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UNITED STATES ENVIRONMENTAL PROTECTION AGENCY

REGION II

JACOB K. JAVITS FEDERAL BUILDING

NEW YORK, NEW YORK 10278

HUDSON RIVER PCB REASSESSMENT RI/FS

COMMUNITY INTERACTION PROGRAM

Joint Liaison Group Meeting
Thursday, November 5, 1992
Latham, New York

M I N U T E S

On Thursday, November 5, 1992, a Joint Liaison Group meting was held at the Holiday Inn Express in Latham, New York. The purpose of this meeting was to bring together the Liaison Group membership with some members of the Scientific & Technical Committee (STC) in order to discuss questions on the second phase of the Reassessment. Members of the Liaison Groups had submitted their questions for the STC members approximately two weeks prior to this meeting.

Present for U.S. EPA were Ann Rychlenski, Community Relations Coordinator and Doug Tomchuk, Remedial Project Manager for the Hudson River PCB Reassessment. Participating for the Scientific & Technical Committee were Dr. Richard Bopp of Rensselaer Polytechnic Institute; Dr. Daniel Abramowicz of the General Electric Company; Dr. James Bonner of Texas A&M University, and STC Facilitator, Dr. William Nicholson of Mt. Sinai Medical Center.

In addition, two members of the STC attended the meeting - Dr. George Putnam of the State University of New York in Albany, and Dr. Ronald Sloan of the New York State Department of Environmental Conservation.

Ms. Rychlenski opened the meeting at approximately 7:15 p.m., and after introductions and a discussion of the purpose and rules of the meeting, turned the meeting over to Mr. Tomchuk.

Mr. Tomchuk gave a brief status update on the project itself, indicating that the high resolution coring is almost done and referred to the demonstration of coring that EPA held on October 23, 1992. He added that the following (11/9/92), water column and method detection limits studies would be taken*.

*NOTE - These samples were actually taken on December 6, 1992.

He also mentioned that very recently, the General Electric Company briefed both EPA and NYSDEC on their sampling in the vicinity of Baker's Falls, NY, which according to G.E. indicates a source of PCBs in the water column up river from the remnant deposits. These PCBs resemble non-degraded aroclor 1242 and appear to be entering the river somewhere near Baker's Falls. Since the sampling has been done by G.E. and has not been validated by EPA, EPA will not comment on these numbers. He added that if these numbers are valid, EPA will incorporate this information into the Reassessment. In addition, Mr. Tomchuk stated that this information will not change the Reassessment, since the goal of the Reassessment is to determine what to do about the contaminated river sediments, and that the Reassessment already takes into account potential sources of PCB contamination.

Mr. Tomchuk went on to state that the low resolution coring program has not as yet been totally defined, and that some samples could be taken in this area (Baker's Falls), however, the side-scan sonar data did not show a lot of sedimentation in this area.

Mr. Tomchuk then addressed the meeting as to the role of the Scientific & Technical Committee. Mr. Tomchuk read the role of the STC from the Revised Community Relations Plan (August 1992) as follows:

Mission & Purpose:

To assist in the Reassessment by providing technical input to the study process.

To evaluate scientific data collected on the project

To provide technical dialogue on a variety of pertinent project topics.

Members

Members are researchers and scientists familiar with the site, PCBs, modeling, toxicology, and other relevant disciplines.

Tasks

The STC will review and provide comments on documents provided by EPA and identify additional sources of information and on-going research relevant to the Reassessment.

STC members will offer and encourage technical discourse which will be of benefit to the project.

Committee members may be called upon to make presentations in their particular areas of expertise to EPA, HROC, or other groups participating in the Community Interaction Program.

The STC will identify issues or topics of a technical nature that should be raised to HROC.

The STC facilitator (Dr. Nicholson) will guide the STC meetings, ensure that avenues of discussion and investigation are productive and pertinent to the Reassessment, and represent the STC on HROC.

ORGANIZATIONAL DETAILS

EPA will prepare the meeting agenda based on the needs of the project.

Mr. Tomchuk continued to discuss that as illustrated by the nature of some of the questions submitted to the STC, there appears to be a general misconception about the roles of the STC. It should be clear that the STC will not be asked to recommend a remedial alternative. EPA is the decision maker for this project. It is also important to remember that the STC is not an independent review board, meant to confirm or criticize EPA's work during the Reassessment. Therefore, questions that ask about the Committee's opinion on EPA's position may not be answered directly in this forum as they are not appropriate. The members present during this particular Joint Liaison Group meeting may answer as they wish, as individuals, but they are not answering for the Committee. When comments are submitted to EPA on the Committee's recommendations, both sides are given so that EPA recognizes both minority and majority opinion.

Mr. Tomchuk urged the members of the Liaison Groups to read the Responsiveness Summary and the Final Phase 2 Work Plan; and added that while all the members of the STC were not present at this meeting, those who were present were chosen for their expertise in the subject categories presented for discussion and also for diversity in opinion to ensure balance. He then proceeded to introduce the members of the STC.

Dr. William Nicholson: Facilitator of the STC. Dr. Nicholson's background is in physics and epidemiology and he is with Mt. Sinai Medical Center in New York. Dr. Nicholson has previously published an epidemiological report on PCBs. He was nominated to the STC by EPA.

Dr. James Bonner: Associate Professor in the Civil Engineering Department of Texas A&M University. His research interests are

in transport, fate and effects of modeling of particulates and associated contaminants and aquatic chemistry. He was nominated by NYS Senator Stafford.

Dr. Dan Abramowicz: Head of the Bioremediation Research Department at General Electric R&D Facilities in Schenectady, NY. He was nominated by G.E.

Dr. Richard Bopp: Professor at Rensselaer Polytechnic Institute. Previously with NYSDEC and Lamont-Dogherty Geological Observatory. He was nominated by NYSDEC.

Before beginning the topical discussions, Mr. Tomchuk stated that questions regarding health risk would not be addressed. He stated that a Joint Liaison Group meeting on this subject was held in February on 1992, and that nothing has changed significantly since that time. In addition, since many of the questions had to do with EPA's policies on health risk, Mr. Tomchuk stated that he felt that this was not appropriate to this committee, and would not create a fruitful discussion.

Mr. Tomchuk then turned the meeting over to Ms. Rychlenski who read the items for discussion as taken verbatim from the questions received from the Liaison Group members. She reminded the audience that questions or clarifications would be entertained only at the end of each category, and that questions or comments should be restricted to the specific categories of discussion as per the agenda.

Please note: From this point on, the meeting minutes will take the form of a verbatim transcript as transcribed from tapes recorded during the meeting. This is being done to preserve the scientific and technical discussion exactly as it took place between the members of the STC, EPA and Liaison Group members.

Ms. Rychlenski: The first category of High Resolution Coring, and the first question is regarding the accuracy of radioactive dating techniques. The question is, **"What is the Committee's position on the accuracy of the radioactive dating technique being used to assign calendar dates to the sediment slices made from the samples taken from the river? And does the Committee view this accuracy as sufficient to use the radioactive dating method?"**

Dr. Bopp: The radioactive dating technique is based on the location of identifiable time horizons within the column of sediments. And the primary time horizons that we look for are the early 1950's which corresponds to the beginning of significant global fallout from the atmospheric testing of nuclear weapons and the maximum atmospheric fallout from the atmospheric testing of nuclear weapons that occurred in 1963. So we look primarily for those two time horizons.

We also have above that in the sedimentary column, the top of the core, where we use another radioactive tracer Beryllium-7, which is produced in the atmosphere. It is a natural radioactive tracer and it has a half-life of only 53 days. So half of it disappears every 53 days. The Beryllium-7 tracer and the fallout tracer, which is the one we use primarily, is Cesium-137, are useful in this technique because they

behave similar to the compounds that were are interested in studying. The have a significant affinity for the particles and the sediments.

Beryllium-7 will only be found in sediment samples that have a significant component deposited within about the last year, otherwise it would have all gone away. SO this gives you a way of unambiguously identifying the most recent sedimentation. Further down the core when you come to the Cesium-137 maximum, the fallout maximum, you have a marker for 1963, and where you go from having some Cesium to having no Cesium you have a marker for the early 1950's.

Between those points, you are essentially extrapolating, or interpolating, sorry. And what is the accuracy of the dating technique to assign calendar years? Well, one easy way to think about that - near the top of the core, if you have a Beryllium-7 bearing sediment, you have it nailed to about within a year. If you get a good sample right near the beginning of Cesium-137, you have it nailed to within a couple of years around the early 1950's. In general in the middle of the core, you're looking at four centimeter slices and you're looking at sedimentation rates in general in most of these cores, on the order of one centimeter a year where they're taken, so your accuracy is on the average, plus or minus a couple of years.

Is this adequate? I think this is very adequate; and I think you can get useful information out of this technique as we will see when we get to the answer to the next question.

Ms. Rychlenski: Does anyone else want to add anything?

Dr. Abramowicz: I think that one of the things you'll see is that the Committee has expertise in various areas, and Richard has obvious done a lot more on the high resolution coring than obviously I have or Jim.

Dr. Bopp: Can I also make one more point. You can't take a good high resolution core anywhere you want it. You have to look for the spots where sediment is hopefully deposited year after year, layer upon layer; and then use the radioactive technique to confirm that or to say that is not correct. You can't say I want a core that we can date and get a chronology at this point in the river. But you might be able to take a five mile stretch of the river and say here's a nice little cove off the side where there's quiescent water, there's not a lot of bottom scour, so this looks like a good place for sediment accumulation. This is a spot where we would take a core and try to date.

The samples that are being taken, by requirements have to be analyzed within five to seven days (extracted within seven days of collection, added by Mr. Tomchuk). So what we have been relying on for taking these samples, they will be counted, they will be radioactively dated the ones that are analyzed. However, for picking the sites for taking these cores, we are relying on past cores that have been taken and counted for the radionuclides. So on just about all of the spots that have been identified for high resolution coring, there is previous data on radionucide activities.

Ms. Rychlenski: On to the next question, which deals with the comparison of archived cores -**"What information will be gained by taking new coring samples, and then comparing the new samples with archived cores? What conclusions will result from doing this comparison?"**

Dr. Bopp: Again, I should probably take it. I don't know if you can see this - (showing chart to audience). Here's an example of the type of thing you might be able to learn. This represents the Cesium profile in two cores. This core was taken in July of 1983, this core was taken in May of 1991. So this may represent an archived core for example, and this may represent a core that we have just taken. This shows the Cesium profile in these two cores. What you see is that this profile has been

offset by 16 centimeters and the 16 centimeters then represents the sediment that has accumulated at this site between 1983 and 1991. That would be the way that I would interpret it, and I would welcome any other possible interpretation.

One thing you can gain from looking at archived cores, we take this sample from the archived core, we look at the distribution of PCB congeners in that sample. That gives us a reading of how much dechlorination took place from the time that sample was deposited until 1983. We go to the 1991 core, take that same sample, this has now been sitting from the time of emplacement until 1991, or 8 more years. By comparing what the PCB congener compositions look like in that sample with that sample, we can get a pretty good idea of what has gone on in situ in the 8 years that this mud has been sitting in the bottom of the river. I think this is a really useful thing.

