Day One: September 11, 1996

Session Two: Questions and Answers

After each session, there was an opportunity for questions and answers and group discussions pertaining to the speakers' presentations.

Q (Dave Michaud, Wisconsin Electric Power Company): Dr. Rubinstein, on your last slide you referenced the need to perhaps establish a minimum criteria for reference site selection. What are your thoughts in terms of groups of chemicals, or on a chemical-specific basis?

Norm Rubinstein:

Without having the residue effects data, we are limited in regard to interpreting chemicals and concentrations. I am talking more in terms of determining what represents a healthy ecosystem. What are the things we are measuring that satisfy our need to insure we are maintaining environmentally consistent conditions? This involves developing much broader databases, much like is done in the Puget Sound Dredged Disposal Analysis (PSDDA) Program. From information developed by this program, we know that the benthic communities are functioning and the animals that are supposed to be there are there.

Q (Dave Michaud): So, the concept might be, for example, going to an area where you have a healthy benthic community, taking sediment samples, and analyzing them for a suite of possible contaminants.

Norm Rubinstein:

Right, and then using that as your point of comparison. This is known as a reference comparison.

Q (John Zambrano, NYS Department of Environmental Conservation): Dr. Rubinstein, in your definition of reference sediment you have three components. The first and the third could be in conflict. The first component is one that is substantially free of contaminants, and the third component reflects the site if no material had been disposed of there. What do you do when they are in conflict, and could you explain how those two components relate to the purpose of using a reference sediment?

Norm Rubinstein:

I will be the first to admit that I think the definition does need to be revisited. The intent, when we started thinking about this issue, was to recognize the fact that many of the areas that require maintenance dredging are in highly industrialized and urbanized areas where the benthic habitat has been degraded over long periods of time. We have never looked at this program as a remediation program. The intent was to insure that we do not cause further degradation. So, as impractical as the language sounds, it was in fact a realistic way of getting a handle on what was there. We do have to go back, look at this definition, and establish what we are now considering to be environmentally acceptable material.

Q (Gayle Garman, NOAA): Dr. Rubinstein, I am familiar with the PSDDA program. I think it is interesting that you hold that up as an example for us, and yet the data you showed indicated that there was a lower survival rate for the amphipods at the control or reference sites in Puget Sound than for the other harbors. So, the Puget Sound approach did not seem to be the most protective approach for the data that you showed us. I would also like you to address the fact that you are talking about a healthy benthic community, and what we are focusing on here is bioaccumulation. A healthy benthic community does not necessarily indicate whether or not there is a potential for bioaccumulation.

Norm Rubinstein:

Yes, amphipod mortality was a little higher in Puget Sound and that is exactly the point of the utility of a toxicity endpoint in a given species. Mortality at 20 or 23 percent in a test species may not be indicative of a significant impact at a population level. When you look at the sediments in Puget Sound, it is my understanding that for factors other than chemical constituents like grain

Back

size or ammonia, this is about the typical response in terms of the toxicity exposure for these animals. So, it is consistent with what I am saying, which is that percent mortality alone may not be a useful tool. Mortality has to be put into a more ecologically relevant context. And I agree with your statement that a healthy benthic community does not necessarily indicate whether or not there is a potential for bioaccumulation.

Q (Edward Zillioux, Florida Risk-Based Priority Council): Dr. Mount, I noticed that you said you limited your data collection to fish and invertebrates. I realize that this is a tremendous undertaking and there may be logistical reasons that you did not go further. But I would recommend that you consider including wading birds, because not only do they provide useful residue effect relationships that are in the literature, but they could also be a good link to higher trophic levels. We looked at this for mercury and found quite useful relationships that showed up in work conducted by Don Porcella and Jani Benoit. And we also found that the residue effect relationships derived from the field samples correlated fairly well with residue effects derived from laboratory studies with a typical white rat and mallards.

David Mount:

You raise a good point. I should emphasize that our decision to limit our database to aquatic species was not any sort of biological judgment, but purely a logistical decision. And you are absolutely right that there are lots of issues regarding bioaccumulation that extend well beyond the aquatic community.

