I have a few comments on the formula, and the variables in it, which I will submit in writing. And my largest concern is that it is a formula, as all formulas would be, that is ripe for abuse; and that is simply not your fault.

I'd like to comment a little bit on El Dorado County and my personal home in El Dorado County. In my front yard, I had one vein of slip fiber tremolite asbestos that weighed about 27 tons. It was one of thousands of veins in residential neighborhoods. I have discovered about ten additional areas in El Dorado County that are similar. My estimate is that there is about 10,000 people in El Dorado County being exposed to these fibers in their everyday activities around their homes, and their homes are simply full of the fibers. I'd just like you to consider that with your considerations here. Thank you very much.

Day 2 (morning), Comment 6: Suresh Moolgavkar, Fred Hutchinson Cancer Research Center

I am Suresh Moolgavkar, Fred Hutchinson Cancer Research Center, and University of Washington. I just wanted to reiterate a few of the points I made yesterday. Because it's 20 years since EPA looked at asbestos, I think this is a real opportunity for a thorough evaluation of the epidemiological literature, and there are three points I would like to make.

First, exposure-response relationships. I think these need to be investigated in detail and Drs. Berman and Crump need to go beyond just showing that the EPA models used in 1986 are adequate. There needs to be a thorough evaluation. This is a real opportunity to do so. For example, for mesothelioma, there is a 1999 paper in *Inhalation Toxicology* by G Berry, who applies a dose-response model to the Wittenoom data and considers what the evolution of risk might be after exposure to asbestos stops. Certainly, considerations of this type and other models that have been developed should be looked at in this document.

Related to that is the issue of the proper exposure metric for asbestos. Now, it's clear that, for mesothelioma, duration of exposure, or time since first exposure, is an extremely important variable, and it's probably more important than the daily intensity of exposure. Now this has been seen for cigarette smoking as well, and it's characteristic. This kind of an exposure-response function is characteristic of any agent that is believed to be a promoter in the carcinogenic process. And so it is quite possible that the same kind of exposure-response relationship, namely one in which the duration of exposure is more important than the intensity of exposure, might operate with lung cancer as well, as mesothelioma. So the fact that the cumulative exposure provides a satisfactory fit to the data is not sufficient. One has to show that

a model that considers daily intensity of exposure and duration of exposure does not do a better job. And, with grouped data, this is extremely difficult to do, because you lose a lot of information in grouped data. But, Drs. Berman and Crump do have two data sets with individual-level data. I wish they could get more. I wish they could get the Libby data to look at this problem as well, but I encourage them to pursue their analyses of the Wittenoom and South Carolina data sets and not abandon the analysis at this point. The write-up that we pulled off the Web indicates that they ran out of time, and they could not complete their analyses. I would be happy to help them, collaborate with them, in the analyses of these data sets.

And, finally, I think again that it is extremely important to look at the interactions of this carcinogen, asbestos, with other lung carcinogens, and most importantly, cigarette smoking. The perceived wisdom here, based on work done by Hammond in the 1970s, is that the relative risk for the two exposures is multiplicative. But, as Dr. Case point out yesterday, there is a very recent paper by Liddell and Armstrong, which I have not had the opportunity to look at, but apparently it indicates that the risk is probably much closer to additive, than multiplicative. Now, if this is true, then all the risk tables for smokers are going to change considerably, and so I think it is extremely important to investigate to the extent possible the interaction of asbestos with other carcinogens. Thank you.

Day 2 (morning), Comment 7: Eileen Kempel, NIOSH

Eileen Kempel from NIOSH. I wanted to make some general comments and also follow up on a few things from yesterday, and first I wanted to thank EPA and the organizers, the panelists, and Drs. Crump and Berman for putting this all together. It's clearly a tremendous amount of effort on a very important topic, and I think it really provides an excellent opportunity to interpret the scientific data, both animal and human, to make very important public health recommendations, which clearly are of great impact. At the same time, I think it's also very incumbent upon us to use the best available science in doing that, and so I agree with comments that have been made that it's important to use some of the methods that have been suggested to evaluate how robust the proposed method is to the data and assumptions and models that have been used.

I think it is encouraging that the proposed revised index does provide improved risk estimation, at least for the mesothelioma. But, at the same time, we are dealing with imperfect data, particularly with the human data, where the exposures in the epidemiological studies are very poorly characterized, and that's clearly an area in exposure estimation that needs to be looked into. So, we are relying on the animal data, to a large extent, because we don't have size-specific data in humans, and I think that's appropriate. That's a good example of using all the available science in the risk assessment. But, at the same time, I agree with the comments that it's important to look at, in the extrapolation from rat to human, what the appropriate dose metrics are in humans. For example, what's respirable in a rat is not the same as what's respirable in humans, and that comment was well taken.