Darryl Decker: Are you assuming that no dechlorination has taken place..

Dr. Bopp: No, no..I want to see how much dechlorination has taken place!

Darryl Decker: In the archived material, during the last 8 years.

Dr. Bopp: The archived material was stored dried and ground in cans and not only that but we have extracts of the archived cores that have been stored since the date of collection.

Darryl Decker: You're looking at the dechlorination now in the archived material, you're not taking the potential dechlorination that was there perhaps at the time you took your sample.

Dr Bopp: Right. To look at what happened between the time this was emplaced and the time it was sampled, you would compare it to an original aroclor composition. So you have really three time points. The original aroclor, this time point in 83 and this time point in 91. A very useful type of thing for trying to get in-situ rates and changes in PCB congener patterns and dechlorination. We can do that at all these different points where you can draw very nice correlation based on the cesium profile and the neat thing is that these different points span a wide range of total PCB compositions - from a few ppm. to over a thousand ppm. So you can get a nice picture of in-situ dechlorination in progress by looking at these types of samples.

One other point. The other thing that's nice. This core has this extra 16 centimeters on top that I would say deposited between 1983 and 1991. We know what the total PCBs were at that point. And what the pattern was, how rapidly they were decreasing. Wouldn't it be nice to know if they were there in 1983, where are they in the stuff that was deposited between 1983 and 91? All you have to do is analyze that upper layer of that sediment core and you are starting to continue the chronology of PCB concentrations for this point in the river. And the idea is to do this at as many points in the river as you can get cores that look like that.

Dr. Abramowicz: Richard, may I ask a question? How similar will the concentrations of PCBs be in this sample and this sample?

Dr. Bopp: Ah, so you're asking if there's any total destruction of PCBs.

Dr. Abramowicz: That's not what I'm after, but, I'm interested to know based upon representative samples that you've taken how uniform the pcb concentrations will be at different time points.

Dr. Bopp: We tried to overlap - we've overlapped cores at Kingston, and overlapped cores in NY harbor. And the overlap is beautiful, it's in the 1989 paper.

Dr. Abramowicz: I know your profiles of cesium distribution, no pcb concentration...

Dr. Bopp: In the 1989 paper in the National Academy publication there are overlaps for 88.6 has two cores, 88.6 and 91.8, and NY harbor has two cores, -1.65 and -1.7; and both of them have significant at least three or four samples that overlap on the profiles, and they're quite good.

Dr. Nicholson: But the congeners are changing.

Dr. Bopp: Not as much in the harbor as they are up here, and Dan's other question, "What about total destruction?", we don't have the analyses in on this yet, but let's say there's a congener shift, but it remains to be seen if there is significant evidence of total anaerobic destruction.

Dr. Abramowicz: I have a couple of comments. Not about the technique, but about the potential information that could come from it, because that is an area that I am more familiar with. One of the concerns I have with the goal of these samples is that, as Richard already mentioned, unfortunately you can't take lots of them. There are only infrequent spots in the river where the depositional history is uniform enough that you get these good cesium patterns and can then do these analyses. Given that, and that means that we're going to have a fairly limited number of samples that we're looking at.

One of the things about biological processes that you can say rather routinely, is that there can be large numbers of factors that influence the rates of biological processes. We've done samples of the upper Hudson river that show that dechlorination in quite extensive in the upper Hudson river. The only way that we could make those kinds of statements was by literally looking at hundreds to thousands of samples. So I think that one of the challenges here, is you're going to have very few samples, trying to look at a measurement that you know inherent, can be very, very variable. So, based upon those rather limited measurements, you're going to try to make general statements about rates of dechlorination.

Dr. Nicholson: The variability that you get across those that you have would give you a measure of the degree that this would differ in other samples as well.

Dr. Abramowicz: It depends upon the statistics is what I would say. One of my general comments about the limitation of the high resolution coring is just that, the fact that unfortunately, you can't get very many samples and so therefore, the generalizations you can make about it are potentially more limited. Particularly when you are talking about something not like pcb concentration, which one would imagine is a much less effective variable, than something like biological dechlorination, which could be affected by many different variables.

Another issue that I have is we know that dechlorination appeared to have occurred rather rapidly in the river. Samples taken in 1973, rather routinely, before the dam was removed, showed that there was very little dechlorination that had occurred in what is known as the remnant deposits, the sediments behind the dam.. If the dam is removed, now back by 77, dechlorination is being routinely seen in samples by 84 - the survey taken by DEC, dechlorination is now quite extensive through the upper Hudson.

If you're looking at later time horizons, one of the things about dechlorination is, it only removes a certain number of chlorines. Unfortunately, it doesn't convert PCBs to non-PCBs, it just reduces the chlorine level from highly chlorinated to lightly chlorinated. I think that much of that initial dechlorination will have occurred before this sampling period that we are looking at, so you might be trying to determine rates of in-situ dechlorination on a process that's already been completed...or near completion, from that perspective. So, I think that that's a potential limitation.

I guess the third point I'd make is I'm trying to figure out how accurately one needs to know rates of dechlorination in the Hudson River. It seems pretty apparent from the sampling that been done that it has occurred at least within the period of a decade within a very extensive amount whether the additional information we could gain by this process, these samples, will refine that more or what the added value is, I'm not clear on. But my major concern is that there will be very few of these samples that will have the right chronology and so we are going to be trying to make general statements on the rate of this biological process on those very limited numbers of samples.

Dr. Bopp: When we started several months ago, I was the one who brought it up that we had really one good dated core, one really good chronology core in the upper Hudson and several other spots to try. As part of the confirmatory sampling that went on, with the high resolution coring, we were able to get really good chronological cores at just about very spot that was suggested in the Phase 2 sampling plan. We have done quick counts on cores, before we went back and revisited the spots to sample. It's not an infinite number, but it's no longer one in the upper Hudson, it's more like a half a dozen in the upper Hudson.

Dr. Abramowicz: That doesn't comfort me too much. One of the things we found in the sampling that we have done in the river - we sampled in 1990 about 18 sites. Pretty extensively. What we found was there were different aroclor mixtures in about 4-5 of those sites. So, in other words, the distribution, the starting material, may be relatively non-uniform in many locations. I just think that there is a lot of inherent variability in the concentrations of PCBs, in the type of PCB that was started. In some cases we found aroclor 1254, in some cases aroclor 1270. There's going to be variability in the biological data, you're going to be trying to make lots of generalizations on this process on perhaps as many, or as few as six samples.

Dr. Bopp: No, six cores, so fifty samples - or sixty samples. They have different starting concentrations, so you can get some extra information by analyzing each sample within the core. Six locations. That was always the idea of this type of sampling - the high resolution coring - the EPA proposal.

Dr. Bonner: That's the grain of salt that everybody has to take along with them. Six locations, you must realize that if you're sitting at one point in the river, and if you're one foot away and get an entire different result. So, everyone has to be aware of what that really means. Now, it can be real useful to reflect mechanisms, it's very useful to compare from one point to the other, but you have to take it for what it is - it's a tool to help us understand this system.

Dr. Bopp: And it's not very useful for doing inventories of PCBs. The other thing that is useful to point out is that in areas where you do - are able to reconstruct the chronologies, you do very often find among the very highest levels of PCB among those chronologies. Because you do have areas of primary sediment accumulation, so we do have the 1,000 ppm. samples in those two cores that I showed you. So that's another thing that makes it a little bit more relevant.

Dr. Abramowicz: We have indeed done sampling as he described where we found extensive dechlorination and types of aroclors varying on distances of a few feet. So, the perspective we took is that to make any generalized statements, we had to look at literally hundreds of samples.

Dr. Nicholson: Those dated samples - where you know that the same composition you see a few feet away, entirely different dechlorination processes where you know them to be historically the same?

Dr. Abramowicz: None of the samples I've taken are dated. There are samples where we've just analyzed for PCB distribution and PCB concentration. We haven't done any of the radionuclide type.

Dr. Bopp: Exactly. By dating the samples, what you're trying to do is to remove some of that type of variability.

Ms. Rychlenski: Okay, let's move on to the next one - the next question has to do with documentation of sampling and storage of archived cores. The question specifically is "**What documentation does this committee have concerning how these cores were obtained and how they were stored over the years?**"

Dr. Bopp: Okay, well here I brought along our field notebook from the extensive sampling in 1977. (Holding up field notebook for audience viewing). I can read you out of here locations, times, sampling places, and water depths of seventy of the cores that would be used of the archived cores. Core 91.8 is here, core P143.4A is in here. They're all in here, and if anyone wants to come up here and look at it, there are maps and little "x's" on USGS maps in the back and there are core descriptions. We have this for everything we've done.

In addition, for every PCB sample we've extracted, we have this type of data on extract weights, organic contents, date of extract, where the sample was from, what the control number of the sample is. Every sample we have has a control number recorded in an independent book. Here's the way the samples are stored, in boxes. (Here holding up boxes of core samples stored in small cans.) Each section in its own individual can, with the control number, the core identification, the depth in centimeters, the sampling date, the count date, the total weight, and on the side of the can we record anytime we open the can and remove any sample from it. These cans, they look like pudding cans, they're nice, they have air tight seals. We need air tight seals to do some of the radionuclide counting that we do.

So there are dried sections of sediment cores in these cans. We have literally, scores of these boxes at LaMont. This box contains cores SF-D.

Dr. Abramowicz: Richard, you said they were dried?

Dr. Bopp: Yes, all the cores were dried at 35 degrees centigrade, in a controlled environment, under a flow of air through a fluorocil column to minimize any atmospheric contamination.

Dr. Abramowicz: Did you do the radioactive traces before they were stored?

Dr Bopp: No, these were done after they were dried.

Dr. Abramowicz: These were done years ago.

Dr. Bopp: Yes. When you go out and collect say, six cores, you want to count the top eight centimeters or so for beryllium because it goes away in 54 days, so that limits the number you can count. You've got to do that quick. That's all been done.
But this is actually the geometry that you count it with. You use a standardized geometry, and so we use the cans for the counting. You just sit the cans right on the detector.

Dr. Bonner: You dry in a controlled environment?

Dr. Bopp: I dry in - I stole an old gas incubator and I plumbed it with an air flow from a teflon piston pump through a fluorocil column in through the incubator, and I've run off shore samples of ancient mud to get a handle on my blanks, and that's the best way to get the blanks down and stored right as samples.

Dr. Bonner: The off gas from the incubator did you have it go through XAD or to locate any off...would that be significant in your opinion?

Dr. Bopp: What I've done is I've run some wet, freeze dried and our types of dried as well as heated samples and saw very little difference - these were lower Hudson samples, so they didn't have many monos and di's, so we couldn't really see the monos and di's. Well, but when we do the upper Hudson samples, we see lots of monos and di's. And I understand that most of the GE samples that were run have been air dried well prior to extraction...

Dr. Abramowicz: Without a flow of air, yes.

Dr. Bonner: And you're seeing monos and di's?

Dr. Bopp: Yeah, I see monos and di's too.

Dr. Abramowicz: We haven't had much of a problem with loss of the monos and di's, but we don't flow any air, we just let it dry. We've looked at extracting wet and extracting dry, and it hasn't made too much of a difference.