Q (Arthur Asaki, U.S. Army Center for Health Promotion and Preventive Medicine): Dr. Mount, I congratulate your work in this area. It is something that has been needed for a long time, and I am glad somebody has done it. I looked at your data, your effect data and no-effect data, graphically represented. You had yellow squares for no-effect data and red diamonds for effect levels. There was quite a bit of overlap in those data points, which is to be expected. Later in your presentation, you showed a table for chlorpyrifos and kepone where the lowest effect level and the highest no-effect level did not overlap. Could you explain that?

David Mount:

I will try to explain what I think you are asking. The figure I presented included all data that were reported for that chemical, regardless of whether it was just one data point or several data points. The tables showed results of individual studies. If you look at the sheepshead minnow data for kepone, you will see several entries for sheepshead minnow in the table with different values for different studies. Each line of the table corresponded to a single study. The comparisons were reduced to just effect/no-effect pairs, in a sense kind of culling the data set, which probably reduced some of the variability. But all sources of variability, such as intra-species and intra-experiment, were represented in the figure.

Q (Maurice Zeeman): This question is for Dr. McCarty. There is a lot of talk going on today about dose-response relationships and tissue residue-based paradigms for toxicity assessment. I was wondering if we are going to have to start looking at some of these old chemicals in new ways, because of some new ways of looking at endpoints. Endocrine disrupters research is getting to be very interesting and it is suggesting, in essence, that dose-response relationships may not be all that important for these kinds of chemicals. When you are exposed to this trivial level of chemical may be more important than giving it to an adult later on at a much higher level or at different levels. What effects do you think that will have, if any, in terms of looking at dose-response relationships, tissue residue concentrations, and bioconcentration?

Lynn McCarty:

I do not think that the endocrine modulator people really believe they are going to modify the basic assumptions of toxicology. The description you presented is that their perception of dose response is giving a strong dose and getting a strong response. Everything is clear and understood. I think it is even more important in the sorts of things they are talking about for low level responses. We are still talking about a dose response. It is just down at low doses and at different endpoints than what we have previously looked at. I do not think it is any different at all. The standard toxicological paradigm applies. We do not have to throw out the paradigm because we do not understand the specifics of this case. I think the paradigm applies; we just have to get a greater understanding of what is going on. I think we see the echoes of this problem with PAHs. The residue for PAHs is no longer a marker of exposure for the organism, because it is so readily metabolized that what you measure today is not what the organism got a year ago. It may not be the dose that was reflective of what is causing effects to the organism today. That is the very same problem that the endocrine people are talking about, and it is simply the next level of effort. We have been very lucky in that we have many organic chemicals which are very recalcitrant to degradation, and so they can serve as their own markers of exposure. For these chemicals, the tissue residue that we see today is fairly reflective of the exposure that the organism received in the past and, therefore, is recently attributable to the effect that we see today. But we know there are situations where that does not occur. However, it still does not negate the need to know what the dose was at the time that the effect was initiated, and we have to develop procedures for estimating that. But I think there are people suggesting to simply bypass the whole scientific process and assessment of this. I think there is good science to be done and, if you throw out dose

response, then you throw out all science that is applicable to things. That certainly can be done, but I am not suggesting that.

Q (Peter Chapman): All chemicals are not the same for a variety of reasons. Do you think it is possible that we will be able to develop body burden-to-effect relationships for all chemicals within a reasonable time frame with reasonable effort? Or should we dedicate our efforts to those chemicals we think we will be able to do that for? Some of them may require so much effort and time it may not be worthwhile.