Another area we mentioned yesterday is there is a lot of question about—we just don't know—there's lacking data on what the role of the short fibers are, and the reality of "short" meaning, you know, less than 5 µm. Of course, the 5 µm cut-off was established primarily because of convenience in the analytical method and didn't have a direct biological connection, but those are the data that we have to deal with. But there are still questions on what are the role of the particles or fibers below that size range, whether it's a direct effect or an indirect effect involving increased inflammation, cell proliferation, and fibrosis. We just don't know. And, in reality, humans are going to be exposed to the mixed fiber or particle situation, and we really ought to consider our area of uncertainty in that.

And also with regard to dosimetry, it's important to realize that lung clearance in humans is about an order of magnitude slower than it is in rats, and this gives increased opportunity for the particles to be translocated into the interstitium. And this has been shown in lung dosimetry models for humans, as well as in a study by [inaudible author name] and colleagues, where they found that the pattern of particle retention in humans was preferentially in the interstitial area, as opposed to rats, where it was in the alveolar lumen. So, this increases the probability that, for a given exposure, the dose in humans over long term may be greater than predicted from the rat studies. So there is an opportunity to use the dosimetric information that we have in the risk assessment, and this has been done for risk assessment for fibers recently. Dr. Moolgavkar and Dr. Yu have done that, for example. So, I think it's just very important to consider the dosimetry in going from rats to humans.

And then finally, with regard to the rat data that are used, the Davis studies from the 1970s and 1980s, which were used to derive the exposure index, they're very good in their own right. But, again, they were only exposed for 12 months, and there are some additional studies as I mentioned yesterday. There's one by Bernstein with short chrysotile in 1998, and I understand there's—and I'm sorry, there is one by Bernstein, yesterday I mentioned one by Hesterberg. The point is, there have been some more recent studies since the 1995 evaluation, and that these really should be considered in the revised exposure index, because those animal studies and the data from that are what are being used to derive the exposure index for humans. And, with regard to the exposure in those studies, the Hesterberg study used almost 11,000 fibers per cubic centimeter, but the Davis study used up to about 6,000 fibers per cubic centimeter. And, from what I understand, the reason for that is that it is very difficult to get fibers into the lungs in rats, and that's because of the complex nasal structures, and that's why the fiber concentrations have to be so high. But given that the main crux of this proposed method is this revised exposure index based on fiber length, and that this is relying heavily, and exclusively, on the studies from Davis and others, which use the 12-month exposure, and there have been some more recent studies which use the full 2-year bioassay, I think it's very important to include the additional recent data and see what influence they might have on the derivation of the human exposure index. Thank you.

Day 2 (morning), Comment 8: Betty Anderson, affiliation not stated

Good morning. Just a couple of comments, the first two, historical in context. I was director of EPA's carcinogen assessment group, with Roy Albert as chairman, when this work in 1985 was done. One point that I think gets lost in this historical context is the insistence that we had agreement we had with science advisory boards at the time that, whenever we were using the linear, non-threshold model, there would be—mine which that was quite descriptive, and I can certainly share it with you—that would recognize that where there was no model that could confidently describe the data, we would be certain to include language that described the results as establishing a plausible upper-bound on the risk, meaning the risk would be considerably less, even approaching zero. So, I think this gets lost, and when we use the model for background levels, we lose the context for what we were talking about. Now I fully endorse trying to go forward with getting some better modeling results so that we can go beyond the 1985 work.

I think also, the historical context of the 1985 work, we had an exposure metric. It was the PCME metric. Now, for sure, what was captured in that metric were the bond fibers and whatever else was not being seen by PCM, but then we were in search of using the metric with the incidence to do a dose-response work for risk assessment. As we now change to finding a different exposure metric, we are shifting the focus to that exposure metric, and I think we have to be certain that we are almost flipping the coin and taking the exposure metric in search of the right incidence relationships for risk assessment. And so, therefore, I think this committee needs to really focus on that exposure metric and what it means in terms of the incidence data, because we are shifting.

A couple of other points just from some recent work we've done. I think when we are looking at this *ad hoc* or whatever bright line we choose for the cut-off point, whether it is 10 µm or 20 µm, we have to be very careful. I think Eric has said it very well earlier today. We have seen in several data sets high risk if we use what Wayne and Kenny, their model; and very low risk if we use ours. Now, we shouldn't have these discrepancies, unless we are going to have a way to explain them. So, I think we certainly don't want to have a method that gives us some very high risks and we don't have a scientific basis for having chosen that 10 µm or that 20 µm level. So I certainly think the committee will, and certainly needs to, focus on how we, and as well as Kenny and Wayne, can get some information to better describe how to deal with something below 40 µm.