Ms. Rychlenski: Nothing else from you guys? Alright, let's go on to the next one and that deals with the use of this technique in riverine systems, and specifically, **'We have heard at these meetings that the radionuclide dating technique has not been used in dynamic river systems such as the Hudson. Does this committee question whether this technique will produce accurate information?'**

Dr. Bopp: One of the systems it has been used the most in is the Hudson. So that's a misconception. It's also been used in Newark Bay, the Passaic River, Chesapeake Bay and several of the Chesapeake tributaries, the James River. I reconstructed the dioxin story in Newark Bay on a similar basis. So although many of the initial applications were used in dating lake sediments, especially in the Great Lakes, it has been used fairly extensively in riverine systems. It has been reported on in the literature, the National Academy of Science reports and meetings.

Dr. Nicholson: But you still have to select your sample spots.

Dr. Bopp: Yeah. Well, if you're crazy you can go out and sample every fifty feet and count the radionuclide data and get nothing you can interpret. So, as the years go on you do get a little bit better. In '77 most of these cores were not excellent , shall we say, chronology cores. In '77 our collection of 70 cores yielded say 20 cores that would provide this type of information.

Ms. Rychlenski: Alright, at this time we will take questions from the audience on the subjects covered so far. Are there any questions or comments to what you have just heard?

Pete Lanahan (G.E.): Richard, what are the standards you apply as to what's a good core, a usual core...?

Dr. Bopp: Okay, what you're looking for..you're looking for the beryllium-7, you look for beryllium-7 in the 0 to 2 centimeter layer and no significant beryllium-7 beneath. If there's significant beryllium-7 beneath, then that means that you may have problems with significant mixing. You look at the top of the core and at the cesium maximum and you generate a sedimentation rate and you say, well, is this a smooth curve. If this is a smooth decline like you'd expect from the fallout records, you should get about the same sedimentation rate that you get from that section compared to the sedimentation rate you calculate between '63 and the early fifties. So, if you've got a factor of 3 difference, you'd probably

be suspicious of that. However, if you saw an area in the core that interrupted that smooth decline, you may be able to interpret that as being indicative of a single event, a high flow event, and you would then compensate for that. You do your best job looking for the shapes of the profiles, the single peak, or if you're at Indian Point or below, a double peak...

In the best cases we can make the point by showing the picture of the cesium profile to a group like this, and people can in general, very quickly, come up with the same types of interrelations.

Pete Lanahan: One more question. The archived cores. You archived these before anybody knew that the end of this process was going to be radionuclide dating. SO, I'm wondering whether whole thing meets the current way the EPA does quality assurance/quality control.

Dr. Bopp: It probably meets what you guys (EPA) call level 2, or something?

Mr. Tomchuk: Different data quality objectives is what Richard is referring to. One through five, with one being the lowest. And without a full chain of custody , which is what I think you are referring to (to Mr. Lanahan), it may not reach the highest data quality use level. It definitely has limitations as far as that goes.

Dr. Bopp: But, there are all sorts of ways of checking it out. There are many analyses that have been done on these samples that could be redone. So there are internal consistency checks that could be performed to ascertain that you get the same results now for types of analyses that I conducted ten or fifteen years ago on the same samples.

Dr. Brown: I have two questions, Richard. This particular study, we're going to be collecting a lot of cores, and there are going to be a lot of decisions, I suppose, on which ones are good ones and which ones are bad ones.

Dr. Bopp: No.

Dr. Brown: No? Possibly some decisions as to dating. Who is going to be appraising the cesium data?

Dr. Bopp: Unfortunately, or maybe, fortunately, the way the system works, the PCBs must be extracted wet from the samples within seven days of collection. So we took a look at the cesium profiles that we've gotten from the areas here high resolution cores were on the agenda. Then we took sediment cores on this spot and took a plug out for pcb analysis, and that's already sent out for analysis, then the rest of that section will be dried and counted for radionuclides. So the radionuclide data and the PCB data will be generated together, and will come out together as part of the data base that is generated as part of this project.

Dr. Brown: My question was, who will make the conversion of the cesium 137 data to dating?

Dr. Bopp: You don't have to do that. The cores will be good enough that you can argue reason from the cesium 137 profiles from the primary data.

Dr. Brown: That was my question, whether it was going to be you or somebody else.

Dr. Bopp: I don't think it's right to bury the primary cesium 137 data and have things reported as sort of in the artificial way as the approximate year of deposition. Everything I believe will be presented in the final data package. I wouldn't think of offering an interpretation of the data without presenting all of the

primary data on which that interpretation is based. I will be involved in looking at the cesium data and the pcb data.

Mr. Tomchuk: Richard will be involved as well as Jim Simpson from LaMont and Ed Garvey from TAMS.

Dr. Brown: My other question is a more technical one. When you fellows at LaMont were developing this technique was the idea of controlling for non-radioactive cesium explored?

Dr. Bopp: No. Not to any significant extent.

Mr. Putnam : Richard, when you were back kind of probing blind, taking the first series of these cores. How many of them turned out not to have a useable or normal kind of chronology, and, in the act of taking these first groups of cores, were there any that you threw out by eyeball examination to begin with?

Dr. Bopp: No. You can't do that. Boy, you can really get mislead if you try to start throwing things out by eyeball examination.

Mr. Putnam: Okay. So there was no clear evidence of disrupted bedding that you used to toss them out?

Dr. Bopp: That's a risky thing to do in a dynamic system. The only criteria we used was where we could collect the cores. And in general, you can not collect gravity cores of the type we take in sandy or very coarse sediments. So we initially, by the nature of our technique, preselected for fine-grained sediment areas.

Mr. Putnam: So how many of those were abnormal?

Dr. Bopp: There are different degrees of useability. For one thing, you can imagine where it would be quite useful to know some integrated PCB levels or lead levels of any cesium bearing sediment, of any sediment that's more recent than 1950. So, it's not necessarily that you get an interpretable profile that you can date within a couple of years or you don't do anything with the sample. That's not the way it works. But in terms of producing profiles, we look for the best areas that we had. And, I would say, you're lucky maybe 10% of the first group. But it's not a 'these are good, these are no good', it's 'these are the best' and then there's a gradation. so for the criteria that we're using here for trying to get dates within a couple of years, I would say that the first time we went out was about a 10% shot.

But, at that time we were also interested in getting the other types of information, some idea of the geographical distribution of sediment accumulation. So we were interested in how much of the river was this year by year, layer by layer accumulation. And how much of it was much slower and how much of it was much too fast. And we found all of those types of areas. So, in a sense, one out of ten, shooting in the dark isn't bad. That's a very hard question to answer, it doesn't have a yes or no answer.

Mr. Putnam: But I gather that intrinsically the data is probably not there to be able to take a given core that's a little bit abnormal, or doesn't fit outside your 10% range and try to compare it with the discharge record on the river to see if we're talking about a given flood event five years apart as having stirred things up enough to have disturbed a certain section of it and not the whole thing.

Dr. Bopp: Pretty tough.

Mr. Putnam: Pretty tough, alright.

Mr. Tomchuk: I would like to ask Richard and Dr. Brown about that last question - controlling for non-radioactive cesium. The counting that you do for the cesium is just on gamma radiation. I'm not sure where the non-radioactive cesium, if that would interfere at all, if you're just counting radiation.

Dr. Brown: It wouldn't interfere with the cesium 137 count. The purpose would be to find out - you know, you've got sediment sorting out in a river and some of the particles have a natural tendency to absorb cesium, either non-radioactive or radioactive more than others, the question is, instead of looking at the ratio of cesium 137 the total mass of sediment, look at ratioed to the cesium-absorptive capacity of the sediment.

Dr. Bopp: Unfortunately, the cesium absorptive capacity of the sediment is very difficult to get, and a lot of people believe that there are very different types of absorption sites and that cesium has been around a long time. Even fallout, radioactive cesium can get incorporated into the interstitial layers of clay lattices and become essentially non-exchangeable. So, it's not a straight forward thing to do. There are a lot of geochemical aspects of cesium and sediment interactions that are quite complicated.

Darryl Decker: The high resolution coring that's going on now is about one core per mile in the upper Hudson. Is that adequate to get an accurate representation of the depositional length of the river?

Dr. Bopp: It's much less than that, it's one per every five or ten miles.

Darryl Decker: How could you get an accurate record?

Dr. Bopp: That's not what it's for. The low resolution coring will address the distribution of sedimentation rates as well as the mass balance of the PCBs. This is for chronological information.

Mr. Tomchuk: Looking for the mass balance we also use side scan sonar work and previous sampling efforts. I have one further question for Dan (Abramowicz) - you said something about you don't understand the necessity to get rates at this time for biodegradation in the river. It just seems that a lot of the press that goes along with this project is saying that 'the river is cleaning itself'. So that's the reason that EPA originally went to try to get rates was to see if we could expect in the future to see further cleansing by dechlorination and degradation, possibly. So, if you want to expand further, why you think the rates would not be necessary in order to make that extrapolation?

Dr. Abramowicz: I'm differentiating two very different biological processes - the dechlorination from the more classic aerobic biodegradation. There have been samples taken on a time span of about five years apart back in the late seventies in 1984, as well as in 90, as well as the historical information that was available before the dam was removed. Those samples indicate that dechlorination occurred quite extensively within a ten year time period.

So, I'm convinced that the first phase - the dechlorination step, occurred within a fairly rapid period of time. I actually argued it could have been significantly less time than that. But, let's just be pretty conservative and say over a ten year period pretty extensive dechlorination occurred. I don't see how six additional cores are going to give us more information than those thousands of cores that have already been taken..on that part of the question. If they could yield information on the rate of aerobic biodegradation, that would be a very interesting part of the question. But I don't see that potential there. We've been approaching that in a couple of different ways - by looking for metabolites of aerobic PCB biodegradation in the river. We've actually discovered that in undisturbed cores, we see those expected metabolites, much as in the field tests we did. So that at least answers whether aerobic degradation is occurring.

Although we haven't yet done statistical sampling that we would need to do, in something like a dozen cores we find it pretty uniformly occurring. So, I think that we are going to find that aerobic degradation is occurring, the question is how fast? I think that's a challenging question to ask. I was assuming that those rates would be pretty slow. With unmixed systems, with only diffusion of oxygen into the sediments, we would probably see very slow rates of aerobic degradation. We set up some model systems in the laboratory that seemed to indicate that the rates
are much more significant than anything that I would have anticipated.

The reason for that is probably is that the congeners are so dechlorinated already, that we're talking about monos and di's that are much more aerobically degradable. So I have a feeling that there may be other ways to approach the rates of in-situ aerobic biodegradation which I think is a very important part of this puzzle. But as far as the dechlorination, whether we can say that it occurred in ten years or in five years, how will that change about what you're going to say about the need to remediate the sediments?

I think there's a lot of very convincing data that dechlorination has occurred in the upper Hudson River and that it has occurred on a very extensive scale. We get in the lower Hudson river, it's clearly a different situation, we know a lot less about it. But in the upper Hudson it's really quite clear that the dechlorination is extensive, and widespread. If we determine that it occurred over a shorter or longer period of time, I'm not sure what the additional value of that information is.