Lynn McCarty:

We are not going to look at every chemical. There is no question about that. I used the example I took from the EPA laboratory in Duluth on the modes of action. I think it is a brilliant piece of conceptual work in saying that we, as toxicologists, have been oppressed by the chemists for so long, because they are the people doing all of the chemical work and they always tell us the chemical relationships in terms of chemical descriptors. Well, I am a toxicologist and I want to have groupings according to toxicology groupings. I do not care what the structure of the chemical is. I want to know about the chemical based on effects. That is the first step in going in that direction. I think as we apply this tissue residue approach, it is going to allow us to get better estimates of those things and begin to categorize things on the basis of the effects that they have. We will be able to classify those effects into mechanistically related groups. It will also allow us the ability to look at mixtures, and hopefully that will allow us to address larger groups of things, conserve the limited resources available, and still improve our ability to do the tasks that have been set for us.

Q (Phillip Rury, Arthur D. Little, Inc.): Burt, since the tissue screening concentration (TSC) method seems to have validated the pertinence of aquatic water quality criteria to protecting aquatic biota from residue effects, how would you respond to Lynn McCarty's assertion that the superiority of chronic tests as a basis for regulatory criteria is a "myth?"

Burt Shephard:

I do not know if I would totally call it a myth. We are really measuring different sides of the same coin. If we expose the animal to the same concentration of chemical for a longer time, you begin to see chronic effects first. And if you keep exposing that hypothetical animal to that same concentration for a longer and longer time, you keep bioaccumulating more and more chemical. Eventually you will begin to run into acute toxicity, where you will reach a lethal body burden and the animal will expire. So, I think what we are really looking at is a temporal difference involving how long organisms are exposed to a given concentration. This is especially the case for chemicals that just keep on bioaccumulating the longer we expose them. You can start to see chronic effects at low tissue residues. As you gain more and more residue, you begin to get mortality.

Q (Phillip Rury): Lynn had not really elaborated on that comment, that zinger up there about it being a myth, and perhaps he would care to take this time to do so now?

Lynn McCarty:

What I was trying to caution against was the feeling that all we need is more chronic toxicity data and we will be able to solve all our problems. I definitely think that is not the case. And I think that there are better ways of obtaining that information than doing chronic toxicity testing in the way that we are doing it now. The point I am trying to make is that I think there are better ways of achieving the same end more cost-effectively using our knowledge, rather than having to create specific data points for every chemical and every situation that we want to look at. Chronic toxicity testing will not be our salvation and it is not the holy grail. It has to be taken into context.

Q (Peter Chapman): When I look at chemicals, including organics that come in via lipids and metals that are taken up via evolutionary mechanisms for uptake, I view them for our purposes here in two ways. Some chemicals that accumulate in organisms can be measured and this information may tell us something we can relate to effects. An example would be PCBs. Other chemicals, such as PAHs, accumulate in some organisms, but not in others because they are metabolized. Either we can look at the metabolites for organisms that metabolize the parent compounds or, as Jay pointed out, we can measure these chemicals in an organism that does not metabolize them. But in addition to that, within the group of chemicals that accumulate in organisms without forming metabolites, there are also chemicals that are regulated and those that are not. For instance, consider the essential metals. I think Burt made my point very well in his talk when he mentioned that copper and zinc proved to be problems for him. They proved to be problems because he was using bioconcentration factors that will not work for essential metals. These organisms must take up the essential metals to survive, and they will fight against the concentration gradient to retain them. I am wondering if we really should not look at the way the chemical acts. Maybe certain chemicals work better than others and we should focus our attention on these. Some chemicals may be a lost cause, and we should not put our effort into them for a variety of reasons. They have got complications that we should leave until later to address. Is that a reasonable way to look at this situation, or do you think, as a panel, that we should just go for it as a whole lot? What are your feelings?