And, finally, I think the committee was charged with looking at the role of cleavage fragments. I wonder how compelling a discussion we can have based on the document that's in review here, since I believe this document doesn't go into that discussion. As many of you know, and many of you I'm sure have been involved, there is a lot of information, a lot of data, the OSHA hearings being a part of that record. So, I'm not quite certain how this committee at this point can fully address that particular issue. It may take another convening of another committee with that information to fully address the role of cleavage fragments and health effects. Thank you.

Day 2 (morning), Comment 9: Stan Dawson, CalEPA

Good morning. I wanted to follow up on yesterday's presentation with somewhat improved technology on the slides; they came out better this morning. [Referring to a figure displayed on an overhead.] I'll just explain a little bit, the diagram; maybe some of you have had a chance to look at the handout with the methodology on it. Basically, what we are plotting here is the cumulative proportion of expected total deaths in the studies versus the potency of each study in terms of lifetime unit risk. This is a little bit reminiscent of when we were taking courses and had the final exams and the teacher gave your grades and your teacher made a cumulative distribution of the scores and then you could find the median score and the 90th percentile score, and this is a way of displaying this data. I should explain that this total deaths, expected total deaths, is used to kind of weight the size of the study, as it were. For example, the last time I said that incorrectly that this was the Liddell study. In fact, that is the [inaudible author name] study—a tiny, tiny study; it only has that much, a tiny amount, of this y-axis. The Liddell study has this whole sweep in here; it's the Liddell point. Anyway, there are the chrysotile studies, the mixture studies, the amosite, and crocidolite. And also, tremolite has been mentioned a couple of times; it's right there. The point I made yesterday is that at the 50th percentile you see quite a spread here. At the 90th percentile, the spread is very, very much less. Dement is not the only study that has a fairly high potency, which gives a little bit above 0.1 of unit risk. And I know surely that the CalEPA potency from 1986, which followed U.S. EPA, was 0.1. So, this is, for lung cancer, the unit risk that is being used right now in California. Are there any questions on the lung cancer slide? [Several questions of clarification regarding the lung cancer plot followed.]

Then we have mesothelioma. Now this is a somewhat similar picture. I want to emphasize that, if this was a log-normal distribution, we would have seen the classical sigmoid shape. And, of course, I have plotted these things that if they were log-normal they would come in on straight lines and they don't, by a long shot. Anyway, here are the values of lifetime unit risk of mesothelioma, and the whole thing is shifted up by almost a factor of 10 for chrysotile. And you can see again that the chrysotile studies come up pretty high with Dement here, compared to the rest. And then there's the mixtures. The amosite now is separated much more from crocidolite than before. Of course, this is consistent with Hodgson and Darnton, as it should be. And if you then look at something like the 90th percentile, and you have to extrapolate up a bit here, you can see there is about a factor of ten difference between crocidolite and amosite, and then another factor of ten down to the 90th percentile for chrysotile. And then tremolite is hanging out here by amosite. [Several questions of clarification regarding the mesothelioma plot followed.]

Day 2 (afternoon), Comment 1: Leonard Burelli, Environmental Profiles

I'm Leonard Burelli. I'm with Environmental Profiles in Baltimore, Maryland. Currently, I'm an industrial hygienist, so I get involved with sampling and writing reports. In my past life, I was a microscopist. I worked in a micro-analytical lab, so I am somewhat sensitive to sampling and preparation and analytical techniques. In the report, specifically the conclusions and

recommendations section, there is a call for addressing the validity of the risk assessment in the protocol, and what's pointed out is to use a TEM method—ISO method 10312—and then later on it says that indirect preparation could be used: ". . . should indirect preparation be required due for example to problems with overloading, a sufficient number of paired samples will need to be collected and analyzed." I just want to point out that indirect preparation could artificially create higher structure counts, and you might want to revisit the idea about indirect. I have one question, too: Will there be opportunity to have written questions presented, because I'm going to get back to Baltimore tomorrow and think of something else? Will there be an opportunity to submit additional comments, observations, that sort of thing, in writing? And who would we direct those to? [A representative from EPA indicated how observers could submit follow-up comments on the proposed protocol.]