Mr. Tomchuk: You're saying that the dechlorination is virtually complete in the upper Hudson.

Dr. Bopp: Would you like to see the results from these two cores? I think you would be really interested in seeing the results from these two cores. I don't have them....

Dr. Abramowicz: We had to look at literally thousands of cores..

Dr. Bopp: But you didn't have the stratigraphic control that's here, Bill's point.

Dr. Abramowicz: But I don't care about stratigraphic controls...

Dr. Bopp: Sure you do....

Dr. Abramowicz: But the one thing you can't do, unfortunately, and I wish you could, is do the statistics to convince me that the trends you see here are general. And that's my basic problem with this technique.

Dr. Bopp: What if these trends show the same trends as your large...

Dr. Abramowicz: That would be interesting, but I don't think it would be as strong.

Dr. Bonner: I think along these lines, we just did a literature review with regard to bioremediation of chlorinated phenols. It wasn't biphenyls, but single phenol rings, chlorinated to varying degrees. We probably looked at fifty to seventy different studies, and we saw a range of rate coefficients from the bare
minimum all the way to the maximum that one could expect for bacterial growth rates and bacterial utilization rates. These were primarily controlled engineered systems, not natural systems. I guess what I'll throw back out is even in the most controlled situations, you're not going to get uniformity across and so I would say that from one location to the next the generalization of that data has somewhat limited use.

If you want that information, and I think that information would be valuable - I wouldn't go out to the core data to get it, I would go to the laboratory, or I would do some study where that is the specific objective of the study. I think you will gain a lot more bang for your buck.

Dr. Bopp: You have one of the hottest hot spots of the upper Hudson that's represented by this core. It would be one of the top ranking hot spots for remediation on anyone's list - it would cost..what does it cost to analyze 20 PCB samples, $250 bucks a sample, is that a good investment of money?

Dr. Bonner: I think it should be done. I'm just questioning what you do with the information.

Dr. Bopp: One of the things that must be done is be very careful about using the word "rate". You have to explain about all of the things that Dan talked about...

Dr. Abramowicz: That's my concern. I'm not saying that this isn't a valuable thing to do, and great if it yields us more information on the system then that's added value. But, I can just predict that people are going to try to make general trends out of this limited data.

Dr. Bopp: But Dan, you're going to be there to comment on this. I'm going to be there. I'm aware that you can't extrapolate a linear rate over three time periods. I'm aware that the congeners change and there may be different rates for different congeners. I talk to Rhee all the time (referring to Dr. G. Yul Rhee, another member of the STC). I think you have to give the scientists and the scientific process time to work and a chance to explain what they find out, and I think this is a very useful thing to identify.

Dr. Abramowicz: Well then, let me ask Doug. Are you counting on this information to give you general results about rates of biological processes in the river?

Mr. Tomchuk: I don't think that that's the major objective of the program.

Dr. Abramowicz: Is it an objective of the program?

Mr. Tomchuk: I think that's it's one of the things we would like to see whether we can get out of it.

Dr. Abramowicz: I think that we've just said that we will never have the statistical information from it to be able to make that claim on any scientifically valid...

Dr. Nicholson: You have to wait to see what you get. Maybe the analysis shows that you don't have any statistically valid points to be extrapolated.. On the other hand it may show that you do. You get a measure of the quality of the data when you look at it all and that tells you how much reliance should be put on it.

Dr. Abramowicz: John Brown talked about six cores of the upper Hudson River, well, grab samples actually, that showed dechlorination in the Upper Hudson River. No one assumed, or in fact, actually, people probably became a little bit aggressive about generalized statements based on that limited number of samples, although they were quite consistent. Richard took another thirteen, that again showed fairly uniform trends of dechlorination occurring on a fairly broad scale on the upper Hudson River.

Statistically, unfortunately, that limited number of samples meant almost nothing. It said it is happening, and it may be happening on a broader scale. The only way you could get that information that you'd like is to go out and do the larger kind of sampling. That's my issue. River sediments - we know that there are different depositional rates in different areas, some always scour, some always deposit, most of

them oscillate between the two. And we are going to pick out some rather select, rather unique environments, and we're going to learn some interesting information about what is happening in those environments.

I think we will learn the things that Richard hopes we will. We will learn something abut deposition, etc., we may learn something about in-situ biodegradation rates. But, I think it's going to be limited to that very narrow subset of the population. The problem that I have is that everyone is going to try to generalize that for the entire system.

Dr. Nicholson: I don't think you can assume that.

Dr. Abramowicz: I just heard it. That's what Doug is hoping it will give them.

Mr. Tomchuk: I think I said that that's one of the things we are looking at it for. And obviously, it is. You have to look and see how valid it is. Any experiment that you do, you're trying to prove something. We're trying to analyze this data to see if it will yield the type of information that we would hope.

Dr. Abramowicz: Okay, so the results are that you get variable results and you know it didn't work, or you get consistent results and you still only have six and statistically you don't have anything. Those are the possible options or some mixture in-between.

Dr. Bonner: It might be more than that. But the issue here to stress that you are going to get a qualitative indication of what is going on in the system. It is very important to know that dechlorination is occurring, yes or no, in the system. That's very important. It's very nice to know that it occurs over a ten year period, but I don't know if I'd want to start using the delta difference in concentrations over a one year period to determine a rate coefficient. So, you might use the data point to calibrate a model, but you'd better not use that series of data from a core, or even six cores, to determine a rate coefficient that you then apply in a model, an exposure assessment model perhaps, over the whole river. That's a very dangerous proposition.

Mr. Tomchuk: That was never our intention.

Dr. Bonner: The information is very valuable, but I think we're going to have probably a gray range here on just how far you can take it. And I'd like to caution the scientific community and the public...

Dr. Bopp: And the modelling community. Well, you said the information may be good enough to put in a model, I thought that was hilarious.

Ms. Rychlenski: Okay, any more on this one?

Ron Sloan: I think there's a nice experiment. But you're going to be quantifying a lot of dechlorination data on the sediments. One thing that's struck me, and you know I'm as big a champion of dechlorination as anyone - is that we see virtually none of it in the fish at any period of time, '77, '78, '84, '91, that dechlorinated PCB is not moving up, at least in what I've seen.

Is this information going to be coordinated with anything coming out of fish analyses? Are we going to be trying to match the PCBs in the sediments to those in the fish?

Mr. Tomchuk: I think we'll get to that in a little bit.

At this time a ten minute break was taken by the participants.

Meeting resumes.

Mr. Lanahan: I was wondering what are the various techniques that you are going to use to get at some of the questions that we talked about tonight? I know that coring is not the only thing. And I was wondering if you could say what the relative degree of confidence in each of the techniques and the relative degree of importance you place on the answers you get.

Mr. Tomchuk: I don't have a definite answer, Pete. I just wanted to say though, that a lot of the things confirm each other. You compare a sediment core with the transport, basically records what's been transported in the water column. So, sediments transported in the water column, you compare that to PCB numbers in the water column historically. And everything can fall upon each other. Combine that with new data on water column samples and look at what's in the top layer of those sediments cores. Everything is overlapping. Each thing in itself may not have a guaranteed use, but when you combine it with everything else it completes the puzzle.

Dr. Bopp: Just to illustrate that idea, I think that at one of the first meetings that you and I were at together you presented an analysis of the USGS transport data that showed that the PCB transport in the river was decreasing with a half time of on the order of three years or so, between 1977 and 1989. I actually plotted it out myself and got the same answer. If you use two years prior to that, I used dated sediment cores from the Kingston area, and actual analysis and got a half time of decrease indicated by the dated sediment cores of about 3 and a half years for that same time period. So the strongest information will be where you get the same answer from a various number of sources.

Ms. Rychlenski: Okay, we're going to move into the next category, the Accuracy of the Data Base, and the specific question for the group: "**Will a combination of information previously obtained from Phase 1 and the data from Phase 2 be adequate to make an accurate judgement on remediation? If not, why not?**

Dr. Abramowicz: I'll take a shot at this one. It's a very broad question and we can probably spend the whole night talking about it. One of the things that the Phase 1 Report showed is that risk is based upon primarily, exposure through fish. So, the issue really comes down to what is going to effect PCB levels in fish. Although that's a simple question, the answer to it is really pretty complex. You need to understand a lot of things, like the food web, PCBs are in the sediment, they're also in the water. Some small organisms typically somehow take up the PCBs, they're eaten by other organisms and eventually by the fish. The components of that which I won't try to address because I am not a modeler, so I must be a scientist, are very complex, and as far as I can tell, I can't argue on how well that is being addressed in the model.

There are a few things that I think would have a significant effect that I think aren't included in that the way it is today. One of them has to do with the different kinds of PCBs. There are 200 kinds of PCBs. We typically in the regulatory arena, like to think of them as all the same, we use the same cancer potency factor, we use the same bioaccumulation factor, so on and so forth. Because of the dechlorination that's occurred in the upper Hudson, primarily now you have mono and di-chlorinated PCBs present there, or PCBs containing one or two chlorines.

One of the fortunate advantages of that is that those congeners, those individual PCB molecules, do not biomagnify through the food chain to nearly the same extent. The differences can be significant on the orders of magnitude, factors of ten. In addition, they're also cleared from humans in different ways. You know we actually contain enzymes that metabolize PCBs. Typically, the more highly chlorinated PCBs are cleared more slowly than these lighter monos and di's. Again, order of magnitude type effects. Nothing I've seen in the model takes that into account at all. So I guess the phraseology would

be a homologue-specific model. It doesn't take into account the degree of chlorination of PCBs in the sediment in interpreting how that will affect concentration in the fish which seem to drive the entire risk.

So , I think that's a critical issue, and from what I've been able to learn about it, it can have a rather significant effect on the result that's obtained. From that perspective, I think that's a lack in the data base that's going to be used to try to determine the effect of remediation.

Dr. Bonner: When you said model, what model were you referring to? Were you referring to a specific model, or a concept?

Dr. Abramowicz: I guess I'm referring to the model that the EPA is going to be using to figure out what the remediation will do to the fish levels.

Dr. Bonner: Is there a model, Doug?

Mr. Tomchuk: Well, we're doing three different models, as outlined in the work plan. One is just the mass balance for river sections B and C, one would be a fish uptake model, and I guess that's the one that Dan's referring to, and the other one would be a sediment scour model.

Dr. Bonner: Is that homologue-specific?

Mr. Tomchuk: We will be looking at homologues and in some cases specific congeners that would typify a homologue group within the models to the extent possible. We are relying on the historic data base a lot, and there are certain applications where we can't do that, but when it's possible we are going to try to go to that level.

Dr. Bonner: Well, there's one extra thing in terms of homologues and congeners, etc. there's also different toxicity levels associated with different congeners. I know you don't want to get into that but...

Mr. Tomchuk: It's actually a really valid point though. As an agency, EPA regulates on total PCBs and I think we've been through that several times with this group. So, when you go to do modelling on homologue-basis or congener-basis, then it's kind of difficult because you are doing a refinement in your scale to characterize that part of it, but it's regulated on total PCBs. We will be making the decision on total PCBs, that's agency policy. We here have been hedging around it. You know it's kind of a torn type thing - we're trying to be technically sufficient and efficient in recognizing the differences in transport and uptake, etc. But, at the same time the regulation is on total.