Lynn McCarty:

I think that the sort of thing you want to do is what Burt has done. I only wish that I had done what he has done. I would have at least liked to have had the

opportunity, because I think it is an excellent example of how you both improve the understanding of the situation and point out the limitations. You have the opportunity to see where things work and, more importantly, to understand why they work. It also allows you to focus on the exceptions, yet they happen to be particularly problematic based on other information. So, I think the general danger in doing any of this residue-based approach is simply that we are looking at the basic paradigm of toxicology dose response and are trying to get a better understanding of the dose so we understand where our response comes from. There are situations where trying to make the methodology apply to all chemicals will make it so incredibly complicated and expensive that it is almost impossible to do. We basically do as is done in the sediment program by applying a tiered-testing approach. Essentially you focus on chemicals that can be addressed by simple assumptions with simple approaches. You only use more complicated evaluations and approaches to address the chemicals that do not fit into a simplified scheme. Understanding that they all are basically surrogates, it is simply a matter of not picking the right surrogate. This is related to Maurice's point earlier about hormone modulators. It does not negate the whole concept of the dose response in toxicology. It simply means that the dose surrogate you were using is not good enough for this particular situation.

Q (Peter Chapman): I know, but I think you are simply adding to my point that we need to be very careful and not delude people. If we do go to a tissue residue versus effects relationship, this is not going to work for everything. There are going to be some exceptions, and I think some very important exceptions. People get deluded in their thinking when they look at some of the data, because we are not always clear that we are talking about organics, lipids, and relationships that may be a little easier, than say, for the essential metals. And we have to be very clear about this. I agree with you whole-heartedly about the tiering approach, but I think my working hypothesis at this point is that we are going to reach the end of the rainbow. Eventually, we will develop a relationship for some chemicals under some circumstances, between effects and tissue body burdens. But we will not successfully do it for a number of others for a variety of reasons.

Lynn McCarty:

I appreciate that, but I just wanted to point out that I recognize that problem. I have been very careful in writing about this to try not to make it a be all and end all. The appropriate cautions or caveats are in there. Whether people actually see them, when they read it, is another story. But at least I think it is very, very important to do exactly what you said. The worst thing that could happen is to present this as the solution to everything, because it is not.

Burt Shephard:

I just might add to that a little. There is certainly no holy grail in this business. Clearly, the tissue residue approach is not going to work for everything. If we want to take the time and effort and money to quantify residues of metabolites of PAHs that are related to adverse effects, we can certainly do that. But if, on the other hand, we already have an approach in sediment quality criteria that seems to be pretty protective of our biological resources from the effects of PAHs, why do we need to look at metabolites at all? We have a method that works, so we should use it. If we need to use multiple methods for the laundry list of chemicals that we have to look at in this business, I certainly do not have a problem with using multiple methods. We should use sediment criteria where they are appropriate. If tissue residues work better for some chemicals or some situations, we should use them.

Q (John Connolly, HydroQual, Inc.): I have a comment and then a question that I think is related to the comment. The comment is that we have been using the term "ecological risk assessment" a lot, and yet everything I have heard is really referring to some sort of screening to evaluate chemicals of potential concern. I do not know if that is ecological risk assessment as much as it is just deciding whether or not there is a potential problem at a site. I think we need to make that distinction. The question is directed to Dave Mount. When we look at body burden relationships to toxicity, there have been some studies that have looked at relationships across the population, and they have shown that there is a range of body burdens. So there is a sensitive organism that responds at a low body burden, and then there is a very hardy organism that does not respond until you get to a very high body burden. That distribution of body burdens gives us information about population response that presumably would allow us to take the step beyond the screening tool to evaluate whether or not body burdens are potentially going to have a population effect. Given the way are you structuring the database, are you going to incorporate some of that kind of information that may allow us to take that step?

David Mount:

The answer, of course, is yes and no. There are several issues that you bring up. One is where there were ranges of concentrations for individual organisms within the population that were evaluated. You can consult the original citation to get more information on the ranges given in the database. There is also some variation in the literature. Studies either analyze the organisms that died, those that survived, or some combination thereof. Those notations are made in the database, so we may be able to use this in our analysis. I really believe that one of the critical uses of the database will be more as a pointer to answer specific questions and less as the endpoint in itself.