Day 2 (afternoon), Comment 2: Chris Anaya, resident of El Dorado Hills, California

Thank you. I think my fears are unfounded. When talking about age and how long people live, it made me a little nervous, and I wasn't sure where you were going with that, and I just want to get clarification: is this just a matter for determining lifetime exposures, I'm assuming? Because, surely we're assuming that a child, maybe 1-year-old, crawling on the floor in asbestos and growing up in this stuff is going to probably live long enough for the mesothelioma, or whatever it may be, to take effect if something was to take effect. I just want to make sure that we are taking into consideration the worst-case scenario. I know with the drinking water, they do; they have orders of magnitude to allow for the sensitive populations. I just want to make sure I understood you correctly. I think you should assume that the exposures take place at the youngest age and then determine how long that person is expected to live from there. That's all.

Day 2 (afternoon), Comment 3: Jay Turim, Sciences International, Inc.

Just a very quick comment. When I first became aware of the work that Wayne and Kenny were doing, it started back in 1995 in that paper—the *Risk Analysis* paper. That paper had the 40 µm break point; I spoke about that yesterday a little bit. And also the 0.4 µm diameter. We said that the paper showed that, in terms of the animal data, most risks was long fibers greater than 40 µm and fibers less than 0.4 µm. Each one of those numbers has seemed to been hacked away by this committee, and I just wanted to call the committee's attention to that. The 40 µm went down to 10 µm, and that was debated, and I think the consensus of the committee was, well, 10 or 20 µm. Not on the basis of the animal data, but on the basis of other considerations. A half an hour ago, you debated the 0.5 µm with the 0.4 µm. Wayne said he made it 0.5 µm because it was easier to read; Eric said, well, 0.4 µm or 0.5 µm can be read equally easily. Dr. Lippmann and others said 1.5 µm is a break point. Fair enough. But I think everyone has to realize that the whole underpinning of the 1995 paper has been abandoned. The 40 µm; the 0.4 µm is gone. Everything is going to rely now upon Table 6-15, coming up with K_L's and K_M's. And, fair enough, that's a way of doing it, but I think we all have to admit that this is the way you are going, and this is the way the Berman/Crump method is going. I just wanted to point out which, to me, seemed a

different take on the way things started out. What had been the basis or the genesis, which was the animal data with some very concrete numbers, have been completely abandoned, and I wanted to bring that point to the attention of the panel. Thank you.

Day 2 (afternoon), Comment 4: Eric Chatfield, Chatfield Technical Consulting, Ltd.

I guess my comment is very similar to that in that the discussions of respirability this afternoon indicated that we should be measuring widths up to 1.5 µm, or thereabouts. The problem with that is that if we retain the 10 µm cut-off again, we then bring in a whole population of cleavage fragments in places where there basically is no risk because there is no asbestos. So, I would point out that one of the recommendations I made at a Denver conference some time back—one of the Libby meetings—was that you can use whatever model you like to estimate risks, but, for the purposes of comparability with past data and with the IRIS method to doing things, then there was very little incremental cost to measuring all widths up to 3 µm, in the longer than 5 µm count. The actual extra cost going from just, say, the 0.4 µm or the 0.5 µm width, to all widths up to 3 µm, was not a great deal of extra costs, and there you would have then the means of comparability with previous work. If you measure the widths, it doesn't mean to say you necessarily have to use all of them in the model, and I would hope that you would retain the concept of either a 0.4 µm or a 0.5 µm—I don't care which—width. And I don't particularly like 10 µm, but I do say that the 20 µm cut-off to the extra toxicity, to the extra potency; a 20 µm number there would, in fact, eliminate pretty well all cleavage fragments. You would just not think of cleavage fragments at that point in one of these counts. But, I became a little bit disturbed; I got the impression that the intention was to be increasing that 0.5 µm width and actually using that in the Berman/Crump model. If that's the case, then I would say that you are going the wrong way; you're going to be bringing all the cleavage fragment arguments over here.

Day 2 (afternoon), Comment 5: Catherine Simmons, Bolter & Yates

My name is Catherine Simmons, and I'm an industrial hygienist. I work for Bolter and Yates in Park Ridge, Illinois. My comments have to do with the importance of the exposure assessments that were conducted originally, that all this work has been based on. Most of that work, or a lot of that work, was done by industrial hygienists, and the determination of appropriate sampling strategies normally is best done by persons trained in the field of industrial hygiene and would best be performed by persons certified in the practice of industrial hygiene. And I guess what I would like to see is that industrial hygienists be included in evaluation of that data and with the factors that are figured into the importance of the information and if there are deficiencies in the data and how they are weighted. That's all I have to say.