Dr. Abramowicz: I wasn't trying to get into the fact that there may be different biological effects, different cancer potencies of different PCBs, because that's obviously still an area of a lot of discussion, debate, research and so forth. Given the assumption that they're all the same. Which we were going to try to start with today. My point is that from a physical perspective, and that's something that there's no debate about - it's physical chemistry, it's partitioning, it's things that are understood very well.

The differences in these congeners can be very dramatic. And I think that there's a great deal of concern in the public's eye, at least in the lower river, about what a flood might do to pcb levels in fish. I think that most people look back to the mid-70's, and I think what people have to recognize is that we're talking about different PCBs now, then we were in 1975.

In 1975 what you had were primarily tri and tetra-chlorinated PCBs, those are most heavily chlorinated, more hydrophobic, more water-insoluble, more fat-soluble. They adsorb into the fat tissue of these organisms more readily. So, what occurred back in the late to mid seventies, if a similar flood were to

occur today, I would have some significant issues on whether it would have the same effect on fish levels. Because now, primarily, we're talking about the monos and the di's. And I think there's a general assumption that there might not be any difference, and the physiochemistry might say that it can be a several orders of magnitude kind of effect. I think it's an important issue that needs to be factored in.

The other model that you described is the mass balance model, and I wish you good luck on that. We took an incredibly fine resolution sample of hot spot five, as you know. We've made the data available to you, and that data showed heterogeneity on twelve foot centers, on six foot centers and on one foot centers, that was for all practical purposes indistinguishable. In other words it was pretty noisy out there when you looked at the PCB levels. And when we talked to experts who tried for example, to do mass balance of metals for mining, using sophisticated techniques like kriging, they basically came to the conclusion that even the twelve foot center resolution that we had - we had about two hundred samples in a say, two hundred by fifty foot range- would not be sufficient to do accurate mass balances in that two hundred by fifty foot range.

So, another fifty low resolution cores in the upper Hudson is unfortunately not going to get us any closer to accurate mass balances. Again, that's a statistical argument, but I think it's an important issue. I just don't think there is any feasible way to get those kinds of mass balance numbers.

Mr. Tomchuk: I think that one of the things that's going to be helpful rather than just the analytical data with the PCB analysis, is the side scan sonar, defining areas of fine grain sediments that we can locate and characterize through historical or new samples and get a general range within those fine grain samples sediments and make approximations from there.

Dr. Abramowicz: So you're saying that you think you can use side scan sonar to give you estimates of pcb concentrations?

Mr. Tomchuk: No. To define areas of fine grain sediment.

Dr. Abramowicz: Okay, the technique may give you information about the type of sediments, but I don't think you can use that in any way to extrapolate for the PCB levels in those sediments. I think it would be wonderful if we had mass balance numbers, it would be even better if we had mass balance numbers over time. It would be nice to be able to know what it was in 75, in 80 in 85 and 90.

I'm cautioning again based upon the power of the information, the question says is it adequate to make that kind of a judgement? Will you be able to do accurate mass balances? I do not believe so.

Dr. Nicholson: The issue revolves around multiplicity of different activities that are going to be used and the analysis of the results from them. One can't guarantee that everything will be perfect and provide just the answer you want with certainty. We should be able to though, once the study is completed, determine if it's sufficient to make a proper judgement. The concerns that you raise are valid, but that doesn't mean that you throw up your hands and do nothing. You do the best you can, and then you look at the answer you get and see if it's accurate; and if it does indeed provide the information that's required.

Dr. Abramowicz: I guess the way I look at it is - you've got two approaches you can take, you can say I need to know the mass balances to determine this problem. In which case you can look at the samples we took in the upper Hudson, look at the samples the DEC took in the upper Hudson, and you can determine the number of samples you would need to take to accurately get that number. And the

answer would be more sample analysis dollars than any of us have. You either have to decide I want to meet that objective or I don't, and you design a sampling plan that can accomplish that, or...

Dr. Bonner: No, let's ask a question here. You went out to your depositional zone, where you had this high resolution, spatial resolution core, if you took a central tendency in that data set and tested central tendency for low resolution and then compared it again, using the same data at high resolution. Was there a dramatic difference in the central tendency..

Dr. Abramowicz: I don't know what a central tendency is.

Dr. Bonner: The mean or some other statistical measures.

Dr. Abramowicz: I'm trying to see if we did what you're asking for. Cause what we did is we looked at it on the large grid and then as we did higher resolution sampling, we did that on smaller and smaller sets, because again, we were limited by the number of samples we could analyze. So I don't think we were able to say what the mean was on twelve foot centers, on six foot centers, and on one foot centers.

Dr. Bonner: What I'm asking for is if you take the mean on your twelve foot centers, then using the same data set, take the mean using one foot centers. How different are they?

Dr. Abramowicz: Let me just explain it and then I'll try to answer your question. We did twelve foot centers on the whole thing, we did one foot centers on a very small sub set. We didn't do the whole two hundred by fifty feet on one foot centers. So given that limitation, the numbers were very different. In fact when we homogenized the samples in the caissons taking it to another level, the numbers again were very different.

Matter of fact, we did everything possible to localize the caissons to the highest concentration based upon the samples I just told you. All of those analyses told us we were going to be in 125-150 ppm. PCB concentrations. The concentrations in the caissons were 50, 40, 30, 20, 20 and 6.

Dr. Bonner: I guess that the point that the question is coming from is if you were doing a modelling project, if you were going to go out there and model, we couldn't afford the cpu time to work on one foot centers or twelve foot centers. So if we can assess the central - in fact we are probably working on kilometer scales, at best half kilometers, if we can come up with a general idea of what the concentration is in a half kilometer square we can then begin..I think that is still an effective tool.

Mr. Tomchuk: I think I may have mislead things in the beginning of this discussion, and I think I'm just going to go back to our Work Plan and the objective of the mass balance analysis is to predict the PCB levels in water and sediment in a year by year and reach by reach average basis. It will be used to analyze the potential impacts of various remedial and source control schemes and will provide that input component two from which impacts on the fish population will be estimated.

So we are looking at general trends, we are looking at spatial scales of three to five mile reaches, a scale appropriate to estimation of average effects in fish populations. Fish move, so that the spatial scale of foot by foot centers is not what we're looking for. I think we got off the topic a little bit there.

Dr. Abramowicz: No, I'm not trying to suggest that we should sample by foot scales. What I'm saying is that even sampling at remarkably high resolutions like that, yield information that experts interpret as meaningless for determining total masses.

Mr. Tomchuk: I think looking at the movement of PCBs in the water column components as well as a general trend that the fish would be exposed to - I think you can come up with an average level for a certain reach of the river. There might be variation within that, but I think that the data we're getting is sufficient to do that. But the question was up to your group to say whether you could not come up with a reach by reach number if you're going to apply it down to this level, to this mass balance model

Dr. Bonner: I think you can do. The question is the certainty to what you've done. And I think that's what you're addressing, that the certainty is quite low. And I think that's debateable. I don't know if I'd go to the extreme. And then there's other issues I think, you mentioned kriging. Well, kriging is a more statistical based estimation of the spatial distribution. You can go to a mechanistic based estimate of the distribution, which is half mechanistic, half stochastic. There's some tricks. And we can, if necessary, impose a distribution of values in terms as input into a model and move to a Monte Carlo approach via distribution of inputs that has a certain certainty which you can characterize with a probability distribution. You can generate now an output distribution and assess certainty and probabilities for the reliability of those results. I don't know if I can completely agree that it would be meaningless or useless. I think there's definite information to be gained here.

There's a gray scale on models. For example - the most empirical or simple model would be to look at suspended solids or pcb load versus flow for one year versus the next. I looked at that on the plane over here and I saw some pretty dramatic effects. If you generally look at that trend, it's in the earlier years, in the seventies, it was higher. The value of total PCB load as a function of flow was higher than in the eighties.

Dr. Abramowicz: That's in the water. The water is a much more uniform media.

Dr. Bonner: But what I'm saying Dan, is that we've got the Kray model all the way down to a simple empirical plot. They all have some element of credibility and usefulness, but you have to interpret it with the reliability constraint in hand. I think if you're going to talk one by one foot squares, you're going to see a lot of variability. But you, yourself would know where a hot spot depositional zone is.

And whether or not you could say, well the average mass concentration in that zone is this, plus or minus this. You're going to have some certainty with that.

Dr. Abramowicz: We took a hundred and fifty samples. The answer we got for the mean is 50 ppm. plus or minus 120 ppm. The error, one standard deviation unit, was twice the mean.

Dr. Nicholson: That's with a single sample, when you look at the totality...

Dr. Abramowicz: No, that was on a hundred and fifty samples.

Dr. Nicholson: No, my point is that the average for that area has a much greater accuracy - you're giving error bars on what would be the uncertainty of a single sample - uncertainty of the value added...

Dr. Abramowicz: That's the uncertainty of the mean of those hundred and fifty samples.

Dr. Nicholson: Can't be.

Dr. Abramowicz: Why can't it?

Dr. Nicholson: That isn't reasonable. It's the uncertainty of a value at a particular point.

Dr. Abramowicz: It says that if we took a sample within this plot it would likely be 66% of the time within the range of 0 to one standard deviation unit, which is about 0 to 200, 90% of the time it would be two standard deviation units, which would be like 0 to 250 ppm., 97% of the time, if I remember, it would be within zero to three standard deviation units.

Dr. Nicholson: That's right - for a single sample, for the mean the uncertainty is a least an order of magnitude less. Averaging the data from one hundred and fifty samples and calculating the average for this area..

Dr. Bonner: I guess what you're saying is if you went and took ten samples and took the mean..how would that...

Dr. Nicholson: It's going to be less.

Dr. Bonner: Of course.

Dr. Nicholoson: For particular areas...

Dr. Bonner: Another way to look at it is group it in tens and then take the mean of the means. It may not be statistically not correct, but you can see what's going to happen. That variability is going to decrease.

Mr. Tomchuk: And when you're going over three mile reaches of the river you end up compiling, taking smaller groups like that.

Dr. Bonner: I don't want to minimize this problem, I think it's hitting something. There are people now who are trying to address this. This stochasitc variability or this heterogeneity, it's not just Hudson River, it's every superfund site in the country. It's not specific to this.

Ms. Rychlenski: Okay, the next question is on fish sampling - and the question **"The approach by EPA is to use data from the past to predict future PCB concentrations in fish. It would seem that a good test of the prediction methods would be to see how closely it predicts current PCB levels in fish. There are no plans to get current PCB levels in fish. Why hasn't the committee pushed to get current fish samples for this purpose?**

Dr. Nicholson: I understand that NYS is taking samples concurrently - from fish.

Mr. Tomchuk: NY State and NOAA research trustees would like to go to locations where we are taking sediment samples for the eco risk assessment and take fish samples. Since it's not part of our program directly, we feel that we could do the eco risk assessment without that information if it came down to it. It would be useful if we had it. But, we plan to move on with our work. Obviously, they are planning to collect this data, if they get it, it will be incorporated. But, it's not critical to our work at this time.