I might diverge a little bit and address the previous question. Certainly we are looking very actively at tissue residue-based approaches, but I think you have to bear in mind a couple things. One is they are most effective when you already have the tissue residue, which indicates they are directly applicable. An example would be a

bioaccumulation test that is done as part of a dredged material management monitoring program. A lot of the decisions that get made are not related, or are not dealt with, at the level of the tissue residue. You still have to bridge back to environmental concentrations that relate to those residues. And that reopens the whole bag of worms that we were trying to avoid by jumping to tissue residues. So, we cannot fool ourselves that there are no problems just because some data that seemed disparate collapse when we look at it on the basis of tissue residue. There is still the fact that those chemicals that collapse on the basis of tissue residue did not necessarily collapse on the basis of environmental exposure.

So, the tissue residue approach is not so much a direct interpretive tool, but it may teach us about groups of chemicals or making estimates of acute or chronic effect thresholds for chemicals that we have relatively little data for. It does straighten out some QSAR relationships that were formerly based on water concentrations, but were muddled by differences in uptake or something else. And to me that is the real scientific importance of the concept. I think the direct regulatory significance is almost secondary. We need to make use of all the information we have. But there are a relatively small number of instances where that information is necessarily directly relevant. For example, in a risk assessment, if you have extremely high residues, you have some information about existing risk. Almost always what is of interest in a risk assessment is future risk or risk under various management alternatives. And unless you can link those up, it will not do all the good you want it to.

Q (Hector Laguette, Brown and Root Environmental): A considerable amount of the discussion so far on tissue residues has been based on lipid-normalized values, and I wonder if any consideration has been given to the possible effect of the contaminants themselves on the lipid metabolism of the organisms prior to the moment when we do this normalization of concentrations. How may this artifact be affecting some of the approaches that we are talking about?

Burt Shephard:

On the database that we compiled, less than 25 percent of the papers that we compiled reported the lipid content of the species. So, it is really hard to make a judgement, at least on what I have looked at. I do not know how Dave feels about that.

David Mount:

Very true. Somebody mentioned this morning that if they report lipid data, there are some issues of how it was measured and how relevant that measure may be. To support your point, I think Peter Landrum presented data this morning to show exactly how lipid metabolism affected interpretation of residue-based data. In that case, lipid normalization tended to explain the variation rather than confound it. But it is a relevant point.

Lynn McCarty:

One of the things I have a great deal of concern about is actual lipid normalization of toxicity test results. It is perfectly reasonable to do it for bioaccumulation purposes, but when you normalize whole body residue levels to a standard lipid content, you are making the assumption that the whole body lipid content is reflective of the lipid content at the site of toxic action. This is not an assumption I would care to make. And we have very little information about that sort of thing. So, I think you have to be very cautious in normalizing the data when you are talking about toxicity. However, Burt has done this and I think it has worked out. But we are doing it out of ignorance, not necessarily out of knowledge. The fact that it worked is not a reflection of whether it is right or not. Maybe we were just lucky that time. Until we understand that, we will have to be very careful about normalizing toxicity data to lipid content.

Burt Shephard:

I am not sure I would agree that it was luck. There is good reason to suspect that it would work out, but it is an assumption. I will grant you that.

Q (Hector Laguette): I guess just from the point of view of ecological risk assessment, it is one more of those things that ends up being in the uncertainty analysis. It is something that should be considered at the end.

Burt Shephard:

Another problem with lipid normalization is that the lipid content of many species varies seasonally or annually, so how do you take that into account as well? The types of lipids also vary. There are a lot of assumptions. I do not know if I was lucky or if the EPA data that I based my data on was good. It might be a little bit of both. In this case, it seemed to work out, but there is certainly some concern about lipid normalization.

David Mount:

I think one of the issues that really comes to the floor when we start talking about all these other variables is that some of these principles work very well, in general, and they make good predictions of mean responses across groups of chemicals. But there are subtleties in organismal factors, physical and chemical factors, or all sorts of other things that cause individual chemicals to deviate from that behavior. In a lot of regulatory programs, that deviation is not considered acceptable. Making your best estimate and constructing a worst reasonable case are two very different tasks. There are exceptions that people consider to be quite relevant. Some of these exceptions are not accounted for in some of the very generalized models that we use in this sort of analysis. We all use log log plots, and the noise around a log log plot is important to the decision that gets made.