Dr. Bopp: What do you think about the fish question in relation to the continuing release that was reported right at the beginning of this meeting?

Mr. Tomchuk: I don't know if I'm the one that should make this projection - but I think there are different things that affect fish levels and sediment concentrations that are in the area of over 50 ppm. would have effects, and do have effects of fish levels in those areas; and resident species would be affected by that. And a lot of our interpretations can be based on historical data looking at concentrations in reaches and historical fish levels.

Dr. Bopp: But historical fish levels seem to follow more the sediment decreases that I see in the recently deposited sediments year by year than what the USGS has reported in the historical water column samples.

Dr. Abramowicz: And as John has already pointed out, the congener distribution in the sediment doesn't seem to match the congener distribution in the fish.

Mr. Tomchuk: You wouldn't expect it to, either.

Dr. Abramowicz: You wouldn't expect it to look alike, but there are congeners present that aren't present in the sediments. They've been dechlorinated in the sediments, but you still find them in the fish for example. All of that - you made a statement that you thought that 50 ppm. sediments would have a significant effect on the fish levels in that region. That's an interesting question when you look at how dechlorinated those sediments are and the trends that Richard has just mentioned - that the fish seem to track much more closely the water levels over time and the recently deposited sediment levels over time. I would question that assumption.

Mr. Tomchuk: Obviously, we made the relationship to the summer water column concentrations too in Phase 1. The data on the new releases is not even validated yet. We have not sat down and interpreted how this is going to affect that. I'm not prepared to say how we're going to address that specifically to answer your question. I think that there is still a lot of information for our decision purposes that is valid, based on our model projections. With that input, without that input, a lot of our methodology is sound, either way.

Dr. Abramowicz: Let me take the upstream source out of the equation. Let's say that things haven't changed and we're on the same historical trend. All the data that we have to date seems to suggest that the PCBs in the sediments aren't having or are not coupled to or don't seem to be having an effect on the pcb levels in the fish. It seems to be much more driven by the levels in the water column, the levels in the recently deposited sediments.

So the statement that you started this discussion with - well 50 ppm. sediments would obviously have a big impact on the fish - I think needs to be verified, especially given the...

Mr. Tomchuk: Clarified, at least.

Dr. Abramowicz: No, verified. Especially given the fact that those sediments contain PCBs that data suggest don't accumulate in the fish to the same extent.

Dr. Bonner: I think it's real important to note that, I think it came about with a question a while back that was deferred until now. The congener mix at a specific location in depth or time, I don't see that there is any necessarily and direct correlation between the fish and the body burden congener mix. I would say that based on intuition, perhaps. There is an entirely different set of processes driving it in the sediment than in the fish. If you have, arbitrarily, ten centimeters of sediment there, the odds of this congener mix getting to that fish are very small. Pore diffusion is less than molecular diffusion.

Dr. Bopp: I wouldn't even characterize it in terms of odds and I wouldn't even call it very small. I would call it not possible.

Dr. Abramowicz: Also, given the fact that the congeners are not the ones that accumulate in the fish - I think that's a general assumption by people - but I think it is not substantiated by what we've seen to date and needs to be verified.

Dr. Bopp: What we've seen to date says water column and biologically active sediment layers, which is between a half and two centimeters...

Dr. Bonner: And that layer and the water column are correlated anyway..

Dr. Nicholson: But there also is a correlation, if you have an area that is for one reason or another having deposition, you're going to have higher deposition of the recent layer as well...

Dr. Bopp: If you're comparing it to an area with zero deposition.

Dr. Abramowicz: I think it points to a rather significant issue which is that we know that the exposure is driven by concentrations in fish. So, understanding the origin of the PCBs in the fish is critical in figuring out how you remediate the system. There has been a general assumption over time that if you took out the mass of submerged, high concentrations of low chlorinated PCBs, fish levels would respond.

Mr. Tomchuk: The reason I said clarifications instead of verifications is when I was referring to sediments, I was referring to sediments they were exposed to. The upper layers, the bioactive zone. I wasn't talking about sediments at depth.

Dr. Abramowicz: Most people generally assume that if you were able to take out the hot spots the fish levels would go down accordingly.

Mr. Tomchuk: I agree that there has been a large amount of talk along those lines and that's exactly what we're trying to determine with our study. We've discussed that at length within our processes.

Dr. Bonner: I don't think really there is not a direct exposure path to deep buried sediments in the fish. However, there is an indirect exposure path, and that's if you resuspend those sediments or you do river dynamics that sediment gets put back into the water column, you start with a new deck of cards. The whole process starts over again.

Mr. Tomchuk: And that's the third component of our modeling, the scourability.

Dr. Abramowicz: But again, I'll caution the need for homologue data in that model. You can't assume it's 1260.

Mr. Tomchuk: Why not?

Dr. Abramowicz: Because you won't get meaningful information.

Mr. Tomchuk: Why not..the Work Plan (pointing to the document).

Dr. Bopp: Just a final comment. I appreciate the fact that the new release data hasn't been validated or is preliminary, but I think it's really important. Especially, since I think it could really influence future fish sampling. I think that the evaluation of that should be of the utmost importance in terms of perhaps looking again at the advisability of doing continued or additional fish sampling.

Dr. Nicholson; What is the magnitude of the new contribution to what is already there?

Mr. Tomchuk: No numbers. I'm sorry, I can't discuss it at this point. If you want to ask G.E. reps, go ahead. We can't give out unvalidated data..

Ms. Rychlenski: Are there any questions from anyone here regarding the discussions we've just had on the adequacy of the data base?

Mr. Tomchuk: I have one. I'd like to ask Ron Sloan from the DEC, who has been nominated and accepted as a member of the STC, in response to John Brown's question on the congener analysis of fish..and anything else...

Ron Sloan: Actually, during this last interchange I was biting my tongue very hard. We do see definite patterns in the fish , but it is species dependent and somewhat age dependent. Also, I dare say, body portion dependent. Historically, we have been looking at what we have termed the standard fillet, and certainly the types of PCBs that would be in that fatty portion of the standard fillet may be somewhat different that occurring in the liver tissue. Certainly different from what we see in an age 1 Pumpkinseed versus age 3 and 4. In the age 1 Pumpkinseed, we're looking primarily at light chlorinated components, trichloro-biphenyls. Whereas in the 3 and 4 year old classes, it predominated and takes over showing proportionately higher levels of tetras and pentas in the 1254 range.

So that the fish are far more difficult to figure out in terms of what they are picking up from the sediment from the aspect of some of those physiological and metabolic processes that we don't have a good understanding of and that's probably one of my criticisms for the while reassessment process in that it's not looking at distribution in the sediments versus what's getting into the fish. And that was one of our hopes during this recent round of fish sampling, if NOAA is going to be able to fund it. We would be looking at some of those distributions in the sediment versus what is getting into the fish. Either at specific localities or over broader reaches of the river. We're in the process of developing a work plan now and hopefully we will have it ready for comment by early winter at least.

Dr. Abramowicz: Ron, you mentioned that when they're young you see primarily tri's and as they're older, they're more highly chlorinated. Might that be the result of excretion or, that might be the wrong word for fish, whatever passing water over gills...

Ron Sloan: Probably changes in food habits, they start becoming more carnivorous as they get older. Instead of depending on plankton for their food, they're picking up other larval fish or even adult fish of some species.

Dr. Abramowicz: And when you say trends with PCBs in the sediments. Do you mean the topmost layer or generally, or...

Ron Sloan: Generally, the top most layers. I really don't know.

Dr. Abramowicz: At least what's known to date is that looks a lot like 1242, right?

Dr. Bopp: As you go further down river you see more of what I would call physical lettering that takes it away from 1242 toward 1248 type thing - so even the earlier peaks of 1242 are mostly removing.

Ron Sloan: Just for everybody's information, we do have - during 1992 and as a follow up to the 1991 sampling that was also fairly extensive, we have basically recreated what we did in 1991, and they're under analysis now, so we should have some information on what happens to the fish in light of this new release. But, it should be several months before we have that data, and we want to be very careful when that data starts coming out before we start talking about it.

Ms. Rychlenski: Okay, we'll move on to the next section then which is on the modeling effects of remediation and the next question posed **"Does remdiation of the upper river alone make any real difference to lower river health risk?"**

Dr. Abramowicz: I guess this is something I've discussed at length at several STC meetings, so I'll talk about it a little bit. It's again, one of these general assumptions that most of the PCBs are up river in the up river sediments, you dredge out the hot spots and the fish in the lower river get better and effective remediation is accomplished. That's a nice assumption. The best data that exists to date, which I guess is a study done by Thomann that suggests that the effect is much smaller than people would generally assume.

I think that's a very important part of the question. I don't think you can just assume that remediation of the upper river will make a difference to the fish levels in the lower river. I think you have to understand that with a good model - people have criticized the Thomann model because it's not perfect, I guess that no model is. I think that most people would argue that it's the best model out there and I suspect that you could strengthen it and do an even better job. It suggests that the effect would be quite minimal.

Dr. Bonner: I guess I would second that. One of the issues that I've raised at the STC meetings is the uncertainty of loading, and there is a lot of uncertainty. In fact, it can range anywhere, the loading influence from the upper to the lower, can range anywhere from 10 to 30% to 40%. If your end point is to get edible fish I would guess that people might be disappointed if they go ahead and that's the end point, and remediate the upper Hudson and hit an expectation of immediate low body burdens in fish in the lower Hudson.

Dr. Nicholson: Some of the data that's been collected would provide a better measure of that, certainly. One needs information on other sources in the Hudson. I think at this point it's not fully clear what the relative contributions are. I think it will be better known at the completion of the study.

Dr. Bonner: I think you're right. After the study is done we will have more information. I think it would be very fruitful however, to look at the loading issue as one of the primary objectives. I think, Richard and I - this goes back at the STC and it's not a set in concrete issue. If you want to take a wastewater treatment plant and say okay there's pcb coming in at this discharge point. It's a very difficult task to go in and measure that pcb, because you're going to have a grab sample and chances are you're not going to be able to detect it because it's very small.

But a very small number times a very large flow rate can be a significant mass. The problem is what's the certainty in making that measurement when it's near or below the detection limit. You can use foam plugs - you know there's all sorts of analytical tricks - but, you're still left with a difficult task.

Dr. Bopp: You can also use congener distributions in sediment cores at distance on the river. One of the first suggestions of the importance of the NYC Metro area input came from sediment coring analysis.

Dr. Bonner: That information is useful, but you can't integrate those curves and apply it , you can get some history on a depositional area...

Dr. Bopp: But, to where you can integrate, to the accuracy you can integrate that total mass, and have an idea of relative inputs from the proportions of different congeners at specific points - you can then take that data and get an idea of proportionate inputs. That may be our best shot, given the difficulty of measuring individual plants and averaging, etc. I think one thing that should be clear is that what we're here for is to

do what we think we can do best and try to make sure that things are done in the best possible way from a variety of initial perspectives. We're sharing different approaches.