Burt Shephard:

The decision is also based in part on what you are going to use your data for. Dave and I both did literature reviews, but, in some cases, we had very different criteria as to how we decided a paper could or could not be used in our literature, just because we had different uses for the data.

Q (Tom O'Connor, NOAA): I did have a question for Burt Shephard that addresses this issue of how extensive the problem of coastal contamination is. Your toxic threshold concentration for cadmium, as I recall, was about 0.04 parts per million. If I convert that to a dry weight number, that is something like 0.2 parts per million. I think that number is exceeded by most of the mussels and oysters in the United States. Are we to conclude that the contamination has put all these animals at risk?

Burt Shephard:

We have run into the same problem. I will use cadmium as an example. I have spent part of the summer up in the Aleutian Islands where very few point sources occur. We have some blue mussel data from up and down the Aleutian Island chain that we have been collecting for background information for use in a risk assessment at a military base closure site in the Aleutians. As fate would have it, the typical cadmium concentrations are about half a part per million. We have a number of mussels from various sites with no known point sources over one part per million. That may be just the natural background. For some reason, the mussels seem to be doing fine there. Something I did not talk about at all, especially for metals, is naturally occurring compounds. It is very important in the risk assessment to do a proper background comparison with your site data. Background comparisons can be done several ways. You can do the mean of your sample population versus your background population. You can also do, for lack of a better term, hot spot comparison, comparing a high end mussel versus some part of your distribution. You asked if I thought the mussels are contaminated nationwide and showing effects. No, I do not. But very clearly, some other species is going to show an effect at half a part per million. I mentioned earlier that if you have species specific information, that is obviously the best way to do a risk assessment. If you have a range of data for blue mussels, and you know that half a part per million cadmium causes no adverse toxicological or ecological effect on blue mussels, then you would certainly use that in preference to a tissue screening number.

David Mount:

I think that addresses a real hazard and what I consider a real abuse of a lot of assessment tools. We had a discussion last week about one-tailed and two-tailed criteria. If you are below a one-tailed criteria, for example, you are confident that there is no effect. But there is no implication of effect if you exceed that. If you look at the derivation of the tissue screening number, they are entirely one-tailed from the way that they were developed. There is no reason to infer effect from an exceedance. In fact, if you look at the way water quality criteria were developed and the way they were written, they are really one-tailed criteria. But people consistently infer effect from an exceedance of a criterion, which is not completely wrong. You should recognize, though, that when you make that inference, you are buying into a set of assumptions that may or may not apply. So, the exceedance of one of the screening levels in a healthy organism should not come as a surprise to any of us. The question is whether or not you consider the generalizations that went into the derivation of that number.

Q (Tom O'Connor): In that light at the other extreme, Jay Field had a lot of data for PCBs in the fishes of the Hudson River. Jay, what did you have for effects of these PCBs?

Jay Field:

We were comparing tissue concentrations of PCBs to literature-derived effect concentrations for total PCBs and dioxin, using dioxin equivalent values for coplanar PCBs. We did not measure effects in Hudson River fish directly.

Q (John Haggard, General Electric Company): Jay, one of the things we are planning on the Hudson River is to investigate and remediate active water column sources of PCBs. We believe they are influencing the top surface sediments. The subject of a lot of the debate over the years on remediation has been buried sediments, which have different PCB congener signatures. In your work, Jay, with the congeners and the fish, have you been able to sort out the sources of the PCBs based on the congener distributions, or are you still working on that?

Jay Field:

No, we did not attempt to distinguish among water column, surface sediment or subsurface sediment sources. I think you need other information to do that. You have in-place sediment, recent releases of material through ground water or non-aqueous phase layers as you have at Bakers Falls. You also have sediment that is resuspended and/or transported down river in every spring flood. So separating out what is coming to the fish via the water column (either suspended or dissolved) from recent releases or sediment transport from past years is difficult to determine based on congener pattern alone. But the congener patterns in fish show a clear signal of what they are exposed to at different locations along the river gradient.

Next