Dr. Bonner: Also the outcome and the end point objective. Fish body burdens aren't the only issue, there are in my view the issue of risk associated with redistribution of PCBs, say during a flood event. I think people might be little bit disappointed if they go and dig up every bit of sediment in the upper Hudson River and then go and immediately measure body burden in fish and wonder what happened.

Ron Sloan: Can I jump in here? The other aspect to this question is that we are hoping to rejuvenate and rehabilitate the fishery in the upper part of the river which is now totally closed to any sort of angling.

Dr. Abramowicz: Yes, that's a separate part of the question. I guess one of the questions is an issue that we have discussed at length at the STC meetings - are you going to be modelling this? I thought you weren't going to be modelling the effect of remediation of the upper river on the lower river fish. I thought that was the conclusion we arrived at.

Mr. Tomchuk: After that meeting we discussed it. We believe that the Thomann model has some application for uses in migratory species in the lower river and we're going to try to apply that, maybe with some slight modifications. Also, for resident species that the environment in the lower Hudson is not that unsimilar to the upper Hudson, and that the projections or the analyses that we are making there in resident species, water column and surficial sediment concentrations can be projected in to the lower Hudson also. So we have revised that. Area C, above the salt front.

Dr. Bonner: You're going to apply the Thomann model where?

Mr. Tomchuk: Only to area C for migratory species, doesn't apply for resident species.

Dr. Abramowicz: Okay, because that was one of the issues I expressed at length, that there was some type of tactic assumption, so now you're going to try to...

Mr. Tomchuk: We've expanded that we're taking surficial sediment cores, grab samples in the other 15 locations for the ecological risk assessment, and those numbers can be applied within this for exposure concentrations and put into the models to some extent.

Dr. Abramowicz: The load over the Troy Dam stays pretty constant through the upper Hudson, is that correct?

Dr. Bopp: From the Thompson Island Dam to Waterford.

Dr. Abramowicz: I was trying to figure out from where to where. I thought it was from Griffin Island to Waterford. So again that points to the issue of..I guess I'm coming back to a similar theme. It all seems to be driven by water levels and how will remediation effect water levels is an important part of the question.

Mr. Tomchuk: Right. In the Phase 1 Report we clearly stated that one of the major findings is that there is source upstream of the Thompson Island Pool that almost half the input into the water column comes from above the Thompson Island Pool, above Rodgers Island. Major finding. So, we've been working under that assumption. What we're trying to determine is any contribution that comes from below that or in the case of scouring, is remediation of the contaminated sediments below that necessary? Both rely on each other and both are serious questions.

Dr. Bonner: A clarification point, Doug. You said above the Thompson Island Pool is a major source. Is that a major source of PCB going over the Troy Dam or is that a major source at the source? There's a difference. Well, if you're floating down the river and you're taking measurements and all of a sudden the concentration is elevated and then gradually falls off, or does the concentration elevate and stay elevated al the way down to the Troy Dam?

Mr. Tomchuk: It stays elevated.

Dr. Abramowicz: That was the point again, it seems to be driven by the concentration in the water column, and if you took the PCBs out of the sediments would that be effective?

Dr. Bopp: Just to give proper credit, the USGS water samples, 85, 86, 87, 88 and 89 are the first things to show a significant source above Rodgers Island.

Mr. Tomchuk: Figure 3-6 in the Work Plan. I don't know if you can see that. But that's what we're talking about. I don't know if the congener mix changes along those lines, that's what we're trying to find out.

Ms. Rychlenski: The next question is on future fish concentrations and the extent the model has been specified to the STC. The specific question is **"To what extent have the specific algorithms and underlying assumptions to be used in using data from sediment samples to predict future PCB concentrations in fish been made available to the committee and reviewed by the committee?"**

Dr. Bonner: May I ask a question to the questioner? What algorithms are you referring to? That's a very broad question and there might be an algorithm...Is the questioner here?

Ms. Rychlenski: No, he's not here.

Ms. Garlanda (on behalf of the originator of the question): We also had another thing here - what conclusions and comments has the committee reached on these algorithms and assumptions?

Mr. Tomchuk: Well, that goes to the purpose of the committee that I outlined at the beginning of the meeting, and conclusions and comments. Comments we gladly accept, conclusions the committee doesn't really make a conclusion or recommendation like an oversight committee. That's not the purpose of the committee - we're just looking to get everybody's input and then we (EPA) sort through it. We're (EPA) the decision makers.

Dr. Bonner: Maybe I can field a very generic answer to a very generic and broad question. No, nothing is being hidden from the STC for many of us we seek these models, or we sit and look out the window and dream them up. So, we have all the available models, there might be some that I myself or one of us is not aware of, and if that person or anyone else knows of something like that we would be happy to look at it and try to apply it.

Dr. Abramowicz: Maybe I heard the question a little differently. I don't think it's something we have discussed in the committee.
Like what is the model we're using, I don't know that we have anyone on the committee that has that expertise. But, it isn't a topic that we've addressed in much detail. We've talked about the need for models and the need for good models, and it's been kind of at that level.

Dr. Bopp: I've reviewed the Thomann model, but that's not the specific model that will be used.

Dr. Bonner: I guess you guys are developing models and applying models. And I would hope that as they're developed and applied, calibrated, etc., we could see that and not render a decision, but offer support or comment on it.

Dr. Bopp: Constructive criticism..

Mr. Tomchuk: Right, that's what we're looking for. Actually, I was going to say I don't think we've given the committee "the model". I think we have for our sediment transport. We've specified WASP 5 and given different parameters that we're going to be calibrating with that. We haven't said what we're using for each of those parameters yet, so they can't evaluate whether they agree with those parameters. I think that that's part of the data interpretation that comes after we collect the data from Phase 2. That's what we'll be doing until August, when we come out with the report, so, that's part of the process yet.

Dr. Bonner: And WASP 5 in general is available. It's an EPA supported model that's available to the general public.

Ms. Rychlenski: Okay, we can move on to the second part of this subject, **"What is the committee's position on the adequacy of EPA's procedures on relating PCB sediment concentrations to water concentrations to fish concentrations?"**

Dr. Bopp: Depends on the model.

Dr. Abramowicz: Yeah, that's the same issue - we just haven't looked at it in real depth.

Mr. Tomchuk: I'd just like to say that there seemed to be some agreement before that when you correlate fish concentrations to surficial sediments and water column concentrations there seems to be a general agreement that that's what you're looking at; and that's what basically our correlation model is talking about. If anybody disagrees with me, jump in.

Dr. Bonner: That's sort of a cyclic argument, because water column is correlated to superficial sediments, water column is correlated to fish, therefore fish are correlated to surficial sediments. That's basically what we're saying. We're not saying cause effective, it's an empirical correlation.

Mr. Tomchuk: In our correlation model we have two linear inputs - surficial sediment and the water column. We're not necessarily saying it goes through that, we're saying here's a concentration, here's a concentration, two things into a black box and comes out fish concentration. And that's our approach right now, we're not taking that extra step necessarily.

Dr. Bonner: I think there could always be more work done. That's what every scientist does at the end of the report as future recommendations, I would like to see process level work conducted on trying to tear apart the mechanisms of how sediments interact with the water column. I like to do research in that area, so clearly I have an agenda - but other people do too.

Dr. Abramowicz: Doug, will there be the potential with this model to correlate sediment concentration in the subsurface versus the biologically active one, for lack of a better term?

Mr. Tomchuk: Actually, I'm not sure. I don't know the detail of our data base for the sediment. It might be the top level, zero to four inches, zero to six inches, whatever the samples had been in the past. It might be at depth even more than surficial sediments. How that gets factored in to the model, I don't know yet.

Dr. Bonner: We talked about a couple of things with regard to sediment processes, critical shear stress and so forth. You talked abut the use of a flume device. Where does that stand?

Mr. Tomchuk: I don't remember the actual device, if you have your final work plan, it's back there. They've worked with Clarkson University on that.

Dr. Bonner: As I remember it, the last meeting - Ed gave me a call to ask me about it. So it still may be in the air. Again, you're going to run into the same type of problems with heterogeneity, if you go out and you collect sediments, put them in an annular flume, run the device, get a critical shear stress - how representative is that going be of the river? You're going to have some problems. But, it might give you some mechanisms to understand what's going on in the river.

Mr. Tomchuk: I believe they have an annular device in here still. They're developing it with Clarkson, and you really have to talk with them to see how they came up with that.

Ms. Rychlenski: Okay, moving on to the next question - benefits of dredging, (I believe this is yours, Eleanor) the question is "**If the remedial alternative of dredging is decided upon following this current EPA study, what environmental benefits will this dredging accomplish?**"

Dr. Bonner: I think we've been talking about this a little bit. I mean that's synonymous with source reduction or source elimination in the upper Hudson. Unless you're talking about dredging the lower river. I think we've talked about it a little. There's always the question that if you dredge, what do you do with the material? Maybe that hasn't come up yet, and maybe you don't want it to. I guess as we move into the hazardous waste treatment business, I'd like to see us move away from digging the stuff up and burying it again. There are treatment technologies available for the material - whether it be in situ or dredged and controlled in a reactor. There is precedence now in Waukeegan Harbor, I believe they dredged material and subsequently are trying treatment technology.

Mr. Tomchuk: The hottest material, that's land based, I believe.

Dr. Bonner: That's right. They have some bioreactors and they've had the thermal desorption process.

Mr. Tomchuk: Sheboygan has bioreactors.

Dr. Abramowicz: I agree with Jim. It's implicit in all the things we've ben talking about to date. You need to understand well the origins of PCBs in fish if you have any hope of reducing PCB levels in fish. So you need to understand the effects of the different remediations like removing the surface layer or subsurface layer of PCBs or what volume of PCBs you remove, or what area of PCBs you remove is going to have on fish levels.
The data that I've seen to date would suggest that it may be much less than people had originally believed. As we've already mentioned here, it seems to correlate with the water levels. The Thomann report seems to support this as well.

Ms. Rychlenski: We'll go on to the last question about the Thomann Report and the utilization of the Thomann Report - "**The Thomann model done in 1989 has been extremely accurate in predicting PCB concentrations in fish. Part of that report predicted a minimal positive effect of bank to bank dredging if done before 1992, but no effect if done after that. Given the success of the fish predictions, how much reliance is being placed upon this report by the committee, and has the author of this report assisted the STC?**"

Mr. Tomchuk: To start with the end of that. Dr. Thomann was invited to participate, but he chose not to be a member of the STC. We did receive one comment on the Phase 2 work plan from him after discussing it with him at a meeting with the Hudson River Foundation - about his involvement, personally.

Dr. Bonner: I guess maybe in terms of what the model's predicting - built into the model, it's almost inherent that it has to predict that. Because, number one, there's a low emphasis put on the loading from the upper to the lower. That, coupled with the fact that there's probably a 60% volatilization built into this model too. So, eventually, we're removing through model applications, so forth, we are removing, or Thomann et al, removed the upper Hudson source from fish body burdens. Now, I guess the next $64 question - is that valid - I think he did a credible job with that model.

There's clearly uncertainty, I would ask that the STC and virtually everybody consider why don't we try to reduce the uncertainty of some of those assumptions, and I think that would be a very fruitful approach. The model is a little bit scary because there's a lot of partial differential equations, etc. The bottom line is it's just a sophisticated inventory of the materials, it's a material balance. So, in it allows for rates, transformations between one phase and another - between one type of PCB and another, etc.

There are a lot of model coefficients there, and there is uncertainty associated with those model coefficients. There are also free parameters within the models. They have to be locked in and there are assumptions in the scientific method used to lock in those coefficients. So it's sort of a gray subjective thing, but it's a reasonably credible approach for what he had available and the state of the art, state of the knowledge.

Dr. Bopp: I heard Thomann talk at the Hudson River Foundation and I think it's a reasonably credible approach. I would caution, as I'm sure he would putting to much emphasis on the conclusions. He's well aware of the uncertainties associated with the assumptions and his inputs to the model. One thing that could be pointed out is that you would get essentially the same answer from the model output simply by taking the year by year fish data generated by DEC and drawing a straight line through that data and projecting out to the future.

So in one sense we don't have... We have to be very careful in using these types of models and assigning a value to them on the basis of what in reality is a fairly simple projection from the past data on which the model was calibrated.

Dr. Bonner: Models are also useful and may be even more useful as a research tool. They can generate as many questions as they can answer. One thing that model did was sort out or set an inventory of loading. What it amounted to was an accounting to if you will, of the various sources of PCBs to the river.

Now you have target values. How certain is this particular low point? Is it off one standard deviation or two? It gives us a point to work from.

Mr. Tomchuk: I think it's important to recognize that EPA uses models, we don't use them to answer the questions and make the decision. It's a tool we use in our decision making process. Any model that we do apply, the answers we come up with - we have things to balance also.

Dr. Bonner: The abridged version of the Thomann model was published in a peer reviewed journal. So it did go through objective scientific review.

Dr. Bopp: It was extremely abridged, though.

Dr. Bonner: Yes. It went from this to this (hand motion signifying a voluminous account shrinking down to a much thinner version).

Dr. Bopp: But when you leave out how you determine the loading from the upper Hudson that's a pretty major omission.

Dr. Bonner: It's one of the problems associated with publishing in the peer review literature.

Dr. Abramowicz: Doug, maybe you answered this question - I'll admit, I don't remember. You're going to be using some modified Thomann model to try to determine fish levels forward in time. Will that have homologue specific information in it?

Mr. Tomchuk: We're looking at the Thomann model and hoping to use it. We're looking to modify it slightly. *Didn't he do some stuff with homologues? Yes, it was homologues.*

Dr. Bonner: That's a nice compromise I think.

Ms. Rychlenski: Okay, are there any questions from anyone regarding the subjects we have just discussed?

Chuck Dworkin (Counsel, NYSDEC): A couple of times Dan mentioned many people assumed certain things, i.e., removal of deep sediment PCB - this may not need to be said, but let me say it anyway just to make sure that it's clear. The NYSDEC Project Sponsor Group has never taken the position that the removal of PCBs in sediments that are not bioavailable is going to make that big a difference. Our position has always been that we are very concerned about scour events. It's making the PCBs bioavailable through a scour event, that's going to increase the concentrations in the water column and therefore in the fish. Looking at the other side of that coin, removing the PCBs before they could be made bioavailable by a scour event is what is being looked at as appropriate as a preventative measure. I just want to make it clear on the record as to where the Project Sponsor Group has always come from.

I would add to that and Richard can comment on it - that isn't it true that we have been for the past 10 years or so in an extremely quiescent period relative to historic scour events in the Hudson River. Therefore, it is reasonable to believe that there is some sort of a cycle over some period of time, and there is going to be the likelihood of major scour events in the future. Therefore, what Richard pointed out - if you draw a straight line through the DEC sampling results - isn't that in fact a reflection of the reduced number of scour events in the past. And with increased scour events in the future, that graph will be looking different in the future if there are scour events.

Dr. Abramowicz: May I respond? I feel like we're on Point/Counterpoint here. First of all, my comment about the assumption was based upon not the DEC, but the good fortune I've had in having discussions with citizens from the lower Hudson River frequently. As to the importance of scour events, I think that you need to recognize that all PCBs are not the same, particularly when you are referring to the potential for uptake into fish.

So your concern is that scour will cause greater bioavailability of these PCBs, and therefore fish levels will increase. My response to that is these are mono and di-chlorinated PCBs. Perhaps back in 1974 they were more highly chlorinated, but now they're monos and di's; and the potential for uptake into the

food web into the fish is significantly decreased. So you need to factor that in to your concern about scour events.

Mr. Tomchuk: Can I just say, taking a step along the way, because it hasn't been stated blatantly and I just want to do that - that would also apply to losses during dredging. The monos and di's would be released. Just putting it out there. Because we often talk about the effects of dredging - so you're lessening the effect because they're monos and di's.

Dr. Abramowicz: Resuspension of those submerged sediments would have lessened effect by whatever means because they are lightly chlorinated and are not accumulated in fish the way the more highly chlorinated ones are. I mean that's just common sense.

Dr. Bopp: But there still persists a recognizable difference between the congener composition in water samples and especially under high flow, and the congener compositions in the deeply dechlorinated sediments that points out the fact that we don't know where the PCBs would come from in a high flow scour event. I think your answer is perfectly appropriate for scouring out the dechlorinated areas. But we... essentially there is still the discrepancy in congener composition that needs to be addressed that the water column PCBs look a lot more like aroclor 1242 than the highly chlorinated ones and that source needs to be found and pinned down.

Dr. Abramowicz: I'm agreeing with you.

Dr. Bopp: And I'm agreeing with you. Let me say one more thing. I was at DEC and one thing I can say is that it was a very open scientific environment. Open to whatever I had to say, I never detected anything that was anything like a party line fixed in stone. In that spirit, I will say that I have looked at the data from the 80's, Chuck, and in the 80's there were two years that had five year flood events and one year that had a ten year flood event. I think we really need to look at the flow data and analyze the flow data to really address the question of the probability of scour events and I think that's something that will be done.

Dr. Bonner: Could we have that on the table? Is that an objective? Because I've asked this before - what is the objective of the remediation? In general. I think I have concluded that one objective is reducing body burden in fish in the lower Hudson. (**Mr. Tomchuk**: Not just the lower Hudson). Okay, in general. This is another one. I think you're on target here. But are we to reduce the probability of a high flow event
spreading PCBs all down the river?

Mr. Tomchuk: I think I can summarize that - again I can read the three objectives just because we have it here. (Reading from the Phase 2 Work Plan) **The choice among the remedial alternatives will require answers to several specific questions with regard to the status of PCBs in Hudson River fish populations. Including the following: 1) When will PCBs in the fish population recover to levels meeting human health and ecological risk criteria under continued no action? 2) Can remedies other than no action significantly shorten the time required to achieve acceptable risk levels, or conversely, could it make current conditions worse? 3) Are there contaminated sediments now buried and effectively sequestered from the food chain which are likely to become reactivated following a major flood resulting in an increase in contamination in the fish population?**

Dr. Abramowicz: So this is all driven by effects on fish.

Mr. Tomchuk: Right. We're looking at fish, but sediments to fish.

Dr. Abramowicz: I think another important point is I started off sometime earlier today saying that there is a lot of variability in the upper Hudson River - dechlorination is by no means uniform throughout the upper Hudson River - but, the areas that have been targeted by the DEC for dredging are the areas of highest concentration and the areas where this dechlorination process has occurred most extensively. So those are the areas in fact, that probably contribute the least to potential uptake in fish because the conversion there has been most extensive.

Dr. Bonner: The point I was trying to make - if this is in fact, try, and it looks like it is - we should devote a lot of attention to the quote, unquote scourablity of the sediment material.

Mr. Tomchuk: That's the third phase of our modeling. Scourability analysis.

Dr. Bonner: There might be some field proofing there as well. Specific mechanistic measurement of that. Process oriented measurements of that. I don't know what the approach is, but it's one thing to run a hydrodynamic model, determine velocity distribution and eyeball a critical shear.

Mr. Tomchuk: You're going to have to talk to the people that know that stuff more to make those recommendations.

Dr.. Bopp: Just one other quick point, back in the mid-80's when we looked at the suspended particles and the PCB concentrations they were a few ppm. If you took H-7 sediments, even though they were extensively dechlorinated, many of those sediments had 100 ppm level of total PCBs. If you just looked at the tri's, tetras and above, they would still be several ppm, I would guess. Just because you're resuspending extensively dechlorinated sediments, because they started out with such a high level of total PCBs, even if they only have 2% of their original tri's and above, that could still be of concern and has to be considered.

Dr. Abramowicz: The point you're making is true. It doesn't mean that there are none of the more highly chlorinated PCBs there.

Dr. Bopp: And even only a few percent may be comparable to what we saw historically back in the mid-eighties in the water column.

Dr. Abramowicz: But then you also need to recognize that that's the concentration you're removing.

Dr. Bopp: Right.

Dr. Bonner: One thing that came out in one data set that I saw was that at comparable flows in the seventies versus the eighties, total PCB was a factor or two lower.

Dr. Bopp: At comparable flows, sure. Seventy-seven through eighty-three comparable flows, yeah. Because you can get that back by looking at the concentration in particles, it's gone down. Essentially if you use a single distribution....

Dr. Bonner: Well in fact as I was flying here I calculated a half life and it was factor two within Thomann's fish...

Dr. Bopp: Three and a half years

Dr. Bonner: Something like that.

Dr. Bopp: That's the sediment number, that's the USGS water number, yes.

Dr. Abramowicz: That's the universal constant.

Mr. Putnam: May I comment once on this discharge versus scour? The risk may be decreasing with time. As it turns out, the 1987 event was the third largest since the event after the dam was removed. In fact, the 1970 event exceeded most of the events during 1983. Sediment discharge during the 1987 event exceeded most of those during the 1983 event. So I think it's a very valid test - of another scour event and what it would to PCB contents. It hardly disturbed the declining trend at all. There was no maximum, there was no real bump that accompanied it. Why? Because we're increasingly covering more and more new sediment over these areas that are, let's call, contain buried amounts of PCBs. We can not conclude that it's just going to leap into action when the next fifty year event occurs.

Dr. Bonner: I think what you're saying is that as time goes on it may take a larger and larger flow event to get the same bang for your buck effectively.

Mr. Putnam: Yes. And you can see the same thing in a given year. The first event that comes through is the most important one, regardless of what its' discharge is. You can have a whopper come in, if it's number two or number three, it isn't going to have anywhere near the effect. This can be seen in the sediment loading and the PCB data, when there's enough of it.

Dr. Bonner: It's probably a combination of things that's caused this.

Mr. Putnam: There's the first shot taken. Apparently they become somewhat armored after that. Or whether you just don't get the same effect anymore. There's more to it than just a pure discharge in a given year.

Ms. Rychlenski: Any other questions? Okay, well than I guess we'll just wrap it up. I want to thank everyone for coming out tonight. I want to thank the gentlemen from the Scientific & Technical Committee. Thank you very much, your participation is greatly appreciated.

Ms. Rychlenski closed the meeting at approximately 10:30 p.m.

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LATHAM, NY
NOVEMBER 5, 1992

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